Graphing Theory:
New Mathematics, Design, and the Participatory Turn

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ABSTRACT

In the 1960s mathematically inclined architects involved with academic research advocated for a shift from the points and lines of geometric shapes to points and lines of another kind – ones representing abstract objects and their relationships. A story of propinquities between architecture and mathematics, this dissertation investigates this shift through the lens of the mathematical concept that catalyzed it: the graph. I take the graph as an entity with fluctuating symbolic and operational properties and “follow” it across institutional and disciplinary boundaries to reveal historical connections hitherto unseen. I begin by locating the graph’s entry into architectural theory at transitions and transactions of mathematical and architectural modernism. Mathematical modernism promoted a structural model of disciplinary knowledge free of empirical intuitions, while boosting new mathematical varieties that represented structures and relations. Architects turned to structural abstraction in efforts to purify their inheritance of interwar Modern architecture from stylistic doctrines and empirical conventions. The graph’s amenability both to visual depiction and to mathematical analysis furnished it with a strategic position among modern mathematical varieties: graphs made structural abstraction visible and workable. By virtue of this property, graphs proliferated in architectural theory as harbingers of a veritably modern discipline founded on rationality and geared toward ensuring functional efficiency. The end of the 1960s found advocates of functionalism and rationality turning to ideals of intuition and espousing the “unpredictabilities” of participatory design. By delving into four contexts of architectural theory production in the United Kingdom, the United States, and France, I expose technical and conceptual continuities among propositions sitting on opposite sides of this “participatory turn.” I argue that the “turn” was undergirded and motivated by a new regime of seeing and subjectivity, for which the graph was an instigator, symbol, and facilitator. “Intellectual vision,” as I call this regime, assumes an abstract invariant structure that underlies concrete appearance and delimits the extents of subjective choice in a combinatorial manner. I identify forces that legitimized intellectual vision in 1960s and 1970s architectural theory and critically analyze the ways in which it was used to conceptualize creativity and open-endedness both in architectural design and in theories of participation. I close with an evocation of alternative engagements between architecture and mathematics as pathways to reclaiming shape and recouping perceptual seeing.

Thesis Supervisor: George Stiny
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To Aliki,
for letting this work grow beside her
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# Graphing Theory: New Mathematics, Design, and the Participatory Turn

Theodora Vardouli

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List of Abbreviations

ABY: Architecture-By-Yourself
ACM: Association for Computing Machinery
ARC: Applied Research of Cambridge
ARPA: Advanced Research Projects Agency
ASEE: American Society for Engineering Education
ArcMac: Architecture Machine
BASA: British Architectural Students Association
BRI: Building Research Institute
CAAD: Computer-Aided Architectural Design
Cacci: Committee on the Application of Computers in the Construction Industry
CAD: Computer-Aided Design
CES: Center for Environmental Structure
CIAM: Congrès Internationaux d'Architecture Moderne (International Congresses of Modern Architecture)
CODRA: Comité de la Recherche et du Développement en Architecture (Committee of Research and Development in Architecture)
DGRST: Délégation Générale de la Recherche Scientifique et Technique (General Delegation of Scientific and Technical Research)
DMG: Design Methods Group
DRL: Design Research Laboratory
DRS: Design Research Society
EDRA: Environmental Design Research Association
ERS: Ergonomics Research Society
GEAM: Groupe d'Études d'Architecture Mobile (Group for Studies of Mobile Architecture)
GIAP: Groupe International d’Architecture Prospective (International Group of Prospective Architecture)
GSD: Graduate School of Design
HIDECS: Hierarchical Decomposition System
LUBFS: Land Use and Built Form Studies
MIT: Massachusetts Institute of Technology
MPBW: Ministry of Public Building and Works
NBA: National Building Agency
NIMH: National Institute of Mental Health
NSF: National Science Foundation
ODG: Offices Study Group
OEEC: Organization for European Economic Cooperation
ONR: Office of Naval Research
RCA: Royal College of Art
RIBA: Royal Institute of British Architects
SIGGRAPH: Special Interest Group on Computer Graphics and Interactive Techniques
SMP: School Mathematics Project
SMSC: School Mathematics Study Group
STS: Science and Technology Studies
UMIST: University of Manchester Institute of Science and Technology
USL: Urban Systems Laboratory
Chapter 1: Introduction

A Hidden Protagonist

It was time to pass on the baton. Le Corbusier was 74, one year younger than Mies, Gropius was 78, and Wright had already been dead for two years. Age was not the sole concern. The wake of the 1960s found the sermons of interwar Modern architecture echoing loudly in the ears of architects, yet with a nagging doubt about their adequacy for grappling with a world that appeared to have changed drastically. Not only was Modern architecture potentially fraught with outdated principles,\(^1\) it had also disintegrated into a style\(^2\) of austere geometry and industrial materials that its followers mimetically emulated. An update seemed to be in order. The epigones of the Bauhaus or the recently dismantled International Congresses of Modern Architecture (CIAM)\(^3\) needed to take stock and move forward. Such was the impetus for a series of

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\(^1\) Enough distance seemed to separate the start of the 1960s from 1920s and 1930s architecture for historians to start scrutinizing the period that had unquestionably altered the architectural discipline and profession. Writing from a moment of galloping technological change, architectural historians and critics advanced a view of the Modern architecture of the interwar years as belonging to a time past. A seminal book that shaped such views was Reyner Banham’s *Theory and Design in the First Machine Age* (1960, London: The Architectural Press). Banham posited that the central issues facing the architects of the 1960s were ones of the “second machine age” of “domestic electronics and synthetic chemistry” and not of the “first machine age” of “powers from the mains and the reduction of machines to human scale” (Banham, *Theory and Design in the First Machine Age*, 10). Banham portrayed the Modern architecture of the “first machine age” as seeking to express a machine aesthetic. He dismissed this stylistic attitude for inevitably producing obsolete architecture. “The architect who proposes to run with technology,” Banham wrote, “knows now that he will be in fast company, and in order to keep up, he may have to […] discard his whole cultural load, including the professional garments by which he is recognised as an architect. If, on the other hand, he decides not to do this, he may find that a technological culture had decided to go on without him” (Ibid., 329-330).

\(^2\) In the book companion of an architectural exhibition showcasing works of Modern Architecture at the Modern Museum of Art in 1932, architectural historian Henry-Russell Hitchcock and architect Philip Johnson advocated that Modern architecture had produced a distinctive visual style. Hitchcock and Johnson dubbed this “The International Style” (Hitchcock, H.R., & Philip Johnson. 1995 [1932]. *The International Style*. New York, N.Y.: London: W.W. Norton). The idea that Modern architecture was yet another visual style, despite Modern architects’ claims that their decisions about the physical form of the building were the result of rational principles pertaining to functionality and construction, became a widely circulated argument among Modernisms’ revisionists in the 1960s.

\(^3\) The Bauhaus was a school of industrial design and applied arts that was founded by German architect Walter Gropius in Weimar and operated from 1919–1933. Assembling leading painters, architects, and designers such as Marcel Breuer, László Moholy-Nagy, Josef Albers, Mies van der Rohe, Paul Klee, and others, the Bauhaus came to stand for the avant-garde of interwar Modern art and architecture. Not a formal subject until Swiss architect Hannes Meyer assumed the school’s directorship in 1928, architecture was conceptually central to the Bauhaus as a metaphor for a unified view of the fine arts and practical skills with an eye toward social and human needs. The Congrès Internationaux d’Architecture Moderne (CIAM) was founded in the summer of 1928 in La Sarraz,
presentations and discussions held from March 20 to May 1, 1961 at the Columbia University Morningside Campus. Titled “The Four Great Makers and the Next Phase in Architecture,” the six-week event self-advertised as: “A program in celebration of the great founders of contemporary architecture. A call for the critical re-examination of the central issues facing us today. A plea for a new formulation of principles and perspectives for the future.”¹ The program featured Le Corbusier, Walter Gropius, Mies van der Rohe, and (in spirit) the late Frank Lloyd Wright—the “great makers” who, in Columbia Department of Architecture dean Charles Colbert’s words, “furnished the bedrock upon which all contemporary architecture rests”⁵—alongside “distinguished groups of international architects, educators and writers in a critical re-examination of the central issues of contemporary architecture.”⁶ Throughout the program’s eight “cycles,” celebratory convocations and gala dinners as homage to the “great makers” alternated with anxious questions such as “conformity, chaos, or continuity?,” “cul-de-sac or open end?,” and “obsolete or viable?” from their successors.

Among the participants of the fifth cycle on “The House for the Modern Family”⁷ was Serge Chermayeff, a Russian émigré architect who had joined the faculty at Yale after directing the Institute of Design in Chicago and serving as professor at the Harvard Graduate School of Design (GSD). Because of his personal work and institutional affiliations, Chermayeff was in Switzerland by initiative of French architect Le Corbusier and architectural historian and critic Sigfried Giedion. Until its dissolution in 1959 after some years of internal dispute, the CIAM held eleven conferences with a strong social and urban agenda. The international character of the CIAM and its highly influential members, made the organization one of the definitive sources for the status of the Modern Movement—as the project of socially oriented architecture was often called. For a comprehensive history of the CIAM debates see: Mumford, Eric. 2002. The CIAM Discourse on Urbanism, 1928-1960. Cambridge, Mass.; London: The MIT Press.


⁶ Ibid.

⁷ The full title of the Cycle in which Chermayeff participated was “The House for the Modern Family: Urban Towers or Suburban Idyls?” and it was chaired by Pratt Institute Department of Architecture Dean Olindo Grossi.
close proximity with the visions of Modern architecture: the Chicago Institute of Design was an outgrowth of László Moholy-Nagy’s “New Bauhaus” while the GSD operated under the staying influence of Bauhaus founder and subsequently GSD chair Walter Gropius. With a talk initially announced under the title “The Socially-Disciplined Housing of Walter Gropius,” Chermayeff chastised his co-panelists for “very eloquently propound[ing]” “aesthetic principles.”

“Architects,” he continued, “still voice piously their belief in the[se] aesthetic principles […] and then parody these principles miserably in acts utterly contemptible.” Chermayeff portrayed his contemporaries as “hiding their artistic, ostrich-like necks in the sand where decision and power lie elsewhere” and urged shifting attention to Modern architecture’s “programmatic,” as he called it, inaugural agenda—the one that it carried before being reduced to an aesthetic style. “The kind of fashionable millinery which distinguishes our present architectural era,” he declared, “surely calls again for the formulation of principles as serious as those which originally moved the men honored at these exercises.”

Pointing to CIAM’s founding agenda “to see to the resolution of architectural problems,” Chermayeff implored: “Let Us Not Make Shapes: Let Us Solve Problems.”

A history of proximity between architecture and mathematics, this dissertation is about endeavors to do precisely that. Routed within the boldly interdisciplinary and science-oriented

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9 Ibid.

10 Ibid., 265.

11 Ibid., 259.

12 Ibid., 263.

13 Mumford provides an excerpt of how CIAM secretary Sigfried Giedion described the goals of CIAM to the Dutch architect and urbanist Cornelis van Eesteren. Chermayeff’s urge “let us solve problems” referred to Giedion’s fourth goal. The four goals were: a) to formulate the contemporary program of architecture. b) to advocate the idea of modern architecture. c) to forcefully introduce this idea into technical, economic, and social circles. d) to see to the resolution of architectural problems [emphasis mine].” (Mumford, The CIAM Discourse on Urbanism, 10).
setting of the 1960s research university and identifying as “researchers” rather than “creators,” architects with mathematical proclivities enlisted modern mathematics of relations and structures to rework Modern architectural theory. These architect-researchers advocated a turn from manipulating geometric shapes with eyes and hands to interrelating structures of space and society through mathematical reason. Such a turn would purify the teaching and practice of architecture from aesthetic conventions and transform the functionalist tenets of economy, efficiency, and social betterment from an unrealized slogan to precise theory. Architects saw in modern mathematics the opportunity to produce rigorous theories of architectural design—ones that would be built on rationally verified principles instead of empirical traditions, would be applied across multiple design situations, and would help architects relate the physical form of building with its social or environmental performance.

The hypothesis driving my inquiry is that modern mathematics formed a *worksite* for transformations of Modern architectural theory in the 1960s and 1970s. I use the worksite metaphor deliberately. Architectural theories were produced through active work with specific mathematical *entities*, which I here take to mean concrete manifestations of mathematical

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14 The term “research university” broadly refers to academic institutions whose main activity was the production of knowledge through research as opposed to its dissemination through teaching.

15 When I speak of the mathematics of relations and structures in the context of modern mathematics, I refer to set theory, matrix theory, and graph theory. These mathematical subjects had ties to each other, were relatively young in the history of the field, and allowed representing and mathematically reasoning with structures of relations between entities.

16 In *Origins of Functionalist Theory* (1957, New York, N.Y.: Columbia University Press), architectural historian Edward De Zurko defines functionalism as a term used to describe any architectural theories that link “fitness to purpose” with aesthetic and moral values. He illustrated that although emphasis on utility is most commonly associated with Modern architecture, questions concerning the practical needs of a building’s occupants have been present since the beginnings of writing about architecture. Although not all Modernists were adamant about utility, standardization, and economy, the rhetoric of economy and efficiency was characteristic of the Dessau Bauhaus under the influence of Gropius and subsequently Meyer. It also pervaded the first decades of the CIAM meetings, with themes such as The Minimum Dwelling (1929) or The Functional City (1933) being oft-referenced examples of a function-oriented attitude.
concepts. These mathematical entities participated in architects’ negotiation of their discipline’s status and their roles as professionals – a negotiation that unfolded both in relationship to architects’ Modern inheritance and to horizontal pressures for the modernization of their discipline to acquire a unified, rational, and well-ordered form. I argue that the symbolic and operational meanings of the mathematical entities that architects put to work not only infiltrated theoretical statements about architecture, but also accommodated, and ultimately gave form to, shifts, breaks, and turns of various kinds in the agendas that drove architects’ mathematical engagement.

Toward the end of the 1960s, architects’ search for mathematical certitude came into question. The transcription of architectural operations in arcane mathematical terms started being viewed as deliberate obscurantism, while the impetus “to solve problems” became accused of being a Trojan horse for architectural despotism—the architects’ desire to predict and control the behavior, habits, and lifestyle of the inhabitants of the buildings and cities that they designed. Amidst rising suspicion of technocracy, the questioning of positive science, and the rise of environmental and social rights movements, many architect-researchers became self-critical of their effort to deliver the Modern image of architects as “form-givers, creators, and controllers of human environments,” as architectural historian and critic Reyner Banham put it. Instead, they repurposed the sophisticated technical repertoire and funding alliances that they had built during the 1960s to serve a new agenda: giving inhabitants the liberty to choose and change their living settings on the basis of their personal preferences, values, and intuitions. The reorientation

17 My construal of “mathematical entity” should not be confounded with the use of the term in mathematical Platonism—the metaphysical stance in the philosophy of mathematics that purports that mathematical ideas exist, that they are abstract, and that they are independent from human thought and activity. As I elaborate further down this introduction, I use “entity” here to point precisely to the concreteness and situatedness of mathematical concepts.
toward “participation,” a broad label that architect-researchers invoked to denote work in this spirit, was often explicitly counterpoised with the ethos of Modern functionalism – a term that by the end of the 1960s had acquired a pejorative meaning. I use the term “participatory turn” to point to such recalibration of values, redirection of research efforts, and change of rhetorical style among architect-researchers in the late 1960s and early 1970s.19

Because of their rejection of the architects’ authority and their focus on “user” (inhabitant) agency, theories and methods of participatory design were promoted and historically registered as breaking from the autocratic inclinations that tainted socially-oriented Modern architectural theory or the scientific tendencies of its mathematically based revisions. Discontinuity between the scientific culture of the 1960s and its negation in the 1970s through the architects’ self-annihilatory engagement with participation sits squarely in cultural, social, and political histories of the period. Yet, I argue that below this discontinuity of rhetoric lies a continuity of technique. After converting to participation, the former acolytes of rationality were fast to dispel their belief in science and reason but held on to the mathematical ideas on which they had built their theories. One would be quick to attribute such persistence to familiarity and habit only, or to trivialize it by assuming mathematics as neutral instruments that unproblematically adapt to divergent goals. One would also be remiss to not recognize that the concepts of mathematically based architectural theories were constituted on the basis of mathematical concepts. “Trees,” “patterns,” “systems,” and “networks,” to name a few examples that populated 1960s and 1970s architecture-theoretical activity, were not just metaphors that architect-researchers picked from

adjacent scientific and technological fields. This mathematics of relations and structures operated in concrete ways within architectural theories, enabling certain actions such as counting, dividing, matching, combining, etcetera that made sense both in mathematics and in architecture. It also colored architectural theories with symbolic meanings that emanated from the communities of mathematicians and other researchers that developed it and put it to use.

Attentiveness to the specific mathematics that undergirded the “rational” architectural theories of the 1960s and the “participatory” architectural theories in the 1970s opens new readings of these theories and a remapping of the relationship between them. While architects were ruminating about their relationship to modernism (both capital and lowercase), drastic reforms that shook mathematics in the late nineteenth century and intensified in the interwar years were coming into the mainstream. These reforms also adopted the qualifier “modern” to point, on the one hand, to a purification of mathematics from knowledge that relied on convention and intuition and, on the other hand, to a rebuilding of its disciplinary edifice in an organized, structured manner that interrelated disparate mathematical topics. Modern mathematics placed “abstraction,” as both a form of reason and object of study, on an intellectual pedestal and inspired structural attitudes in different fields anxious to achieve rigor. The visions

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20 A characteristic example of architectural history that takes “network” as a metaphor (in this case from media theory), is architect and author Mark Wigley’s 2001 article “Network Fever” (Grey Room 4: 82–122). Wigley provides a rich account of the proliferation of talk and images of “networks” in 1960s architecture, some of which intersects with this dissertation’s focus. However, Wigley views the network predominantly as a cultural object that came to symbolize architects’ imagination of a connectivity enabled through electronic media. Despite the insights that it offers about the emergence of a new “invisible architecture” (Ibid., 92), Wigley’s analysis of networks exclusively at the level of cultural metaphor omits the concept’s practical, operational aspects. It overlooks that architects concretely used network representations to think through architectural problems, make theory, or rethink their disciplinary identity, and that this concrete use, in turn, influenced the network’s dissemination and cultural significance, as well as the kinds of statements that architects made about it.

of mathematical modernism infiltrated Modern architecture’s quivering and drew a path toward the 1960s imperative of architectural disciplinary modernization.

My first move, therefore, is to examine how ideas, ideals, and techniques from modern mathematics participated in the momentous transformations that architectural theory underwent in the 1960s and 1970s. I bring to the fore an underdiscussed, yet formative, mathematical entity that was routinely used both in the context of rational and of participatory architectural theories: the graph. When I say graph I do not mean curves representing fluctuating quantities along two axes, such as the ones we are used to seeing in weather or stock market charts. I use the term to refer to a specific mathematical entity that is the purview of a branch of mathematics named graph theory. The graph consists of points that represent discrete elements and lines that connect the points according to rules. The lines represent relations. The graph as a whole represents a structure of relations. To give a concrete example: Imagine you have an architectural drawing of a house plan. In graph theory, the shape of the rooms does not matter, only their relative positions to each other. Now visualize the house plan drawn upon a rubber sheet. Deforming the sheet by stretching and pulling can change the shapes of the rooms, but relationships of adjacency do not alter. Finally, imagine that you put a dot at the center of each room and connect the dots of rooms that share a wall. If you erase the walls, you have a graph, where the points are also called “vertices” or “nodes” and the lines are also called “edges” or “arcs.”

I take the graph as a mobile entity that can be “followed” in its traversal of geographic, disciplinary, and institutional sites and reveal connections and reciprocities among these contexts that are perhaps invisible to conventional historical analysis. Viewed through the lens of the graph, the story of the participatory turn stops being one about a shift in the architect-researchers’ ethical, philosophical, and ideological commitments. It becomes instead a story of architectural
modernism’s profound ambivalence toward shapes and their characteristics of ambiguity and contingency. From the points and lines of geometry—traditionally the pinnacle of architectural theory—architect-researchers turned to points and lines of another kind. The points represented discrete entities of architectural interest (functional requirements, building elements, spatial units, individual inhabitants, and others), while the lines captured relations of connection or containment among these entities.

Replacing geometric representation, the graph cultivated the idea that geometric shape was undergirded by abstract structures that could be reasoned about mathematically. This was a key premise of a new visuality—a culturally constituted way of seeing, which I call “intellectual vision.” I use intellectual vision in juxtaposition to the perceptual seeing of shapes, which architects aspired to tame through structural abstraction. Graph-enabled intellectual vision appeared to resolve the architectural impetus to “solve problems” and “not make shapes,” to think and not just to see. However, I propose that the graph’s popularity among architect-researchers was precisely due to its concrete visual and manipulable appearance, the aspect of architectural representation that they enlisted it to eradicate in the first place. Furthermore, I

22 In their seminal book Objectivity (2010. New York, N.Y.: Zone Books), Lorraine Daston and Peter Galison use the term “structural objectivity” to suppress visual representation entirely and replace it with a logical structure. They view this as one stage in a trajectory of efforts to suppress the subject through particular regimes of making and reading images in scientific practice. “Intellectual vision” presents cultural affinities with “structural objectivity” but it is not the same. Intellectual vision is to be seen not as negating images and subjectivity but as instilling a new relationship between the subjective and the objective, image and reason, with the latter underpinning and constraining the possibilities of the former.


24 Historian of mathematics Alma Steingart introduces the term “mathematical manifestation” to point to the productive conflation of the concrete and the abstract in the work of mathematicians. “Manifestation” stands for aspects of mathematical research that can be displayed and presented in concrete, material form, thus leading to an embodied understanding of abstract mathematical ideas (Steingart, Alma. 2015. “Inside: Out.” Grey Room, no. 59 (Spring): 44–77). If mathematicians capitalized on the generative potentials of reclaiming concreteness in a field that operated under the reign of abstraction, architects did the inverse: they sought to instill abstraction in a field that relied on the concrete and the empirical. In architecture, therefore, the graph’s mathematical manifestation (visual presentation) eased the transition to abstraction—a realm to which architects gave intellectual priority.
argue that the graph’s oscillation between the visual and the mathematical, the concrete and the abstract, provided a way of thinking about the relationship between intuition and rationality. The graph became a model for the way architect-researchers conceptualized participation in architectural design: as a “subjective” operation on top of an unchanged abstract structure that defines the extent of choice.

The Stories of Techniques

This dissertation is not only a story about recent pasts that reverberate in contemporary architectural culture, be it through mandates of “research,” interdisciplinarity, and rigor that continue to be heard in architecture departments, or bold visions of technologically-enabled social emancipation that are frequently romanticized in architectural studios. It is also a story about our computer-saturated presents and the mathematical representations in the background of computer screens. This is because the history of architect-researchers’ engagement with mathematics that I tell here is in great part the history of the introduction of computation and computers to architecture. Mathematics, computation, and computers are neither synonymous nor interchangeable. However, as I will elaborate on in chapter 2, many of the mathematically based theories of architecture that I discuss in this dissertation were premised on an algorithmic understanding of architectural design as a step-wise process—as computation. Additionally, these

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25 Computation is a term as nebulous as it is pervasive. It has been defined in various ways in different disciplines, contexts, and historical periods. Some of these meanings are discussed in computer scientist Peter Denning’s opening statement of the 2010 Ubiquity symposium “What Is Computation?” (Denning, Peter J. 2010. “Ubiquity Symposium ‘What Is Computation?’: Opening Statement.” Ubiquity 2010). A definition that was widely accepted in the twentieth century relied on the Turing machine model, articulated in the mid-1930s by British mathematician Alan Turing. Broadly, this model implied the existence of a computational agent (human or machine) that transformed inputs based on an algorithm (a set of rules or instructions) (Ibid., 2). In Turing machines, inputs were numerical symbols and computation relied on a mathematical representation including such symbols. In recent years, there has been discussion in computer science about expanding the term “computation” to encompass any transformative process (Frailey, Dennis J. 2010. “Ubiquity Symposium ‘What Is Computation?’: Computation Is Process”).

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theories included some of the first uses of computers in architecture. Here, I view the translation of architectural concepts and operations in mathematical terms as a necessary (albeit not sufficient) step for architecture’s engagement with computation and computers in the 1960s and 1970s. I further claim that this step both shaped and can help interpret the intellectual stakes of this engagement.

In recent years, there has been growing historical interest on individuals, groups, and institutions that prepared the ground for the introduction of computation and computers in design and architecture. There appears to be a shared sense that historical inquiry into these early encounters of architecture with computation and computers provides more than a record of a particular historical period: It also helps understand and critique contemporary design culture—its persistent commitment to research, its continued interest in interdisciplinarity, and its growing engagement with computational tools and concepts. The specific actors or sites on which I focus this dissertation are not virgin territory. Christopher Alexander, the Land Use and Built Form Studies Centre in Cambridge, Yona Friedman, and MIT’s Architecture Machine Group ring bells to most architects and historians. Nor is the novelty of my endeavor to discuss harbingers of computational architecture comparatively, although this has not been done to date. Rather, it is to discuss them as chapters of the same story: a story about graph theory’s transformative trajectory through architectural theory.

Turning the graph into the vehicle by which to traverse different geographic and institutional sites, I advance a new historiographic approach to 1960s and 1970s transformations in

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26 A recent and indicative example of inquiry into the pasts of digital and computer-related approaches to architecture is the *Archaeology of the Digital* program, organized by the Canadian Center of Architecture (CCA) and comprised of three exhibitions in 2013, 2014 and 2017. The program was also accompanied by a series of digital publications on the projects featured in the exhibitions and two printed books in 2013 and 2016 probing the origins, and originators, of the “digital” in architecture. This dissertation presents different lineages than the CCA program by moving beyond the production of architectural objects and forms, or the instruments and technologies used to produce them, and looking at fusions of architectural and mathematical thought that preceded “digital architecture.”
architectural theory. This is, to a certain extent, a corrective to geographically localized and externalist approaches to the topic. Geographically localized histories stem from the implicit assumption that developments in architectural theory can be adequately explained through recourse to deliberating actors operating within social, political, and economic institutions. The methodological choice of studying a particular group or actor within the bounds of a specific institutional setting emphasizes locality and difference but misses the opportunity to discern commonalities in concepts and techniques cutting across geographic and institutional boundaries. The assumption that actors and institutions are prime historical agents perhaps also explains the tendency towards externalist (context-oriented) histories of architecture, mathematics, and computation. Externalist historical readings often view the engagement of the architect-researchers with formal and mathematical techniques as a symptom of postwar scientism and rationalism. Doing so tends to disregard subtle, yet important, nuances in the ways that different propositions construed the relationship between architecture and mathematics, while being quick to stereotype architects who engaged with mathematics as de facto. Intellectual histories that acknowledge institutional forces but avoid institutional determinism can offer

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28 A characteristic example of an institution-oriented approach to postwar architectural theory is Dutta’s edited collection *A Second Modernism*. The volume offers valuable institutional histories of architects’ engagement with logic and mathematics at MIT during the 1960s and 1970s, but only a few of the essays address intellectual history.

29 In his manuscript “What Is Intellectual History? A Frankly Partisan Introduction to a Frequently Misunderstood Field” (2013, Harvard University) historian Peter Gordon distinguishes intellectual history from the history of ideas.
more nuanced accounts of architecture-theoretical activity in 1960s and 1970s. However, such intellectual histories cannot stay blind to the entanglement of architectural concepts with mathematical concepts or focus on the development of architectural discourse as if it happened autonomously. Mathematical techniques were not passive instruments in the hands of theorists and researchers but participated, both operationally and culturally, in the making and circulation of architectural theory. Bringing the mathematical basis of architectural theories to the fore can reveal reciprocities and continuities among seemingly disparate agendas and their respective contexts.

Techniques tell stories. And sometimes they even make them.

These claims may raise eyebrows. Taking a technique as a “vehicle,” a thing that can be “followed” across contexts, is bound to stir concerns about reification and fetishism. By reification, I mean viewing the graph as an unchanging object that exists independently from the contexts in which it is invoked or the actors that use it. By fetishism, I mean making the story all about the graph; making the graph the end of the story itself. By assuming context as merely a background for the graph’s trajectory in architectural theory, such moves would arguably flatten rather than nuance the recounting of architects’ engagement with mathematics after the Second World War. Certainly, the graph as an “entity” that can be recognized in different contexts of architectural theory is a key premise of this dissertation, and there is no hiding that the graph is its persistent focus. This is a deliberate choice. As cultural anthropologist Arjun Appadurai argued in the seminal 1986 book *The Social Life of Things*, a degree of “methodological

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He explains that intellectual history considers ideas as tied to the contexts in which they are encountered, be it social contexts or contexts of discourse.

30 In her 2009 dissertation, Sachs composed a rich and expansive history of the development of the concept of “research” in U.S. architecture. Her history offers key insights into the claims and deliberations that characterized the engagement of the architectural discipline with rigorous research, but makes no reference to the technical content of particular research propositions that emerged during the period under study.
fetishism” (focusing the story on things and their trajectories rather than social actors) can shed light on the human operations that enliven them.\textsuperscript{31} Appadurai proposed that engaging in “biographies”\textsuperscript{32} of things bears analytical potential “as it is the things-in-motion that illuminate their human and social context.”\textsuperscript{33} In history of science, Lorraine Daston has spoken of “biographies of scientific objects”\textsuperscript{34} as shedding light on the formation of knowledge through attentiveness to the fluctuating cultural, material, and theoretical meanings of objects of scientific inquiry. Hans-Jörg Rheinberger has proposed “a shift of perspective from the actors’ minds and interests to their objects of manipulation and desire”\textsuperscript{35} and proposed tracking “things embodying concepts”\textsuperscript{36}—what he termed “epistemic things”—as a way to navigate sites of knowledge production. In similar spirit, by focusing on the graph as opposed to different actors or institutions where mathematically based architectural theories were produced, I intend to thread together disparate contexts and illuminate their social, intellectual, and institutional specificities.

But what about my second claim—that the graph is more than a vehicle; that it is an \textit{actor} in the current story? My goal here is not to anthropomorphize the graph by attributing to it agency or intentions. Nor do I regress to some form of technical determinism that looks at the use of the graph as automatically leading to specific kinds of theoretical or intellectual commitments regardless of context and human agendas. Yet characterizing graph theory’s trajectory through architectural theory as “transformative” admittedly implies that graphs \textit{did} have some measure of

\begin{thebibliography}{99}
\item Ibid., 13.
\item Ibid., 5.
\item Ibid., 3.
\end{thebibliography}
agency in changing architectural theory. Attributing “agency” to “non-human” entities has been a productive analytical convention for Science and Technology Studies (STS), helping to explain the interplay of human intentions with a world that resists, assists, or modifies them. Scholars in STS have emphasized the importance of the working tools of scientists in the development of ideas, knowledge, and theory, even in the most abstract of fields. In their accounts, the properties of “non-human” entities, such as instruments, lab animals, and representational devices, are viewed as enabling, constraining, shifting, and transforming intentions of human actors. By choosing these working tools as central characters in their stories and by tracing their meanings and circulation across different working sites, historians of science have offered compelling accounts of the development of ideas and theories in fields as diverse as physics, molecular genetics, and marine biology. In a similar vein, when I speak about the graph’s transformative trajectory in architectural theory, I talk about the ways that the graph’s properties inflected architect-researchers’ agendas.

Reference to the graph’s “properties” is not to be understood as allusion to some platonic, unchanging essence of the graph. Although recognizing that the graph “acted” on both a symbolic and an operational level is key for this story, its symbolic and operational properties

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37 The parlance of “non-human agency” is typical in Actor-Network Theory (ANT), an approach to the analysis of social phenomena, with origins in science studies. Key figures in ANT’s development were anthropologist and sociologist Bruno Latour, sociologists Michel Callon and John Law, and others. ANT views agency as distributed in networks of physical-material entities (humans, animals, things) and concepts. Literature on the subject is vast, and applications beyond science studies are numerous. For an introduction of the principles of ANT and demarcation from adjacent approaches see: Latour, Bruno. 1990. “On Actor Network Theory. A Few Clarifications plus More than a Few Complications.” Sociale Welt 47: 369–81.

changed depending on the aspects of empirical reality that it was called to represent and
calculate, and the actors that put it to use. As social anthropologist Tim Ingold has argued, the
properties of things or materials cannot be described independently from the activities and
contexts in which these manifest. Describing the graph’s properties means, to paraphrase Ingold,
to tell their stories.\footnote{Ingold, Tim. 2007. “Materials against Materiality.” \textit{Archaeological Dialogues} 14 (1): 1–16, 3.} Construing the graph as an entity with both abstract mathematical
properties, which define operational capacities, as well as mutating symbolic and cultural
meanings, I set out to trace its entry, dissemination, and transformative effects for 1960s and
1970s architectural theory. I ask questions such as: How did the graph enter architectural
debates? What sanctioned it as appropriate? And how did its properties change discourse about
design?

Addressing these questions requires to move between two different scales of analysis. Chapter 2 begins by tracing broad cultural and intellectual debates that qualified the graph with
the symbolic properties that compelled architects or promoted its use for certain kinds of
operations. This mode of analysis is about discipline-wide phenomena in mathematics,
architecture, and adjacent disciplines after the Second World War. For this analysis, I engage
histories of mathematics and architecture, as well as broader institutional and intellectual
histories of the postwar period. From the aerial view of entire disciplinary terrains in chapter 2, I
zoom in to the working desk of architect-researchers in chapters 3, 4, and 5. In these chapters, I
look at how the broad symbolic and operational properties of the graph were particularized in the
context of architecture-theoretical production. I discuss what architect-researchers \textit{saw} in the
graph and how working with the graph influenced their theoretical statements. I use the verb
“saw” here both figuratively, as in what kinds of agendas they saw the graph as capable of
delivering, and literally, as in what the graph’s visual, geometric representation evoked for them.
At this level of analysis, I engage original architecture theoretical texts published and disseminated in newsletters, conference proceedings, journal articles, research progress reports, and book treatises. In keeping with ethnomethodologist Michael Lynch’s discussion of visual “inscriptions” in (scientific) theoretical texts as another layer of argumentation (parallel to the written word), I pay attention to where and how graphs were drawn and what claims architectural theorists made about them. Invaluable in helping develop intuitions about how to navigate this diverse material, but also in revealing personal and experiential engagements with graph theory that did not make it into the texts, were interviews with authors of the architectural theories that I discuss here.

Ultimately, by focusing on the graph, I assemble a continuous field of discourse that has been fragmented on the basis of guiding agendas, human authors, encompassing institutions, or styles of exposition. Within this field of architecture theoretical statements produced on the basis of graph theory, I trace transformations of architectural concepts in order to tell the story of how architects graphed (architectural) theory. Aside from its historiographic or historical stakes my discussion also aims to highlight some of the problems in the assumptions, implicit motivations, and outputs of graphing architectural theory. Many of these outputs persist until the present, either as concepts assimilated in architectural language or as operational frameworks for design embedded in computer applications. By chronicling how architects graphed theory, I aim to

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41 Interviews with professors Lionel March, Philip Steadman, Marcial Echenique, Sara Ishikawa, and with Yona Friedman are presented in the dissertation’s appendix. I also conducted interviews with professors Nigel Cross and Tom Maver, which I do not present in full, as they do not directly relate to the focus of this dissertation’s chapters.
reveal the contingency, hidden stakes, and fallacies of this process, and suggest that the present could and can be otherwise.

*Graphing Theory*

Chapter 2 traces the forces that brought graph theory into architectural theory in the 1960s. As the chapter’s title suggests, the context of this entry was transitions and transactions between modernisms, of various letter cases and disciplinary origins. I say transitions because graphs entered architecture at a time when academic architects were launching a project of disciplinary modernization. A modern architectural discipline would replace the stylistic canons and operational conventions of Modern architecture with rigorous, unified, and rational “architectural theory.” I say transactions because in order to know about graphs, let alone entrust them with visions of disciplinary reform, architects had to venture into mathematics. Architects’ mathematical expeditions took place within specific sites and at a distinct historical moment, in which particular mathematical subjects, as well as mathematical cultures, prevailed. Neither the intellectual stakes of graph theory’s entry in architecture can be understood, nor practical questions about it answered, without considering how architectural agendas encountered, and were inflected by, debates about modern mathematics after the Second World War.

In the first section of chapter 2 (“Architectural Modernism”), I paint the intellectual and institutional landscape that led to the emergence of “architectural theory” in the setting of the 1960s research university. There, discontents about Modern architecture intersected with the new condition of research, with its institutional formations, technical infrastructures, and cultural connotations. I engage this context, in tandem with statements from some of the following chapters’ key figures, to describe how architects rethought and reworked the idea of architectural modernism. Preserving faith in interwar Modern architecture’s social orientation and
commitment to reason, architects declared war on its geometric conventions and sought a rational foundation for their discipline.\textsuperscript{42} Such “rational” foundation was in continuum with traditional meanings of “rational architecture”—buildings designed in a reasoned manner, with material economy or functional purposes in mind. These demands, however, were recast through the lens of a distinct genre of goal-oriented, rule-based rationality that characterized postwar intellectual life. I begin the section by dwelling on the meanings of a core concept in 1960s architecture-theoretical activity that reflects these meanings: design. I discuss how an abstract and rational construal of design was popularized within academic engineering debates and picked up from academic architects as affording opportunities for rationality. Design, as a stepwise “process” that included decisions and resulted in the description of a physical form, catalyzed, as much as it was shaped by, engagement with mathematics.

Mathematical scrutiny of design revolved around either devising “rational methods” (structures of decision sequences so as to achieve certain goals) or constructing mathematical models that would inform various decisions in the process of producing physical form. I continue the section by exposing the stakes of mathematical engagement in relation to architects’ modernizing agenda. Architects sought mathematical rigor so as to tap into basic research funding, establish academic legitimacy within the setting of the research university, and reclaim their professional role as legitimate authors of the built environment—an authority that they saw threatened by various outside experts (from sociologists to building scientists). At an operational level, mathematics would ensure that physical form derived from the consideration of social or environmental objectives, as opposed to the architects’ personal preferences or inherited

conventions. More broadly, though, mathematics would help develop a well-organized architectural theory purified of empirical baggage and founded upon confirmed knowledge. Within the intellectual ecology of the 1960s, this is what being modern meant.

In turning to mathematics, architects, more or less deliberately, participated in a model of modernization that mathematicians were pioneering and disseminating. The second section of chapter 2 (“Mathematical Modernism”) traces a model of disciplinary knowledge both modeled upon and delivered through the modern mathematics of relations and structures—the mathematics that dominated 1960s architectural theories. When architects ventured into mathematics in the search for rigor, they did not encounter an undifferentiated field. Mathematical subjects pertaining to structures and relations enjoyed a privileged status, by virtue of their accessibility, popularity, perceived operational capacities, and associated intellectual visions of modernization. I discuss how influential mathematicians in the 1950s and 1960s used the modern mathematics of relations and structures to construct an abstract, unified, and structural model of their discipline—a model that several academic disciplines emulated in search of ways to organize previously scattered empirical knowledge. I touch on these phenomena to distill a distinctive attitude that became associated with modern mathematics: one that dispelled visual intuitions and experience and recast rationality as a process of constructing, recognizing, and reasoning with structural abstractions (patterns of entities combined with relationships). Because of its alignment with mathematics’ modern self-image but also its useful applications, the mathematics of relations and structures was met with a surge of interest in university education and research.

In the 1960s, aspiring to bring secondary school students, and society at large, up to date with the mathematics that supported the scientific and technological edifice, academic mathematicians
and schoolteachers allied in drastically reforming school mathematics. These reforms, first in the U.S. and subsequently in Europe, became known as the “new mathematics” or the “new math.” The new math provided channels of dissemination for modern mathematics, thus endowing a generation of architects with the mathematical aptitudes that would define their mathematical engagements while also providing collective inculcation into a structural outlook of the world. I activate debates about the new math because they expose both the values that mathematicians saw in modern university mathematics, and their ambivalences. This helps shed light on meanings that modern math had when architects encountered it, either in university or in secondary school. I center my discussion on how U.S. and British new math treated geometry. Geometry, traditionally seen as the realm of shapes and spatial intuitions, was a bone of contention both for architectural and mathematical modernism. By translating shapes in structural terms, modern mathematics sought to purify geometry from its visual aspects and subjugate it to abstract reason. While the U.S. contingent of the new math embraced an abstract view of geometry, the British new math continued to relate geometry with the experiential world—a world, however, which it helped recast in structural terms. Through new math textbooks, British students learned to see structures and patterns in the environment. This was in keeping with the British modern mathematics’ emphasis on mathematical modeling of real-world phenomena, as opposed to the more purist and inward-looking attitude of U.S. modern mathematicians. As the subsequent chapters come to show, these local nuances echoed in British and U.S. architects’ deployment of mathematics.

Chapter 2 ends with a discussion of graph theory’s status in the postwar mathematical edifice. In the 1960s, graph theory was a new and upcoming mathematical subject, with convincing advocates and a growing number of enthusiasts in various disciplines. Because of its capacity to
represent and calculate relations and structures, graph theory capitalized on the postwar structuralist thrust and figured as agent of modernization. I show that graph theory’s geometric presentation was a key force that boosted its circulation, especially in architecture. Graphs rendered relations and structures visible and workable, thus making the unfamiliar world of pure abstraction appear more palatable to visually trained architects.

In chapters 3, 4, and 5, the graph’s visual allure figures as a key force that propelled its circulation in architecture-theoretical activity. At times, architects’ embrace of the graph was by virtue of the congeniality of pictures and, at other times, it was because the graph’s skeletal appearance evoked actual architectural objects or concepts that architects came to see as existing in the environment. The chapters also expose ways in which architects used the graph to negotiate their ambivalent relationship with shapes and appearances. Architects conceptualized abstract structures underpinning physical form and geometric shape, just like the mathematical abstraction of the graph underlay its visual presentation. Finally, the chapters show how the graph’s oscillation between an abstract and a concrete entity provided latitude for continuous theoretical transformations from demands of rationalism and control to claims of intuition and freedom. The end of the 1960s found pioneering figures of rational architectural theories renouncing the objectivist and controlling stance of their own early endeavors and espousing subjectivity and open-endedness. The pervasiveness of graph theoretic concepts and techniques in participatory architectural theories that these figures advanced reveals conceptual and technical continuities despite the blatant rhetorical discontinuity.

Chapter 3 traces the entry, establishment, and formative effects of graph theory in the work of Christopher Alexander, the Cambridge University-trained mathematician and architect whose Harvard doctoral dissertation is widely perceived as the first comprehensive rational
theory of design in the 1960s. The first part examines the building of this theory through Alexander’s doctoral studies activity. I discuss how Alexander deployed graph theory to give a mathematical expression to the views of his doctoral consultant and collaborator Serge Chermayeff. In keeping with Chermayeff’s polemic against “making shapes,” Alexander proclaimed that geometric shape was undergirded by structure, which he called “pattern” or “form.” In agreement with the ethical tenet of “solving problems,” he also argued that functional requirements of an architectural “problem” had a structure that could be revealed by examining the interrelations among functional or other requirements (whether they conflicted or not). Alexander posited that architectural design was about producing physical things whose underlying structure was congruent with the structure of the problem. He used graphs to represent requirements (points) and their interrelations (lines). He deployed graph theoretic techniques to break down (decompose) the whole set of requirements into smaller groups of requirements that the designer should handle together. The graph thus calculated and depicted the structure of the problem. It also defined a method (“program”) for creating shape: architects were to draw “diagrams” (sketches) that satisfied the smaller groups of requirements and then combine them in the order specified by the graph to produce the drawing of the whole physical thing. In the first stage of his work, Alexander used a particular type of graph (the tree) to advance an anatomical view of architecture, in which physical things were viewed as having a hierarchic structure.

In the second section of chapter 3, I discuss how Alexander—from his new position as a professor at UC Berkeley—invoked other mathematical structures (the semi-lattice, the cascade, the network) to disavow hierarchy as a symptom of dehumanizing rationality and venture toward *A Pattern Language*, an architectural treatise and method of architectural design that he vested
with humanizing, participatory, and anti-Modern proclamations. Parallel to Alexander’s shifts, I
examine the reception of his early work by architects in North American research universities,
who followed his lead and embarked on the project of “rational design methods.” Chapter 3’s
second section mainly focuses on the work of the Center for Environmental Structure (CES),
Alexander’s independent research group staffed with Berkeley graduates and faculty. Scrutiny of
CES’s activity shows how Alexander gradually started seeing structure as existing in the built
environment. I examine the gradual transition from a structural outlook of physical things and
settings toward the production of a “generative” theory of architectural design—one that could
be used by anyone to produce architecture. This theory was based on a sequenced combination of
what he called “patterns”: relations between activities and physical form. In this chapter the
geometric and visual content of the graph figures at a rhetorical level only—to signpost changes
in Alexander’s theoretical commitments from hierarchy to overlap, from rationality to humanism,
from control to participation. Yet, the evolution of his theories was undergirded by a structural
understanding of shape, a precarious sense of mappings among abstractions of dissimilar things
(physical shape and human behavior), and a combinatorial, component-based view of creative
design.

Although on a symbolic level the geometric presentation of Alexander’s graphs was key, on
an operational level Alexander’s use of graphs was purely modernist (in the mathematical sense).
He used them to represent and reason about abstract structures with no real-world referents. His
disregard for geometric presentation, which brought him into the crosshairs of mathematical
critique, was consistent with the abstract-prone view of modern mathematics in the U.S.

Moving to the British context, chapter 4 examines a different use of graphs as mathematical
models—ones that allowed mapping real-world phenomena and reasoning about them
mathematically. This chapter looks at the mathematizing work performed in the Land Use and Built Form Studies (LUBFS) Centre, the research center of Cambridge University initiated by the modernist designer Sir Leslie Martin. Under the subsequent directorship of Lionel March (Alexander’s classmate at Cambridge and also a former mathematician), LUBFS Centre researchers consciously engaged in a process of modernization: the production of a well-structured, unified discipline with a solid mathematical foundation. Instead of producing an all-encompassing theory, LUBFS Centre researchers engaged in a systematic translation of architectural operations in mathematical terms. Implicit in this view was that relationships between mathematical structures would reveal the internal unity of architecture. LUBFS Centre researchers used the vocabulary of modern mathematics to move from the “old geometry” to a “structural” understanding of the environment, as they often termed it. This structural understanding would not only interrelate scales and contexts of operation, but also enable rational thought in all steps of architectural production.

I begin chapter 4 with a discussion of LUBFS Centre’s first published book, titled *The Geometry of Environment*, which deployed the language of the new math to develop an architectural textbook. I discuss how the model-oriented view of the British new math influenced the way that LUBFS members used graphs: to represent structures that they saw as underlying the built environment. I also discuss how by mapping architectural drawings into structures, the book reinforced in architecture the kind of visuality that British new math proponents were promoting—one that saw abstract structures under visual appearances. The relationship of the abstract and the geometric appearance of the graph recurs in my discussion of architectural modeling work in the second section of chapter 4. Through three specific research contexts that respond to traditional problems of Modern architecture (namely university timetabling,
pedestrian circulation in offices, and generation of minimum standard house plans), I examine the role of the graph in mapping and matching the realm of the social and behavioral with that of physical form. I examine specifically the way that graph theory enabled new theorizations of visuality, choice, and possibility in creative design, which were crucial in enabling a “participatory turn” in architectural theory. Although the LUBFS Centre’s mathematical models fully subscribed to the tenets of Modern functionalism, claims about the graph’s representational realism and the extents of choice are key for understanding mathematically based participatory architectural theories developed elsewhere.

Chapter 5 looks into the development of such theories through the intersecting work of the prolific Hungarian-born French architect Yona Friedman and a forward-looking computer research group at the MIT School of Architecture known as the Architecture Machine (ArcMac). ArcMac makes for a compelling case both for examining graph theory’s relation with the growing field of computer aided design (CAD) in the late 1960s, and also showcasing the shifting cultural role of the computer during the same period—a shift that ArcMac largely orchestrated. I investigate these aspects by engaging British and U.S. organizations that intersected with ArcMac and which engaged with rational design methods and CAD research.

The first section of chapter 5 looks into Friedman’s theoretical activity from 1964 to 1971. Friedman undertook this activity both in contact with North American research universities and as member of avant-garde architectural groups in France. The section shows how in an iconoclastic move, Friedman disavowed the architectural imagery of gigantic inhabitable space-frames that had made him famous and sought a mathematical justification for the architectural forms that he produced. The result was Pour Une Architecture Scientifique (Toward a Scientific Architecture), a book that used graph theory to offer scientific justification for the necessity of a
participatory architectural theory and to provide a method of participatory architectural design. I discuss how the realities of architectural production and Friedman’s aesthetic predilection with skeletal space-frame structures sanctioned the graph as appropriate for his theory. I also examine the duality of the graph as both a rational and a visible entity, in allowing Friedman to reconcile scientizing and emancipatory claims. In Friedman’s work graphs figure as intuitive, accessible mathematics that democratize rational mathematical thought. Ultimately, I show how the graph amounted to a model of controlled participation—one that conceptualizes subjectivity and freedom of choice as a combinatorial operation on an underlying invariant structure.

In chapter 5’s second section I discuss how this model permeated debates of interactivity and computer aided design that ArcMac pioneered. The connective thread of the section is ArcMac’s computer implementation of Friedman’s graph-theoretic method of participatory architecture. The main focus is on work that led up to this implementation and defined its technical specificities. From this, I venture to a broader discussion of architectural research in computer aided design and interactive computer graphics at the start of the 1970s. The section highlights graph theory’s role in facilitating transactions between participatory architectural theories that proliferated during the period, computer aided design, and interactive computer graphics. Graphs, as an abstract structure underpinning geometric shape, not only underlay geometries displayed on the computer screen, but also allowed matching them with structures of human behavior or programmatic requirements. Imbued with these capacities, the graph came to be viewed as a natural model of interaction, participation, and creative design on a computer.

The chapter ends with a moment of transparency: a screenshot of the graph on one of ArcMac’s touch-sensitive IMLAC displays. There, the graph manifests as a facilitator of do-it-yourself participatory architecture. It appears as a congenial model of human-machine
interaction—purportedly visual and malleable, speaking both the objective language of mathematical structures and the subjective language of the “user.” It also figures as a natural model of architectural design—one that architects had come to construe as manipulating abstract structural representations of functional descriptions to derive the physical, geometric form.

With these assumptions, graphs were hidden behind the screens, where they continue to reside. Despite the seemingly image-rich computational world in which architects operate today, Chermayeff’s wish seems to have been fulfilled. Shape has been ostracized from creative design. Shape is mitigated by abstract, invariant, structural descriptions that purify it from ambiguity and predefine the operational freedoms of the designer. We reside within and unknowingly enact the intellectual vision of a staying architectural modernism. Critical awareness is the first step for activating alternative possibilities. I close this dissertation with an epigraph on a theory of design that deployed modern mathematics not to exile, but to embrace appearance—a theory for reclaiming shape.
Chapter 2: Architectural Modernism | Mathematical Modernism

2.1. Common Grounds

In opening the proceedings of Basic Questions of Design Theory, an NSF-funded symposium held at Columbia University in May 1974, academic engineer William Spillers cast the mathematical study of abstract concepts in engineering and architecture as foundation of a “common ground.”\(^{43}\) This common ground would reveal similarities among disciplines that Spillers viewed as held apart merely by professional circumstance. “The dissimilarities of design activities,” he posited, “become less important as design is approached on an abstract level.”\(^ {44}\) In invoking abstraction, Spillers specifically alluded to methodological and theoretical operations of concretizing, in structural mathematical models, immaterial concepts of organization and connectivity that he saw as underpinning “all of design.”\(^ {45}\) Far from launching a new intellectual agenda, Spillers was documenting the intimate intertwining between design theory and graph theory throughout the 1960s. In chronicling this trajectory, he wrote:

> The roots of this Symposium run back to the work in engineering analysis and design of the past ten to fifteen years which has been centered in graph theory. During this period […] an awareness of the importance of the ‘connectivity’ of the elements of discrete systems developed. Subsequently, graph oriented network analogies have allowed a wide variety of physical problems to be treated as generalized network problems […] connectivity is still central since questions of assembly of pieces are common to all of design.\(^ {46}\)

Illustrative of graph theory’s role in instituting both the “state-of-the-art of engineering and architectural design”\(^ {47}\) and a “common ground among the engineering and architectural


\(^{44}\) Ibid.

\(^{45}\) Ibid.

\(^{46}\) Ibid.

\(^{47}\) Ibid.
disciplines” was also the invitation of eminent graph theorist Frank Harary as keynote speaker for the event. A professor of mathematics at Michigan and Oxford, winsome speaker, and prolific writer, Harary (or Mr. Graph Theory, as he was introduced at the symposium) had gained a reputation not only for his mathematical contributions to the field of graph theory, but also for campaigning its usefulness and applicability in virtually all domains of human knowledge—sociology, anthropology, philosophy, ecology, zoology, astronomy, and architecture, to name a few examples. Harary remarked that he “was delighted to observe that in most of the papers graphs were explicitly used either in the figures or in the text.” His keynote address was a compilation of references to symposium papers by architects, urban designers, mechanical, chemical, electrical engineers, and applied mathematicians, who all made use of graph theory.

In his talk, Harary put the papers’ specific models and examples in the context of graph theory research, recast them in more general mathematical terminology, and pinpointed common concepts and operations. “When both the points and the lines of each graph used in the various papers are analyzed to see which entities and relationships they represent,” he wrote, “one can see vividly the impressive variety of natural uses of graphs as mathematical models.” Harary continued to highlight that these “natural uses” were all tied to various disciplines’ turn toward describing their subject matter in structural terms, as discrete entities (points) connected with relationships (lines). He wrote:

I say structure, deliberately, because structure is the key word in the pervasive usefulness of graph theory as a

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48 Ibid.
50 Ibid., 395.
51 Ibid., 398.
52 Ibid., 400.
mathematical model. Let me editorialize to be even more emphatic. I know of no exception to the following rule: Whenever a chemist refers to the structure or to the topology of a molecule, or an electrical engineer speaks of the topology of an electrical network, he always means its graph.\footnote{Ibid.}

Harary strove to “convince [the audience] that graph theory is a basic concept which has been found most useful in many different aspects of the theory of design.”\footnote{Ibid., 404.} Among the operations that he identified as bridging the previously disparate fields that he reviewed were “‘tearing’ of a structure”\footnote{Ibid., 400.} (decomposing something into parts), counting possible combinations of a structure,\footnote{Ibid., 401.} and finding one-to-one correspondence of structures (“isomorphism”) even when they “do not look obviously alike.”\footnote{Ibid., 397.} As Harary explained, \textit{isomorphism} (etymologically defined as equality of form) denoted a one-to-one matching between the points of two graphs that preserves their lines (relationships).

Isomorphism became the conceptual model of the “common ground” that Spillers referred to in the symposium: One-to-one mappings of “real-world” (physical) phenomena to abstract structures of entities and relationships would furnish the disciplines of engineering and architecture with a mathematical foundation. At the same time, abstract structures underpinning the concrete particulars of each field would reveal commonalities among disparate domains of human knowledge and activity enabling the streamlining of research efforts and responding to intellectual visions of unification that proliferated after the Second World War.\footnote{Visions of unification also characterized Modern architectural culture, especially of Bauhaus descent. Although the craft-based studios of the Bauhaus outputted a recognizable style of pure geometries and industrial materials, the school advanced a “scientific” construction of form through combinations of elementary formal and color entities and with an eye toward utility. Galison has concretely shown the relationship of the intellectual project of unification of the arts and rational construction of physical form that was underway at the Bauhaus with the tenets of the Vienna Circle’s logical positivism. Galison argues that both the Bauhaus and the logical positivists’ “Aufbau” (a
Spillers’ articulation of the “common ground” falls within a specific approach to disciplinary unification through “localized sets of common concepts,” to use historian of science Peter Galison’s terms. Galison has described this local and transactional view of unification as native to postwar U.S. research and distinguished it from the prewar Vienna Circle view of unification as “global metaphysical reductionism”—the reduction of all knowledge to a logical construction of elementary units. Yet the boundaries between the two views of the misty intellectual project of unification are not entirely clear-cut. Returning to the case in point here: the description of common concepts undergirding engineering and architecture in the language of mathematics (graph theory, specifically), opened possibilities for communication not only between these two disciplines but also with other disciplines that adopted mathematical vocabulary after the Second World War. Mathematics as a common language cultivated prospects not only for unification of the local kind that Galison described, but also of global communication among disparate fields. Furthermore, as I will elaborate on in this chapter’s second section, postwar modern mathematics cultivated a new disciplinary self-image as a well-ordered, cohesive, unified structure. The internal unity of mathematics would reveal the internal unity of fields that spoke the language of mathematics—even if these fields made use of different

60 Ibid.
61 The Vienna Circle, with key members Rudolf Carnap and Otto Neurath, advanced a view of knowledge as empiricist and positivist (relying on what is immediately given) in which all scientific theories were constituted through a “building up method” from “elementary bits of perception” (cited in Galison, “Aufbau/Bauhaus,” Critical Inquiry 16 (4): 732). Historian George Reisch has shown that the logical empiricism of the Vienna Circle, stripped of its progressive political imperatives, formed an inaugural program for the philosophy of postwar science (Reisch, George A. 2005. How the Cold War Transformed Philosophy of Science: To the Icy Slopes of Logic. Cambridge ; New York: Cambridge University Press). I will dwell more extensively on the relationship of unification rhetoric with “structure” after the Second World War in the second section of this chapter and in following chapters. As a provisional note, visions of unifying disparate domains of human knowledge and activity through some notion of “structure” extended to structuralism (study of relationships between objects instead of their properties), and to systems theory (mathematical study of organization in disparate entities, regardless of scale or material).
concepts. In the 1960s, architects followed their contemporaries in viewing disciplinary consolidation and interdisciplinary unification through the adoption of a mathematical vocabulary as the hallmark of a modern discipline. The qualifier “modern,” as distinct from its capital-M precedent, pertained not to a visual style but to an attitude toward a discipline’s subject matter. In architecture, being a modern discipline came to mean breaking from empirical, conventionalist, dispersed knowledge disseminated through practical instruction and developing a rational, verified, well-structured, and teachable discipline.

This chapter situates architectural theory’s adoption of graph theory in the transitions and transactions of mathematical and architectural modernism. Modernism is one of those misty and controversial keywords whose meanings and expressions have steadily invited philosophical and critical rumination. I use the term here both as descriptor of a cultural imperative to be modern, which this story’s actors shared, and as an analytical category for looking at their work. Architects and mathematicians invoked the slogan “modern” in multiple and productively ambiguous ways. The specific meanings of modernism as cultural imperative will emerge from my discussion of particular actors and contexts in the following sections and chapters. However, from these specificities surface common themes: the ambivalence toward intuition and experience, a predilection for structural abstraction, and the ideal of a responsible professional operating on organized knowledge and verified methods. This leads to modernism as a historical analytic.

In an oft-cited definition, art critic Clement Greenberg construed modernism as “the use of the characteristic methods of a discipline to criticize the discipline itself—not in order to subvert it, but to entrench it more firmly in its area of competence.”62 Architectural theories of the 1960s

complicate this view. As I will elaborate on in the following section, a reflexive critical spirit aiming to firmly entrench architecture in the production of the built environment impelled the production of architectural theory during the period. However, architects sought to develop new “characteristic methods” through deployment of concepts and techniques from another discipline: mathematics. In other words, I am referring here to an architectural modernism in which the inward look to the discipline, which Greenberg described, was contingent on the translation of architectural concepts and operations in mathematical terms—to an architectural modernism enabled by and modeled after mathematical modernism. Continuing to undergo a modernist transformation\(^6\) initiated in the late nineteenth century, mathematics had come to be viewed as a model “modern” discipline—one symbolizing a formal, abstract, and inward-looking attitude toward its subject matter. Mathematics linked this modern outlook with another keyword that would come to pervade postwar intellectual life: *structure*. Modern mathematics promoted a structural image of disciplinary knowledge, while also boosting research in mathematical subjects that broadly represented structures\(^6\)—set theory, matrix theory, topology, and graph theory. By looking at the large phenomena of architectural and mathematical modernism, this chapter illuminates the cultural imperatives that motivated the graph’s entry into architectural theory and the symbolic meanings with which it was vested upon this entry.

In the section “Modern Mathematics and Graph Theory,” I discuss what architects found when they turned to mathematics. In the section “Modern Architecture and Design Theory,” I examine disciplinary impasses and intellectual questions that motivated this turn. The section

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\(^6\) I use the term “structure” loosely here to denote any set of relationships among entities. For a precise discussion of how the concept of mathematical structure was described and defined in modern mathematics see: Corry, Leo. 1996. *Modern Algebra and the Rise of Mathematical Structures*. Birkhäuser Verlag.
also sheds light on the institutional contexts in which this turn toward mathematics occurred. Architects’ engagement with mathematics took place mainly in the setting of British and U.S. research universities. The research university was a privileged site of knowledge production after the Second World War, because of its relative fencing from the exigencies of practice and its alignment with “basic research,” a term that Vannevar Bush famously defined as “research [that] is performed without thought of practical ends […] [that] leads to new knowledge” and positioned as a “pacemaker for technological progress.”

Because of its link to academic institutions and its alignment with ideals of science, the architects’ turn toward mathematics can also be viewed through the lens of “academic drift” or “academization”: the social and cognitive process by which educational curricula of formerly practical fields of knowledge increase their reliance on the mathematical and basic sciences. In architecture, this academic drift is not to be seen only as resulting from the pressures of a technocratic and science-oriented research university. In basic research, architects found an enclave in which to address vexing intellectual questions of their Modern architectural inheritance without the pressures of professional practice.

Architects’ engagement with mathematics was contingent on the recasting of design as a step-wise goal-seeking process. This produced a new disciplinary focus for architecture, from the physical form of the final artifact (object, building, city) to the steps and decisions that lead to it. This new meaning of “design” did not grow from within architecture, but was pioneered in the

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Head of the U.S. Office of Scientific Research and Development Vannevar Bush promoted the agenda of basic research in his 1944 letter to President Roosevelt. The letter advocated for the creation of the National Science Foundation that came to be an indispensable source of funding for academic research in the decades following the Second World War (Bush, Vannevar. 1945. “Science: The Endless Frontier.” Washington, DC: Government Printing Office).

other “design discipline” that Spillers described architecture as sharing “common ground” with: engineering. Before focusing on architecture, I briefly recount a parallel tale of the academizing pressures and professional anxieties that led engineers to seek a “rational” definition of design. Engineering analysis, the field in which Spillers located graph theory’s first contacts with design, figures in this story as a precursor of design’s redefinition. This short exposition also touches upon formative events in the lineage of “design theory”—the abstract study of design among different disciplinary varieties—of which “architectural design theory” was a particular case. With these events as a backdrop, I then focus into architecture. I discuss the implications that the process-oriented view of design had for the discipline and the promises that it was seen as having for remedying architects’ own professional and intellectual anxieties, especially as they pertained to the negotiation of their Modern legacy and horizontal pressures for academization. This provides context for architects’ mathematical scrutiny of design processes or steps of these processes, which became the ground on which they engaged with structural abstraction and structural and relational mathematical ideas.

2.2. Modern Architecture and Design Theory

The Design Process

In thanking the symposium’s NSF donors Charles Babendreir and Howard Moraff for “their willingness to recognize an idea and let it run its course,” Spillers was positioning Basic Questions of Design Theory in a line of efforts to distill the common characteristics of design activities from among its many manifestations and disciplinary varieties. A landmark event for

67 Spillers, Basic Questions of Design Theory, n.p. Charles A. Babendreir was associate program director at the NSF and Howard Moraff was director of the techniques and systems program computer applications in the NSF Research Office of Computing Activities.

68 Ibid.
these pursuits was the 1962 Conference on Systematic and Intuitive Methods in Engineering, Industrial Design, Architecture and Communications (henceforth Conference on Design Methods) organized at the Imperial College London by a ten-member interdisciplinary committee. The committee included the industrial designers John Christopher Jones and Bruce Archer, who would come to play a key role in the institutionalizing design as a discipline in its own right. In the Foreword of the conference proceedings, co-organizer Peter Slann of Imperial College identified the integration of “conscious,” systematic methods of problem solving with “uninhibited creativity” as the main aim of the conference. It was with this goal, he continued, that the conference brought together previously separate groups of professionals and researchers who shared a common concern with the “methods, processes and psychology of the Design Act.” The aim was to collect existing knowledge on the subject, to map possible connections between domains sharing similar concerns, and to establish “a common language for communication between disciplines, especially those hitherto completely unrelated.” Put in Peter Slann’s terms, the objective of the Conference on Design Methods was “to test the existence and the quality of a bed rock upon which it is hoped to construct a sound system for design.”

Engineers and architects engaged with this endeavor motivated by distinct discipline-specific debates. These debates converged, however, in common demands for educational reform and professional refashioning. These demands were consistently expressed as a turn toward “fundamentals” and “theory,” the core of every field’s activity that could be made explicit, rendered rigorous, and disseminated through teaching. Opening the Conference on Design

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70 Ibid.
71 Ibid.
72 Ibid.
Methods, famed British educationist and engineer Derman Guy (D. G.) Christopherson\textsuperscript{73} likened the event to recent U.S. efforts to situate design at the core of engineering activity as a systematic yet creative process. A key event in this direction was the First Engineering Design Education Conference held in September 1960, at the Case Institute of Technology in Cleveland, Ohio, with representatives of major research universities in the U.S.\textsuperscript{74} The Case conference was an outgrowth of educational evaluation committees, initiated in the early 1950s by the American Society for Engineering Education (ASEE) and spread in universities nation-wide. Coordinated by the ASEE, which co-sponsored the Case conference, these committees aimed to differentiate the training of the engineer from the scientist and the practical “subprofessional”\textsuperscript{75} technician and to provide recommendations on new educational standards that would address “the changing demand upon the engineering profession in the United States” and the “rapidly increasing advances in technology and science.”\textsuperscript{76}

\textsuperscript{73} D. G. Christopherson. 1963. “Opening Address: Discovering Designers.” In Jones and Thornley (eds.), Conference on Design Methods. 1-10.
\textsuperscript{74} The 1960 Case Conference brought together around seventy-five people from colleges and universities throughout the United States who formulated recommendations for innovations in engineering design education. The conference participants were affiliated with leading institutions in engineering education and research, such as Stanford, the University of California Berkeley, Purdue, UCLA, MIT, Carnegie Institute of Technology, and others. (Reswick, Allan B. 1960. “Foreword.” In Proceedings of the First Conference on Engineering Design Education, I–VI. Cleveland, Ohio: Office of Special Programs, Case Institute of Technology, I).
\textsuperscript{75} The 1950s was a decade of introspection for engineering departments of American academic institutions. In May 1952 the American Society for Engineering Education (ASEE) initiated a Committee on Evaluation of Engineering education in order to bring engineering education up to speed with rapid scientific and technological change and educate leading engineers. The Committee on Evaluation was tasked with updating accreditation standards to meet the engineers’ emerging professional responsibilities, and specifically “to clarify the curriculum content that differentiates engineering education from that in science on the one hand or in subprofessional technology on the other.” (“Summary of the Report on Evaluation of Engineering Education.” 1955. Journal of Engineering Education, 25–60, Reprinted in the January 1994 issue of the Journal of Engineering Education, 74–94). The Committee of Evaluation invited all accredited colleges to conduct internal evaluations via Institutional Committees. Under the coordinating efforts of the ASEE, these reports were consolidated and redistributed among institutions, articulating and disseminating criticisms about the status of American engineering education. The need to differentiate the engineering designer from the technician was also voiced by several Case Conference participants.
The Case Conference participants positioned a redefined version of “design”\textsuperscript{77} as the basis of a “unified” engineering curriculum. In a conference address that detailed developments in design-centered unified engineering education at UCLA, Allan Rosenstein and J. Morley English relayed the conviction that by emphasizing “generality”\textsuperscript{78} of disparate engineering disciplines, beyond specific subject matters and applications, such a curriculum would provide the engineer with a “much stronger fundamental base.”\textsuperscript{79} The teaching of fundamentals,\textsuperscript{80} itself aligned with the federal government’s emphasis on basic undirected research and concerns with academic status in the competition between science and engineering cultures, was until then mainly associated with courses on “engineering analysis.” With a model course running in the Carnegie Institute of Technology under the leadership of B. R. Teare\textsuperscript{81} since 1939, engineering analysis sought to train students in the use of an “orderly method” for solving engineering problems, which was called “the engineering method.”\textsuperscript{82} The method-orientation of engineering analysis, as opposed to subject matter, allowed it to transcend academic fields.\textsuperscript{83} “Subject matter,” in this context, stood for inherited practical knowledge of the kind that one would find in engineering handbooks. At stake was “creativity,”\textsuperscript{84} a keyword that came to mean an aptitude in pulling from scientific fundamentals and past experience to address problems one had not encountered before.

\textsuperscript{77} Reswick, “Foreword,” II.
\textsuperscript{78} Rosenstein and English, “Design as a Basis for a Unified Engineering Curriculum,” 1.
\textsuperscript{79} Ibid.
\textsuperscript{83} Ibid.
\textsuperscript{84} Reswick closed the Foreword of the Case Conference proceedings by quoting MIT professor and dean emeritus of engineering Richard Soderberg on the importance of creativity in engineering design: “Engineering activity, above all, feeds on ingenuity and imagination […] No amount of formal education can take the place of inspired imagination, and our system of engineering will surely atrophy unless we attend to this issue. This is why the present wave of activity in creativity and design is of such paramount importance” (Reswick, “Foreword,” VI).
Familiarity with precedents was perceived as inadequate for tackling the kinds of unprecedented problems that characterized high-level technical positions.

Because of its abstract and methodological nature, engineering analysis was soon found to be flooded with mathematical subjects that were hard to fit in other parts of the engineering curriculum. A turn toward design—traditionally a register of the more practical and concrete aspects of engineering activity—sought to counterbalance this trend by preserving a scientific status in engineering while drawing boundaries from science proper. In order for design to serve as the basis of a reformed engineering curriculum, and the refashioned practice to which it aspired, the term needed to be intellectually and socially upgraded, and controversies pertaining to it effaced. Both in the U.K. and the U.S., the 1950s found the engineer-designer with “no social status” and lacking social recognition compared to the engineer-researcher or the engineer-businessman, recognized respectively for published work or for administrative capabilities. Within engineering design itself, there was a growing rift between “systems design” and “detail design.” “Detail designers” were portrayed as merely drawing out and embellishing the basic characteristics of new products that “systems designers” specified in an abstract, “scientific” manner. Rosenstein and English argued that despite engineers’ inclination to use “design” for component detailing, closer examination revealed the techniques used by detail component designers and complex system designers to be the same.

Aspiring to a unified and unifying definition of design, the Case organizers held workshop groups with topics such as “The Design Process,” “The Essence of Design Education,” and

87 Ibid.
“Creativity and Invention.” Among other observations, these discussions concluded that learning design was not too far apart from learning methods essential to the practice of engineering, but that it was necessary to distinguish between design “of a relatively straightforward synthesis type” and design that was “essentially creative.”

“Creative” design pointed more to the novelty of the outcome rather than the experience of the designer during designing. It alluded to obtaining unprecedented, far-reaching goals, similar to the ones sought after by the methods of engineering analysis. Following from the engineering analysis tradition, Rosenstein and English defined design as “an iterative decision making process for developing engineering systems or devices whereby resources are optimally converted into desired ends [underlined in the original].”

This definition drew heavily from the goal-seeking sciences of operations research and cybernetics that burgeoned during the war and were available to engineers in its aftermath. It also reflected a broader turn to instrumental reason in most of the social sciences, including economics and political science.

Recast as a problem-solving, goal-oriented process, design could be provided with a teachable method. The Case workshops concluded that the “design method [emphasis mine] could be represented very appropriately as the integration of the four principles of conception, analysis, synthesis and evaluation.” Far from mechanizing design, the articulation of a step-wise process was seen as supporting the ideals of “ingenuity” and “inspired imagination” by freeing designers from traditional modes of thinking and doing and allowing them to combine “creative imagination” with “comprehensive knowledge of scientific fundamentals and past

90 Reswick, “Foreword,” III.
experience in the field of engineering" in order to address the demands of new, complex technical problems. The 1960 conference debates instigated the *Fundamentals in Engineering Design* series, published by Case. The first book in the series was the highly influential *Introduction to Design* (1962), where Morris Asimow, also part of the UCLA educational reform efforts, presented a process of design including the steps of analysis, synthesis, evaluation and decision, optimization, revision, and implementation. These American developments found attentive ears in the U.K., where the 1962 Report of the Feilden Committee on mechanical engineering design advocated for radical changes in the engineering curriculum so as to remedy the detrimental lack of concern for “the creative process of deciding the best final form of the finished product.”

The process-oriented view of design advanced within engineering debates had crucial theoretical repercussions. First, this view recast the final (physical) form of an object as the result of a sequence of steps and decisions—of a process. Second, it positioned the process of design as the essence of education—an education leading to a new type of research and professional practice. Designers trained to follow processes rather than traditions would be better fit to face the needs of a rapidly racing technological society. By replacing traditional tricks of the trade with teachable rational *methods*, designers would align with two main tenets of modern ethos: their decisions would be open to rational scrutiny and they would be future-oriented rather than past-conformant.

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96 I take “process” broadly here to mean a succession of actions, a procedure. Process shifts focus from a final static outcome (for example, a description of physical form) to the sequence of events leading to this outcome.
97 “Method” is like “process” in the sense that it is also a sequence of actions, with the difference that method is goal-directed. It implies a plan and a predefined procedure aimed at attaining a particular objective.
By virtue of these credos, the development of step-wise design methods also attracted industrial designers and architects, who lent an attentive ear to developments in technology-related disciplines. Such interest was manifest in the 1962 Conference on Design Methods. This is the conference that I mentioned in the beginning of the section as an inaugural event in efforts to build a “common ground” among the design disciplines through an abstract study of the design process. In his conference address, co-organizer J. C. Jones presented a graph theory-based method for deriving performance specifications (p-specs) for a design by analyzing parameters of use, function, economy, and other factors, then devising a “solution” by systematically combining “partial solutions” for each specification, and finally “evaluating” this solution in order to identify and correct problems “before final manufacturing drawings have been started, before production begins, before the product has been sold, before it has been put to use [emphasis in the original].”

The aspiration to anticipate and fine-tune relations between the physical form of the building and its contexts of use was also embraced by architects who participated in the conference. D. G. Thornley of Manchester University, for example, presented an educational method that followed a similar analysis – synthesis - evaluation procedure, with the analysis also including the accumulation of information about the users, the environmental conditions of the site, material performance, and other parameters. One of the current story’s key actors, Christopher Alexander, also participated in the conference with a worked example.

99 Ibid., 63.
100 Ibid., 65.
101 Ibid., 69.
102 Thornley, D.G. Design Method in Architectural Education. In Jones and Thornley (eds.) Conference on Design Methods, 42.
of a design method that he developed in his doctoral dissertation at Harvard, which used graphs in order to “fit” “physical form” with its encompassing “contexts.”

Architectural Design Research

The development of methods indicating how to design to achieve certain performance objectives was only a subset of architects’ engagement with mathematics and rationality in the 1960s. Yet the view of design as a process either responding to or setting a problem was a common denominator of the endeavors that occupy the following chapters. Architects enlisted mathematics to scrutinize either the way that the decisions of the design process should be made and sequenced, or the kinds of information that designers needed at various steps of the process. Almost invariably, architects’ engagement with mathematical techniques unfolded in (or adjacent to) the institutional setting of the postwar research university under the umbrella of “research.” In her study of architectural professionalism and higher education after the Second World War, Avigail Sachs succinctly highlights the prosperous nebulosity of the term “research” and its rhetorical invocation by architects to solicit resources. Within the “military-industrial-academic complex” of the U.S., the term that senator William J. Fulbright used to

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104 In his review of the Conference on Design Methods published proceedings for the journal Ergonomics, R.S. Easterby highlighted the use of “topological nets” by Jones and Alexander as being of great promise for design methods, especially in relation to the advent of computers. He wrote: “The substance of the collection is probably contained in the three separate papers by Norris, Jones, and Alexander. Each paper is most informative on design method, and each progresses beyond a simple statement of the technique to illustrations of application. A common characteristic of these three papers is their concern with topological nets, which manifest themselves as block diagrams and linear graphs. In terms of future exploitation, it is apparent that more attention to the techniques of manipulating flow graphs and matrices will increase the power of these methods. Thus, the role of the digital computer in design becomes clearer when it is seen as a tool for manipulating parameters cast in net or matrix form, to exemplify a multidimensional design problem” (Easterby, R.S. 1964. “Review of ‘Conference on Design Methods.’” Ergonomics 7 (4): 499–500, 500).


describe the “golden triangle” of military agencies, the high technology industry, and research universities. The term “research” was equally captivating for European architects. Standing for values of impartiality, rigor, and objectivity, “research” was also formative in the British and French contexts that I examine in following chapters.

A keyword in academic debates around the creation and distribution of architectural knowledge, “research” stood broadly for any kind of systematic inquiry that produced generally applicable knowledge. Architects’ engagement with “research” signaled different kinds of interdisciplinary alliances. These ranged from cooperation with engineers on building-related technical inquiries, to collaboration with planners and social scientists in gauging public “needs” and social repercussions of architectural projects, labeled respectively as “building research” and “environmental studies” in the Anglo-American context.

The start of the 1960s found a new variety of “research” emerging in the university, which Sachs labels “architectural research.” Distinctive of this variety was a tendency for theoretical introspection of architecture as an intellectual discipline. “Architectural research” broke from other kinds of systematic inquiry that focused on gathering information or identifying standards about building. The postwar research university, permeated by the ideology of basic research—to rehash historian Roger Geiger’s famous designation—provided a protective enclave for such introspective work. Central figures of this story acknowledged such privilege celebrating the freedom of taking a “long, cool view away from the hurly-burly of day-to-day decision-making and compromise” or escaping the constraints of “the incongruous mixture of restrictive

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legislation (with its good intentions), ruthless vested interest and an ubiquitous public indifference.”111 The university not only enabled but also mandated moves of disciplinary consolidation. As historian of science Charles Rosenberg suggested, the scrutiny of disciplinary history reveals how social expectations “intrude even upon the internal texture of academic discourse.”112 With “science-orientation,” to use historian Jonathan Harwood’s term113, becoming a measure of social standing, architects turned to the mathematical sciences so as to upgrade their social and intellectual status. These efforts of disciplinary reform, fueled by academic antagonisms, reflected broader professional anxieties and public expectations that seemed pressing in the 1960s.

Calls for a turn to “research”—be it of the fact-finding or “fundamental” type—were usually accompanied by arguments about the architects’ responsibilities toward society. Information about “the development of Architecture as a Public Service and what the public expects of the architect”114 was the basis upon which the participants of the 1958 Oxford Conference—a turning point for a rigorization of architectural education in the U.K.—discussed “the standards of entry and training and […] the ultimate and desirable level of performance in the profession.”115 Along with the establishment of the British Welfare State after the end of the Second World War came a growth in public building with domestic, healthcare, educational, or governmental uses. As was the case with the British postwar urban reconstruction program of the “new towns,” public projects absorbed the principles of economy and efficiency that

113 Harwood, “Understanding Academic Drift.”
115 Ibid.
characterized Modern functionalist architectural theory and permeated the CIAM meetings. Research agencies, funded by ministries or philanthropic organizations, collected information and established standards about the production of buildings and the needs of their inhabitants. In the wake of the 1960s in the U.K. such inquiries were undertaken by the Building Research Station, the Ministry of Education, and the Nuffield Foundation.\textsuperscript{116} In these settings, as Oxford Conference chairman and key figure in this story Sir Leslie Martin remarked, architects came “at work” with many other parties: “structural engineers, mechanical engineers, production engineers, management and time study experts […] clients, sociologists, psychologists, physicists and physiologists.”\textsuperscript{117}

Similarly, in the United States, agencies operating under the 1949 Housing Act initiated wide-ranging research into technical and social aspects of housing. Although these inquiries were oriented toward standards to be deployed by private industries rather than information to aid public projects, they brought together different specialties and confronted architects with a kind of information-based collaborative work that they were not accustomed to performing. In France, large-scale social housing projects undertaken after the Second World War (the so-called “grands ensembles”) had brought architects and planners around the same table with social scientists. With the political investment in these projects running strong and the stakes of their failures becoming painfully visible as the 1960s progressed,\textsuperscript{118} an architect familiar with building technologies, able to speak the scientific language of sociologists and planners, and to articulate architectural proposals in a language that could be open to critical scrutiny by these disciplines, became a pressing mandate.

\textsuperscript{116} Ibid.
\textsuperscript{117} Ibid.
Architects’ implication in large interdisciplinary teams exacerbated a growing sense of irrelevance. Architects felt that they were merely decorating a technological environment that was being shaped in their absence. This was especially disconcerting for young students of architecture training to join the profession in the late 1950s and early 1960s. The first editorial of The Architects’ Journal section dedicated to the British Architectural Students Association (BASA)—a consortium of student representatives from twenty-five British architecture schools lobbying changes in architectural education founded in 1959—uttered confusion about their teacher’s “big talk about art and technology,” which “somehow sounds hollow when the one is so often used to embellish the other.”119 Christopher Alexander conveyed a similar sentiment in a talk that he gave along with Indian modernist designer B. V. Doshi in the 1962 International Design Conference in Aspen. Alexander and Doshi bemoaned architects for having “no real power to change the world. In spite of his boasts, his buildings do not really alter the face of the earth. For him the shaping of the environment is merely a phrase.”120 Other experts commanded the built surround:

The chemist and physicist can manufacture entirely the materials […] economic policy […] is drafted by economists and politicians … the expert [in indoor climate] is the heating engineer […] the experts in [the social repercussions of physical planning] are anthropologists and sociologists […] [for] the psychological conditions […] we will more likely consult the psychologists […] [for] the economic effects of placing a building in one place rather than another […] the economic planner is the expert […] And even in the simplest environmental problem of all, that of building dwellings, the builder rather than the architect reigns supreme […] So we are led to the strange conclusion that the architect at present plays almost no useful part in the creation of the environment.121

121 Ibid., 17–18.
It was in the university and not in the world of professional practice where architects turned to reclaim their authority in shaping the built environment. Speaking from within academia, influential architectural voices posited that a renewed architectural education founded on research-generated knowledge was the path forward. Unable to profess expertise over the technicalities of building or the socioeconomic specificities of the sites on which they operated, these voices sought to carve a new territory proper for architecture in providing integrative frameworks of information collected by outside experts. Architecture’s new disciplinary identity was to be found in organizing disparate research snippets in holistic, total views of the built and natural surround. This “total” view of architecture had precedents in the Bauhaus and had made its way to the U.S. through the leading presence of Bauhaus émigrés in architecture departments of highly visible academic institutions.\textsuperscript{122} It also built on an age-old identity of the architect as the “master-builder,” an authority over all aspects of the domestic and urban environment. In the 1960s the Modernists’ “total” design appeared as an ideology, a slogan. The “Masters”\textsuperscript{123} of Modern architecture had failed to produce a systematic body of principles that could be taught and refined through research—to produce “theory.” Instead, they had outputted unfounded doctrines.


Contrary to the common attribution of the modernist transformation of architecture’s teaching and practice in postwar America to the influence of Bauhaus émigrés—for example, Walter Gropius (head of Harvard GSD), László Moholy-Nagy (director of the New Bauhaus in Chicago and subsequently of the Illinois Institute of Technology (IIT) Institute of Design), Ludwig Mies van der Rohe (also at IIT)—James-Chakraborty argues that these changes were a result of the Great Depression and the war economy. Margret Kentgens-Craig makes a similar argument in her 1999 book The Bauhaus and America: First Contacts, 1919-1936. (Cambridge, Mass.: The MIT Press).

\textsuperscript{123} Art and architectural historian Nikolaus Pevsner used the term “masters” in his seminal Pioneers of Modern Design: From William Morris to Walter Gropius (1936, Faber) to refer to prominent architects of the Modern Movement, which he saw extending before the Bauhaus. Reyner Banham also used the term to refer to influential Modern architects, who professed to be form-givers of the human environment and society (Banham, Reyner. 1986. Age of the Masters: A Personal View of Modern Architecture. London: Thames & Hudson). As we saw in the Four Great Makers program at Columbia University, these figures were accepted to be Walter Gropius, Mies van der Rohe, Frank Lloyd Wright, and Le Corbusier. “Masters” evoked both the idea of a “genius” leaving a mark on a discipline and characterizing an entire age, and also to architecture as apprentice-able artistry.
From Capital to Lowercase - M Modern Architectural Theory

In the aftermath of Modern architecture’s “heroic” period, architects found themselves having inherited a predilection for science and rationality, alongside a socially oriented, problem-solving ethos, that seemed to have left an inheritance of questionable architectural conventions rather than actionable methods. Faced with the imperative of “research” and a fraught professional status, a younger generation of students saw the teachings of the “fathers” as perpetuating baseless dogmas. The BASA student section in The Architects Journal denounced the generation of educators as being attached to obsolete ways of thinking and doing, which left students ill-equipped for the demands of practice. “We are learning as a son learns from his father. Not to progress, but to be the same. For what we have to do, we are learning wrongly.” Sources of such conservatism were not only to be sought in Beaux-Arts education that perpetuated the ideal of the architect as a demiurge—an inspired genius. Acolytes of Modern architecture that populated academic institutions disseminated a theory that had lost its “original validity as an approach to the creation of form” and “dwindled into the decorative by its retention of absolute concepts belonging to outworn systems of thought.” This critique echoed internal confrontations among proponents of Modern architecture.

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124 The generation of architects that succeeded the instigators of the Modern movement and founders of the CIAM used the term “heroic” to denote the revolutionary spirit of Modern architectural theory in the 1920s and 1930s. The term appeared in a 1965 article by Alison and Peter Smithson (Smithson, Peter, and Alison Smithson. 1965. “The Heroic Period of Modern Architecture.” Architectural Design 25 (12)). The Smithsons were leading figures of the avant-garde group Team 10, which consolidated CIAM participants critical to the Le Corbusier or Giedion’s dogmatic approach to urbanism and which rose triumphant from CIAM’s 1959 dissolution at Otterlo. The term “heroic” was subsequently used by architects Robert Venturi and Charles Jencks in pejorative terms to point to what they viewed as an arrogant search for original, imposing, and society-altering architectures.
126 Ibid.
127 Ibid.
128 In their 1965 article on Modern architecture’s “heroic period” Alison and Peter Smithson diagnosed in the works of architects that subscribed to the Modern Movement “a loss of force, of revolutionary intent […] in which the
Anderson has argued, these internal conflicts were testaments of Modern architectural theory’s staying power after the Second World War, as opposed to its demise.\footnote{Anderson, Stanford. 1997. “The ‘New Empiricism- Bay Region Axis’: Kay Fisker and Postwar Debates on Functionalism, Regionalism, and Monumentality.” Journal of Architectural Education 50 (3): 197–207, 197.}

Among the ranks of Modern architecture’s questioners were architects who felt that the Modern project needed not be abandoned, but instead, revised.\footnote{In their edited volume Anxious Modernisms, Sarah Williams and Rejean Legault expose an “evolving and powerful modernism” that unfolded in the interim of what they refer to as an “expiring modernism” and its perceived antithesis: the postmodernism of the 1970s. (Goldhagen, Sarah Williams, and Rejean Legault, eds. 2001. Anxious Modernisms: Experimentation in Postwar Architectural Culture. Montréal, Cambridge, Mass: The MIT Press and Canadian Centre of Architecture).} These figures, many of whom occupied key positions in British and North American universities, embraced the Modern functionalist tenet of socially oriented economic and efficient building. In the university, they found space, funds, and new techniques to place the methods for achieving functionalism under rational scrutiny. Such a mandate was consistent with a culture of “re-examining fundamentals freshly and fearlessly,”\footnote{Taylor, Walter A. 1947. “The Architect Looks at Research.” Journal of Architectural Education 1: 13–24, 18.} as U.S. architectural research spokesperson Walter Taylor had put it in the late 1940s. Rational reexamination would transform architectural theory from a compendium of arbitrary doctrines to a well-structured discipline with certified knowledge. Capitalizing on the resources and “fundamentals”-seeking culture of the postwar research university, academic architects strove for another kind of lowercase-m modernism—one that pointed not to visual style, but a style of disciplinary knowledge. The role of architecture departments would shift from producing trained practitioners to producing “theory,” universally applicable principles, through “research.”\footnote{In the 1958 Oxford Conference Martin argued that it was “theory” advanced through the tool of “research” that would give teaching its “cutting edge.” (Martin, “RIBA Conference: Report by the Chairman,” 775).}

A transition from Modern architecture to lowercase-m architectural modernism is a consistent pattern behind the production of mathematically based theories of architectural design.
that I study here. The researchers that advanced such theories were mentored in some capacity by Modern designers or designers implicated in the perturbations of Modern architecture in the late 1950s: Serge Chermayeff, Sir Leslie Martin, and Yona Friedman, to mention some names that will come center stage in the following chapters. Chermayeff at Harvard and Martin in the University of Cambridge, recruited architecture students with advanced mathematical understanding to elucidate ideas of total design or functionalism. These students, in turn, became leading pioneers of new mathematically founded theories of architectural design, which they initially related to Modern quests for efficiency, economy, and rationality. More critical of efficiency and economy, which he sought to redefine from the inhabitant’s point of view rather than the designer’s, Friedman worked directly with mathematicians. His mathematical theory of architecture influenced MIT researchers investigating applications of computers to architecture. Invariably, the reworking of architectural theory was performed either by researchers who had formal university training in mathematics (as was the case with Christopher Alexander and Lionel March) or had a working knowledge of mathematics sophisticated enough to communicate with academic mathematicians.

The architect’s proximity with the discipline of mathematics is a crucial aspect of the current story. Either as direct or indirect participants of 1960s mathematical culture, architectural theorists amalgamated intellectual visions outside architecture with internal disciplinary quests. Mathematics, carrying connotations of rigor, reason, and exactitude, provided architectural theorists a standpoint from which to critique the “absurd,” unfounded status of architectural theory—a critique that resonated with the discipline’s internal sentiment. It also offered them techniques for remedying the situation. At times, mathematical techniques were deployed to

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provide a-contextual and universal theories of design that made no reference to the discipline’s history. Other times, mathematical tools were viewed as an opportunity to look at the edifice of architectural tradition in a new light and amend, rather than fully replace it. Sometimes, architectural theories departed from specific practical questions (such as the location of a highway interchange or the layout of mass customized building) and extrapolated a more general framework applicable in all of design. In other cases, theories’ tackled open intellectual questions in the discipline of architecture (such as the relationship of function to physical form or the possibility to anticipate the results of decisions made on the drawing board) or scrutinized aspects of the design process. The relationship with practical problems\textsuperscript{134} influenced the style of exposition, which also depended on the priorities of the funding source or the context in which the theory was presented. Books and journal articles were more amenable to “pure” theory than progress reports, which tended to emphasize potential applications. Proposals for science foundations and research councils, in turn, permitted abstraction and generality more than governmental building or housing department commissioned reports. These differences will color the chapters that follow, ultimately pointing to Gray’s proviso that lowercase-m modernism holds a complicated relationship with the day-to-day world.

Along with the language of mathematics, architectural theorists subscribed to a different style of advancing disciplinary knowledge and resolving intellectual problems that tainted architecture—a style of sober research. Alexander’s polemic in his early article “The Revolution Finished Twenty Years Ago” is revealing:

\textsuperscript{134} Generally, the architectural theories that I study in this dissertation spanned the “free” (with unknown application) and “objective” (of practical importance) spectrum that Walter A. Taylor, director of the American Institute of Architects Department of Education and Research, carved in 1947 as variants of “fundamental” research in architecture (as opposed to basic). Taylor saw in architecture possibilities for reconciling “pure” and “applied” science. He wrote: “The practical but humanitarian attitude of the architect will help to break down the artificial barrier between so-called pure and applied science) emphasizing the dual role of the scientist as discoverer and interpreter” (Taylor, “The Architect Looks at Research,” 16).
Certainly, it’s always fun to play at soldiers. But there’s no time for fun when there is serious work to do. The work of this generation, and of succeeding generations will be the work of refinement. Work that must proceed slowly, doggedly, methodically. There are no longer battles to be fought.135

Alexander’s diatribe encapsulated a broader sentiment about the agents of progress in architecture—no longer the solitary geniuses of heroic Modern architecture but teams of trained experts. The 1960s found this belief being also espoused by Modern “Masters” who themselves turned to collaborative teamwork.136 Proponents of such an attitude tasked architectural education with instilling a new attitude and working methods in the next generation of architects. As Alexander provocatively argued in the same article, “We cannot train for genius. But we can achieve a universal competence—a climate where there is no doubt about which way to do things.”137


justifiably rings bells of conformity. Yet, in the 1960s a universal and rational theory of architectural design carried positive connotations of democratizing design competence. As Friedman later put it, an explicit “scientific” theory of design was necessary in order for architecture to shift from a “prenticeable” to a “teachable” discipline.\footnote{Friedman, Yona. 1975. Toward a Scientific Architecture. Translated by Cynthia Lang. Cambridge, Mass.: The MIT Press, 12.} Friedman used these terms to denote, on the one hand, a situation where architecture could only be taught to an elite population granted the privilege of direct contact with a “master” and, on the other hand, one in which it can be disseminated through textbooks that alluded to the universal human trait of rational thought.

In the late 1960s, the development of “universal competence” through “teachable” architectural processes cultivated the prospect and responded to the mandate of involving wider sections of the population in the design of physical form. The declaration of physical form as deriving from a \textit{process} that included deliberations and decisions, which earlier in this chapter I positioned as a foundational assumption of mathematically-based theories of architectural design, instigated debates about the legitimate actor of decision-making. The effort to salvage architectural professionalism and carve a territory proper of architectural expertise that motivated the quest for rationality in the first place gave way to a complete dismissal of the professional architect’s decision-making legitimacy. This is not to say that the roots of participatory design are to be sought exclusively in architectural theory’s engagement with rationality in the 1960s. Participatory design was a notoriously muddled area\footnote{From the onset, architectural critics denounced the term “participatory design” as politically fuzzy and methodologically confused. In 1971 Reyner Banham characterized participation as “one of those ideas that everyone has heard of, everybody can discuss, everyone knows what it means” (Banham, Reyner. 1972. “Alternative Networks for the Alternative Culture?” In Design Participation: Proceedings of the Design Research Society’s Conference, Manchester, September 1971, edited by Nigel Cross, 15–18. London: Academy Editions, 15.) Social worker Sherry Arnstein had voiced similar skepticism of “participation” rhetoric in planning in her 1969 article “A Ladder of Citizen Participation.” There she argued that the term was merely a slogan, without any concrete} both ideologically and methodologically.
It incorporated motives, agendas, and practices as varied as its many lineages and political colorations, of which this story’s actors tried to make sense. When I speak of participatory design in the current work, I refer specifically to a subset of participatory design’s history that has ties to the modernizing project of 1960s “architectural theory” and “architectural research.”

This abridged exposition of another kind of architectural, lowercase modernism that unfolded after the uppercase Modern architecture sets the context for my inquiry. Accounts of architects’ engagement with research in the 1960s often tell a story of how an extreme predilection with rationality motivated, and was expressed through, a repurposing of techniques with military origin—from operations research to cybernetics to ergonomics—within the funding and ideological ecologies of the military-industrial-academic complex. Many of these expositions see architecture’s modernizing moves after the Second World War as results of a context that “imposed particular methodological and ethical choices.” These accounts fall back on a discussion of mathematical techniques in terms of application, deliberately putting something to use for a purpose. Here, I tell a different story that views the research university as an enclave for processes of intellectual and methodological fusion between architecture and mathematics. The operative term here is not application, but what I will subsequently frame as mathematization—an intellectual and methodological process that was both a keyword for the actors of this story and a productive term for analyzing their work.


2.3. Modern Mathematics and Graph Theory

*Mathematization*

A notion that should be dispelled from the onset is that mathematics implies quantification. Architects who turned to mathematics in the 1960s did not do so in search of numbers, but in search of “structures”—abstract descriptions of relations between entities of architectural significance that could be reasoned about mathematically. This was new in the history of architectural theory, where exchanges with mathematics had revolved chiefly around metrics and measures (proportions, dimensions, modules, distances, and averages). The appeal to mathematical concepts and vocabulary fluctuated proportionally to the architectural theorist’s claims to rationality. As such, the Modern architectural theory of the interwar and early postwar period routinely invoked mathematics. The exigencies of mass production motivated scrutiny of proportional systems, dimension standardization, and modular coordination, with numbers acquiring meanings ranging from the quasi-metaphysical\(^{141}\) to the utterly pragmatic. The commitments to economy and efficiency promoted perusal of statistical averages and circulation distances.

However, the end of the Second World War came with the realization that the amassment of numerical information and the performance of tedious calculations failed to deliver the optimistic promises of well-functioning homes and cities that animated in the CIAM meetings. Worse yet, quantification started being perceived as mechanizing and dehumanizing: statistical averages catered for fictional entities to the discontent of actual humans, while dimensional calculations said nothing about a building’s relationship to social organization and public needs—the focus of a new generation of architects that saw CIAM to its dissolution at the end of the 1950s. The new

\(^{141}\) For example, Le Corbusier’s Modulor—a proportional system based on human measurements and mathematical ratios such as the golden section or the Fibonacci numbers—was promoted as humanizing the mechanical universe of industrial mass production (Le Corbusier. 1950. *Le Modulor I*. Boulogne).
surge in architectural theory that occurred in the 1960s did not deploy more of the same mathematics. Instead, architectural theorists turned to a new kind of mathematics that entered public and intellectual debates full force. Despite the fact that its assertive entry has now been forgotten, architects of the 1960s unloaded professional anxieties and hopes of disciplinary renewal to this new mathematics. The “mathematics of quality,” as famed French anthropologist Claude Lévi-Strauss famously baptized the mathematics of relations and structures, fostered new optimism for a truly modern architectural discipline with well-organized knowledge and a solid mathematical foundation.

Intellectual and methodological possibilities aside, the mathematics of relations and structures, including graph theory, reached architects prepended with the qualifiers “modern” and “new.” These epithets made for attractive rhetoric in the writings of architect-researchers. Despite claiming the opposite, architect-researchers remained committed to a culture of novelty, continued to idolize scientific and technological advances, construed their profession as shaping human life and society, and insisted to “play soldiers” — this time against a methodologically flawed and un-rigorous Modernism. That “modern” mathematics would deliver a truly “modern” architectural theory, or that “new” design methods would be produced through “new math” made for an intuitively convincing argument. Architectural theorists in the 1960s repeatedly, explicitly, and vocally announced their alliance to “modern” or “new” mathematics. In looking back at the history of the systematic study of design in architecture, design methodologist Geoffrey Broadbent posited that despite the perception that new theories and methods grew out of Second World War disciplines such as cybernetics and operations research, their foundation was really

143 Alexander, “The Revolution Finished Twenty Years Ago,” 181.
“the new maths.” Broadbent wrote: “The new maths, with a certain amount of statistics, has been almost as influential in the development of new design methods as all the other sources and disciplines put together.” As he explained later in his book, by “new math” Broadbent referred specifically to mathematics representing structures of relations: topology, set theory, and graph theory.

Such parallelism of “modern” math and “modern” architecture or “new math” and “new design methods” was not just wordplay. Architects did not simply apply novel mathematical techniques to serve independent and already existing theoretical agendas. Instead, they engaged in translation of architectural ideas and processes in mathematical terms, a procedure that both actors of the current story and philosophers of science refer to as “mathematization.” In theoretically contemplating the term, French historian and philosopher of science Sophie Roux uses the term “forms of mathematization” to highlight the many and divergent manifestations that such translation can take. Mathematization, she argues, is contingent on the specific branches of mathematics used and their meanings (cultural, symbolic, operational, etcetera) for the discipline that performs the translation. This observation is crucial for my inquiry into

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144 Broadbent used “new” to point to new research in mathematics more so than to the educational movement of the “new math.” However, as I will elaborate on soon, some of the mathematical varieties that he included in the “new math” were the focus of the “new math” movement in secondary school mathematics.


146 French historian and philosopher of science Sophie Roux defines mathematization as “the application of concepts, procedures and methods developed in mathematics to the objects of other disciplines or at least of other fields of knowledge.” (Roux, Sophie. 2010. “Forms of Mathematization (14th-17th Centuries).” *Early Science and Medicine* 15 (4/5): 319–37, 324). The term is not only a historical analytic but was also used by some of the actors of the current story. For example, as we will see in chapter 4, Lionel March used the term to describe the work of his research center at the University of Cambridge.

147 Roux argues that mathematization escapes general definition as it always relies to specific kinds of mathematics that carry their own meanings. In that sense, “quantification,” traditionally linked to measuring and counting, is different from “geometrization,” which adds the layer of spatial and visual representation, and different from “algebraization,” which points to a procedural manipulation of signs detached from physical referents. These connotations, in turn, played different functions within the receiving disciplines—from intellectual legitimacy to practical applications in discipline-specific operations. Roux also points attention to the distinction between “application” and what Louis Althusser called “true mathematization”—the phenomenon where objects of a
architecture theorists’ mathematizing efforts during the 1960s. This was the time that architects stopped viewing mathematics as a “science of quantities” or “magnitudes in general”\textsuperscript{148} and turned to “new” and “modern” mathematical varieties that ventured toward the “qualitative.”

Here, I investigate the meanings that the “qualitative,” “modern,” “new” math carried before entering architectural debates and the position of graph theory within the structure-oriented edifice of mathematics. I argue that shedding light on these meanings is imperative for grasping the modes and implications of architects’ engagement with mathematics in the 1960s; for unveiling the forces that brought graphs to architecture and unearthing the symbolic loads that graphs carried.

\textit{Structures of Modern Mathematics}

The emergence of “modern mathematics” is a multifarious phenomenon that does not reduce to any single lineage.\textsuperscript{149} One can trace an instantiation of “modern math” not only as a set of subjects and practices, but as a culture and a distinctive way of thinking about disciplinary knowledge, to the work of Nicolas Bourbaki. Often referred to as a “multi-brained” mathematician,\textsuperscript{150} Bourbaki was the pseudonym of a secret collective of mathematicians formed in 1935 by École Normale Supérieure affiliated with the goal to modernize and democratize discipline are “constituted” through a mathematical language. (Althusser, Louis. 1974 [1967]. \textit{Philosophie et Philosophie Spontanée des Savants}. Paris: F. Maspero).

\textsuperscript{148} Roux, “Forms of Mathematization,” 324.
\textsuperscript{149} Gray locates mathematics’ “modernist transformation” between 1890 and 1930 (Gray,\textit{ Plato’s Ghost}, 1).
\textsuperscript{150} Bourbaki grew out of the “Committee for Writing a Treatise on Analysis,” a group formed in December 1934 during one of the meetings of the bimonthly Julia Seminar at the Poincaré Institute. A history of Bourbaki’s trajectory is available by historian of mathematics Maurice Mashaal (Mashaal, Maurice. 2006. \textit{Bourbaki: A Secret Society of Mathematicians}. Translated by Anna Pierrehumbert. Providence, RI: American Mathematical Society). Mashaal explains that academic mathematicians Henri Cartan, Claude Chevalley, Jean Delsarte, Jean Dieudonné, René de Possel, and André Weil met ten times until May 1935 to write a new textbook on analysis for higher mathematical education to replace existing textbooks on the subject, particularly Édouard Goursat’s early twentieth century \textit{Cours d’Analyse Mathématique}. Mashaal cites Weil’s modernizing visions to produce a textbook “as modern as possible” (Ibid., 7) and “as robust and universal as possible” (Ibid., 9) and his democratizing intentions for the textbook to be “used by anyone: by researchers (both students and professors), by future teachers, by physicists, and by engineers” (Ibid.).
higher mathematical education by distilling the substance of mathematical tools and presenting them in a universal and robust way. Bourbaki presented these three traits as a remedy to the polyphony and balkanization of mathematical knowledge in the 1930s and 1940s. In the group’s most influential polemic, felicitously titled the “Architecture of Mathematics,” Bourbaki likened mathematics’ status to “a Tower of Babel of autonomous disciplines, isolated from one another in their goals, methods, and even in their language.” This led to the disconcerting question, “Do we have today a mathematic or do we have several mathematics?” Bourbaki’s answer was to reorganize mathematics like modernizing a city. The article famously read:

[Mathematics] is like a big city, whose outlying districts and suburbs encroach incessantly, and in a somewhat chaotic manner, on the surrounding country, while the center is rebuilt from time to time, each time in accordance with a more clearly conceived plan and a more majestic order tearing down the old sections with their labyrinths of alleys, and projecting towards the periphery new avenues, more direct, broader and more commodious.

In articulating disorganization as pathogeny and proposing “structure” as a cure, Bourbaki put forward a new attitude toward disciplinary knowledge that linked abstraction and structures with...

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151 In August 1952 the Bourbaki group acquired an official presence as a non-profit organization named “Association of Collaborators of Nicolas Bourbaki” with its headquarters in residences of its members, first in Nancy and then in Paris. Since 1972 the association headquarters are located in the École Normale Supérieure in Paris. (Mashaal, Bourbaki, 17).

152 Bourbaki’s efforts to develop a new textbook on analysis developed into a full-blown mathematical treatise that came to be known as Éléments de Mathématique—a deliberate reference to Euclid’s Elements of geometry. Bourbaki’s Éléments currently consists of ten books, the first six of which were published before 1958 as the work’s first part. The first book published in 1939 was dedicated to set theory, which by describing relationships between groups of elements formed the foundation of the axiomatic and structural rendition of mathematics that Bourbaki was promoting (Ibid., 52).


154 Ibid.

ideals of unification and modernization. Bourbaki envisioned mathematics as an expanding structure built up from combinations of what they called mathematical entities. The entities or their properties were not important. Instead, what mattered were their relations and the ways in which they combined. This was what would give rigor and abstraction. For Bourbaki the mathematical edifice was built through the “axiomatic method.” Axioms were postulates without proof that held true for the objects involved in a theory. These could be combined through logical operations to form more complex structures called theorems. The construction of theorems relied solely on rules-based method, not intuition or experience.

Bourbaki suggested that using the axiomatic method made it possible to build all of mathematics as a relational network of structures. The focus of mathematics would then shift from studying specific mathematical objects and their properties to studying relations between mathematical structures that connect them. Because the nature of the objects was insignificant, the same “pattern” or structure could apply in completely different sets of objects. Apart from organizing mathematical knowledge, the proposition to move from the properties of objects to the relations between them decisively severed mathematics from any intuitive or experiential referents. This continued an abstracting project initiated by German mathematician David

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156 Bourbaki identified three types of structures: algebraic structures (associating any type of elements to a third), order structures (comparing elements of a set), and topological structures (representing neighborhoods, limits, and continuity).

157 Bourbaki’s theory of structures assimilated category theory, developed in 1942 by American mathematicians Samuel Eilenberg, who eventually became a Bourbaki member, and Saunders MacLane as part of their study in algebraic topology. The goal was to describe structure-preserving processes. Category theory included “-morphisms” between classes of objects, and became an indispensable part of talking about structures, as Mashaal notes (Mashaal, Bourbaki, 83). Bourbaki founding member Claude Chevalier characterized category theory as more “true to the spirit of Bourbaki than their theory based on structures” by virtue of being “more structuralist” (Ibid., 84). Pierre Cartier, also a Bourbaki member, observed that in the 1960s Bourbaki talked about categories without really talking about them, as an explicit espousal of the term would bring too many changes in their already published work (Ibid.). “-morphisms” of various kinds permeated the American new math curriculum and, as we will see in many cases, penetrated debates in other disciplines, architecture and design included.

158 Mashaal, Bourbaki, 75.
Hilbert, who in his 1899 *Foundations of Geometry* reworked Euclid’s axiomatic method to cleanse it of spatial intuitions. Hilbert espoused the axiomatic method as a valid model of constructing and organizing mathematical truths, but reworked it to correct what he perceived as a weakness: Many of Euclid’s theorems were based on spatial intuitions about points, lines, and planes rather than pure reason. In a move frequently construed as an originary moment in mathematical modernism, Hilbert dissociated the fundamental objects of geometry from any spatial and visual properties and strove to produce a geometry whose statements would hold true even if points, lines, and planes were replaced by “tables, chairs, and beer mugs.”

The shift from the scrutiny of objects’ properties to the study of abstract relations that connected them found ample emulators in the 1950s and 1960s. As historian of mathematics Maurice Mashaal writes, while Bourbaki preached mathematical introspection and a purism that denounced mathematics’ ties to neighboring sciences, the group’s style and methods had many followers. The physical sciences and also anthropology, sociology, archaeology, literature, and other fields that previously relied on empirical knowledge emulated Bourbaki’s move toward structural abstraction. “Structure” became a stand-in for disciplinary renewal, modernization, and rigorization, and a keyword in the intellectual tide of structuralism that swept the so-called “soft” sciences after the Second World War. Structuralism notoriously evaded definition. In the late

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159 Structures, axioms, and modern mathematics have an intertwined history. Outside geometry, axiomatizing efforts were also made in arithmetic by Richard Dedekind and Giuseppe Peano. Van der Waerden’s work *Moderne Algebra* also advanced a discussion of algebraic structures (Waerden, Bartel Leendert van der. 1930. *Moderne Algebra*. Berlin: Springer). As Mashaal notes, Bourbaki’s contribution was to extend structures and axioms to all mathematics.


161 Mashaal, *Bourbaki*, 125.

162 Structuralism was a multifaceted cultural and intellectual phenomenon that comprised many different strands. Key figures in the movement, such as anthropologist Claude Lévi-Strauss, have linked their ideas with early twentieth-century developments in structural linguistics, and not mathematics. Most scholars and historians have also studied structuralism as conceptually emanating from linguistics. However, structural mathematical ideas formed the technical back-end of many structuralist analyses. Lévi-Strauss’s *Elementary Structures of Kinship* (1969 [1949] Edited by Rodney Needham. Translated by James Harle Bell and John Richard von Sturmer. Boston: Beacon
1960s comparative and historical sociologist Garry Runciman was questioning whether it managed to amount to a “distinctive doctrine or method” of analysis, while philosopher Michel Foucault spoke of structuralism as “not a new method” but as “the awakened and troubled consciousness of modern thought.” Historian of structuralism Francois Dosse has characterized structuralism more as a “phenomenon” rather than a movement, associated with promises of a “rigorous method and some hope for making progress toward scientificity” but also a “key moment in critical consciousness,” a “desire for modernism in search of new models.” In Runciman terms, “The term 'structure' serve[d] to mark off questions about the constituents of the object under study from questions about its workings.” By representing such a shift from how objects are or what they do to the structures that constitute them or of which they are part, Bourbaki operated as a “cultural connector” for structuralist tendencies—to borrow and extend a term that historian of science David Aubin has proposed to describe Bourbaki’s influence in the French context.

In “Mathematics of Man” Lévi-Strauss pointed to the dovetailing between modern mathematics and structuralism. He wrote:

The sciences of man […] at first sight seem the most remote from the idea of exactitude and measurement […] their qualitative object of study made it impossible for them to ‘tag along’ behind traditional mathematics […] and forced them to turn […] to certain novel and daring methods of mathematical thought.

Press) featured an appendix by Bourbakist André Weil, while structuralist developmental psychologist Jean Piaget made frequent reference to the Bourbaki methods.

Ibid., xix.
Ibid.
Ibid., xx.
Indeed, apart from advocating for a structuralist approach to disciplinary knowledge, Bourbaki also disseminated the mathematical devices for representing and reasoning with relations and structures. To reiterate historian of mathematics Ivor Grattan-Guinness’s observation, although Bourbaki strove for the “pure” and steered away from the “applied” it played active role in disseminating applic-able mathematics. The modern mathematics of relations and structures—set theory, topology, matrix theory—not only described the mathematical discipline’s new structural self-image but provided tools for mathematically elucidating the structures toward which other disciplines, including architecture, turned in efforts to modernize their subject matter.

These modern mathematical subjects did not remain enclosed in the realm of academia, nor were they privileged knowledge available to an elite of mathematical prodigies. Starting in the 1950s, academic mathematicians allied with schoolteachers to bridge what they perceived as a gaping chasm between university mathematics and schoolchildren’s mathematical education. This resulted in drastic changes in mathematical education that swept the Western world in the 1960s under the label “new math.” The new math encapsulated a plurality of approaches to curricular change in secondary mathematical education, with as many common threads as local divergences. Although many historical accounts link the impetus for educational reform with the 1957 launch of Sputnik and locate its beginning in the United States, the new math did not have one singular geographic or chronological origin. Sputnik merely opened the funding faucet

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and impelled organizations such as the NSF to generously support curricular changes that were already being discussed among mathematicians. Changes in mathematics education emerged from many centers and did not follow a simple model of transatlantic transfer. These changes required intricate alliances between governmental organizations, funding bodies, educational councils, teachers, journalists, and the public and were driven by a whole gamut of deliberations. These were both internal to the discipline of mathematics and pertaining to mathematics’ relationship with other disciplines and with the public. Invariably, however, the new math brought some version of the modern mathematics of relations and structures into the classroom, alongside the assumptions and cultural meanings that these carried in academic intellectual life.

Structures of New Mathematics

Historian of science Christopher Phillips describes the American new math as driven by the realization that traditional school mathematics was at odds with the mathematics that propelled the scientific and technological supremacy of the United States. It was the perception of the public and the government alike that science and technology had won the Second World War and would help the United States to prevail over its Cold War adversaries. Not only did learning to memorize theorems and solving equations leave students ill-prepared for university education, but it also cultivated the “wrong” kind of intellectual capacities. Memorization and manipulation were viewed as mechanical habits that countered the ideal of the active and free-minded U.S. citizen. Phillips compellingly exposes the American new math as responding to the exigency

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173 Bob Moon makes the point about “parallel” innovation in his international history of the new math (Ibid.).
175 Ibid., 2.
176 Ibid.
177 Ibid., 135.
to train the population to reason about complex social and scientific situations, in universities, the industry, and everyday life. In Cold War America, governmental entities and the public put emphasis on mathematical training, both as providing techniques to put to work, and mental proclivities to practice, so as to decipher patterns undergirding a growingly complex and fast-changing world. Phillips identifies these mental habits as an aptitude for formal, structural, mathematical reasoning—the kind of reasoning that the structure-oriented and abstraction-prone modern math promoted. In the United States, debates among academic mathematicians and mathematics teachers throughout the 1950s culminated in the NSF-funded School Mathematics Study Group (SMSG) that ultimately became synonymous with the American new math. Led by Edward Begle of Yale University and initially composed of prominent academic mathematicians, the SMSG pushed modern mathematics in the classroom. Set theory and group theory figured centrally in the curriculum, while SMSG members propagandized their intellectual benefits.

Similar claims about mathematics offering a basis “for a first rate education” and being “a good training to be an efficient member of society” accompanied the beginnings of the British new math movement. In the U.K. the start of the 1960s found the topic of mathematical education acquiring a “feeling of […] breathtaking urgency” for members of British schools, universities, and the industry. An impactful conference on the teaching of mathematics that took place at the University of Southampton in April 1961 echoed the American new math conviction

178 As Phillips succinctly puts it: “the new math’s development and deployment were based upon claims about the special nature of ‘modern’ mathematical knowledge, the relationship between this nature and the mental habits resulting from its study, and the importance of these particular habits for the shaping of U.S. Citizens” (Phillips, The New Math, 4). In other words, the condition of modernity required a “modern” mind that could be shaped through modern math (Ibid., 118–119).

179 The specific quotes come from British new math pioneer Bryan Thwaites, who argued that “quite apart from any narrow interpretation of usefulness […] A training in mathematics is a good training to be an efficient member of society and a mathematician’s approach to a non-mathematical problem is often a vital one, reflecting in its effectiveness the mathematical training he has received.” (Thwaites, Bryan. 1961. On Teaching Mathematics: A Report on Some Present-Day Problems in the Teaching of Mathematics Being the Outcome of the Discussions and Lectures at the Southampton Mathematical Conference 1961. Pergamon Press & The MacMillan Company, 47).

180 Ibid.

181 Thwaites, On Teaching Mathematics, xiv.
that “whilst mathematics provides one of the finest educational backgrounds—and a useful one into the bargain—an unsuccessful mathematical education can be disastrous.”\(^{182}\) The Southampton Mathematical Conference was the culmination of conversations about problems and challenges in the teaching of mathematics, already underway in the United Kingdom since the late 1950s,\(^{183}\) and laid the seeds of the most influential initiative of the British new math in secondary education, titled the “School Mathematics Project” (SMP). Soon after the Southampton\(^{184}\) conference, Bryan Thwaites co-founded the SMP with the double agenda to evolve syllabi for ages 11 to 18 that would “adequately reflect the modern trends and usages of mathematics” and to produce “a complete set of associated textbooks and teachers’ guides.” Within ten years from its foundation SMP books were being used by around half the schools in England, giving it a leading position among many other organizations that engaged with the development of mathematical syllabi and textbooks in the U.K. throughout the 1960s.\(^{185}\)

\(^{182}\) Ibid., 48.

\(^{183}\) The Southampton Mathematical Conference was preceded by two other conferences aiming to create communication bridges between those involved with mathematical education and the industry. These conferences were initiated and chaired by applied mathematicians. The first was a conference at Oxford (1957) initiated by statistician and applied mathematics advocate John Hammersley, and a second at Liverpool (1959) exploring the theme “Mathematics in Action,” which was chaired by Louis Rosenhead, mostly known for his work in fluid mechanics. The novelty of the Southampton Mathematical Conference was its “highly organized structure of committees” (Ibid., xiv) on university mathematics, school mathematics, and industrial mathematics.

\(^{184}\) The Southampton Mathematical Conference not only consolidated critique that vouched for educational reform, but also suggested a path forward. Part of the answer was to familiarize both students (through textbooks) and teachers (through pamphlet guides or, if need be, courses offered by the Institute of Education and Universities) with “modern developments” in mathematics (Thwaites, On Teaching Mathematics, 30). The lack of textbooks that “present the subject from a modern point of view” was blatantly identified at the conference, where delegates were unable to “name a single British school textbook which does so” (Ibid). On an administrative level, the answer to the shortage of well-trained mathematicians proposed at the Southampton Mathematical Conference was to double the intake of mathematics students in universities, not necessarily in honors degrees, but more general subjects or ones coupled with the study of engineering or scientific subjects. In this way universities would output more technologists and schoolteachers (as opposed to professional mathematicians) (Ibid., 99). This required changes in the university entry procedures, including a common entry process for all mathematics students (regardless of whether they pursued an honors degree) with tests on a single mathematics subject. By the mid-1960s, prominent universities such as Oxford and Cambridge agreed to modify these requirements. (Thwaites, Bryan. 1972. The School Mathematics Project: The First Ten Years. London: Cambridge University Press, 99).

Before starting SMP, Thwaites prophesied “a decade of ferment” in education brought by the growing numbers of students pursuing higher education in “the Sciences.” Mathematics, he argued, would be at the cyclone’s eye because of its position as “the one discipline common to all the sciences.” Faced with growing demands of well-qualified mathematicians not only in academia, but also in industry and in the government, the number of school and university mathematics teachers proved grossly insufficient. With technological skills being “the prime raw material” of the “tiny crowded island” of the U.K., this shortage took the dimensions of a “national disaster.” In arguing that mathematics was “crucial for science and technology” Thwaites was not implicitly lobbying for “applied” mathematics. Since the end of the Second World War, a feud between “pure” and “applied” mathematics was animating American

Thwaites did not only see mathematics as a common foundation for all sciences but also a bridge between humanistic and scientific cultures. He introduced the Southampton Mathematical Conference report by saying: “The scientific advances which have captured people's imagination - space travel, the harnessing of atomic power, and many others - would have been impossible without the participation of men and women trained in mathematics and applying their training to the study of the physical world. At the same time mathematics enjoys an independent existence as one of the finest products of the human mind, testifying along with the other arts, by its nature and content, to man's creative ability.” (Ibid., 1).

Thwaites, On Teaching Mathematics, 46.


Alma Steingart has convincingly argued that the rift was more between “applied” and “pure” mathematicians: the kinds of activities they performed, the institutions with which they interfaced, and the kinds of personalities they typified. (Steingart, “Conditional Inequalities”).
mathematics and had made it into the new math debate. Thwaites saw the distinction between “mathematics as a purely intellectual activity and mathematics as a problem-solving technique,” or “Pure” and “Applied,” or “Abstract” and “Applicable,” or “Theory” and “Practice” provisional and likely to diffuse in view of new modes of technological and scientific research, including the advent of electronic computing.

At Southampton, participants were asked to speak on the theme of “Mathematical Models.” Shifting the focus of “applied” mathematics from finding statistical means or calculating currents to constructing mathematical models presented opportunities for exposing the common essence of “pure” and “applied” mathematics. Mathematical modeling was about identifying and interrelating variables and implicated processes of abstraction, mapping, and comparing—structural ideas that “pure” modern mathematicians embraced. Such coalescence between abstract structures of axiomatic mathematics and empirical structures of mathematical models was also highlighted in a panel on mathematical education organized by the still-young American Society for Industrial and Applied Mathematics a few months after Southampton. The question was whether the structures of mathematical models should derive from empirical observation and abstraction of “reality” or grow from within mathematics by manipulating mathematical structures. Academic mathematician and SMSG author Paul Rosenbloom, for

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195 The rift between “pure” and “applied” math penetrated the American new math debate. With members such as Marshall Stone of the University of Chicago, vocal proponent of mathematics’ abstract nature and independence from physical applications, the SMSG was widely perceived as channeling a kind of mathematical purism into the classroom. As such, it received rebuttal from the opposing front, represented by figures such as New York University mathematician Morris Kline, who found mathematics’ dissociation from the physical world not only pedagogically ineffective, but hindering progress in the field. For the American controversy around the new math see Phillips, *The New Math*, 69–70.
197 Ibid., xii.
198 Ibid., 60.
example, argued that models should derive from reasoning with abstract structures of mathematics and then “look around for a physical interpretation.” The power of the new math, Rosenbloom seemed to suggest, came from instilling in students the ability to see patterns and structures both in mathematics and in the real world. This claim is not far from the British new math attitude toward mathematical modeling.

The British new math assimilated topics of set theory, matrix algebra, topology, and various modern mathematical varieties, both as intellectual training and useful skill: teaching students to reason with abstract and empirical structures. In reminiscing on SMP’s first ten years, deputy director Douglas Quadling declared the teaching of mathematics motivated by “a social function” to “help us interpret and control our environment.” The increasing impact of “mathematical patterns of thought on society,” an impact visible in “industrial practice, ecological studies, urban planning or putting spacemen into orbit,” mandated that the teaching of mathematics aimed not only to “knowledge and accuracy” but also to what Quadling called “appreciation”—learning to see patterns and structures in the world. The SMP used “modern” to variously denote the content of what students were being taught and the attitude with which they were taught. As Thwaites explained, “modern” referred to both new mathematical topics that had made their way into universities under the label “Modern Mathematics” as well as “a modern approach” towards the teaching of traditional math with a view “to inspiring in children something of the modern attitude towards the structure, pattern and beauty of mathematics.” The SMP textbooks both familiarized students with mathematical topics other

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200 Ibid.
201 Ibid., 217.
202 Ibid.
203 Ibid.
204 Thwaites, On Teaching Mathematics, 29.
205 Ibid., 30.
206 Ibid.
than the traditional arithmetic and geometry, such as sets, groups, matrices, transformations, and topology, and promoted a culture where “structure and pattern” were viewed as aesthetic and intellectual ideals. Providing technical training and inclusion in structural thinking, the new math is important background for understanding architects’ engagement with mathematics and their adoption of graph theory. Pioneering figures like Christopher Alexander and Lionel March acquired formal mathematical training in university. Yet, as I mention in following chapters, architecture students who advanced Alexander’s theories or staffed March’s research lab at the University of Cambridge were children of the new math. At the same time, public debates that popularized the structuralist culture of modern mathematics created a receptive climate for architectural theories that reworked architectural modernism through modern mathematics: sets, matrices, graphs. Architects could understand the math that these new architectural theories contained. The new math prepared architects both as audience and workers of architecture’s mathematization. It also amplified, instilled even, in architectural culture one of modern math’s key ambivalences: its contested relationship with the visual, empirical world—an ambivalence that is key for understanding the graph’s entry into architecture.

Abstraction, Experience, and the New Geometry

The SMP invoked the term “modern” with less gravitas than the U.S. contingent. Although the American new math formed an exemplar in terms of funding structure and organization,207 its

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207 The Southampton Mathematical Conference concluded by commending the “very great indeed” “scale and […] calibre” of American efforts to ameliorate school mathematics and advance university mathematical research,
influences by the Bourbaki style and implicit promotion of a purist agenda did not seem viable in the U.K. There, the “new” in new math pointed more to a commitment on being “up-to-date” rather than to a wholesale espousal of mathematical modernisms’ intellectual tradition. The value of relating mathematical topics with the “living and thinking experience of the pupil”208 was an active debate since the Southampton Mathematical Conference and made it into the SMP. As deputy director Douglas Quadling wrote in accounting for SMP’s first ten years, “Our own approach to this was more conservative [than the American new math]: we acknowledged amongst our objectives the development of an awareness of structure, but saw abstraction as a process in which the pupils should engage actively as a result of concrete experience, not as a system of laws to be imposed upon them.”209 No subject exemplified the controversial relation with experience, and in ways relevant to architects too, better than geometry. The new math’s relationship with geometry was complicated. In SMP’s ten-year retrospective Quadling characterized geometry “the most difficult area of all.” He attributed this difficulty to the fact that “in the traditional syllabus [geometry] stood apart from the rest of mathematics, and there was considerable doubt as to the motive for teaching it, or indeed whether it had any place at all.” These sentiments toward geometry held even truer for the American new math, where the teaching of geometry broke from acts of drawing and interpreting figures210 and shifted toward systematic reasoning with axioms and theorems.211

supported by the federal government and the NSF, and highlighted the need to “follow their example here and now” (Thwaites, On Teaching Mathematics, 100).

208 Ibid, 28.
210 Historian and philosopher of mathematics José Ferreirós offers a compelling reading of Ancient Greek geometry as a kind of visual practice, where mathematical rules provide ways of interpreting “diagrams” (the objects of geometry) and that doing geometry involves visual reasoning. He writes: “the geometer will act (drawing) and reason (inferring) accordingly, employing the diagram not as it is empirically given, but as it is conceived. Nevertheless, the relevant information content is not fully exhausted conceptually or theoretically; the information is...
Such ambivalence dates back to an event that frequently appears in the origin stories of the new math movement. This was a two-week seminar with proposals for bridging the gulf between school and university mathematics that took place in 1959 in Royaumont, France, and was convened by the Organization for European Economic Cooperation (OEEC). In the seminar, prominent Bourbakist and alleged author of “The Architecture of Mathematics” Jean Dieudonné argued that old methods of teaching and doing geometry should be ostracized from mathematical syllabi. With the provocative slogan “Euclid must go!” Dieudonné called for a re-evaluation of geometry’s place in modern mathematics. Such re-evaluation required “separating what is fundamental [in geometry] from a chaotic heap of results with no significance except as scattered relics of clumsy methods or an obsolete approach.” Dieudonné posited that aside from the axiomatic system, all other content that filled geometry textbooks was irrelevant “dead weight” taught in a “fantastically laborious and complicated process.” Geometry could be salvaged by a modernization of its methods of teaching that would cultivate “intuition of space” yet placing it “in the logical frame which will enable them to use it later.” For Dieudonné, intuition was the first step in the students’ evolutionary trajectory to full

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213 Mashaal hypothesizes that, because of its combative tone, “The Architecture of Mathematics” was written by Dieudonné (Mashaal, Bourbaki, 71).
215 Ibid. 216 Ibid.
217 Ibid., 38.
218 Ibid., 37.
219 Ibid., 46.
220 Ibid.
abstraction. Yet, the intuition that he embraced was of a particular kind. Dieudonné construed intuition as an acquired fluency built through constant reasoning and experimentation with mathematical material, and not as a kind of visual precognition about shapes and their properties. Seeing was not a privileged realm that escaped structural and axiomatic processes. In “old geometry’s” claims that there was a special relationship between geometry and the visual experiential realm, Dieudonné saw nothing but excuses for the sloppy reasoning that plagued it.

At the Royaumont seminar, Dieudonné sketched a new method of teaching geometry that did away with “artificial playthings as triangles” and put emphasis on “basic notions which will command and illuminate every question in which geometry intervenes.” Triangles were to be replaced with vectors—lines with magnitude and direction that were subjected to arithmetic definitions and operations such as adding, subtracting, multiplying, etcetera. Divorced from metric properties, geometry would become a part of linear algebra and its focus would shift from figures to “symmetries, translations, composition of transformations, etc.” The algebraization of geometry would not only make it congruent with the rest of mathematics, but also provide opportunities for using the modern methods of set theory and matrix algebra to describe operations between vectors. Dieudonné’s talk stirred the expected controversy among mathematicians who were not ready to do away with triangles—“a figure that has deep roots in human intellectual development […] the only rigid polygon.” Yet SMSG leading figures

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221 Ibid., 45.
222 Ibid., 41.
223 Dieudonné claimed that his wish was not to minimize the importance of geometry but to modernize its methods: “My quarrel is […] with the methods of teaching geometry, and my chief claim is that it would be much better to base that teaching not on artificial notions and results which have no significance in most applications, but on the basic notions which will command and illuminate every question in which geometry intervenes. For instance, whereas the notion of vector has paramount importance everywhere in modern science, the notion of triangle is an artificial one, with practically no applications outside the highly specialised fields of astronomy and geodesy.” (Ibid., 46.)
224 Ibid., 41.
225 Ibid., 47.
Edward Begle and Marshall Stone, who attended the seminar, assimilated Dieudonné’s program. The cleansing of geometry from its visuospatial content and its purification as a form of reasoning made it into the SMSG teaching material.

The SMP members were less adamant about replacing visual intuition and experience with axiomatic reasoning. Affine transformations of “rotation, reflection, translation, and enlargement”\(^{226}\) prevailed in its O(rdinary)-level textbooks directed at the general student population. They did so along with algebraic notions of sets, groups, and matrices. Yet, if the SMSG used algebra to recast geometry as a form of reasoning, the SMP used bodily experience and visual intuitions as the basis upon which to present mathematical notions of sets, matrices, groups, and others in a spatial context. The axiomatic treatment that the U.S. contingent valued most in geometry was almost absent from the British new math textbooks. In his 1962–1963 director’s report Thwaites wrote that in the place of a “watered-down Euclid”\(^{227}\) (axiomatic treatment of geometry) the SMP strove to provide students with “a feeling for [...] spatial relationships”\(^{228}\) and to emphasize the “interplay between geometry and algebra.”\(^{229}\) A few years later, Quadling was celebrating SMP textbooks’ “lack of awareness at any time whether one is doing algebra, or geometry, or trigonometry, or what.”\(^{230}\) If Hilbert’s axiomatization of geometry lay behind the subject’s modern teaching in the United States, the British new math pledged allegiance to Felix Klein, the late-nineteenth-century German mathematician that historian of mathematical modernism Herbert Mehrtens has positioned at Hilbert’s antipode as an “anti-modern.” Klein was a supporter of mathematics’ interconnection with the experiential world.\(^{231}\)

\(^{226}\) Ibid.
\(^{227}\) Thwaites, The School Mathematics Project: The First Ten Years, 18.
\(^{228}\) Ibid.
\(^{229}\) Ibid.
\(^{231}\) Historian of mathematics Joan Richards discusses the primacy of Euclidean over new non-Euclidean geometries among British mathematicians of the Victorian era. Euclidean geometry was considered a privileged realm of spatial
He believed that mathematical elements carried meanings outside the realms of formal definition and that mathematics progressed as much with logic and reasoning as it did with perception, imagination, and spontaneity. Klein encapsulated these ideas in a famous lecture that he delivered at the Philosophical Faculty of the University of Erlangen in October 1872 upon being appointed full professor. This lecture formed a programmatic basis for what came to be known as the “Erlangen Programm.” The Programm was a new philosophy of geometry that maintained figures (shapes) at the center and used the properties of these figures that remained unchanged (invariant) under certain transformations as a way to define different geometries, and in particular bringing Euclidean and non-Euclidean geometry under the umbrella of projective geometry. This approach formed the basis for the SMP’s new geometry. Qualding wrote:

The approach through transformations, which had been in the background since Klein’s lecture in 1872 […] seemed to offer new hope. In its early stages it gives much scope for practical activity based on children’s experience of movement and aimed at building up spatial awareness. As the work develops, we find central themes of mathematics — sets of points, transformations (or functions), groups, etc.— exhibited in a spatial context. Geometry ceases to be just a series of set piece engagements, but opens up possibilities for the pupils temperate in a more realistic mathematical genre: a blend of experiment, guesswork and deductive reasoning. The emphasis is no longer on proof for its own sake, but as one means of increasing out geometrical knowledge.


Klein’s 1872 Erlangen lecture was titled: Vergleichende Betrachtungen über neuere geometrische Forschungen [A Comparative Review of Recent Researches in Geometry].

Gray, Plato’s Ghost, 10.


The emphasis on acquiring geometrical sensibilities on an experiential level also spoke to the British new math’s intimate relation with real-world mathematical applications. Applied mathematicians pushed for the necessity to “acquire a geometrical sense.” As one professor of applied mathematics was quoted to remark in the 1963–64 SMP report, such sense “cannot be acquired by pretending that geometry is a part of algebra […] It can be acquired only by some form of exercise in which people are forced to think in geometric terms.” Thwaites replied with an assurance that A(dvanced)-level students were being exposed to more abstract, algebraic renditions of geometry, after having first passed through the general O-level course “in which there is great emphasis on geometrical ideas—on the nature and topology of geometrical figures, on patterns, on the effects of transformations, on three-dimensional as well as two-dimensional properties, and on visualization.” Indeed, the SMP O-level textbooks were amply illustrated with examples showing mathematical concepts of mappings, transformations, and topological diagrams embedded in the physical environment—in house plans, electrical networks, geography maps, and more. This did not only tap into students’ experience and intuitions to foster understanding of mathematical concepts, but also provided students with a mathematical lens for seeing the world. It did not only rely on spatial intuitions, but shaped them.

To recapitulate, modern mathematics associated claims of unification, modernization, and democratization with structure. School mathematics promoted a structural way of teaching mathematics through sets, matrices, and topology. These were also used to teach geometry (recast as vectors and linear algebra), with an implicit disclaim of the visual or spatial content of shapes. The outcome was students well trained in sets, matrices, and topology, exposed to the idea that it was best to think geometry rather than draw it, and in some cases trained to “seeing”

236 Ibid.
237 Ibid.
structures in their environment. Architects, traditionally concerned with geometry, acquired a whole new apparatus of thinking about shapes at a time that the field’s preoccupation with aesthetics and seeing was nothing short but anxiety inducing. A new structural understanding of shapes, buildings, and the environment bore promises of renewing the discipline, salvaging professionalism, and securing legitimacy among other sciences by giving architecture a modern mathematical basis. The structural view of geometry, which purified it of its spatial and visual content, and the qualification of “structures” and “patterns” with modernizing and unifying values are key aspects of graph theory’s entry into architecture. Graph theory entered architecture to accommodate, and in retrospect expose, its ambivalences toward seeing and drawing.

Points and Lines

Within a few decades, graph theory moved from scornful disrepute among mathematicians to thriving popularity across disciplines. Cambridge mathematician W. T. Tutte reminisced about the “low […] prestige of Graph Theory in the Dirty Thirties”, what British mathematician Henry Whitehead once called “the slums of Topology.” Associated with recreational mathematics, graph theory was seen as trivial and un-rigorous; “the so-called science of trivial and amusing problems for children, problems about drawing a geometrical figure in a single

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238 William T. Tutte, who enrolled as a chemist major in Trinity College, Cambridge, in the mid-1930s, was a member of the Trinity Mathematical Society and the famous group of mathematicians Trinity Four. The group is best known for the use of a method of graph theory applied in electrical current calculations to solve the geometrical problem of rectangular dissections (dissecting rectangles into squares), a problem that, as I will elaborate later on, was of influence to architects.


241 In 1892 W.W. Rouse Ball of Trinity College, Cambridge, detailed major nineteenth-century advances in graph theory methods in Mathematical Recreations and Essays. The book discussed arithmetical, geometric, mechanical and miscellaneous questions through graph theory and featured a long chapter on magic squares.
sweep of the pencil, problems about threading mazes, and problems about collating maps and cubes in cute and crazy ways.”242 Students “tempted” by the subject were advised “to turn to something respectable or even useful, like differential equations.”243 By the end of the 1960s, the situation had shifted, with graph theory textbooks proliferating and applications of graph theory spreading across disciplines. Graph theory’s infamy gave way to laudation, even hyperbole. For example, the 1972 English translation of a 1968 French textbook on graph theory ended:

No engineer, or physicist, or chemist can ignore this theory; otherwise, complicated structures will become for them labyrinths which lie outside scientific method […] The engineer and the artist will be brought close together by the theory of graphs and its extensions; will it not be a good thing for them to join together in logical and global understanding and thus be more completely human?244

Writing under the pseudonym Blanche Descartes, Tutte synopsized graph theory’s trajectory in poetic verse: “From Königsberg to König’s book, So runs the graphic tale, And still it grows more colorful, In Michigan and Yale.”245 The reference to Michigan and Yale was attribution to mathematicians Frank Harary and Øystein Ore respectively, who were the main American campaigners of graph theory’s universal usefulness. Ore published one of the first introductions to graph theory in 1963 under the title *Graphs and Their Uses*,246 which became a classic in the subject. The start of Tutte’s poem pointed to graph theory’s origin in Euler’s famous solution of the Königsberg bridge problem247 that is acknowledged as the origin of topology—a description

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243 Ibid.
247 In Tutte’s prose from the *Expanding Unicurse*, the Königsberg bridge problem goes as follows: “Some citizens of Königsberg
  Were walking on the strand
  Beside the Rivel Pregel
of space that is not about measuring distance between things but reasoning about relationships between things. Scattered work on graph theory followed Euler’s formulation, but the topic was not presented to the mathematical world “as a subject in its own right, with its own textbook” until Dénes König’s 1936 *Theorie der Endlichen und Unendlichen Graphen.* König, Tutte wrote, “did not just introduce Graph Theory. He strove to lift out of a Slough of Despond and to set it upon a height.” Despite “some undue scorn of Graph Theory [that] persisted into the fifties,” Tutte ruled König and his successors victorious.

Ambivalence about the visual and geometric aspects of graphs, the fact that they were ultimately points and lines, existed from the onset. König’s book steered a course for a “modern” look at graph theory that abstracted its “continuous-geometrical content” and cast it as a branch of combinatorics and set theory. He wrote:

> […] we do not ascribe to the elements of graphs, points and edges, any geometrical content at all: the points (vertices) are arbitrary distinguishable elements, and an edge is nothing other than a collection of its two endpoints. The geometrical notation, which we nevertheless use,

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With its seven bridges spanned
'Oh Euler, come and walk with us',
Those burghers did beseech,
'We'll walk the seven bridges o'er
And pass but once by each'.
'It can't be done', thus Euler cried.
Here comes the QED.
Your islands are but vertices
And four have odd degree.”

In graph theory terms, the Königsberg bridge problem asks if it is possible to construct a path in a given graph which visits each line exactly once. Euler proved that a necessary condition for this is that all points in the graph have an even degree (number of lines emanating from them).

König’s book, a compendium of lectures in the Technische Hochschule of Budapest, was read by famed mathematicians Paul Erdős and John von Neumann. It was circulated in German until its 1990 translation in English as the *Theory of Finite and Infinite Graphs.*

250 Tutte, “Commentary,” 2.
252 Ibid.
gives a very convenient terminology without assuming any geometrical view or geometrical axioms.\textsuperscript{253}

Yet, König found the geometric notation (drawing graphs as points and lines) “without assuming any geometrical view or geometrical axioms”\textsuperscript{254} to be “convenient”\textsuperscript{255} and of “great heuristic value.”\textsuperscript{256} He explained that geometric notation “furnishes ‘natural’ problems and connects quite abstract things with clear ideas, whereby new connections among concepts and problems seemingly distant from one another often come to light.”\textsuperscript{257} What König was pointing to was the value of visual depiction both in elucidating abstract ideas and in revealing structural analogies between things that appeared dissimilar at first glance. Geometric notation also made graphs accessible to more general audiences. As König wrote, understanding graph theory required “a certain background in mathematical thinking […] but not a knowledge of mathematical theorems and concepts.”\textsuperscript{258}

Until the late 1950s, König’s non-translated book was the only available comprehensive introduction to graph theory. The first Anglophone introduction to the subject appeared in 1962 as a translation of Claude Berge’s 1958 book \textit{Théorie des Graphes et ses Applications}\textsuperscript{259} (Theory of Graphs and its Applications). Berge, an advisee of French new math instigator André Lichnerowicz, followed König’s steps (the only precedent that he referenced) and introduced the topic through the abstract language of set theory. This was a deliberate effort to strip graphs from their “diagrammatic” nature and present them in a modern way. Berge opened his book with the observation that diagrams connecting discrete points that represented “people or places or

\textsuperscript{253} Ibid.
\textsuperscript{254} Ibid.
\textsuperscript{255} Ibid.
\textsuperscript{256} Ibid.
\textsuperscript{257} Ibid.
\textsuperscript{258} Ibid., 49.
atoms\textsuperscript{260} with lines or arrows that represented “kinship relations or pipelines or chemical bonds” were “met with everywhere […] under different names.”\textsuperscript{261} Examples were “sociograms (psychology), simplexes (topology), circuit diagrams (physics, engineering), organizational structures (economics), communication networks, family trees, etc.”\textsuperscript{262} which in essence were all graphs. Berge declared the articulation of graph theory “in a formal and abstract manner”\textsuperscript{263} (through set theory) necessary in order for it to be “usefully applied in all these different domains.”\textsuperscript{264} Such applications extended from disciplines studying physical and social phenomena (Berge listed the behavioral sciences, information theory, cybernetics, games, transport networks, etc.)\textsuperscript{265} to “abstract discipline(s)” within mathematics, such as set theory or matrix theory.\textsuperscript{266} Graph theory, in other words, pointed simultaneously toward the “applied” and the “pure”; the “empirical” and the “rational”; the “concrete” and the “abstract.” This duality was crucial for its dissemination.

The 1960s were marked by heightened interest in graph theory. Although graph theory was often taught as part of topology or set theory, either in university or at schools,\textsuperscript{267} a growing number of textbooks became available to students who were being encouraged in self-directed

\textsuperscript{261} Ibid.
\textsuperscript{262} Ibid.
\textsuperscript{263} Ibid.
\textsuperscript{264} Ibid.
\textsuperscript{265} Ibid.
\textsuperscript{266} Ibid., x.
\textsuperscript{267} For example, in the SMP report 1962–63, the Book 2 on Topology listed as subjects:
“Lines, closed curves, networks, surfaces approached through concrete problems, house water systems, maps (one-to-one correspondence), etc.
Map colouring.
Incidence matrices to describe networks, roads, communication s, etc. Study of networks; odd and even nodes.
Konigsberg-bridge type of problems.
Number of intersections of 1, 2, 3, … lines is 0, 1, 3, …”
(Thwaites, The School Mathematics Project: The First Ten Years, 25).
The subject enjoyed vast popularity in disciplines that turned to structuralism in search of scientific rigor. Persuasive preachers, such as Harary or Ore, capitalized on the cultural climate of structuralism to highlight the benefits of graph theory. Graphs remained faithful to the tenet of studying relations instead of properties of objects, an idea carrying cultural connotations of modernization and unification. In essence, graphs were structures to study structures that could be reasoned about and with mathematically. Yet, as Harary incessantly campaigned, their geometric appearance offered advantages of accessibility and congeniality that were missing from, for example, set or matrix theory. Graphs made structures and relations visible and workable. Especially for architecture, traditionally entrenched in geometry and cultures of seeing, the Janus-faced graph theory fostered hopes of reconciling “intuition” and “rationality”—terms consistently deployed by this story’s actors.

The following chapters examine how architectural problems became translated into graph theoretic terms, and how these terms, in turn, transformed the way architects looked at and theorized these problems. As the studies of specific engagements with graph theory will come to show, the aspect that gave graph theory prevalence over other mathematical techniques was precisely its visual perceptibility and its manipulability. Ironically, it is the concreteness of seeing and drawing, which architects enlisted graph theory to ostracize from their discipline in the first place, that made it popular. Concreteness was intellectually scorned but practically necessary. This ambivalence toward seeing and drawing is the first line running through the studies that follow.

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268 Thwaites associated SMP with “want[ing] to encourage in pupils at all levels an enthusiasm for mathematics, a readiness and ability to use the full range of their knowledge when faced with novel problems, and a willingness to make and codify discoveries for themselves,” a demand that he framed at Southampton as “Encouraging pupils to refer to suitable books in the mathematical library as well as to their own textbooks, will not only increase their interest but also help to prepare them for the transition to the university.” (Thwaites, *On Teaching Mathematics*, 21).
Graphs not only exposed such ambivalence, but fostered hopes for resolving it by cultivating a new regime of seeing and subjectivity that I call “intellectual vision.” This is the second connective thread of this story. Architects saw in graphs the possibility of a new kind of vision purified from the subjectivities of perception and underpinned by rational structures—a vision modeled after the graph, whose abstract structure underpins its geometric appearance.

Third, despite subscribing to disparate agendas, architectural theories developed on the basis of graph theory involved the same kinds of operations: mapping and matching structures of dissimilar entities (isomorphism), combining discrete elements to produce possibilities of physical form (combinatorics), and anticipating the performance of physical forms for a number of requirements (predictivity). The same operations were promoted either under the cloak of rationality and scientific certitude or intuition and freedom of choice, sometimes in the work of the same architectural theorist. Depending on the ambience of the theoretical claims, authors respectively emphasized the abstract/structural or concrete/geometric aspects of graph theory, always with the reassurance that the former would subjugate the latter. The studies that follow end with graphs concealed behind the computer screen as a natural and unproblematic framework for designing, hiding their structuralist biases from sight.
Chapter 3: Christopher Alexander and the Center for Environmental Structure

3.1. Of Innocence and Experience

O Rose, thou art sick:
The invisible worm,
That flies in the night
In the howling storm,

Has found out thy bed
Of crimson joy;
And his dark secret love
Does thy life destroy.

— William Blake, “The Sick Rose,” In *Songs of Innocence and of Experience*, 1794

As the anecdote goes, somewhere between 1967 and 1974, beat poet Allen Ginsberg performed a musicalized version of William Blake’s 1794 poem “The Sick Rose” in front of a large crowd at the University of California (UC) Berkeley campus central plaza. The spiritual take on Blake’s pastoral elegy resonated with the late 1960s Berkeley climate of protest against technocracy and the alienating forces of modernity. Not with dissimilar connotations, the poem also figured as the lyrical analogue for a Berkeley-born architectural “treatise” that

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Ibid., 21.

“The Sick Rose” was Plate 39 of William Blake’s *Songs of Innocence and of Experience Showing the Two Contrary States of the Human Soul*; an illustrated collection of poems originally published in two rounds, with *Songs of Innocence* appearing in 1798 and *Songs of Experience* in 1794. The collection juxtaposed an idyllic pastoral state with the destructive powers of industrialization and their resultant pathogenies. “The Sick Rose” was mourning the loss of a protected, rural world, under the lingering threat of modernizing forces.


Saunders characterized *A Pattern Language* as “the most read architectural treatise of all time.”
promoted “an entirely new attitude to architecture and planning.”

A Pattern Language was published in 1977 after more than a decade of development and testing by the Center for Environmental Structure, a non-profit corporation co-founded in 1967 by Austrian-born architect and UC Berkeley faculty member Christopher Alexander along with fresh-out-of-college architects Sara Ishikawa and Murray Silverstein. As Silverstein would later describe it, the publication was the result of a quest for “an evolving system of rules that [...] form a shareable base of knowledge for designing at all levels of scale—from chairs, to buildings, to cities.”

A Pattern Language featured 253 “patterns,” instructions for designing parts of the environment, with each pattern including rules for combining it with other patterns. Although the definition, description, and combination of patterns were markedly characterized by formal rigor, the book was addressed to lay audiences and did not require knowledge of mathematics. Because of its accessible format and underlying formal ideas, A Pattern Language gained popularity among amateur designers and homeowners, was emulated in software design,

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274 Ibid., ii.
276 The theory behind A Pattern Language was presented in its “sister” book The Timeless Way of Building (Alexander, Christopher. 1979. New York: Oxford University Press). As I will discuss in this chapter, this theory was largely based on Alexander’s mathematical explorations since his doctoral studies in the Harvard Graduate School of Design in the late 1950s.
277 The “History, Aims, and Goals” page of the Pattern Language official website (https://www.patternlanguage.com), launched in 2002, states that the website is addressed to both professional and lay audiences: “This website has been created to allow all people: homeowners, architects, builders, planners, and others, to design their own houses, to design large buildings, streets, neighborhoods, and gardens, in a way that will enhance the earth.” In a joint review of Christopher Alexander’s 2002 work The Nature of Order and of A Pattern Language, Harvard Design Magazine editor William Saunders reported that, according to a patternlanguage.com staff member, the website was being visited primarily by homeowners and builders as opposed to professional architects. Source: Saunders, William. 2002. “Ever More Popular, Ever More Dogmatic: The Sad Sequel to Christopher Alexander’s Work.” Architectural Record 190 (5).
278 Ward Cunningham and Kent Beck were the first to apply the “pattern” idea in software programming in 1987 for the design of a semiconductor tester using the object-oriented language Smalltalk. Subsequent efforts by others in this direction were consolidated in a workshop organized by computer scientist Bruce Anderson in the OOPSLA 1991 conference. OOPSLA stands for Object-Oriented Programming, Systems, Languages and Applications and is a research conference of the Association for Computing Machinery (ACM). Among others, workshop participants Erich Gamma, Richard Helm, Ralph Johnson, and John Vlissides further developed programming patterns and published a first catalogue under the title: Design Patterns: Elements of Reusable Object-Oriented Software (1995. Boston, MA, USA: Addison-Wesley Longman Publishing Co., Inc.). For an extensive history of the use of patterns
received skepticism from architectural theorists and critics, was applauded by counterculture, and was celebrated as a systematic tool for participatory design. Featured under an introductory section titled “The Poetry of the Language,” “The Sick Rose” provided not only an exemplar of A Pattern Language’s professed poetic potential, but also a symbol of its social vision. Alexander and his collaborators aspired to reconstruct society by operating on its “mirror structure,” the built environment. Permeated by a “neo-romantic, community-anarchist, structuralist vision for a human city,” as Silverstein would describe it, the book sought to restore within urban environments a kind of serenity reminiscent of the one that Blake portrayed in his Songs of Innocence and whose loss he lamented in his Songs of Experience.

This was not the first time that Alexander was engaging the theme of “innocence.” Some thirteen years before A Pattern Language, Alexander had polemically announced the loss of another kind of “innocence”—one founded on the idea that “design must be a purely intuitive process […] hopeless to try and understand […] sensibly.” His diagnosis of the architects’ loss of “intellectual innocence” and his proposal for a new theory of design that accepted this loss had brought Alexander to the epicenter of 1960s design debates. With a 1964 book publication in object-oriented programming see the pertinent wiki page in the official pattern language website (“History of Patterns.” 2001. http://www.patternlanguage.com/leveltwo/caframe.htm?/leveltwo/..../bios/historyofpatterns.htm.)


280 A Pattern Language was featured in several issues of renowned countercultural magazine The Whole Earth Catalog as a do-it-yourself design compendium (Institute, Portola. 1969. Whole Earth Catalog: Access to Tools. Portola Institute).

281 As we will see in this chapter, A Pattern Language was originally developed and tested largely in the context of participatory design projects, such as the University of Oregon in Eugene campus masterplan or the Projecto Experimental, a low-cost housing project in Lima, Peru.

282 Alexander et al., A Pattern Language, xli.

283 In an interview with his intellectual biographer Stephen Grabow, Alexander reminisced: “There was a general feeling in the 1960s that both society and the environment morrow each other and that if one starts to take the structure of the environment seriously enough one inevitably becomes involved in reconstructing society” (Grabow, Christopher Alexander, 55).

284 Silverstein, Reflections on A Pattern Language, 19.


286 Ibid., 9.
that competed with *A Pattern Language* in terms of influence, circulation, and recognition for its author, Alexander had challenged the conventions of Modern architecture and preached a new kind of mathematically based rationality in design.

According to Berkeley architect and urbanist Roger Montgomery, the *Notes on the Synthesis of Form*, as the book was titled, was the “first manifesto” of a “worldwide movement” to “modernize [emphasis mine] design methods and bring scientific rigor into their (the designers’) ancient craft.” Indeed, as I will subsequently discuss, Alexander’s method was taken up by several architects and planners in the U.K. and the U.S., who deployed it in educational experiments or developed computer implementations. Alexander was a participant of the 1962 Conference on Design Methods in London, an event that inaugurated a series of impactful conferences and symposia that strove to articulate explicit knowledge about processes of designing across their many disciplinary expressions. With Alexander being among the first architects to engage with the design methods endeavor, the *Notes* came to symbolize a quest for rationality and a research ethos that proliferated in Anglo-American architecture schools throughout the 1960s. Along with a rationalizing mandate, the *Notes* popularized new mathematical concepts of graphs, trees, and sets in architectural parlance.

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288 These remarks appeared in a commentary on the CES contributions to the science of design written by architect and urbanist Roger Montgomery and published in the *Architectural Forum*, a journal in which Christopher Alexander published several seminal pieces, including his famous 1965 article “A City Is Not a Tree.” Montgomery was West Coast correspondent for *Architectural Forum* and sympathetic to Alexander’s and the CES’s activity and aspirations, although he would later part ways with Alexander (Grabow, *Christopher Alexander*, 179–180). A member of the UC Berkeley faculty since 1967, Montgomery had been working on instituting educational bridges between the departments of architecture and city and regional planning where he was jointly appointed. Previously, he had put to work an urban viewpoint on housing as first urban designer of the U.S. Housing and Home Finance Agency and urban design officer with the U.S. Urban Renewal Association. The citation for the remarks quoted here is: Montgomery, Roger. 1970. “Pattern Language – The Contribution of Christopher Alexander’s Centre for Environmental Structure to the Science of Design.” *Architectural Forum* 132 (1): 52–59, 52.
In a review of the *Notes*, Chinese-American geographer Yi-Fu Tuan synopsized Alexander’s endeavor as the introduction of a “logical structure made up of mathematical entities.” He showcased this structure with an illustration taken from the *Notes* [Figure 1, Figure 2]. This depicted two tree-like figures with downward- and upward-pointing arrows, respectively captioned “program, consisting of sets” and “realization, consisting of diagrams.” From a mathematical perspective, these figures were both “trees,” graphs where any two points are connected by exactly one link. These trees were also directives for a rational design process for synthesizing physical forms that meet specific functional or other requirements. The deployment of trees for the analysis and synthesis of design would, Tuan described, come to replace the designers’ “unexamined preferences,” often misleadingly marketed as “inspired intuition.”

At first glance, *A Pattern Language* appears to stand far apart from *Notes* in terms of rhetoric, style, readership, and goals. Where one speaks of rationality, the other speaks of beauty and feeling; where one parades eclectic mathematical vernacular, the other deploys a colloquial informal language; where one speaks to academic and professional audiences, the other seeks to appeal to laypeople; and where one signals technocratic rationality, the other emits much of the countercultural flair of its time. This disjunction sits squarely within a widely circulated narrative about Alexander’s mid-1960s famous rejection of rational design methodologies for not offering

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289 Shortly after the release of Christopher Alexander’s Harvard dissertation in book form, the winter 1964–1965 issue of *Landscape* published a review by Chinese-American geographer Yi-Fu Tuan titled *Notes on Computer Architecture*—a play on the book’s original title *Notes on the Synthesis of Form*. At that time, Tuan was still a young professor at the University of New Mexico and some years away from enjoying the international recognition brought by his ideas on “humanist geography.”


291 Ibid.

292 Ibid.
any useful insights in how to design a building—a rejection that has been likened to a father abandoning his offspring. Alexander’s ambivalent relationship with rationality has resurfaced several times in subsequent interviews and oral histories. From the perspective of rhetoric, Alexander is arguably a par-excellence case of what I have been referring to as “the participatory turn”—the shift of research efforts from controlling the outcomes of a design process and the subjects it implicates, to the design of processes that foster choice, subjectivity, and open-endedness. It is precisely this apparent break in rhetoric between Notes and A Pattern Language that makes Alexander’s technical practices worth examining. By focusing on the techniques supporting Alexander’s theories, it is possible to escape the strong authorial tone that characterizes his writing and to seek continuities and breaks in his body of work that go beyond his personal claims and intentions.

In this chapter I trace the mathematical entities and concepts that Alexander put to work, from the time he enrolled as a PhD student at Harvard University in the late 1950s, to the publication of A Pattern Language in the late 1970s. I identify operations of mapping, combining, and anticipating in the theories and methods that Alexander put forward, and discuss how ideas such as isomorphism, combinatorics, and predictivity were rendered in his theoretical statements. I also examine the role that these ideas played within the institutional settings in which Alexander operated: how isomorphism, for example, opened the way for interdisciplinary

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293 One of the pioneering figures of the design methods movement, Alexander soon after came to disassociate himself from the field. “There is so little in what is called ‘design methods’ that has anything useful to say about how to design buildings,” he wrote, “that I never even read the literature anymore … I would say forget it, forget the whole thing” (Alexander, Christopher. 1971. “The State of the Art in Design Methods.” DMG Newsletter 5 (3): 3–7). Another well-known article expressing skepticism of the design methods idea was Alexander’s interview to Max Jacobson that was published in Architectural Design in 1971 under the title “A Refutation of Design Methodology” (Jacobson, Max. 1971. “A Refutation of Design Methodology.” Architectural Design 42: 768–70).


collaborations and translation of concepts from one domain to the other; or how the idea of combining independent components (atoms) enabled Alexander to negotiate between basic and applied research and solicit pertinent funding within the context of the research university.

The chapter draws from primary published work, scholarship on Alexander, oral resources, and archival material. It combines Alexander’s seminal book publications and his extensive corpus of scientific articles with biographies and dissertations written on him and his activity. It also builds on personal narratives of his and the CES’s activity, as rendered in Alexander’s many interviews, as well as an oral history interview that I conducted with CES cofounder and A Pattern Language developer Sara Ishikawa. Of particular value in shedding new light on Alexander’s story were the archives of Serge Chermayeff, who served as a member of his doctoral committee at Harvard. Personal letters, PhD progress reports, and research proposal drafts found in Chermayeff’s archives provided new insights on the making of Alexander’s theories. I tell the story of the graph’s trajectory in Alexander’s work against a shifting context of design methods-related organizations that echoed, resisted, or developed his methods and theoretical ideas. Despite his vocal rift from design methods traditions, Alexander’s work remained a steady reference in conferences and publications on the subject. Apart from absorbing and recasting the outputs of Alexander’s research, design methods endeavors offered Alexander a convenient register against which he promoted the technical, intellectual, and ethical superiority of his proposals.

In this chapter we will see species of graphs competing, expanding, and replacing each other. Although not always equally pronounced, “trees,” “cliques,” “simplices,” “semi lattices,” “cascades,” and “networks” marked different “eras” in Alexander’s body of work. At times, the symbolic significance of these graph varieties outperformed their operational function. In other
words, Alexander used the general structural characteristics of these varieties (for example, if the graph is hierarchical or not, how connected its points are, etcetera) to signpost both conceptual and ideological changes in his work. Alexander saw the graph as both an instrument and a schematic representation—a diagram—of his theory’s shape. Other scholars have picked up on Alexander’s flirtations with different mathematical structures, and have used these structures as categories for parsing the evolution of Alexander’s thinking. However, such work has so far treated Alexander’s invocation of mathematical structures in almost exclusively metaphoric terms. There has not been much discussion of how different graph varieties came onto Alexander’s radar, the assumptions that made the one or the other kind of graph appear congruent with his theoretical statements, or the ways in which the graph’s mathematical properties contradicted Alexander’s theoretical intentions—for example, his claims to freedom, openness, fluidity, and multidimensionality. In this chapter, I turn the analytical lens to these questions. Ultimately, I argue that even when they were not foregrounded, as was the case in *A Pattern Language*, graphs lingered behind the scenes of Alexander’s theories, undermining the visual polyphony of the theories’ concrete, empirical aspects and standing at odds with their emancipatory visions. To circle back to Blake, Alexander’s enlisting of structural concepts and graph theoretic techniques to obliterate the “invisible worm” of modernity compromised the “life” he so eagerly strove for—*And his dark secret love, Does thy life destroy.*

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296 For example, in her 2014 doctoral dissertation Molly Steenson used “trees,” “semi lattices,” and “networks” as analytical categories for the development of Alexander’s theories, with a focus on the architectures that he devised for organizing information and their mutually constitutive relationship with architecture theoretical debates. (Steenson, “Architectures of Information”).
3.2. Notes, HIDECS, and the Search for Rationality

Mental Preparation

In his 1983 intellectual biography,297 Stephen Grabow has colorfully detailed Alexander’s tortuous trajectory toward architecture. Other scholars298 of Alexander’s work have since retold the story of a young mathematical prodigy educated at the renowned establishment of Oundle, who countered his school’s long-standing science orientation in pursuit of his architectural inclinations, much to the dismay of his father, who dismissed architects as disreputable and idiotic.299 The story continues with Alexander’s compromise to study mathematics at Trinity College, Cambridge, as the “most fundamental mental preparation for any other activity”300 and the coupling of an intensive and specialized two-year mathematical education with a self-initiated instruction in aesthetic theory. It then moves to Alexander’s enrollment to the Cambridge Department of Architecture in 1953 and his quick and violent disillusionment about the discipline’s disorganized status and pervasive arbitrariness, especially with respect to what Alexander’s quest of a precise method for attaining the elusive and philosophically muddled ideal of beauty in buildings.301 The story fast-forwards to 1958, with Alexander hurriedly completing his architecture degree and fleeing Cambridge in hopes of escaping the “absurdity”302 that he had encountered in what was presumably one of the leading institutions of Modern architecture. After rejecting a PhD in aesthetics under the advisorship of British logical positivist

297 Grabow, Christopher Alexander.
299 In conversation with Grabow, Alexander spoke of his father’s mandate to study chemistry and his blatant disappointment with Alexander’s choice to pursue architecture. (Grabow, Christopher Alexander, 29).
300 Ibid.
301 When asked about his motivations from journalists and historians, Alexander consistently portrays his work as a life-long quest for “what makes things beautiful” (Ibid., 29). During his architectural education in Cambridge he saw this question answered by means of arbitrary judgments and nonsensical explanations. In conversation with Grabow, he likened his architectural education with the experience of being in an insane asylum (Ibid., 30).
302 Grabow, Christopher Alexander, 31.
philosopher Alfred Jules Ayer and a post in the London Building Research Station, Alexander found the “other” Cambridge.

This transatlantic transition initiates the current story. The story starts with Alexander accepting a position in the Harvard Graduate School of Design’s newly announced PhD program to continue the work he began in the U.K. on the relationship between the dimensioning of industrialized housing components and human perception. His pre-Harvard explorations were presented in an article published in the *RIBA Journal* under the title “Perception and Modular Coordination.”

The paper is usually mentioned in passing among Alexander’s publication achievements that came before *Notes*. However, the topic of perception and modular coordination is significant for this story’s unfolding in three key ways.

First, as the topic of Alexander’s original thesis proposal, it gained Alexander a scholarship to study at the GSD.

Second, by falling in the interstices of the human perceiving subject and industrialized building, the topic connected Alexander with two formative collaborators at Harvard who would eventually become members of his dissertation committee: cognitive psychologist Jerome Bruner, who pioneered research in human cognition and perception, and Chermayeff, who extensively theorized on technology and the organization of the built environment. Third, the way that Alexander approached the topic formed preparatory ground for ideas that would come to define Alexander’s work: the turn from the arithmetic basis of modular coordination to structural mathematical ideas as a way of casting a new look to key issues in Modern architectural theory; the declaration of form as a structural concept; and the adoption of structural mappings as a technique for establishing correspondences between divergent contexts and realms. For example, in this paper the concept of isomorphism established correspondences

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304 Grabow, *Christopher Alexander*, 32.
between physical things with subjective perceptions. Soon after, Alexander would extend the structural mapping idea to bridge the analysis of programmatic requirements with the synthesis of the form designed to satisfy them. Later, he would argue for isomorphisms between the built environment and social situations. The fundamental assumption that underpinned such mappings was that design was about “forms,” entities with structure and organization, as opposed to structureless and ambiguous shapes.  

**Forms, Not Shapes**

In “Perception and Modular Coordination,” Alexander sought to devise an operational and workable definition of the commonly accepted principle of “visual order,” and to explain the visual appeal of using certain modular dimensions in industrialized building. Focusing on the mystified “golden section,” Alexander countered a growing number of arguments about its arithmetical attributes, simplicity, dynamism, naturalness, or formal properties. He debunked well-circulated explanations of the golden section’s aesthetic value found in Le Corbusier’s *Modulor*, Romanian polymath Mathila Gkyka’s *Geometrical Composition and Design*, or American artist Jay Hambidge’s *Dynamic Symmetry*, to name a few, either by exposing their

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306 In Alexander, the term “shape” had both a literal meaning as two-dimensional geometric shape and also a broader metaphorical meaning as perceptual appearance, in the sense that Chermayeff used it in his “Let Us Not Make Shapes, Let Us Solve Problems” paper that I discuss in the Introduction.

307 In an interview with Stephen Grabow, Alexander narrated studying proportional systems and modular coordination (Ibid., 32) alongside psychological theories (Ibid., 30) as part of his quest for “how to make something beautiful”—something that the “dry” field of aesthetic philosophy did not seem to address (Ibid., 30).

308 Ibid.

309 Debunking Jay Hambidge’s claim that the golden ratio is a “dynamic” as opposed to a “static” ratio, Alexander wrote: “When we see, we see, not a rectangle with the mathematical properties that (1-root 5)/2 has, but a shape whose sides are in a ratio somewhere between 1.6 and 1.65” (Ibid.)


use of pseudo-mathematical vocabulary for purposes of deceiving the public or revealing fallacies in their reasoning.

The visual appeal of modular systems such as the golden section, Alexander argued, was structural: It had to do with “relations between the parts,” or what he called “patterns.” 310 “It is the sizes of these components that characterize the patterns,” he wrote, “And it is the relations between the sizes that is responsible for what we have called ‘order.’”311 The aesthetic appeal of such “patterns,” he argued, stemmed from a fundamental principle that had grown out of new research in the psychology of perception: “that whenever an object is perceived, its form re-occurs somehow in the nervous system,” or, in other words, the form of the physiological configurations in the brain is isomorphic to (structurally analogous to) [emphasis mine] the form of the object.”312 The building of such cognitive isomorphisms, Alexander argued, was energy consuming, and therefore the “lazy” human brain preferred forms that made this mapping less effortful.

Alexander was pointing to research in gestalt perception, initiated in the 1920s by psychologists Kurt Koffka, Max Wertheimer, and Wolfgang Köhler at the University of Frankfurt and disseminated through their subsequent teaching and research activity in the United States and in Europe. Gestalt perception was a quintessentially structuralist endeavor, as it studied how combinations of simple perceptions led to the emergence of meaningful wholes. Isomorphism was a central and controversial concept in gestalt perception that was used to identify and represent mappings between the phenomenal and psychophysical world (à la

310 Alexander, “Perception and Modular Coordination,” 425.
311 Ibid., 428.
312 Ibid., 425.
Köhler), between the geographical and the phenomenal world (à la Wertheimer), or among all three domains.313

The conceptual and historical nuances of the gestalt debate on isomorphism were not particularly crucial for Alexander. As a process of structural matching, however, isomorphism was a crucial opportunity that resolved Alexander’s anxieties: It established a mathematically describable link between the physical world that was the purview of the designer and the subjective percepts of architecture’s inhabitants. This link, in turn, enabled “objective” directives for the production of the physical world, to meet certain requirements (in this case, visual order).314 As Alexander wrote in closing the paper: “[...] our faith in the visual order we produce will no longer be mysterious, but may be in some measure understood.”315 The mathematical description of a form’s structure also opened communication pathways and alliance possibilities among previously disparate professional capacities. In the case of modular coordination, structural mathematical ideas316 served as common ground for addressing both the interests of the manufacturer, who was interested in the maximum combinability of industrialized components, and of the architectural theorist, who was interested in selecting and repeating a small number of component combinations so as to convey a sense of rhythm.317

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314 The development of a mathematically describable link between perception and building component dimensioning also demystified, explained, and operationalized the visual appeal of the Modulor series, the Miesian Farnsworth dimensioning, the regular building grid, and the 4-inch module, established by the British Standards Institution and the British Building Research Station.
315 Alexander, “Perception and Modular Coordination,” 429.
316 The group theoretic mathematical idea of a commutative additive group, an algebraic structure that used two numbers, a and b, in order to produce a set of linear combinations, alongside some limitations on the maximum values of a and b so that their dimensional relations are perceivable by the human eye, satisfied the disparate intentions of these professional groups. Note that linear combinations of two numbers, a and b, can be produced by multiplying a and b with integers and then adding the results.
317 Alexander, “Perception and Modular Coordination,” 429.
The structural understanding of form and the insistence on operational definitions were key for Alexander’s next steps. The progress reports\(^{318}\) that punctuated Alexander’s meandering route toward his 1963 dissertation “The Synthesis of Form: Some Notes on Theory” offer a glimpse not only into how Alexander’s interests shifted from industrialized housing to urban organization and from visual order to functional organization, but also on how his structural understanding of form linked together a widely diverse set of projects and problems through constant abstractions, mappings, and cross-domain translations.

**Needs, Games, and New Design Representations**

If asked about his dissertation topic circa 1958, Alexander would not have spoken of “misfits,” “requirements,” “conflicts,” “trees,” or any of the concepts that would come to characterize the *Notes*. In an October 1958 doctoral progress report he described his dissertation as “formulation of [a] mass-produced house design procedure as a cooperative game between architect and society.”\(^{319}\) This game would help salvage the architect’s role as “reformer,” “form giver,” and teacher of “visual sophistication”\(^{320}\) against a growingly suspicious public and donors. It would also enable architects to “maximize the […] educational effect on society”\(^{321}\) by teaching them how to avoid “being extreme” and how to “give a little, and take a little;”\(^{322}\) The game was


motivated by what Alexander called “the need for choice.”\textsuperscript{323} This was the need for architects to start taking responsibility for their formal or functional choices, and stop delegating them to a flawed and groundless “logic” or “rationality.”\textsuperscript{324} This co-operative game would be founded upon new mathematical techniques that would allow for operational definitions and evaluations of such choices.

Well versed in mathematics, already with strong opinions about the status and role of the design professions, and immersed in seminars and experiments in the psychology of aesthetics\textsuperscript{325} and in social relations and space, Alexander met applied mathematician and mathematical psychology pioneer R. Duncan Luce.\textsuperscript{326} At that time Luce was pursuing a three-year lectureship at Harvard and was reaping recognition from psychologists and social scientists for his 1957 book \textit{Games and Decisions}.\textsuperscript{327} In the book, which is now a classic in game theory, Luce and his

\textsuperscript{323} Alexander had developed the idea that design inevitably involves choice and arbitrary decisions in a paper on logical aspects of critical theory for art historian Eduard Sekler’s Harvard seminar. This is striking, especially in relation to Alexander’s later quest to develop non-arbitrary design criteria.


\textsuperscript{324} Ibid.

\textsuperscript{325} Alexander attended a seminar and pursued experiments in the psychology of aesthetics with Jerome Bruner. Bruner was a seminal figure of cognitive psychology. In 1960, he co-founded, along with George Miller, the Harvard Center for Cognitive Studies under a National Science Foundation grant. According to Grabow, Bruner first nominated Alexander to the Harvard Society of Fellows, where he became a Junior Fellow in 1962. Alexander described to Grabow his encounter with Bruner: “I went to see him initially because he was interested in perception. He invited me to take some of his courses—which I did—and then very soon afterwards gave me job as a research assistant. I worked on him doing research on various experiments in cognition and also taught a section of his and George Miller’s psychology course at Harvard College. Eventually I was given a lab of my own in the Center for Cognitive Studies” (Grabow, Christopher Alexander, 193).

In his September 1958 PhD progress report, Alexander also mentions meeting with gestalt psychologist Hans Wallach.

\textsuperscript{326} By the time that he met Alexander, Luce had already gained recognition in the field of so-called “mathematical psychology”—a field that uses formal, mathematical, and computational methods to model psychological processes and interactions. Educated at MIT in the 1940s, Luce had come in contact with psychology through the Research Center for Group Dynamics at MIT, founded in 1945 by social psychologist Kurt Lewin. Lewin promoted a dynamic, interactional, and holistic understanding of human behavior going beyond the cause and effect (stimulus response) model of behaviorist psychology and using concepts such as “fields” and forces. Lewin’s students formalized some of his ideas through the use of topology. For a discussion of Lewin’s relationship to Luce see: Erickson, Paul. 2015. \textit{The World the Game Theorists Made}. Chicago, IL.: University of Chicago Press.

\textsuperscript{327} Luce, R. Duncan, and Howard Raiffa. 1957. \textit{Games and Decisions}. New York, N.Y.: Wiley.
co-author Howard Raiffa formulated mathematical models for social negotiation and decision-making. Graph theory prominently figured in the book’s mathematical repertoire, as a representation of a game (the so-called “game tree”), possible moves at each step, and decisions made in the process. Luce had a long-lasting relationship with graph theory, which he had used to model group dynamics and social structures during his MIT doctoral studies. This was at a time that graph theory was still being presented as part of other branches of mathematics, and the only comprehensive book dedicated to the subject was König’s *Theory of Finite and Infinite Graphs*. Despite its close-knit relation to social and psychological questions, some of Luce’s doctoral work resembled gymnastics in pure graph theory. Yet, even work in “pure” graph theory was motivated by possible applications in mathematical psychology. A telling example of such work was Luce’s 1952 publication “Two Decomposition Theorems for a Class of Finite Oriented Graphs” in the *American Journal of Mathematics*. The article discussed a method of decomposing (dividing into component sub-groups) a group of entities linked by relationships. The problem of dividing a graph into sub-graphs based on some property of the relationships between its points was a mathematical one, but could be readily applied in the study of social groups. Graphs and decomposition would come to be central elements in Alexander’s future mathematical toolkit.

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328 Luce and Raiffa were not the first to deploy graph theory in the context of games and decision-making. In their seminal 1944 book *Theory of Games and Economic Behavior* (Princeton University Press), John von Neumann and Oskar Morgenstern also presented elements of graph theory in the context of “extensive-form games”—game specifications representing explicitly the player’s possible moves, decisions, decision-making information, and other important aspects of the game.

329 Erickson, *The World the Game Theorists Made*, 144.


331 A side-note is that Luce later declared isomorphism as one of the most important and general mathematical concepts. This does not indicate direct influence of Luce to Alexander but reveals the potency of isomorphism as a mathematical technique and conceptual practice (Luce R. D. & Patrick Suppes. 1968. Mathematics. In D. Sills (Ed.) *International Encyclopedia of the Social Sciences*. Vol. 10. 65–76 as quoted in Luchins and Luchins, *Isomorphism in Gestalt Theory*, 208).
In his September 1958 doctoral progress report, Alexander documented having consulted with Luce on applications of game theory and linear programming techniques. The purpose of the exchange was to find a solution to the problem of “decision making in house design” and to discuss the possibility of using an IBM machine to plot “utility functions for various domains of decision.” Utility was a key term in rational choice theory and game theory, measuring the satisfaction of different stakeholders for a given choice. Alexander believed that it was possible to find operational ways of defining subjective utility for each player of the game—the designer and the public. He envisioned deploying the game-theoretic formalism as the mathematical core of a new design process consisting of the following steps [Figure 3]: first, collection of information about public needs through questionnaires or interviews; second, use of this information to design an ideal physical form as seen from the architect’s perspective; third, field work to gauge the public’s reactions to the architect’s ideal form and use of the game-theoretic formalism to negotiate choices; and finally, mutual settlement to “a balanced solution”—a happy medium between the absurdity of “cultural democracy” and the “cultural dictatorship.” Alexander emphasized that the architect’s design needed be represented in a way transparently relatable to the information collected and the evaluation mechanisms (i.e., the various utility functions), but did not expand on what kinds of new representations these

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333 Linear programming was a technique for maximizing the utility function for a specific choice.
334 Ibid.
335 Apart from game theorists who grappled with economic decision making, the question of “rational” choices was famously tackled by the then Carnegie Mellon professor of administration Herbert Simon in his 1957 book *Models of Man: Mathematical Essays on Rational Human Behavior in a Social Setting* (New York, N.Y.: John Wiley & Sons), which Alexander cited in his doctoral dissertation.
338 A transparent relation between the representation of a design, the information supporting it, and its means of evaluation was crucial. Without such a system, Alexander warned, designers may continue to “go their own sweet way in spite of the research information” (Ibid., 1).
would be. It would take another three years for Alexander to explicitly use the graph in his explorations, although it is arguable that his familiarity with Luce’s work also implied a familiarity with graph theoretic techniques.

However, the fundamental conceptual step that prepared the ground for the introduction of graph theory in Alexander’s work had already been taken before he enrolled at Harvard. Apart from anticipating the graph, the structural construal of form enabled a next conceptual step that was key for the development and transformation of Alexander’s “cooperative game” idea. This was the mapping between the programmatic requirements of a design and the form developed to satisfy them. Alexander called such programmatic requirements “form-determinants.”

In Alexander’s game, “form-determinants” included both the public needs collected through extensive empirical work and the designers’ aesthetic preferences or other ideals. The identification and documentation of each “form-determinant” was the subject of an entire research program, enlisting various experts, designers, and consultants. Alexander framed this polyphony of “form-determinants” as a corrective to the dominance of singular arbitrary ideals such as “beauty,” “social status,” “structure,” “taste,” “economics,” “function,” or “social structure” characteristic of non-vernacular architectural production. Alexander contended that despite efforts to “educate designers in this total grasp of form-building, the only good architecture that is produced is the work of a few men of genius who happen to have a grasp of

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340 Alexander called such non-vernacular cultures “self-conscious” to differentiate them from “unselfconscious” cultures, where there was repetition and gradual adaptation to the environment (broadly construed to encompass physical, social, and other aspects) without imposition of external design concepts. These arbitrary external concepts governed the making of form and obscured what once was “the whole of man’s existence.” (Ibid.).
everything that matters.” Everyone else, according to Alexander, was drowning in a sea of technical information and an impossible number of irreconcilable public needs.

Form-determinants, Failures, and Interactions

Alexander’s diagnosis and cure for the problems of mass-produced housing found attentive ears in Chermayeff. Chermayeff highlighted the exigency of practically applying Alexander’s ideas in the context of industrialized housing. He also broadened the scope of Alexander’s proposal to the urban scale and the built environment. In a letter following the successful defense of his dissertation, Alexander acknowledged Chermayeff’s influence in his shift toward urban questions. Alexander wrote: “If I ever am lucky enough to get somewhere in the problems of modern urbanism, it will always be because of you—it was you, not Cambridge, who opened my

341 Ibid.
342 Such information was becoming available at growing rhythms as building research became intensified both in academia and in industry in postwar America, with the backing of governmental initiatives such as the 1949 Housing Act. However, this information did not seem to improve the process of design or empower designers to better respond to the numerous demands they were faced with. In his doctoral dissertation Alexander invoked psychological literature pointing to this problem, such as: T. W. Cook. 1937. “The Relation between Amount of Material and Difficulty of Problem-Solving” Journal of Experimental Psychology 20(1): 78–83.
343 After his graduation Alexander attributed his shift of interest to urban issues to Chermayeff, a European émigré who had succeeded László Moholy-Nagy’s presidency of the Institute of Design in Chicago after the recommendation of Walter Gropius, and had joined the faculty at the Harvard Graduate School of Design in 1953 after a brief stay at MIT. Chermayeff was a key figure in reinforcing the urban design agenda put forward by Harvard dean Jose Luis Sert. Former CIAM president and, starting 1953, GSD dean Jose Luis Sert, responding to some of the late CIAM challenges, put forward the new field of “urban design” as a middle ground between large-scale planning and smaller-scale interventions in the level of residence and community—between the planner and the inhabitant. Alongside Siegfried Giedion and British town planner Jaqueline Tyrwhitt, Sert launched the GSD Urban Design Conferences in 1957 and designed pertinent coursework that would result in the institution of an advanced interdepartmental program in urban design first offered in 1960–1961. The preoccupation with the urban scale absorbed the programmatic, technology-oriented, interdisciplinary, and future-centric architectural ethos and disseminated his totalizing vision of design as an activity traversing scales, from objects to buildings to cities, that Gropius had installed during his GSD tenure (1937–1953). It was also in proximity with reformative moves taken by avant-garde European groups such as Team 10. Urban design had a stronger research orientation and was, in that sense, akin to planning, where more systematic research was already underway. Opposite to Gropius, Sert reintroduced history in the Harvard curriculum and started an appreciation and reappraisal of traditional forms of urbanity, which, however, should now be analyzed rationally and their principles extracted.
eyes to the real problems.”

From 1959 and on Chermayeff’s archives include research proposals and research progress reports jointly written with Alexander, which culminated in the co-publication of the renowned 1962 book *Community and Privacy: Toward a New Architecture of Humanism.* The book conceptualized urban organization as a hierarchy of components and subcomponents and presented a formal method, similar to the one Alexander articulated in *Notes,* for designing such components [Figure 4] to account for various “pressures” (the equivalent of form-determinants) and then combining these components to produce desirable hierarchies [Figure 5].

The directionality of conceptual influence in Chermayeff and Alexander’s close collaboration is difficult to disentangle. However, in personal correspondence with Chermayeff in June 1965, Alexander characterized his role as clarifying and mathematizing Chermayeff’s thought. “When we worked together in Cambridge,” Alexander wrote, “part of the little help I was to you, came from the fact that I tried to re-state, more clearly, your own thoughts as you saw them.” This clarifying work, which ultimately transformed Alexander’s early expeditions with game theory to the *Notes,* capitalized on the financial, cognitive, and technical infrastructure of the postwar institutional construct of the university-affiliated, interdisciplinary research center: the MIT-Harvard Joint Center for Urban Studies, founded in 1959.

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According to the faculty and staff listing in the 1967 MIT Alumni Directory, Alexander was associated with the Center from 1959–1960. There, he worked with Chermayeff on a research project called “The Urban House.” The project was the offspring of an effort initiated in one of Chermayeff’s Harvard seminars in 1952 and revisited in 1956 and 1959, to redefine and seek “a vocabulary capable of describing the infinite variety of elements, situations, activities, or events that make up the complex organism ‘house’.” This endeavor was not a first. Alongside architecture’s postwar turn to technology and industrialization came attempts to devise schemes for classifying building components at different scales. The goal of such classifications was to help designers address new technological “forces” or accommodate technological change in their designs. Amongst the most influential efforts in this direction was Knud Lönberg-Holm and Theodore Larson’s 1953 Development Index, which in the authors’ words, sought to “outline the various series of factors involved in development relationships.” In Lönberg-Holm and Larson, “development” stood for “continually perceiving new needs and transforming the various environmental relationships into new forms or patterns of activity that will serve man to ever better advantage.”

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349 Chermayeff and Alexander, Community and Privacy, 152.
350 Knud Lönberg-Holm and Theodore Larson were collaborators of technological icon Buckminster Fuller, and members of the Structural Study Associates (SSA), a group of productivist architects promoting a technology-driven architecture and a scientific, information-based view of design. The group replaced “architecture” with the term “environmental controls” and operated under the motto “Don’t fight forces use them.” Alongside Fuller, they engaged in an effort to develop an evolving catalogue of design information. For an extensive discussion of the SSA’s approach to design and its relationship with information systems see: Strum, Suzanne. 2010. “Informational Architectures of the SSA and Knud Lönberg-Holm.” In Nexus 2010: Relationships Between Architecture and Mathematics. Porto.
352 Ibid., Index Development.
353 Ibid., Ia. Development Goals.
while thinking about how to organize the “factual data” collected in the empirical research part of his proposed design process, but dismissed them as “a little awry.” A similar nagging sense of arbitrariness disturbed Alexander’s attempts to develop other systems of classification. It was not long until he identified the source of his discontent. In 1960, he wrote to Chermayeff:

After going on for a while trying to produce meaningful categories etc, I realised [sic] my one great fault so far. Through I had been talking a great deal about logic, I had not yet used it, put it to work. I began to examine questions of information storage and retrieval (which is a subject closely relevant to ours, with a fairly sophisticated theory), had a number of conferences with members of IBM research teams, and went on as a result of all this to dig far deeper into the logic of our problem. At the same time I kept my eye much more closely on the fact that we have to be able to use [underlined in the original] the system devised in the very near future. The practical problem immediately confronting us is to isolate groups of ‘failures’, areas for research, so that within each one of these limited areas the design problem becomes manageable.

What we had been trying to do was to isolate these groups “by eye” so to speak: a priori; and this is what was wrong [emphasis mine]. I realized that the groups were actually given by the logic of the relations tying our failures to one another — if one only knew how to look for them. And that if we could set the system up suitably, the logic would allow us to extract the groups of failures we wanted, quite NON-ARBITRARILY [emphasis mine].

The concept of “failure” was the precursor of “misfit” that Alexander used in Notes. “Failure” denoted a kind of physical event that prevented a need from being satisfied (for example, sleep prevented by bioclimatic discomfort) [Figure 6]. Because of their construal as physical events, “failures” not only established relationships between “form-determinants” (needs) and aspects of

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354 Without a classificatory scheme, Alexander worried that the data for various “form-determinants” would “dissolve into chaos” (Alexander, “The Design of the Urban House and Ways of Clustering It,” 6).

355 Ibid.


356 Ibid.
physical form, but also established “interactions” between the “form-determinants” themselves. Sometimes failures shared data, other times they were corrected by the same operations, and other times the correction of one failure aggravated the other. Similar relations of overlap, reinforcement, or conflict were then established among the failures’ corresponding form-determinants or, as Alexander also later called them, “requirements.” By considering “the relations themselves, or links” between failures, it would be possible to achieve Chermayeff and Alexander’s main deliverable: “a working programme for design.”

In one of the footnotes of his finished dissertation, Alexander identified the term “program” as deriving from emerging, at that time, literature on the psychology of problem solving to indicate a partial attack on a problem in a stepwise manner. To the interest of scholars who have commented on the parallelism, Alexander also referred to famed British architectural historian John Summerson’s singling out of “program” as the unifying and distinguishing characteristic of Modern architecture. Program, for Alexander, was between architectural reality and mathematical abstraction. It denoted both a structure, representing a logical organization of design requirements, and a process, indicating in what order the designer should address these requirements in order to produce a form whose physical organization would be isomorphic to the design requirements’ logical organization. As

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358 Alexander, “Letter to Chermayeff Re: Failure Cards.”
360 Alexander, “Programme (The Urban House).”
361 Alexander, Notes, 208.
362 In her doctoral dissertation Alise Upitis juxtaposes Alexander’s and Summerson’s “program”: she finds them both reliant on a biological understanding of unity, with Alexander’s version extending the biological to the mechanical and unifying them through the medium of information. Alexander’s program is also different in its sequential nature and the possibility of automatic execution (Upitis, “Nature Normative,” 78).
Alexander first publicly announced in 1961 the mathematical device that would allow designers to “picture”\textsuperscript{364} this structure and enact this process was the “topological 1-complex” or, more simply, the linear graph.\textsuperscript{365}

Problems, Programs, and Their Trees

After completing his Joint Center appointment, Alexander requested $166,000 from the Building Research Institute (BRI)\textsuperscript{366} to pursue a three-year experiment\textsuperscript{367} on the means and effects of correlating information about building with the design problem at hand. His proposal, titled “Information and an Organized Process of Design,” was presented in the Spring 1961 New Building Research Conference of the BRI Division of Engineering and Industrial Research—a biannual “research correlation conference”\textsuperscript{368} bringing together a professionally, geographically, and disciplinarily diverse audience of companies, associations, societies, and individuals broadly engaged with building science. Since 1956, the BRI had issued a comprehensive guide listing “sources of information on research and technical developments in the industry”\textsuperscript{369} with quarterly supplements and annual indexing. The production of this building information compendium raised the question of how to document building science literature effectively and

\textsuperscript{364} Alexander, “Information and an Organized Process of Design,” 117.
\textsuperscript{365} Ibid.
\textsuperscript{366} The BRI was organized in 1952 under the auspices of the National Academy of Science and the National Research Council to encompass “the whole of building research and technology” (BRI, New Building Research 1961, 172) and to liaise between building research agencies internationally. Its membership was open to companies, associations, societies, and individuals. As is generally the case with National Academy of Sciences agencies, funding came from a mix of Congress and federal agencies, the private sector, philanthropic institutions, and individuals.
\textsuperscript{367} The experiment that Alexander proposed to the BRI would be pursued by a five-person group including himself, a librarian, a practicing architect, a communications designer (the equivalent of what we would today call a data visualizer), and a staff member. (Alexander, “Information and an Organized Process of Design,” 121).
\textsuperscript{368} The BRI “research correlation conferences” were biannual events open to the public that sought to combine cross-industry experience in the application of new building products and construction methods or address specific building problems. These were different than the committee, workshop, and round-table activities that happened by invitation only.
\textsuperscript{369} BRI, New Building Research 1961, 172.
efficiently. This figured as the theme of the fall 1959 BRI conference, featuring seminal librarians and information specialists, such as coordinate indexing inventor Mortimer Taube.

The problem posed by the BRI, or at least the way that Alexander interpreted it, had to do with developing a proper organization (structure) on which the abundant knowledge and data about building that were becoming available could hang from. This was a concern that had preoccupied a good portion of his doctoral work. To the arbitrary matching between the organization of building information and the needs of a particular design situation, Alexander counter-proposed “to set up temporary isomorphisms [emphasis mine] between the library’s organization and the cognitive organization of the process.” This necessitated “some logical or mathematical relation between the two classification systems,” the source of which would be “the topological structure of the problem.”

In his BRI proposal, Alexander presented one page with five mathematical figures [Figure 8]. Figure 1 was an entanglement of straight lines, connecting multiple points (nodes). Citing König’s and Berge’s books, Alexander explained that the figures were graphs. The points represented “requirements,” the set of which defined what Alexander called a “design problem.” The lines represented “interactions” among requirements. Alexander continued to suggest that the problem’s abstract structure became visible by considering conflicting relationships between its elemental units (the requirements). These conflicts were mathematically translated into

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371 Although Alexander’s proposal to the BRI was one of optimizing the use of available information for a design process, it was not his main interest. In the post-panel discussion session responding to Texas Engineering and Experiment Station member Ben H. Evans he argued that what was fundamental was not the library organization. “That relation is important only if you must gave the information at your fingertips and must have it in an organized way. The key to aiding the problem solution is the structure of that hierarchy I described, which has nothing to do with the information store.” (Alexander, “Information and an Organized Process of Design,” 124).

372 Ibid., 120.

373 Ibid.

relations that bound different requirements together and helped identify the problem’s “functional units”—subsystems of strongly connected requirements that could be handled separately from other requirements. Figures 2 and 3 showed an isomorphic transformation of part of figure 1’s graph into sub-graphs that revealed the subsystems emerging from the consideration of interactions among the entangled requirements of figure 1. Alexander annotated figures 2 and 3 with hand-drawn circles that in figure 2 indicated the two independent functional units, and in figure 3, an “arbitrary” functional unit or category that designers used by convention. The use of graph theoretic analysis to identify such subsystems would ultimately order the entangled graph of figure 1 into a neat hierarchical structure. Pointing to figures 4 and 5 Alexander wrote:

If we do a complete analysis of a problem’s graph, we get something that looks like Figure 4, a nested system of systems within systems. Each system consists of a densely connected set of requirements. [...] This nest of systems clearly has a hierarchical character. We can make another picture of the same structure which brings out its hierarchical form more obviously, and It looks like a tree. This tree really prescribes the process of design [emphasis mine]. You start at the bottom, solving the simplest systems of requirements, and work your way to the top.

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375 As Alexander had written to Chermayeff in his 1960 update letter a mathematical method made it possible to group requirements non-arbitrarily, in a way that escaped arbitrary and conventionalist groupings imposed by linguistic categories. In his BRI proposal he wrote: “In other words, the various specific requirements which deal with noise problems, though they have a common linguistic none, do not really make a functional subunit of the problem. I can’t explain here precisely how to carry out the analysis which defines these subsystems properly. It depends on a formal, mathematical way of picking out the same entities which we can see by eye in very simple cases” (Alexander, Information and an Organized Process of Design, 117).

376 Alexander disclaimed any polemical attitude against the rationalizing operations of classifying and segmenting. The requirements were so many that they were impossible to address in one single stroke: “This is not to imply that problems of design must be seen as wholes, that you cannot possibly break them apart without interfering with the unity of the creative process. In fact, this would be a very romantic kind of idealism. What we know about cognitive processes and the brain suggests strongly that human beings always tackle complicated problems in pieces, even when they experience the strongest illusions of creative unity” (Ibid.) However, he took issue with the arbitrary nature of the subdivisions: “What I have been working on for some years is the possibility of breaking a problem to pieces which are more intimately connected with its structure [emphasis mine] than arbitrary classifications like ‘acoustics’” (Ibid.).

377 Ibid., 119.
In the Joint Center, Alexander had already started developing a computer program for producing such design programs on an IBM machine. The first fully functional version of a computer program that implemented Alexander’s theory was not developed for architecture, but for application to the analysis of problems in highway engineering. Alexander developed HIErarchical DEcomposition System 2 or HIDECS 2 during his consultancy at the MIT Civil Engineering Systems Laboratory, in collaboration with Laboratory member Marvin L. Manheim.\textsuperscript{378} The computer program was implemented in the IBM 709 of the MIT Computation Center, under the control of the Fortran Monitor System in use at the Center during the second half of 1961. Despite its development in the context of highway engineering, HIDECS 2 was not domain-specific. As Alexander and Mannheim affirmed in the report: “the nature of the analytical methods and of the specifications of the program allow for broad application to other subjects.”\textsuperscript{379} In the report, Alexander and Manheim reiterated the assumptions and definitions of the BRI proposal, namely that a design problem was in fact a relational entity that had a structure; it consisted of requirements (aka points) and interactions between requirements (aka links). The design problem was a graph and the graph represented the structure of the design problem. This isomorphism between a problem and its graph enabled developing an “orderly scheme for dealing with the requirements posed by the particular problem”.\textsuperscript{380} which requirements should be considered together and which should be combined and considered [Figure 9].

\textsuperscript{378} Alexander, Christopher, and Marvin L. Manheim. 1962. \textit{HIDECS 2: A Computer Program for the Hierarchial Decomposition of a Set Which Has an Associated Linear Graph}. Cambridge: Civil Engineering Systems Laboratory Publication 160, MIT.

\textsuperscript{379} Ibid., 3.

\textsuperscript{380} Ibid., 7.
HIDECS 2 took as input a graph, in matrix form (rows and columns corresponding to vertices and 0/1s indicating if the vertices are connected or not) [Figure 10]. It then analyzed the structure of the graph by breaking it down into subgraphs, using a method not dissimilar to the one outlined in Luce’s 1952 decomposition paper. This breakdown or partition was based on information theoretic considerations having to do with the statistical correlation between the variables associated with the points of each subgraph. The program started by making trial cuts of a graph into two subgraphs [Figure 11]. Each time a partition was made, an INFO parameter was calculated on the basis of the number of links that the partition cut and the number of vertices at each side of the partition. The goal was to minimize INFO, therefore minimizing the interaction between the two subgraphs. Because the evaluation of all possible partitions was computationally intractable, Alexander suggested sampling the set of all possible subsets and deploying a heuristic optimization method of hill climbing. The HIDECS 2 output was a tree [Figure 12], which specified “an order in which the designer should consider the requirements he tries to meet in the process of evolving a design.” The HIDECS 2 decomposition method, which made it into Alexander’s dissertation and ultimately into Notes with minimal modifications, was analytical and largely automatic. The output (tree) represented

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381 The IBM 709 supported a word length of 36 bits. This made it possible to completely describe a graph with less than 36 vertices as an array of 36 words. Upitis argues that such pragmatics of programming the IBM 709 defined the list of requirements and number of interactions that Alexander identified in “The Determination of Components for an Indian Village.” This was an example of the decomposition method that he developed in his doctoral dissertation applied in the Indian village of Bavra, where Alexander spent the summer of 1961. The example was presented as an appendix in Notes and, prior to that, as a conference paper in the 1962 Conference on Design Methods (Alexander, “The Determination of Components for an Indian Village”).

382 The “INFO” parameter calculated the information transmitted between the different possible subgraphs that a graph could be partitioned into at each step. This calculation was made under the assumption that each link of the graph represented a statistical correlation between the variables associated with its endpoints. The goal was to choose the partition that would give the least interacting subgraphs or, in other words, the subgraphs that have the least information transmitted between them. This was done through hill-climbing, an optimization heuristic that would become popular in the field of Artificial Intelligence. The program moved toward a direction that improved (minimized) the INFO parameter until no further improvement could be made.

383 Alexander and Manheim, HIDECS 2, 4.
the “logical structure of the problem.” Alexander saw the synthesis of form, however, as a kind of cooperation between the designer and the tree.

Diagrams, Overlays, and the Synthesis of Form

In the 1959 proposal for the Joint Center “Urban House” project Alexander had written: “In many kinds of research the findings lead straight to the answer—given the findings, the result is completely determined. In our research this will not be the case. The design [underlined in the original] comes from the designer.” 384 In the final iteration of his method Alexander still claimed to leave space for the designer. As Tuan remarked in his Notes review: “The logical structure does not prescribe [physical] form; but it does express pattern, order and relations which can then be translated, through processes still largely intuitive, into an orderly complex of forms.” 385 The “program” (tree) allowed the design to be developed step by step, by developing schematic designs for smaller groups of requirements and combining these designs in the order specified by the tree. Alexander called these schematic designs “diagrams.”

The term was also used in Community and Privacy, where Alexander and Chermayeff described “diagrams” as “schematic statements that summarize (visually) the physical implications of the various components of the problem.” 386 These required “total specificity as regards the requirements under discussion, and complete generality as regards pressures not under immediate discussion.” 387 The designer started by developing diagrams for the lower levels of the tree (the simpler subgraphs) and then used the hierarchy of the tree to combine them into “composite diagrams.” The process of combination would be frictionless, as the

386 Ibid.
387 Ibid., 162.
subprograms’ independence would ensure that no “conflicts” among requirements would arise. Because of the way that they were produced, the organization of these “composite diagrams” was also hierarchical. There was a one-to-one matching between components of the problem and the components of the physical form as specified by the diagrams. As Alexander would close in _Notes:_

The hierarchical composition of these diagrams will then lead to a physical object whose structural hierarchy is the exact counterpart of the functional hierarchy established during the analysis of the problem; as the program clarifies the component _sources_ of the form's structure, so its realization, in parallel, will actually begin to define the form's _physical_ components and their hierarchical organization [emphasis in the original].

Both in _Community and Privacy_ and in the famous worked example of determining the components of an Indian village that figured in the Appendix of _Notes_, the process of diagram composition or form synthesis was mainly a process of superimposing component diagrams, without, however, disturbing their independent nature [**Figure 13**]. The component diagrams were still discernible in the composite diagram. Interestingly, the most suggestive experiment about the possibility of visual fusion during the process of combination occurred in a non-architectural context. In March 1962, Alexander and Manheim issued a research report documenting how the design “programs” (trees) outputted by HIDECS 2 could be used in the context of an actual design situation: locating a section of the I-91 Interstate Highway System in Western Massachusetts along a 20-by-10-mile area of the Connecticut River valley. The project

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388 Alexander, _Notes_, 131.
had a footing in a real-world problem, for which a design proposal had already been developed but which the authors did not consult.\footnote{Alexander and Manheim’s highway location project was sponsored in part by the Massachusetts Department of Public Works in cooperation with the U.S. Bureau of Public Roads of the U.S. Department of Public Works. It deployed MIT Computation Center’s computer and associated facilities. The highway’s location had already been determined by the sponsoring organizations, but the authors refrained from consulting the official plan before finishing their analysis. (Ibid., 1).}

This was a “demonstration project”\footnote{The work extended William Little and Brian Martin’s term paper for an MIT course on a set theoretic decision method to defining and structurally analyzing the route location problem, who Alexander and Manheim consulted when developing the project. (Little, William, and Brian Martin. 1961. “An Attempt to Apply a Suggested Set Theoretic Method to the Definition and Structural Analysis of a Route Location Problem. Term Paper for the Course, 1.25-Transportation Route Location (Dept. of Civil Engineering, MIT).” Unpublished manuscript).} meant to illustrate a method “for analyzing engineering design problems” and, more broadly, “to illustrate certain aspects of a new approach to physical design problems.”\footnote{Alexander and Manheim, \textit{The Use of Diagrams in Highway Route Location}, 1.} In the context of highway route location, diagrams were simply “lines and areas on a map.”\footnote{Ibid.} The method for generating such diagrams was based on the evaluation of the “utility” of various locations from the viewpoint of each of twenty-six location requirements that Alexander and Manheim identified [Figure 14]. Unable to calculate the utility of one-dimensional elements, such as lines on a map, they represented the utility of points, with black being the most favorable and white the least favorable from the perspective of each requirement. The process resulted in twenty-six diagrams, or “utility maps,”\footnote{The superimposition of “utility maps” was a retake on a 1957 method developed by Paul Roberts of the MIT Department of Civil and Sanitary Engineering (Roberts, Paul. 1957. “Using New Methods in Highway Location.” \textit{Photogrammetric Engineering} 23 (3): 563–69). Roberts proposed combining photogrammetry data and computer analysis to produce models of the terrain with their points ranked according to various requirements. These rankings were then added to produce an integrated model addressing all the requirements. Alexander and Manheim were critical of the addition process, as several requirements were “non-comparable.” (Alexander and Manheim, \textit{The Use of Diagrams in Highway Route Location}, 112).} each corresponding to one of the “problem’s” requirements. The order by which to combine these maps into a single utility map would be taken care of by the “program”—a tree produced by HIDECS 2 after “interacting” the twenty-six requirements [Figure 15].
However, despite their mathematically stringent generation, which differentiated them from the Indian village diagrams, for example, the twenty-six highway location utility maps could not simply be added to produce a singular utility map. Alexander and Manheim cautioned that each map represented potentially incommensurable utility functions. Furthermore, a simple addition of utility did not account for the properties of a highway as a whole.\textsuperscript{395} “Even if we combine the 26 diagrams in the order which the tree prescribes,” the authors pointed out, “we shall still always hit the same resist if we do no more than add them; we shall \textit{still not overcome the objections to straightforward combinations} [emphasis mine].”\textsuperscript{396} At each level, the combination should emphasize what the authors called the “pattern” properties required by a highway, to ensure that the resulting diagram would also present these new properties.

Although the HIDECS 2 analysis was automatic, the synthesis required seeing and judgment. Alexander and Manheim superimposed the diagrams photographically, projected them on a drawing board, and then sketched over the projection to identify desirable areas in terms of utility, while preserving the configurational characteristics of the highway [Figure 16, Figure 17]. The computer needed a special supplement that was none other than the designer’s eye [Figure 18]. Alexander and Manheim wrote:

While it may be possible in principle to deal with these matters analytically and program them for digital computers, in practice, present digital computer techniques and utility theory are too little advanced to be of much use. […] Of course people have used their eyes and heads before. But the idea that the human eye is a special purpose computer for solving problems of this type, shows us the process outlined as a framework in which the computer can be used intelligently and efficiently.\textsuperscript{397}

\textsuperscript{395} Alexander and Manheim emphasized the “gestalt” [holistic] properties of the highway as guides to the integration of the various utility maps. They wrote: “[…] a highway is an organized entity, and must be treated as such during its design” (Ibid., 90) […] “If the physical entity we design does not have these configurational properties, we can no longer call it a highway, for they are essential to our concept of a highway. The twenty-six requirements, on the other hand, are only ways in which a highway can be more or less desirable” (Ibid., 91–92).

\textsuperscript{396} Ibid., 109.

\textsuperscript{397} Ibid., 117.
Apart from asserting the sophistication of the eye and the inevitability of architectural judgment, HIDECS 2 brought forward some “irritating anomalies” when performing the analysis that identified important flaws in Alexander’s theory. In June 1963 Alexander published a revision of the program, that he named HIDECS 3, which addressed some of the issues. The new program put forward a different conception or definition of the “subsystem.” In HIDECS 2, the system of requirements was a graph represented in the computer by a binary matrix. The subsystem was a subgraph of this initial graph that was produced through decomposition, breaking down the system (graph) each time into two subsystems (subgraphs) so as to ensure minimum information transfer between them. Alexander cautioned that because the decomposition took place step by step by a two-way partition algorithm, the program “had no way of seeing into the future lower levels of decomposition not yet carried out.” This resulted in misclassifications of elements into subsystems with which they shared less information than others [Figure 19, Figure 20]. HIDECS 3 sought to “account for the holistic relatedness of the system” based on mathematical insights from sociometric analysis. Initiated by Austrian psychiatrist and psychosociologist Jacob Levy Moreno as “science of group organization,” sociometry was concerned with the mathematical study of social relations. As such, it was fertile ground for applications of

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399 Ibid., 1.
400 Ibid., 9.
401 Ibid., 3.

Although Moreno characterized sociometry as “science of group organization,” his aspiration was less to gain scientific knowledge and more to make the groups under study conscious of their inner structure and dynamics that produced their form, leading to their improvement.
graph theory—starting with Moreno’s graph-like “sociograms” and continuing with sociometry-related graph theory work by mathematicians such as Luce and Harary.

In HIDÉCS 3, Alexander cited Luce’s and Harary’s graph theoretic work on “clique” detection, which had been published in sociometric journals in the 1950s. Apart from a term for characterizing an exclusive social group, “clique” was another graph theoretic name for a “subgraph” with all its vertices connected. Alexander observed that it was possible to replace such graphs with shapes. He wrote: “For three points all connected to one another, replace them by a triangle, for four such points replace with a tetrahedron. Clearly the vertices of the resulting topological complex are precisely the elements of G, and its edges are the links of G.” Such shape-like constructs defined by a set of vertices were called “simplices.” Alexander suggested a new method of decomposition pursued by searching for “maximal” simplices in a graph [Figure 21]. In other words, searching for triangles or tetrahedra whose number of vertices was not smaller than other triangles or tetrahedra in the graph. The crucial attribute of this method was that a vertex could be part of multiple subsystems. Abandoning the idea of decomposing into discrete (independent) systems, Alexander confessed, led to a “most natural

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403 In Moreno’s method, participants were called to select another member of the group that satisfied a certain criterion. The data collected from this process were represented in the so-called sociogram, a graphic method where individuals were represented as nodes and relations as lines (Moreno, J.L. 1953. Who Shall Survive? Beacon, N.Y.: Beacon House, xix.)

404 Alexander, HIDÉCS 3, 16.


406 Alexander, HIDÉCS 3, 16.
decomposition [...] into subsystems [...] [that] presents no intuitive difficulty”\(^{407}\) and could easily be performed by the eye.\(^{408}\) The output of HIDECS 3 would therefore not be a tree, but a “lattice.”\(^{409}\) Alexander invoked the set-theoretic term rather loosely, to highlight the move away from a system consisting of independent subsystems to one with overlapping subsystems.

The technical concerns outlined in HIDECS 3 had serious repercussions in architectural debates. Soon after the circulation of Notes, Alexander published a bold self-corrective article in which he admitted that he had gotten the mathematics wrong.\(^{410}\) The architecture of humanism that he and Chermayeff were evangelizing did not have the form of a tree. Trees, with their relentless hierarchy and independent subsystems, led to “entirely unsuccessful” results “from a human point of view” such as Levittown, Chandigarh, and the British New Towns.\(^{411}\) Alexander even admitted that the small Indian village whose development he planned during several months in India was organized as a tree.\(^{412}\) The order of the natural city, Alexander claimed, was a “semi-lattice”\(^{413}\) [Figure 22]. Alexander articulated this idea in his seminal article “A City is Not a Tree.” The article was published in a 1965 issue of the Architectural Forum and earned him the Kaufman International Design Award, alongside established architecture critic Ada Louise Huxtable and renowned architectural historian Lewis Mumford.\(^{414}\) In it, Alexander

\(^{407}\) Ibid.
\(^{408}\) Ibid.
\(^{409}\) Lattices are partially ordered sets in which every two elements have a unique upper and lower bound. An upper bound of a set’s subset is an element of the set, which is greater than or equal to every element of the subset. A lower bound is an element of the set that is smaller than or equal to every element of the subset. Alexander pulled the lattice idea from Garrett Birkhoff’s 1940 book Lattice Theory (Birkhoff, Garrett. 1940. Lattice Theory. New York: American Mathematical Society Colloquium Publications). Birkhoff was a Harvard- and Cambridge-trained mathematician who advocated for a modern view of geometry as a form of pure reasoning. “Pure” mathematics advocate and SMSG leading member Marshall Stone was Birkhoff’s doctoral student.

\(^{411}\) Ibid., 58.
\(^{412}\) Ibid.
\(^{413}\) The mathematical concept of the semi-lattice is similar to a lattice, with the difference that every two elements should have a unique upper bound only. Intuitively, this gives the semi-lattice a sense of scalar hierarchy, which, as we will see in the following section, Alexander found useful for his subsequent work.

described the physical fabric of the city as the \textit{invariant} part of a larger living system consisting of a large number of overlapping components. The semi-lattice was a more adequate description of this living system than the tree, because it afforded a “much more complex and subtle structure.”\textsuperscript{415} Alexander argued this mathematically:

We may see just how much more complex a semilattice can be than a tree in the following fact: a tree based on 20 elements can contain at most 19 further subsets of the 20, while a semilattice based on the same 20 elements can contain more than 1,000,000 different subsets. This enormously greater variety is an index of the great structural complexity a semilattice can have when compared with the structural simplicity of a tree. It is this lack of structural complexity, characteristic of trees, which is crippling our conceptions of the city\textsuperscript{416}

[...]
If we make cities which are trees, they will cut our life within to pieces.”\textsuperscript{417}

Alexander put forward the semi-lattice, either as an operational mathematical entity used to develop a decomposition computer program, or as a device for conceptualizing the city, as a stand-in for a holistic structure. This motivated several derivative studies in North American architecture and planning departments, which were marked by a surge of interest in rational design methods in the second half of the 1960s. It also invited rebuttal from architects and planners, who rejected the structural view of the city altogether.\textsuperscript{418} Alexander’s embrace of complex overlap was not a move away from mathematical precision. The semi-lattice’s overlap was mathematically describable and calculable. Not unlike \textit{Notes}, Alexander strove to extract the

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\textsuperscript{416} Ibid., 55

\textsuperscript{417} Ibid.

mathematical principles observable in “natural cities”\textsuperscript{419} and use them to accommodate the functional needs of modern society:

As the relationships between functions change, so the systems which need to overlap in order to receive these relationships must also change. The recreation of old kinds of overlap will be inappropriate, and chaotic instead of structured.\textsuperscript{420}

A novel aspect in “A City Is Not a Tree” was a more explicit argument for a correspondence between physical form (the invariant parts of the system) and social forces, which together made a “living system.” This idea of a socio-physical isomorphism operating within the “living system” along with the assertion that “the overlap must be the right overlap”\textsuperscript{421} would form the ground for Alexander’s work on environmental structure and ultimately the pattern language.

3.3. Relational Complexes, Pattern Languages, and the Prospect of Participation

Basic Projects, Language, Compatibility

In March 1963, UC Berkeley Architecture Department chairman Charles Moore received an enthusiastic letter of recommendation for a young applicant considered for an assistant professorship at the newly established Berkeley College of Environmental Design.\textsuperscript{422} The letter lauded the applicant’s “fortunately unorthodox and inventive” way of applying his “remarkable intelligence” and testified to the ability of his “brilliant,” “analytical mind” and “mathematical intelligence” that would compensate for his young age.\textsuperscript{423} The applicant was Alexander, who, after successfully defending his Harvard dissertation, was orchestrating his second move West.

\textsuperscript{419}“Natural cities” was another term for the products of what Alexander termed “unsconscious design” in \textit{Notes}.
\textsuperscript{420} Alexander, “A City Is Not a Tree,” 55.
\textsuperscript{421} Ibid.
\textsuperscript{423} Ibid.
The recommender was Chermayeff, who, as Alexander’s advisor and collaborator, had witnessed Alexander “acquire an unusual amount of experience in a few years” through his “capacity of work and power of retention.” The profile painted by Chermayeff resonated with the major institutional reforms underway at Berkeley, namely the restructuring of design-related departments into a College of Environmental Design with departments of Architecture, Landscape Architecture and Planning. Alongside this change came an increased demand for systematic research in architecture and landscape architecture that could match the already established research tradition in planning. Alexander’s mathematical background, research experience, and involvement with design in all scales of the built environment aligned with the department’s unfolding agendas.

Alexander joined Berkeley alongside other acclaimed figures pioneering research in design and architecture, with mathematician, theoretical physicist and Ulm Hochschule für Gestaltung former director Horst Rittel being a notable example. Eventually, the theoretical and methodological explorations of Berkeley architecture professors materialized in a one- to three-year graduate curriculum with a “Design Theories and Methods” concentration, including Alexander, Rittel, and others. The second half of the 1960s also found Berkeley becoming a

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424 Ibid.
425 Ishikawa, Sara. 13 October 2016. Interview by Theodora Vardouli.
426 A pioneer of rational approaches to design, Rittel imported techniques from his background in operations research, socioeconomic modeling, sociology, and mathematical logic to promote what he called a “science of design”: the exteriorization of design processes for purposes of communicability, teachability, and improvement via critical scrutiny. (Churchman, C. West, Jean-Pierre Protzen, and Melvin M. Webber. 2007. “In Memoriam: Horst W.J. Rittel.” Design Issues 23 (1): 89–91).
427 In 1966 the University of Berkeley began a new one- to three-year graduate curriculum with a Design Theories and Methods option, which accepted students with a bachelor’s degree in any field. The syllabus included courses on the psychology of perception and communication, problem-solving procedures and prescriptive operations research models, program development and evaluation procedures, methods of architectural research, design methods used in specific environmental problems, and a seminar by Christopher Alexander on environmental structure—an idea that he developed in the mid-1960s. Students entering the program were also encouraged to take some architectural design and city planning courses, and courses in creativity and problem solving, mathematics, probability and decision theory, and operations research. (Moore, Gary T., ed. 1966. “Graduate Studies in Design Theories.” DMG Newsletter 1 (1): 3).
central hub and supporter of North American design methodological activity—the transatlantic counterpart of British design methods. Although Alexander participated in the eponymous 1962 conference in London, and was a key reference in subsequent U.K. events on the subject, he disavowed the design methodologists’ abstract study of design activity (whose main Berkeley proponent was Horst Rittel). Instead, he directed his theoretical and methodological efforts toward issues specific to architecture and the urban environment. Alexander’s orientation, which continued the research trajectories that he initiated with Chermayeff at Harvard, participated in cultivating a focus to architecture and environmental design in U.S. design methods.

Soon after his arrival at Berkeley, Alexander wrote a research proposal titled “Ten Year Program for Research on Environmental Design.”428 There, he outlined his plans for attacking nothing less than the “problem of designing the form of the entire urban environment.”429 “Despite its deeply theoretical hard core,”430 Alexander projected, the program would appeal to a cornucopia of individual agencies and would potentially ultimately acquire the character of a service or consultant organization. The program was a colossal undertaking with the goal “to create, over a period of ten years, a conceptual design for the modern city.”431 This conceptual design would be presented “as an organized assemblage of component subsystems,”432 derived through a definition of harmful characteristics of existing environments (what Alexander had previously called “failures”) and producing forms in which these no longer arise. The program’s goal would be to devise new “components” (physical and conceptual) that would come to replace the conventional building blocks with which designers were shaping “our picture […] of the

429 Ibid., 10.
430 Ibid., n.p.
431 Ibid., n.p.
432 Ibid., 1.
physical environment” and control “our conception of the city.” The new “components” (subsystems) would be subsets of the city or larger environment, where no elements could be added or removed without disturbing their coherence according to some “well-defined criterion.”

“The search, in each individual project,” Alexander wrote, “is for a system of requirements which is so coherent and independent that its implications for the form of the environment can be studied in isolation.” Each of the subsystems, alongside the data about the needs and forces, would be published as a separate book and the data collected in a “permanent information store.”

Apart from feeding into a “central bank of requirements,” each subsystem would also yield a detailed academic course that would remedy the “appalling” lack of substantive knowledge and generality in the teaching of urban phenomena, and would offer specific directions for a three-year PhD. At the same time, the ten-year program would serve as a framework for enlisting other disciplines to address “specific operational questions” pertaining to the requirements (“needs” or “forces”), thus grounding the vague concept of interdisciplinary collaboration. Such collaboration would furnish the College of Environmental Design curriculum with new ideas and qualified personnel, while providing material for new courses. Alexander envisioned the program as a “federation of individual basic projects,” with new “basic projects” emerging every so often from the analysis of the “requirements bank” for new subsystems, using the method that

434 Ibid., 7.
435 Ibid., n.p.
436 Alexander estimated the number of requirements for the “entire urban problem” to be in the order of ten thousand. He envisioned these requirements constantly updated in the course of research: “The list would be in constant flux with items added and removed […] Each basic project, as it creates and revises its own list of requirements, will feed them into this central bank. Further, this bank will be the source of new basic projects.” (Ibid., 13).
437 Ibid., 18.
438 Ibid., n.p.
439 Ibid., n.p.
Alexander had developed during his years at Harvard. Although the research program’s conceptual substrate and the way that the basic projects were derived and linked to each other had its basis on abstract mathematical reasoning, the basic projects themselves would be relentlessly empirical. Alexander wrote:

All the projects will have one method in common: the minute observation of details, aimed at getting a clear picture of the demands, needs, requirements, and stresses which arise in a problem area. Although some formalized methods of observation do exist, it is intensity of observation, and attention to detail which will yield results, rather than exactly defined methodology.  

Basic projects would vary in duration and would each be staffed by one to four full-time members convening in weekly seminar meetings. Each project would also have a geographically dispersed board of expert consultants (the international sages in each field), answering “hard” questions about requirements pertaining to the project. It would also have a director (in other words, Alexander himself) coordinating the work, adding new projects, and ensuring with “a firm hand” the compatibility of all individual projects. For Alexander, such compatibility was key.

In order for the “federation of individual basic projects” (urban or environmental subsystems) to amount to a whole (the “city” or the “environment”) it was necessary to consider their interrelations—the ways in which they could be combined, “fitted together.” Statements about basic needs, requirements, and problems, the format of operational questions, and the conceptual solutions for individual projects should be compatible – “all of a kind.” This led Alexander into a discussion of language. Since his doctoral studies, Alexander had been preoccupied with the communication of information to the designer in a way that would “reduce the hopeless

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440 Ibid.
441 Ibid., 10.
inefficiency of words. In his proposal, Alexander argued that neither discursive natural languages (like English) nor mathematics were adequate in order to “develop thoughts about the structure which the environment requires.” “In spite of many recent attempts to use operations research, linear programming, statistical decision theory,” Alexander wrote, “these techniques were not invented to solve the kinds of problem which environmental designers face.” The best medium for casting thoughts on form were doodles, which he claimed were of little help because of their “unstructured” nature. Alexander observed:

The best medium we have is that of doodles at the edge of our drawing boards. This is a totally unstructured medium, and because this does not itself help guide us towards the design. In the case of natural language and mathematics, the inner structure of the medium itself guides you towards the kinds of objectives you seek. Doodles do not do this. However, doodles, because they are concerned with form, are still better than English or mathematics.

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442 In the “Urban House” proposal, Alexander placed emphasis on how to communicate research information to designers. He wrote: “We might use graphic representation—which does NOT mean just drawing statistics in terms of silly little men, as is so often done. It would require an entirely new approach, that might be worth developing even if it took some extra time.” (Alexander, “The Design of an Urban House and Ways of Clustering It,” 7).


444 Ibid.


Alexander’s solution was a “part mathematical, part visual” language. Recalling his most recent finding in HIDECS 3 and his arguments in “A City Is Not a Tree,” Alexander conceived the elements in the language to be structured as semi-lattices, or other collections of sets “ordered by coherence and inclusion.” This gave rise to an important question: the relationship of the language’s structure to what Alexander called “real world structures.” Alexander here made a case for isomorphism. Although in natural languages, the structure (syntax) had no direct correspondence with the semantic content conveyed or the “real” world that the language described, in mathematics the language was the structure. There were rules for “connecting the internal structure to real world structures” that gave the language’s syntax “tremendous power” as it “actually itself represents the world.” With this in mind he enumerated a series of topics that needed to be addressed in the process of language-building: “rules, internal structure, relation to world, results of using it, problems stated, what structures can it handle that other languages can’t, possible transformations in the language.”

When Alexander submitted the proposal, he had already completed a “basic project” to show as proof of concept: the conceptual design of Bay Area Rapid Transit (BART) District stations in San Francisco. The work was performed as a consultancy to the firm of Wurster, Bernardi & Emmons, founded in 1945 by former School of Architecture dean and instrumental figure for the creation of the UC Berkeley College of Environmental Design William Wurster. Alexander started working on the BART project almost immediately after his move to California, in collaboration with 1963 Berkeley graduate Sara Ishikawa, who had heard Alexander’s “A City Is

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447 Ibid.
448 Ibid.
449 Ibid.
450 Ibid.
451 Ibid.
452 The BART project employed three principal investigators and two assistants. The project ran for eleven months and its total cost was $140,000 (Ibid., 22).
Not a Tree” lecture while in her thesis semester, and Berkeley student Van Maren King. The team was soon after joined by Michael Baker and Patrick Hyslop. Over the course of eleven months the team performed extensive research into and visits to numerous transit systems to collect 390 requirements. These were then “interacted” through HIDECS to produce a “program” (functional analysis structure) on the basis of which the schematic diagrams could be generated and combined. The process proved arduous and of little practical output, as HIDECS ended up breaking the problem down into rather obvious components (subsystems), such as entrances, ticketing platforms, and so on. The sheer labor put into the BART diagrams’ invention and the gradual linking of human behaviors with physical relations cultivated the idea of reusing some of the BART diagrams as generic entities for the design of rapid transit stations irrespective of geographic location or specific requirements.

That arduousness vexed the Notes method (and its HIDECS implementation) was a shared sense by its proliferating users. In 1965 Murray Milne of Yale University and Charles Rusch of the University of Illinois Center for Advanced Study disparaged the method’s tediousness. This stood on the pragmatic side of a wide spectrum of critiques that condemned Alexander’s

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453 In personal conversation, Sara Ishikawa remarked: “I was very taken by Chris’s lecture at the school the summer I graduated (“A City Is Not a Tree”), as it was the first time I heard of a theory that could help answer the question, ‘How should one go about designing something?’” (Ishikawa, Interview by Vardouli).


456 Ibid., 17

457 Alexander identified the BART study as the turning point from the problem-specific orientation of Notes to the generic and reusable character of Alexander’s next research focus: patterns. In Notes, diagrams were developed specifically for one design problem. In the BART study Alexander et al. were faced with the practical challenges of deploying the Notes method for a complex situation such as a transit station, and the impracticality of starting the analysis from scratch for every single design problem. “It did become plausible that perhaps this material could become valuable if it could be used over and over again. So the idea that the diagrams were generic entities playing a combinatorial role—such as patterns do in the current theory—began to emerge as a necessity forced by the reality of that huge situation.” (Grabow, Christopher Alexander, 43).

458 Ibid.
method either for neglecting aesthetic and cultural symbolism or for failing to identify the full spectrum of goals and requirements that would ensure a good fit between physical form and human behavior. Yet, despite its arguable impracticality, Milne and Rusch found the Notes method to have positive value for architectural education. This observation stemmed from a one-year educational experiment, under the support of the University of Oregon and the AIA Education Research Project, during which Milne and Rusch used the Notes method in lieu of a traditional architectural design studio—a fifth-year studio at the University of Oregon and a third-year studio at UC Berkeley, respectively. In a 1968 report on the 1965 “Cal-Oregon Experiment,” as it came to be called, Milne and Rusch argued that the method’s “process-orientation” challenged the Beaux Arts apprenticeship model by “demand[ing], and hence develop[ing], in the student a rigor of thought process, a disciplined approach to thinking, […] extremely beneficial to the student’s education.” They also highlighted its conceptual value in fostering understanding of architecture as generating environmental structure—a statement that would become the core of Alexander’s explorations after the BART study. The method, Milne and Rusch argued, taught its users (in this case architecture students) to “come to grips with the nature of environmental structure [emphasis mine] and the nature of the design process which attempts to create that structure [emphasis mine]” and to begin “see[ing] the environment as a network of physical relations [emphasis mine].” It also accustomed its users to “the concept of multiple levels of organization” that could be decomposed and recomposed, leading to the “exciting” insight that an environmental design problem has a “relational structure.”

460 Ibid.
461 Ibid.
462 Ibid., 26.
463 Ibid., 26.
464 Ibid., 27.
report, Milne and Rusch absorbed the vocabulary with which Alexander set out to reframe his Notes investigations post-1965. From the abstract decomposition of design problems, Alexander moved to the specification of “environmental structure.”

Relations, Complexes, and Environmental Structure

In 1965, having already initiated his ambitious research project, Alexander took a two-year leave of absence from Berkeley for visa-related reasons and temporarily moved to London. At the beginning of his leave, Chermayeff invited him to join the faculty at Yale, where he was newly appointed department head in the School of Architecture. In June 1965, Alexander wrote back declining the position with the excuse of his many open projects and dependent students at Berkeley. He also updated Chermayeff on his ongoing concerns:

I am giving a lot of thought, just now, to the question of integrating all the various systems that make up the whole urban system— and how they interlock. This is particularly important, since no one person can ever work on all of it — and since individuals, and individual projects, must concentrate on parts only, it becomes critical to be sure that all these parts will fit together.466

While in London, Alexander developed “environmental rules”467 for an “urban rule system.” He outlined this system in a paper initially titled “A Regional Structure Skeleton,” which he

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465 Writing from London, Alexander thanked Chermayeff for “stirring up the right kind of hornets nest at Yale,” but refused the offer to join the faculty because of his ongoing obligations at Berkeley: “I can’t possibly take a job at Yale. There are almost twenty students at Cal in the middle of special programs with me, waiting for my return. Eleven of them in a totally new degree course, which I am solely responsible for, so they won’t even get their degrees unless I go back. What’s more, I have a consultant’s firm brewing in Berkeley. Barring some kind of catastrophe, these commitments will keep me there for several years to come.” (Alexander, Christopher. 1965. “Letter to Chermayeff Re: Yale Position Offer.” Box 4, Folder “Alexander, Christopher, 1958–1966.” Serge Ivan Chermayeff Architectural Records and Papers, 1909–1980. Dept. of Drawings & Archives, Avery Architectural and Fine Arts Library, Columbia University.)

466 Ibid.

467 Ishikawa worked with Alexander on entrance rules while in London in 1965–1966 (Ishikawa, “Origins of Patterns & A Pattern Language,” 17). This research was published as "Twenty-Six Entrance Relations for a
presented in Basel under the title “The Coordination of the Urban Rule System.” Soon after his return to the United States in May 1966, Alexander saw design theoretical and methodological activity consolidate under the Design Methods Group (DMG). Founded in June 1966 as a “loose coalition of people engaged in research or application of rational theories and methods of environmental design,” the DMG issued a monthly newsletter that identified and connected endeavors erstwhile dispersed in architecture and planning departments of U.S. research universities. Initially sponsored by the UC Berkeley College of Environmental Design and edited by faculty member Gary Moore, the DMG Newsletter prominently featured work that expanded or revised Alexander’s Notes method and routines of HIDECS 2 and HIDECS 3. For example, in the 1966 Design and Planning Seminars at the University of Waterloo—the DMG’s foundational event—Allen Bernholtz of Harvard GSD and Edward Bierstone of the University of Toronto presented an application of a modified version of HIDECS 3 for the design of a house [Figure 23, Figure 24]. Titled “Computer Augmented Design,” the paper tweaked HIDECS 3 in order to define a hierarchy for recomposing the subsystems that it produced (which in HIDECS 3’s initial version were not ordered hierarchically). Alexander did not participate in the growing community of researchers assembled around his early work. Instead, the impact of his Notes work in U.S. teaching experiments and computer applications provided both attentive ears for, and a state of affairs against which to promote, the new direction of his research endeavors. In March 1967, along with BART project collaborators Sara Ishikawa and Murray Silverstein and a


starting grant from the Edgar J. Kaufmann Foundation, Alexander co-founded an independent non-profit corporation to further develop environmental rules and the system for combining them. Until 1974, with the support of various research grants, the “Center for Environmental Structure” (CES), as the corporation was called, engaged in architectural and urban projects and produced several “pattern languages”: diagrams for environmental “components” and a system of rules for combining them.

The CES activity advanced some of the research intentions that Alexander had presented on his arrival at Berkeley in 1963. The projects that the Center pursued were, in a way, like Alexander’s “basic projects.” Each involved extensive empirical work on some part of the environment. In this respect, they were also continuous with the empirical program of cataloguing design requirements that Alexander described in his PhD progress reports. This empirical element was very much in the background of Notes, but perhaps overshadowed by mathematical analysis. In that sense, Alexander was pursuing versions of the same research enterprise. However, there was one important way in which the development of pattern languages broke from Alexander’s earlier work. In a 1970 commentary on the CES contributions, Berkeley faculty member Roger Montgomery highlighted the “surprising [...] carefully intended shift [...] almost reversal” between the decomposition of a problem into a program, as presented in Notes, and the “combinatorial problems” of putting “pre-designed component images” together.\footnote{Montgomery, “Pattern Language,” 52.} In “pattern languages” the structuring work done by the tree or the semi-lattice was eventually delegated to the “diagrams” themselves. Much of this conceptual shift happened in the U.K. and had to do with a part architectural/part mathematical idea that preceded the pattern: the idea of a “relation” between physical form and human behavior as “atom” of the
“environmental structure.” From 1965–1966, with the research experience of the BART study in his repertoire and his ambitious research program in development, Alexander worked as research architect in the Offices Development Group (ODG) in the U.K. Ministry of Public and Building Works (MPBW). During his research appointment, Alexander worked with architect and social researcher Barry Poyner\textsuperscript{473} to develop the so-called “relational theory,” which was published at Berkeley as a report titled \textit{The Atoms of Environmental Structure}.\textsuperscript{474}

The MPBW was a seedbed for the development of design methods, architectural research, and, in the late 1960s, computer aided design in the U.K. Under the headship of Ian Moore, the ODG had done extensive work on user requirements and outputted a method for architectural programming called “Activity Data Method.”\textsuperscript{475} For each new building the method started by listing desired “activities” connecting them in “link analysis” diagrams (graphs), depending on desired proximity and adjacency relations. Techniques from graph theory were then used to arrange spaces in ways that satisfied these requirements in optimum or sub-optimum ways. Coming out of operations research and management science methods for the generation of optimum architectural layouts, the term “activity” denoted actions taking place in discrete locations.\textsuperscript{476} Since the early 1960s, several landmark studies had used “activity” to optimize the layout of hospitals and factories.\textsuperscript{477} Most of these methods deployed graph theory both to

\textsuperscript{473} Barry Poyner was also working on user requirement studies at the Ministry of Public Building and Works.

\textsuperscript{474} Alexander and Poyner, \textit{The Atoms of Environmental Structure}.

\textsuperscript{475} Broadbent, \textit{Design in Architecture}, 288.

\textsuperscript{476} One of the first papers correlating activity, location, and efficiency was published by Gordon Armour and Elwood Buffa in a 1963 issue of \textit{Management Science} ("A Heuristic Algorithm and Simulation Approach to Relative Location of Facilities." \textit{Management Science} 9 (2): 294–309). In the second half of the 1960s, the paper became a steady reference in design methods and computer aided design papers concerned with the topic of architectural layout.

represent activity patterns (activities and their relationships) and their locations in space. Moore and his collaborators used “activity” as a unit of architectural programming, to replace the more passive concept of “need.” Although Alexander and Poyner also disclaimed the subjectivism and elusiveness of occupants’ needs, they stood critically against ODG’s concept of “activity.” Their criticism stemmed from the observation that “activity” described actions separate from their physical environment and that it did not provide guidance on the generation of physical form—designers could still make “arbitrary” decisions in interpreting the topological guidelines resulting from the “link analysis.”

In the *Atoms* report, Alexander and Poyner posited that “environmental structure” was composed of atomic “relations”: geometrical arrangements that had the property of preventing conflict between people’s “tendencies.”

> “Tendencies,” Alexander and Poyner’s alternative to “activities,” were statements that stemmed from systematic observation of people in physical environments and described the needs that people tried to satisfy when a physical arrangement gave them the opportunity.

The next step after defining “relations” was to investigate their “interlock.”

The BART study, together with subsequent work on relations, helped articulate a new keyword that would play a short-lived but important role in the development of Alexander’s structural thinking: the “relational complex.” The “relational complex” was essentially a concept for matching physical relations between elements with functional relations between requirements in a building. It was based on the idea that each building’s form was undergirded by a set of physical relations that “control the way that buildings work” [underlined in the original].


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478 Ibid., 2.
479 Ibid., 4.
481 Ibid., 186.
Ishikawa, Hyslop, and Baker) and titled “Relational Complexes in Architecture.” The article put forward a theory of relational complexes and used the BART study as a worked example. In an introduction to the article, the journal editor described the work as seeking “to make use in architectural design of the new mathematics of relationship [emphasis mine] and the capabilities of the computer, while at the same time remaining fully cognizant of the complexities and subtleties that are an essential part of all architecture.”

Indeed, in their text, Alexander et al. extensively promoted the “transition from number to structure” and reprimanded other factions engaged with systematic design methods for their bias toward “numerical precision.” Such attachment to numbers “obscure[d] basic relationships” and cultivated a false sense of precision leading to complacency. The authors’ structural polemic is worthwhile quoting in length:

Although it is true that relationships of this kind are present in every building, nevertheless the designers of buildings do not, at present, discuss such relational structures openly. As a result, although the details of buildings may be successful, and the buildings may seem good to look at, the fundamental relationships which underlie their form [emphasis mine] are often wrong.

This potentially damaging preoccupation with numbers is a hold-over from the late 19th-century thought that something was not precise unless you could measure it, a belief current in the days when mathematics and physics dealt largely with numbers and quantities. Today mathematics and the older sciences are more sophisticated. People in these fields have begun to realize that the fundamental nature of things depends far more on relationship and structure than on number and quantity [emphasis mine].

482 Ibid., 185.
483 Ibid., 189.
484 Ibid., 188.
485 Ibid.
486 Ibid., 190
The crux of the article was the establishment of a structural mapping between a “simple physical relation” (positional or topological relation of physical elements) and a functional requirement. If a common physical element was found in two or more requirements, then the relations were characterized as “interlocking.” The “relational complex” was a sophisticated structural entity describing how various “simple physical relations” interlocked. Each “complex” therefore also described a structure’s interdependencies and independencies among the functional requirements to which such relations corresponded, and had “inescapable functional significance”—it was connected (interlocked) enough to serve as a functional unit. The densely connected character of the subsystems identified by Alexander’s theory “guarantees in advance that the solution of its requirements, will be a relational complex not just a collection of relations.”

“Design,” the authors aphorized, “is the invention of relational complexes. We must learn to define them, and to design them.”

In their *Atoms* work, Alexander and Poyner framed relations as scientific theories of sorts, falsifiable statements, contingent on two hypotheses: “1. That the conflicting tendencies do occur as stated, under the conditions specified. 2. That the relation proposed is both necessary and sufficient to prevent the conflict between these tendencies.” The value of “relational theory” was that it could “define design in such a way that the rightness or wrongness of a building is clearly a question of fact, not a question of value.”

Because of such claims of truth, objectivity, and factuality, Alexander’s “relational theory” became a bone of contention in the 1967 Portsmouth Symposium on Design Methods in Architecture, a turning point in the history of British design methods. The event signaled a critique of the scientistic orientation of the early

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487 Ibid., 188.
488 Ibid., 190.
489 Ibid., 17-18.
490 Ibid., 1.
years and called for a recalibration of its goals on the basis of humanistic ideals. Organizing member of the symposium Geoffrey Broadbent framed the event as a confrontation between a behaviorist and an existentialist/phenomenologist stance toward design.\footnote{Broadbent, Geoffrey, and Anthony Ward. 1969. \textit{Design Methods in Architecture}. London: Lund Humphries.}

Although Alexander was absent from the symposium and had already distanced himself from design methodologies, the event largely unfolded as a philosophical and ethical quarrel around the “environmental structure” concept. The rift was between researchers who collaborated with Alexander at the ODG or pursued independent work on decomposition methods and the specification of “environmental structure” and critics of these approaches. For example, Janet Daley, at the time an instructor of social philosophy at the Kingston School and at the Bartlett School of Architecture, dismissed Alexander’s “relational theory” as “grotesquely, and rather dangerously, naive and confused.”\footnote{Ibid., 75.} Accusing Alexander of hypocrisy concealed with incomprehensible jargon,\footnote{Ibid.} she exposed hidden value judgments in his assertion that all “tendencies” are worth expressing and all “conflict” necessitates elimination. In a similar spirit, in their \textit{Architects’ Journal} report on the Portsmouth Symposium, Colin Cave and Keith Elvin described the event as marking a move away from the “strictures”\footnote{Cave, Collin, and Keith Elvin. 1968. “Design Methods: Not Only How But Why.” \textit{The Architects’ Journal} 147: 63.} of “grotesquely oversimplifying problem solving structures”\footnote{Ibid.} which represented a “retreat from reality of design situation.”\footnote{Ibid.} The ignition of these debates found Alexander already having relinquished the objectivist rhetoric of \textit{Atoms}, and incorporated his “relational theory” ideas in the “pattern language” investigations—a research that he promoted with humanistic, participatory, and ecological prose.
Patterns, Cascades, and a Language

The baptism of the “pattern language” is said to have taken place in the summer of 1967, a few months after the establishment of the Center for Environmental Structure. During a one-day seminar in Inverness, California, with a small grant from the Bureau of Standards, hosting members of the National Institute of Mental Health (NIMH), interdisciplinary experts from the East Coast including Marvin Manheim, and Berkeley faculty members, DMG affiliates, and soon-to-be CES staff Sim Van der Ryn and Roslyn Lindheim, it was decided that environmental rules should be recast as “patterns” and their relations as a “language.” The term “pattern” was not new in Alexander’s expeditions. In concluding the Notes, Alexander described the nature of components (subsystems) as both atomic units and structures:

Every component has this twofold nature: it is first a unit, and second a pattern, both a pattern and a unit. Its nature as a unit makes it an entity distinct from its surroundings. Its nature as a pattern specifies the arrangement of its own component units. It is the culmination of the designer's task to make every diagram both a pattern and a unit. As a unit it will fit into the hierarchy of larger components that fall above it; as a pattern it will specify the hierarchy of smaller components which it itself is made of.

However, by 1967, Alexander would come to find this formulation mathematically inelegant and argued that a more mathematically correct and elegant definition would be to speak of patterns

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499 Alexander, Notes, 131.
containing patterns. Along with this idea came another important conceptual breakthrough that differentiated “patterns” from the “diagrams” of Notes. Diagrams were schematic renditions of a specific system’s components. Patterns, however, were universal and reusable: They captured relations that recurred in the environment.

After the BART study, Alexander and his CES collaborators worked on a series of consultancies where they advanced and tested the identification, combination, communication, and application of patterns. One of these projects was the development of a pattern language for multi-service centers in Hunts Point, South Bronx, in the winter of 1967. The project was done in collaboration with Urban America’s director and San Francisco Poverty Program participant Kenneth Simmons and overseen by the New York City Human Resources Administration (HRA). The HRA’s desire to develop a general planning scheme for multi-service centers promoted the prospect of the patterns’ reusability—an idea that had already occurred to CES members after the highly laborious pattern development for the BART stations. Sara Ishikawa and Murray Silverstein developed sixty-four patterns [Figure 25] and connected them

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500 In conversation with Stephen Grabow, Alexander discussed the drift away from units and the shift toward nested relations: “During the early years of the formulation of the pattern language we had a very peculiar problem. We had both ‘things’ and ‘patterns’ which were connecting those things. This seemed like a very inelegant formulation. In discussing this with mathematicians it was intuitively clear to them it would be better if there were ‘patterns of patterns’ rather than ‘patterns of things.’ In 1967 this seemed like a beautiful idea but it did not seem to have any reality. It seemed too abstract. It finally became clear that it was much more lucid to say that there were just patterns. … Things are just convenient labels which we give to bundles of patterns, or patterns themselves. Although this is a pretty difficult thing to realize, it is consistent with modern mathematics and physics [emphasis mine]. In that sense it’s not a surprising development” (Grabow, Christopher Alexander, 45).

501 The Hunts Point multi-service center was done by Alexander, Ishikawa, and Silverstein, with the eventual participation of Max Baker. Berkeley architects Sandy Hirshen and Sim Van der Ryn participated, along with New York firm Gruzen & Partners. (Montgomery, “Pattern Language,” 54).

502 The CES was brought as a third party to a contract between NY HRA and Urban America. (Ibid.)

in a mathematical structure that they called a “cascade” [Figure 26]. The “cascade” was a new take on what Alexander previously called “program” and was represented as a directed graph. The graph’s nodes were patterns and its links identified the order by which the patterns should be considered [Figure 27]. Alexander et al. cautioned that the “cascade” was not to be confused with the language. The cascade was merely a path through the language’s structure.

Let us establish one thing at the outset. The language, and the cascade, are two different things. The language contains far more structure than is captured in the cascade [emphasis in the original]; the cascade is merely a partial representation of the language. However, we shall not discuss the additional structure in this report. Here, we confine ourselves, entirely, to those features of the language which are captured by the cascade.

The language’s overall structure was not discussed in the Multi-Service Centers report, yet the cascade had a somewhat hierarchical order, with bigger patterns considered first, and smaller patterns later in the process. The patterns themselves were rendered in an “if-then” format—where “if” corresponded to a (primarily social) situation, and “then” to a physical configuration adequate for that situation.

An evolution of the Atoms ideas, patterns implicitly followed the motto: “A good environment is one in which no two tendencies conflict.” The elimination of conflict carried emancipatory aspirations: “Life can fulfill itself only when people’s tendencies are running free. The environment should give free rein to people’s tendencies; conflicts between people’s

504 The “cascade” representation of the language was also used for the development of a schematic design for a facility for the Southwest Regional Laboratory (SWRL) for Educational Research and Development in Orange County, California. The project was commissioned to the CES by the large architectural office Skidmore, Owens and Merrill (SOM). CES members Barbara Schreiber, Ron Walkey, Denny Abrams, and Jim Smith worked on over one hundred patterns that were then reviewed by the SWRL, SOM, and CES and ultimately implemented. Their implementation came with a positive appraisal on the decision-making liberty that they afforded the designer by virtue of their non-prescriptive organization (the cascade enabled different paths in the language) and some critiques on their strictly human-oriented content (eliminating conflicts among human tendencies) that left out technical or other. (Montgomery, “Pattern Language,” 58).

505 Alexander et al., A Pattern Language Which Generates Multi-Service Centers, 51.

506 Alexander and Poyner, Atoms, 9.
tendencies must be eliminated." This statement resonated with the NIMH’s concern with alleviating environmental stressors and instilling environmental tranquility. In the summer of 1968 the NIMH funded the CES with a $300,000 grant to develop the pattern language and communicate it to the public. \(^{508}\) The NIMH grant was paramount for the pattern language development. \(^{509}\) It boosted theoretical research and turned the CES back toward the more “basic” aspects of the pattern language research; the language’s structure and the formal definition of the patterns. The NIMH grant also helped staff the CES with graduate students who performed a series of “experiments,” both in between and during the project commissions. These experiments addressed the communication of patterns to users (their format and their level of emphasis and prescription), their level of abstraction (how generic they should be), and their structure (how they should be combined).

A driving question that emerged from the pre-NIMH projects and was developed under NIMH funding was the pattern language’s “generativity.” This is a term that has received ample attention from scholars of Alexander or researchers revisiting his theories in the context of contemporary computational design. \(^{510}\) From a theoretical perspective, a discussion on the topic

\(^{507}\) Ibid., 17.

\(^{508}\) Speaking to Stephen Grabow, Alexander described the NIMH’s assignment as the development of “a complete system of patterns which could be put into the environment that would actually alleviate these stresses and strains.” He continued: “They knew I had developed this way of separating hundreds and hundreds of these issues from one another in such a way that they could be handled independently and built up into a complete environment. They wanted me to develop it, catalogue it, and put it into a published and accessible form so that more and more people could become aware of these issues.” (Grabow, *Christopher Alexander*).

\(^{509}\) Alexander attributed the completion of the pattern language to the NIMH grant: “They saw us though about five years which gave us the opportunity to complete the pattern language. There is no question that without that help we would never have been able to do it because the sustained energy that it took to do that work was ferocious” (Ibid., 94).

was launched by Alexander’s 1968 article titled “Systems Generating Systems.”\textsuperscript{511} In the article he distinguished between “a system as a whole” and a “generating system.” The former was a way of representing a structural property of a thing—one that could only be described by considering relations among parts. The latter was “a kit of parts”\textsuperscript{512} along with their rules of combination, and, as Alexander claimed, was necessary in order to produce holistic systems. He wrote: “If we wish to make things which function as ‘wholes’ we shall have to invent generating systems to create them.”\textsuperscript{513} From a practical perspective, the question about the language’s generativity had emerged from efforts to use the pattern language as a design tool in CES design projects and academic seminars by CES staff at UC Berkeley and elsewhere.\textsuperscript{514} These efforts indicated that the early versions of the pattern language could not produce designs by virtue of the language’s structure. Designers needed to bring in external resources (i.e., professional expertise) in order to go from the cascade into a schematic architectural design.\textsuperscript{515} Alexander saw the language’s generative capacity as being contingent on including the right “sequence” or “order” \textit{within} the language’s structure.

Lay participation in design served as a test for whether the language’s structure allowed for the generation of designs without external interventions by the language’s “users.” Against the backdrop of ramping political involvement in Berkeley, the end of 1960s found Horst Rittel developing systems for argumentative problem solving and the \textit{DMG Newsletter} publishing a growing number of papers and events critiquing the limits of rationality and prediction.

\textsuperscript{512} Ibid., 605.
\textsuperscript{513} Ibid..
\textsuperscript{514} Since his appointment at Berkeley, Alexander ran several courses on the development and use of patterns. Ron Walkey tested the multi-service patterns with students from the UC Berkeley Department of Architecture. Murray Silverstein at the University of Washington and Sara Ishikawa at Berkeley also tested patterns in an educational setting. (Montgomery, “Pattern Language,” 56).
\textsuperscript{515} Grabow, \textit{Christopher Alexander}, 54.
Alexander observed these trends while immersing in the techno-spiritualist speak and a hippie lifestyle of West Coast counterculture. However, in the context of Alexander’s work, lay participation figured less as a political mandate and more as testing ground of the pattern language’s generativity (its ability to contain all the rules necessary for someone to create a design) and motivation for further theoretical and technical development. Between 1969 and 1971 the CES engaged with two major participatory design projects, the Projecto Experimentale\(^{516}\) in Peru (winter 1969), published as Houses Generated by Patterns,\(^{517}\) and the campus planning process for the University of Oregon Campus in Eugene in 1971, published as the Oregon Experiment.\(^{518}\) These projects, alongside many others performed under the NIMH funding, were seen as experiments for refining the overall concept of the pattern language and developing new patterns. Through these projects CES members “began to see how this material could be generalized, made part of a language for an entire urban region.”\(^{519}\)

\(^{516}\) In winter 1969, the CES responded to the United Nations invitation to be the American entry for an international competition for the design of a low-income housing project of 1,500 houses in Lima, Peru. The project asked for a site plan, house plans, and a construction system (Ishikawa, Origins of Patterns, 18). The project began with several CES members, including Alexander, living with families in Lima in order to document their lives and activities. These observations were then taken back to Berkeley, where the CES developed patterns in parallel with specific designs. The competition outcome, although not victorious for CES, was the publication of Houses Generated by Patterns (1969), with a lay audience as its target, and the realization of one cluster of houses (this was done for all competition participants). In her 1984 visit of the CES-designed block Sara Ishikawa found inhabitants “very happy and proud of their home and neighborhood” and having “made additions and modifications to their houses” (Ishikawa, “Origins of Patterns,” 19).


\(^{518}\) In 1971, the CES also proposed a campus planning process for the University of Oregon Campus in Eugene, with the Cambridge University individual colleges’ locations and relations as reference. This was published as the Oregon Experiment (Alexander, Christopher, Shlomo Angel, Sara Ishikawa, Denny Abrams, and Murray Silverstein. 1975. New York: Oxford University Press). In the Oregon campus case, the CES outlined principles for the growth of the campus over time through the participation of its occupants. These principles were: organic order, participation, piecemeal growth. Patterns, diagnosis and coordination. The book’s authors collaborated closely with the campus planning office, university officials, faculty and staff.

\(^{519}\) Silverstein, “Reflections on A Pattern Language,” 17.

Other major projects pursued between the reception of the NIMH grant and the CES’s dissolution in 1974, which fed into the development of patterns, were: an eighty-panel exhibit for the Japanese Pavilion in the 1970 Osaka World's Fair titled “A Human City”; the programming and design for Berkeley City Hall Complex in 1970; and a 20,000-square-feet community mental health center in Modesto, California. The Osaka World's Fair exhibition consolidated work on urban and community patterns developed by CES members and students, and was redesigned by Ronald Walkey. The Osaka Expo material eventually became the “Towns” section of A Pattern Language.
projects are reported as being successful, lay participation was a difficult challenge in the
pattern language’s development. Lay people’s use of the language produced “very sketchy,
rambling designs which did have certain holistic properties but which were not yet clearly
formed buildings.”

Alexander saw the sequencing of the patterns as a mathematical problem. In 1972, he
enlisted his PhD students to describe the mathematical attributes of a simple language for
generating Japanese teahouses consisting of twenty-four patterns. Alexander was confident that
the Japanese teahouse’s sequence was successful, because its patterns were structured in such a
way that when they were presented to someone “that person would then form a complete image
of a teahouse in their mind.” This “correct” sequence was one of twenty-four factorial possible
sequences. Alexander gave his graduate students the problem of identifying what made this
particular sequence of patterns work and if there were other possible sequences that could also
work. In reminiscing about the event he asked: “You could tell intuitively that it was great; but
if one had made a cascade of the type we were studying earlier, what would have been the
mathematical properties that got from that cascade to the correct sequence?” This problem
occupied much of Alexander’s cognitive effort while heading the CES. In conversation with his
intellectual biographer Stephen Grabow, Alexander reported having installed in the CES office a
teletype hookup to one of the largest Berkeley computers, with which he tried to extract

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520 In conversation with Stephen Grabow, Alexander framed experiments with lay people on house design as integral
to the pattern language development. CES members provided patterns to lay people and asked them to develop their
own designs, evaluated the results according to their own criteria (Alexander did not specify what those were in his
interview), and modified the pattern language according to how “satisfactory” they found these results (Grabow,
Christopher Alexander, 96).
521 Ibid., 95.
of Building and the Nature of the Universe. Berkeley, Calif.: Center for Environmental Structure, 303.
523 Alexander’s students made algorithms for obtaining sequences that were “consistent” and “backtrack free.” They
also tested other mathematical and computer (LISP) methods for “obtaining good sequences as a function of
524 Grabow, Christopher Alexander, 95.
sequences from pattern language cascades.\textsuperscript{525} Although in personal conversation Sara Ishikawa had no memory of such computer experiments, Alexander’s anecdotes speak to the desire to mathematically derive a “correct” sequence within the cascade, where the only ordering principle so far was scale. Initially, the goal of such sequencing was an automated navigation from one pattern to the next. Soon after, however, Alexander realized that a book format was most adequate for conveying the pattern’s correct sequence. Alexander narrated:

We put all the patterns that we had available to us at that point into the computer, connected them all up in cascade form, and wrote a whole series of programs attempting to extract sequences that actually followed the mathematical logic of what we believed a correct sequence to be. At that point there were still faint inklings that we would actually be able to offer this service. The way I ran the programs was based on the idea that a person would approach the teletype and punch in something like “house” and perhaps some other patterns and immediately get a language that was based on all those patterns, in the correct order, that would then be able to generate a building. Well, needless to say, we finally gave up all that foolishness because it seemed too cumbersome and not very much to the point. And finally we discovered that, actually, it was possible to lay the patterns out in the form of a book that possessed the correct sequence in the sense that you could skip through the book and you would essentially be using them in approximately the right order.\textsuperscript{526}

This short-lived flirtation with the idea of a pattern language computer program is important, both despite and because of its abandonment. In the early 1960s, the HIDECS program had gained Alexander the reputation of a computer frontiersman—a role that he vocally rejected as early as 1964 in the landmark conference “Architecture and the Computer,” organized by Chermayeff at the Boston Architectural Center. Also subsequently published in Landscape under the title “A Much Asked Question About Computers in Design,”\textsuperscript{527} Alexander’s diatribe chastised growing efforts to “apply” computers to architectural design for being “misguided,

\textsuperscript{525} Grabow, Christopher Alexander, 95.
\textsuperscript{526} Ibid.
dangerous, and foolish.\textsuperscript{528} “There is really very little that a computer can do,” he cautioned, “if we do not first enlarge our conceptual understanding of form and function.”\textsuperscript{529} Alexander’s critique focused on the reductiveness of automatic building layout programs—a prime research focus during the first years of computer aided architectural design. By generating layouts on the basis of criteria that could be “measured and encoded”\textsuperscript{530} (for example, circulation costs), leading to the “ironic (situation) that the very tool which has been invented to unravel complexities imposes such severe restrictions on the design problems it can solve that the real source of complexity has to be eliminated before the tool can even get to it.”\textsuperscript{531} Alexander insisted on a conceptual approach to design during his mid-1960s visiting appointment at the Offices Design Group of the Ministry of Public Building and Works, despite the Group’s growing preoccupation with computer aided design of the quantitative optimization variety. Alexander’s designation of computers as tools for taming design “complexity,” coupled with galloping developments in computer graphics throughout the 1960s,\textsuperscript{532} steered the course for the DMG’s early activity. With the DMG lending attentive ears to Alexander’s endeavors, despite his unwillingness to reciprocate, a computer implementation of the pattern language was certain to have an eager audience. Like HIDECS in the mid-1960s, a pattern language computer program would reposition Alexander at the center of a research scene that he, indirectly, had helped create.

\textsuperscript{529} Ibid., 7.
\textsuperscript{530} Ibid., 6.
\textsuperscript{531} Ibid.
\textsuperscript{532} MIT was a center of computer graphics research in North America. The Computer-Aided Design Project (CAD Project) that ran from 1959–1970 under the joint leadership of Douglas Ross and Steven Coons pioneered developments in graphical communication among humans and computers, and fostered ideas of intuitiveness and accessibility. For more on the CAD project and computer graphics research, see chapter 5. The Harvard Laboratory for Computer Graphics and Spatial Analysis, initiated in 1965, was another key center of computer graphics work, albeit with emphasis on cartography and geospatial information representation.
At the same time, the wake of the 1970s found computers becoming increasingly accessible and computer aided design shifting toward participatory design applications, ranging from culling design “requirements” from the “users” to directly involving them in the design process. The Environmental Design Research Association (EDRA), an outgrowth of the DMG founded in 1968 with the goal to bridge design methods and social scientific research so as to foster a more “holistic” approach to environmental design issues, disseminated research blending information technologies and participatory perspectives. The gradual shift of the computer’s cultural connotations from technocracy to participation and empowerment, which I will discuss in more detail in chapter 5, made a computer version of the pattern language less of a contradiction. Alexander’s preference of a Xerox-able book over a computer program bears symbolic load. The idea was to conceal the mathematical formality on which he grounded the patterns under the congenial format of a “cookbook”—the do-it-yourself architecture manuals popular in the U.S. counterculture. This choice was well received by influential countercultural collectives such as the Whole Earth Catalog group, whose members embraced the pattern language work as delivering ideals of ecology and creative individualism in design.533

A Pattern Language

A Pattern Language was published in 1977 by Oxford University Press, as the second volume of a tripartite series that also included the Timeless Way of Building and the Oregon Experiment. Its main authors534 were the founding trio of the CES, which had ceased its operations in 1974. A

534 Under the names of Alexander, Ishikawa, and Silverstein, the 1977 publication of A Pattern Language also featured Max Jacobson, who had been responsible for construction patterns; Ingrid Fiksdahl-King, who had produced key images and photographic material; and CES collaborator Shlomo Angel. (Ishikawa, “Origins of Patterns,” 19).
Pattern Language was a compendium of 253 patterns, arranged in “straight linear sequence”\textsuperscript{535} by order of scale. In keeping with the aim that Alexander had stated upon his arrival at Berkeley to address the “problem of designing the form of the entire urban environment,”\textsuperscript{536} patterns started from the regional scale and trickled down to issues of house ornamentation. Regardless of scale, the presentation of patterns kept a consistent format [Figure 28]. Each pattern was introduced with a photograph showing an “archetypal example”\textsuperscript{537} of its realization. This was followed by a “context” paragraph indicating connections with larger patterns. Then came a short statement of “the problem” (forces or requirements that the pattern was responding to) in bold, followed by a long explanation of the empirical evidence supporting the problem and proving the pattern’s validity. The problem statement was followed by an instruction for how to solve it, again in bold. This was in essence “the heart of the pattern”: “the field of physical and social relationships which are required to solve the stated problem in the stated context.”\textsuperscript{538} The solution statement was also depicted in diagram form, with labels showing the pattern’s components. The presentation of each pattern closed with a paragraph indicating links to smaller patterns.

Despite the visual concreteness of the archetypal images, patterns themselves were to be taken as highly abstract entities. They were generic structural descriptions of form, “relations,” that responded to forces of various kinds.\textsuperscript{539} In terms of their schematic, abstract, and structural nature, they were not unlike the “diagrams” that Alexander had put forward in his doctoral work.

\textsuperscript{535}Alexander et al., A Pattern Language, xii.
\textsuperscript{536}Alexander, “Draft Sent to Chermayeff,” 13.
\textsuperscript{537}Alexander et al., A Pattern Language, x.
\textsuperscript{538}Ibid., xi.
\textsuperscript{539}In The Timeless Way of Building, Alexander described patterns as a “morphological law, which establishes a set of relationships in space” (Alexander, The Timeless Way of Building, 90). This law had the general form “X \rightarrow r (A, B, ...)” which Alexander explained as “Within a context of type X, the parts A, B, . . . are related by the relationship r”; for example, “Within a gothic cathedral \rightarrow the nave is flanked on both sides by parallel aisles” (Ibid.). Although Alexander did not offer a reference, this formulation seems to draw from work in high dimensional pattern grammars by computer scientist King-Sun Fu, which generated visual images and their descriptions through spatial relation rules (to the right of, above, etcetera) that operated on words and visual symbols (Fu, King Sun. 1974. Syntactic Methods in Pattern Recognition. Academic Press, 68–69).
The difference was that the diagrams were the result of an active interpretation of the designer of a set of requirements, whereas patterns sought to capture, as Alexander et al. contended, an “invariant property [emphasis mine] common to all places which succeed solving the problem.”

An essential characteristic of the patterns was their independence. The principle of independence, which Alexander had entertained from his doctoral work (independent subsystems), reiterated in his proposal for “individual basic projects,” and implied in his discussion of discrete “atoms” that make up the environment, performed multiple functions. First, it implied expandability and replaceability of individual patterns without disturbing the overall theory. This had both practical and ideological implications. From an ideological perspective, it allowed the CES members to portray A Pattern Language as what co-author Murray Silverstein would later describe “a stew,” “a healthy ecology of thought” made up of “falsifiable hypotheses” that allowed for “variety and freedom to be wrong.” The falsifiability of patterns and their potential expandability allegedly made A Pattern Language an “open system” instead of a definitive and authoritarian prescription of how to design the built environment. This resonated with ideals of individualism, freedom, and flexibility that were coming into the mainstream in late 1960s California. The CES cofounder and book co-author Murray Silverstein testified to A Pattern Language’s participation in that climate:

The book captures (or, more accurately, was captured by) a cultural moment: the anti-corporate, participatory, left-wing communitarian spirit.

540 Ibid., xiv.
Silverstein portrayed A Pattern Language’s many ambivalences and contradictions as the book’s strong point. He wrote: “[A Pattern Language] is both serious and playful, authoritarian and tentative, open-ended. Somehow it’s both a bible and a first rough draft […] equal parts academic, classical, romantic and analytic; parts of it are striking, parts silly. It’s both obsessive and slope. It tilts you toward coherence but is a minefield strewn with little bombs of inconsistency and contradiction. Made by many hands, the language is like that ‘crooked timber of humanity,’ of which, says Kant, ‘no straight thing can be made’” (Ibid., 19–20).
542 Ibid., 20.
of the 1960s. And, in particular the West Coast 1960s — a place where the culture, the social and economic order, felt so malleable.\textsuperscript{543}

From a practical perspective, this replaceability enabled the proliferation of pattern-language-related research projects. Roger Montgomery recognized this in his 1970 appraisal of the pattern language contributions to the science of design. He wrote:

> Another, perhaps unanticipated, benefit from formalization appears in its potential for effective structuring of much of the rapidly growing architectural research effort\textsuperscript{544} […] Since the patterns are independent, then you can change one at a time, and they can always get better, because you can always improve each pattern, individually. (If the patterns were linked, so that as you improved one pattern, you would also have to change 50 others, the system would be unstable, and you could never improve it cumulatively).\textsuperscript{545}

Second, and most crucial, the independence of patterns had implications for their use, and provides an entry point for their critique. \textit{The Timeless Way}, the theoretical companion to \textit{A Pattern Language}, explained that the links between individual patterns were not only vertical, but also horizontal. In that sense, patterns were a “generating system” because they included the rules of combination; they were both “elements” and “rules,” with the two ultimately being indistinguishable.\textsuperscript{546} These rules of combination gave \textit{A Pattern Language} its structure. This was different than the linear sequence in which the language was presented and used.

In the “Summary of the Language” section, Alexander et al. deployed yet another graph theoretic term to describe this structure: the “network” [\textbf{Figure 29}]. Opposite to the tree or the semi-lattice that had specific and binding structural properties, the “network” was a generic term, almost synonymous to the term “graph.” It implied a dense and intricate connectivity that could

\textsuperscript{543} Ibid.
\textsuperscript{544} Montgomery, “Pattern Language,” 58.
\textsuperscript{545} Alexander, \textit{The Timeless Way of Building}, 345.
\textsuperscript{546} Ibid, 185.
accommodate multiple different pathways (sequences) for combining the patterns. Rather than an operational structure, Alexander et al. invoked the network as a symbol of connectivity and wholeness—ideas that in the turn of the 1960s had acquired a distinctive countercultural flair. If someone turned to The Timeless Way for mathematical sobriety, one would find instead a further mystification of the network:

In this network, the links between the patterns are almost as much a part of the language as the patterns themselves. Not merely a network, but a network of networks, a structure of structures, a vast pool of changing, varying, languages which people create for themselves as they take on their different building tasks. 547

However, this network was always traversed by means of a linear sequence. This raised a challenging question: “How can the process of using patterns be flexible enough to permit one to reshape decisions on the basis of past and future decisions?”548 This challenge was partially addressed by the scalar ordering of patterns. The consideration of bigger patterns before the smaller ones ensured that “enormous changes which might cancel out earlier decisions will not have to be made.”549 This provision did not fully address the pressing mandate to keep the process “as fluid as possible,” “since the design is built up one pattern at a time.”550 The issue of how to compose the individual patterns in the context of an evolving design, which had come up as early as in the highway location determination, was still present and pressing. Alexander et al. argued that the application of each pattern “transformed” the entire design. However, their description of “transformation” approximated more a simple combinatorics of independent elements: the application of a new pattern was not allowed to disturb the structure of the design. They wrote:

547 Ibid., 341.
548 Grabow, Christopher Alexander, 98.
549 Ibid.
550 Ibid.
When you start to think about compromises between patterns, you are not taking account of the fact that every pattern is a rule of transformation. The fact that every pattern is a rule of transformation means that each pattern has the power to transform any configuration by injecting a new configuration into it, without essentially disturbing any essentials of the configuration which was there before.

The design can change as it needs to, so long as you maintain the essential relationships and characteristics which earlier patterns have prescribed. You will see that it is possible to keep these essentials constant, and still make minor changes in the design. As you include each new pattern, you readjust the total gestalt of your design, to bring it into line with the pattern you are working on.

Such statements reveal a profoundly structuralist construal of fluidity, open-endedness, and transformation. The concepts were defined on the basis of the assumption that, at each stage of the design process, there was an invariant structure that needed to preserved and extended by the next design operation. Adding to this the sequencing of patterns by scale, one was left with a design procedure where patterns were nested within other patterns. This process was no different than a simple combinatorial operation. Patterns came together but their structural essence remained undisturbed. Within the structuralist worldview that Alexander was embracing, combinatorial operations were viewed as bearing infinite generative capacity. As Alexander himself stated, the idea of a “generating system,” which he saw as the pathway to holism, fell into a “kit of parts” worldview.

In speaking about the pattern language idea during the early years of the CES, Alexander declared striving for multi-dimensionality: “Where an ordinary language is a system which generates one dimensional strings [of words] called sentences, a pattern language is a natural

551 Alexander et al., A Pattern Language, 465.
552 Ibid.
generalization of this idea to three dimensions.\textsuperscript{554} The work with physical, spatial elements afforded a semblance of three-dimensionality. Architectural elements are, after all, three-dimensional in a geometric sense. However, from a mathematical perspective Alexander’s work did not escape zero-dimensionality. Patterns operated as atomic units that could be nested and combined, but seldom fused and restructured.

In introducing this chapter, I analogized the network with an “invisible worm”—a concealed and compromising force. The network backend of \textit{A Pattern Language} annulled Alexander’s multidimensional aspirations. Alexander admitted so himself by conceptualizing the structure of the language as a network. A network is a graph, a graph consists of points, and points are zero-dimensional entities. Alexander’s design was a combinatorial (and therefore mathematically predictable) game founded upon the assumption of social-cognitive-physical isomorphisms that produced universal invariants.

3.4. The Invisible Worm

In this chapter I traced transformations in Alexander’s theoretical activity from his doctoral work to his magnum opus \textit{A Pattern Language}, effected on the basis of graph theoretic concepts. In the transition from the “tree” to the “network,” graphs of different kinds were markers of both continuity and change in Alexander’s portrayal of architecture and the environment. The structure and density of links among the points of different graph types signaled shifts in thinking: from hierarchy to overlap, from order to complexity, from a prescriptive “program” to subjective roaming through a “language.” However, consistent among these many shifts and reversals remained the points. Be it requirements, diagrams, relations, or patterns, Alexander’s theories assumed a kind of atomic unit. Alexander eventually attempted to escape such atomistic,

\textsuperscript{554} Montgomery, “Pattern Language,” 53.
discrete logic by arguing that patterns were not objects, but structures within structures, networks within networks. Despite his intentions, nevertheless, his mandate to preserve the patterns’ structure “invariance” in the process of designing hindered his efforts to relinquish discreteness. The persistence of the graph as a mathematical underpinning of Alexander’s theories echoed and enforced a structuralist understanding of form and beauty that had motivated his investigations since his doctoral work at Harvard. Alexander and his collaborators portrayed their work as “adding nails to the coffin” of modernism. Yet the structural understanding of physical form that they advanced was in keeping with a modernist architectural ethos promoted by Alexander’s doctoral consultant Serge Chermayeff.

In a 1965 summary of his dissertation published in the Architectural Forum, Alexander aphorized: “The crucial quality of shape, no matter of what kind, lies in its organization, and when we think of it this way we call it form.” The juxtaposition of form (structured) and shape (structureless) resonated with what Chermayeff epitomized as the essence of Modern architecture: not making shapes, but solving problems. The idea that shape can be recast as structured form grew out of Alexander’s expeditions in gestalt psychology. It was legitimized by Alexander’s mathematical background and his exposure to a general shift of modern mathematics from number to structure, which we saw him promoting in the “Relational Complexes” paper. The structural concept of form allowed a much-desired isomorphism: the matching of a shape and its function. Such matching was a pivotal and, until then, unattainable ideal for the programmatically inclined architectural Modernism. The establishment of a form-

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context isomorphism through graph theoretic analysis was, in short, what the *Notes* were about.

As Yi-Fu Tuan telegraphically synopsized in his review of *Notes*:

> The object of design is form. The problem of design is to fit the form with its context. Form is that part of the world over which we have control; context that part of the world which puts demands on this form. A good design is a good fit, one in which form and context are in frictionless coexistence. Such a design is further distinguished by the clarity of its articulation.\(^{558}\)

Catalyzed by the work of Luce, Alexander engaged ideas of games, conflict, and cooperation, ultimately enlisting the graph to negotiate relationships between a complex society of “demands on form” and ensure their “frictionless coexistence.” From the onset, Alexander’s work was part abstract, part empirical. The relentless fieldwork and cataloguing of requirements developed alongside logico-mathematical explorations of their “interlock.” The graph, as a structure *underlying* the physical world, appeared to accommodate this duality. It ensured that, despite their immense empirical variety, requirements and their corresponding diagrams would speak the same mathematical language. This, in turn, enabled matching the programmatic, functional order with the formal order, in striking continuity with Modernist mandates of form following function. Eventually, the “requirements” were transformed to “tendencies” and the “diagrams” to “relations” and then universal “patterns.” The graph persisted as gatekeeper of the “frictionless coexistence” of architecture’s physical receptacles and the life that they accommodate. The mathematical enforcement of environmental tranquility troubled Alexander’s critics.

Yi-Fu Tuan kept a critical distance from the “romantic” view that “blissful equilibrium between men and environment”\(^{559}\) can be established. He also declared himself skeptical of the claim that the conflict-eliminating logico-mathematical structure that Alexander foregrounded

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\(^{558}\) Tuan, “Notes on Computer Architecture,” 12.

\(^{559}\) Ibid., 13.
would pave the path to a “village Utopia in which possible sources of irritation are excluded and no occasion for demanding change would arise.”

In a 2002 review of Alexander’s later work 

Harvard Design Magazine editor William Saunders diagnosed a despotic impulse to impose a “Californian/Mediterranean” conception of “ideal life” that left no space for the potentially desirable elements of “conflict, intensity, and rough edges.” Similarly imperial, Saunders found Alexander's pursuit of a comprehensive structure to determine the presentation and use of the pattern “smorgasbord.” Such preoccupation with structure compromised Alexander’s “best impulses” by “stifling the spontaneity he so clearly loves.” Indeed, from the Notes to A Pattern Language, the structure—be it as a tree-like “program” or as a “cascade” through to a “network”—was a mechanism for controlling the process of design and those who used it. Especially in A Pattern Language, which lay claims to freedom, open-endedness, and fluidity, the existence of such a control structure is arguably objectionable, as with the patterns’ “invariant relations” that presumably captured the “measurable structures of good architecture.” However, Alexander did not fret at this seemingly blatant contradiction.

In the 1962 “Main Structure Concept” paper, where we heard Alexander and Doshi expressing anxiety that specialized experts would displace architects to the point that their professional role would become that of monument-builders, the two authors also came forward with a remedial proposition. Architects, they argued, could still “make a contribution

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560 Ibid.
562 Ibid.
563 Saunders wrote on A Pattern Language: “Treated as a smorgasbord for selective consumption, the book is marvelous. […] But Alexander has always wanted his readers to consume from his work not just morsels but his whole five-course meal of design principles” (Ibid.).
564 Ibid.
565 Saunders attacked Alexander’s objectivist stance toward design as unsympathetic and authoritative: “Even if we could precisely define the objective, measurable structures of good architecture, to demand that people build those structures would be to dictate a robotic and therefore dead and bad architecture” (Ibid.).
566 Alexander and Doshi, “Main Structure Concept,” 18.
which the all-powerful specialists cannot make." 567 That was to specify “the overall organization of the environment but have left it to the individual to control and construct his immediate surroundings.” 568 This is what Alexander and Doshi called “the main structure concept”: the identification of a “structure” and “filler” at any scale of resolution, 569 where the structure was a kind of skeleton necessary for the filler 570 to exist. The “structure”-“filler” idea was already popular in architectural debates, pioneered by avant-gardist groups that grew out of and as corrective to CIAM, with Team 10 being a prominent example. Influenced by structural anthropology, these groups found “structure” to be a useful concept for bridging the lacuna between urban form and the human aspects of the city, and for complexifying the functional determinism of CIAM urbanism. Team 10 pioneered a model of urbanism, where architecture provided a structure (a “stem,” a “group,” a “network”) corresponding to different forms of social organizations and enabling a degree of adaptation to the inhabitants’ needs. Perhaps aware of the conceptual affinities between his mathematical work and Team 10’s structuralism, Alexander had presented his Indian village worked example at the September 1962 Team 10 meeting in Royaumont, to receive mixed reactions. This was weeks before he found a more enthusiastic audience at the first Conference on Design Methods at Imperial College, London.

Despite its participation within a broader cultural climate of revisions to Modernism, the “structure-filler” idea for Alexander was predominantly an abstract conceptual formulation that

567 Ibid.
568 Alexander and Doshi wrote: “If we can develop this trend towards the specific design of main structures only, it will be of enormous human advantage. If designers concentrate on the main structure only, the individual filler unites, whether they are dwellings, offices, cases or gardens will be able to find their own form at the hands of the people who inhabit them. Perhaps we can then again learn the freedom and sense of belonging to the things around us that we once had” (Ibid., 20).
569 Ibid., 18.
570 Ibid., 20.
Alexander and Doshi identified the following characteristics as important in the relation between main structure and filler: “1) the main structure is always more permanent than the filler 2) The functions performed by the main structure are usually more exacting than the functions of the filler 3) in many cases, the development of the filler can be left to the fluctuations of the market and to the whims (remember Alberti here) and special desires of individuals.”
guided his mathematical explorations. It characterizes the relationship between the tree and the diagrams, or the network and the patterns, or the patterns and their particular instantiations by different language “users.” In all these cases, there is an abstract structure—a skeleton—lying under the concrete specificities of a physical thing.

Some of Alexander’s critics saw the graph as what Michael Lynch has dubbed “rhetorical mathematics”: a graphic representation giving the appearance of rationality. In discussing Alexander’s work, for example, design methods seminal figure and Portsmouth Symposium co-organizer Geoffrey Broadbent pointed out that “highly personal views expressed in algebraic or graphic form have been given the semblance of objectivity by the medium of expression.” But the graph’s rhetorical role was not only objectivizing and legitimizing Alexander’s beliefs. As I argued earlier, because of being visualizable, the graph “pictured” the shape of Alexander’s theories in a memorable and communicable manner, and signposted their evolution. As a kind of “brand” or visual symbol, it was paramount for the dissemination and recognizability of Alexander’s theoretical production. Complex formulas were hard to decipher and remember, but trees, semi-lattices, and networks spoke the language of the visually-trained community of architects that Alexander was addressing.

Yet, graph theorist Frank Harary claimed that Alexander did not give the graph’s empirical nature enough justice. In 1975 Harary, along with Angelicum University graduate J. Rockey, published an article in *Environment and Planning A* provocatively titled “A City Is Not a Semilattice Either.” In the paper, they acknowledged Alexander as the originator of graph

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572 Broadbent, *Design in Architecture*, 287.
theoretic analysis in architecture and urbanism, but lamented that, despite his self-issued mathematical corrective, his mathematics was still wrong. Aiming to “clear the ground for future graphical analysis,” their critique hinged on several points, the most salient of which was that Alexander used the graph as a device for doing set theory (logical analysis) as opposed to isomorphically representing empirically significant phenomena. They wrote:

Alexander […] bypasses graph theory and the usefulness it brings to bear on structural models […] introduces graphs artificially as patterns of sets, thereby losing the intuitive advantages [emphasis mine] and structural concepts provided by graph theory. This further complicates the issue by introducing an unnecessary medium between a graph and the phenomenon it represents.

According to Harary and Rockey, Alexander kept a blind eye “to the geometric representation of a graph [emphasis mine], which results in an enhanced understanding of the abstract mathematical concepts and thereby enables the nonmathematician to make more effective use of the model.” In the next chapter, I will delve into the work of a research group that followed Harary’s mandate. In the Cambridge University Land Use and Built Form Studies Centre, a research laboratory founded in the late 1960s by Alexander’s professor Sir Leslie Martin and directed by Alexander’s classmate Lionel March, graphs were viewed as intuitive mappings (isomorphisms) between the “real world” and its abstract mathematical renditions. The Centre’s members mobilized the graph to give a mathematical expression to aspects of architecture naturally and intuitively representable by points and lines, for example connections between the rooms of a house plan or the relations between individuals of an organization. I will focus my inquiry precisely on the forces that vested the graph with ideas of naturalness, intuitiveness, and realism. Such realism, central in the LUBFS Centre’s activity, was not explicitly operative in

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574 Ibid., 375.
575 Ibid., 379.
576 Ibid., 380.
Alexander’s work. As Alexander himself admitted, the structures that he put forward were difficult to intuit.\textsuperscript{577} Alexander’s structures were the result, rather than the beginning, of his mathematical analysis. Alexander used graphs to represent the hidden structures that he saw as underlying, and ensuring the well-functioning and “livelihod” of wholes. There were moments in Alexander’s trajectory that highlighted the problems of using a construct from discrete mathematics to describe or generate holistic phenomena. The visual fusion of the “utility maps” composition in the highway location project, or the turn to multidimensionality in HIDECS 3, are such examples. Alexander turned away from the \textit{Notes} discrete logics and strove for holism and generativity. However, for reasons at times as prosaically pragmatic as developing, funding, and managing independent projects in a research center at Berkeley, Alexander regressed to an atomic, combinatorial, and structuralist understanding of design: studying subparts of the environment in isolation and linking them up in whole-aspiring networks that, nevertheless, did not amend discreteness.

\textsuperscript{577} Alexander, \textit{Notes}, 118.
Figures for Chapter 3

Figure 1: Alexander’s design method. The tree establishes isomorphism between functional analysis of a design and formal synthesis. (Source: Alexander, Christopher. 1964. *Notes on the Synthesis of Form*. Cambridge, Mass.: Harvard University Press, 94).

Figure 2: Example of requirements for the design of a kettle, ordered in a tree. (Source: Alexander, *Notes*, 62).

Figure 5: Composite diagram combining seven urban components. (Chermayeff and Alexander, *Community and Privacy*, 175).

Figure 7: Structured and unstructured design problems. The structure emerges from grouping requirements. (Source: Chermayeff and Alexander, Community and Privacy, 145).
Figure 9: A graph and its decomposition into levels of hierarchy. (Source: Alexander, Christopher, and Marvin L Manheim. 1962. *HIDECS 2: A Computer Program for the Hierarchial Decomposition of a Set Which Has an Associated Linear Graph*. Cambridge: Civil Engineering Systems Laboratory Publication 160, MIT, 5).
Figure 10: The input of HIDECS 2 was a graph, represented in the computer as an incidence matrix. (Source: Alexander and Manheim, *HIDECS 2*, 20).
Figure 11: HIDECS 2 decomposed the graph into subgraphs, which also had matrix representations. (Source: Alexander and Manheim, *HIDECS 2*, 21).
Figure 12: The output of HIDECS 2 was a tree that represented the order by which various requirements should be considered. (Source: Alexander and Manheim, *HIDECS 2*, 48).
Figure 13: Using the tree to combine diagrams developed for the Indian village worked example. (Source: Alexander, *Notes*, 153).

Figure 15: Hierarchic decomposition of the highway location problem. The “tree” is a design “program” in the sense that it indicates to the designer the order in which to address different design requirements. (Source: Manheim, “Problem Solving Processes in Planning and Design.” 37).

Figure 18: Depiction of the importance to take into consideration the “gestalt” properties of the highway in the combination of utility maps. (Source: Alexander and Manheim, *The Use of Diagrams in Highway Route Location*, 111).
related to one another than others. Thus, for example, take the following simple case:

\[ \text{Diagram} \]

It might be argued that this is best described by a 3-way partition. Thus:

\[ \text{Diagram} \]

But the left-hand subsystem is less strongly connected to the center than the right hand one. If we described this by means of a 3-way partition we should be ignoring this asymmetry or inequality. We shall have a more accurate picture of the structure if we describe it by means of two 2-way partitions. Thus:

\[ \text{Diagram} \]

Figure 19: Some shortcomings of HIDECS 2’s two-way partition into discrete subsystems. (Source: Alexander, Christopher. 1963. *HIDECS 3: Four Computer Programs for the Hierarchical Decomposition of Systems Which Have an Associated Linear Graph*. Cambridge, Mass: Civil Engineering Systems Laboratory Publication Report No. R63-27, 6)

\[ \text{Diagram} \]

Figure 20: More shortcomings of HIDECS 2’s two-way partition into discrete subsystems. (Source: Alexander, *HIDECS 3*, 8).
Figure 21: Flowchart for SIMPX, a program that produces simplices. (Source: Alexander, HIDECS 3, 18).
Figure 23: Left: Linear graph showing interactions among seventeen requirements; Middle: Overlapping subsystems of requirements based on HIDECS 3 decomposition. Each subsystem consists of at least three requirements connected to each other; Right: Disjoint subsystems of requirements based on HIDECS 2 decomposition. (Source: Bernholtz, Allen, and Edward Bierstone. 1966. “Computer-Augmented Design.” Design Quarterly, Special Issue: Design and the Computer 66/67: 40–51, 41–42).

Figure 24: Left: A subsystem of a house, corresponding to the garage and entrance. Numbers indicate misfits (requirements). (Source: Bernholtz and Biestone, “Computer Augmented Design,” 47); Right: Diagrams for each subsystem, from top left to bottom right: C, D, E, F, B, A (Source: Ibid., 48-49).
Figure 26: A cascade of patterns at the cover of *A Pattern Language Which Generates Multi-Service Centers* (Source: Alexander, Christopher, Sara Ishikawa, Murray Silverstein, and Center for Environmental Structure. 1968. Berkeley, Calif.: Center for Environmental Structure).
Figure 27: Designing with the cascade. Diagrams of multiservice-center components generated through use of the pattern language (Alexander et al., *A Pattern Language Which Generates Multi-Service Centers*, 27).
112 ENTRANCE TRANSITION

So far we have spoken mostly about houses. But we believe the pattern applies to a wide variety of environments: it certainly applies to all dwellings, including apartments—even though it is usually missing from apartments today. It also applies to all public buildings which thrive on a sense of extension from the world: a church, a cathedral, a cathedral, a public library. It does not apply to public buildings or any buildings which thrive on the face of being continuous with the public world.

Here are four examples of successful entrance transitions.

Each crosses the transition with a different combination of elements.

As you see from these examples, it is possible to make the transition itself in many different physical ways. In some cases, for example, it may be just inside the front door—a kind of entry court, leading to another door or opening that is more definitely inside. In another case, the transition may be formed by a bend in the path that takes you through a gate and brush past the fence on the way to the door. Or again, you might create a trans-
THE STRUCTURE OF A LANGUAGE

Each pattern sits at the center of a network of connections which connect it to certain other patterns that help to complete it.

Suppose we use a dot to stand for each pattern, and use an arrow to stand for each connection between two patterns. Then

![Diagram](image)

means that the pattern A needs the pattern B as part of it, in order for A to be complete; and that the pattern B needs to be part of the pattern A, in order for B to be complete.

If we make a picture of all the patterns which are connected to the pattern A, we see then that the pattern A sits at the center of a whole network of patterns, some above it, some below it.

![Diagram](image)

Each pattern sits at the center of a similar network.

*And it is the network of these connections between patterns which creates the language.*

Chapter 4: The Land Use and Built Form Studies Centre

4.1. Themes of Education and Research

In January 1967 eminent British architect, educator, and influential cultural figure Sir Leslie Martin gave a presentation to the Royal Institute of British Architects (RIBA) reclaiming the value of the Modern project in architecture. By the late 1960s architectural modernism was widely perceived as having failed its social agendas and aggravated the environmental problems that its proponents strove to resolve. Many attributed this failure to a flaw in its theoretical principles: the break with arts and crafts intuition and the wholesale espousal of a rational approach in the design of the built environment. In his RIBA presentation, Martin assessed that it was not the principles of modernism that led to its demise, but modernist architects’ mistaking of geometric systems produced by these principles as design doctrines. This dogmatic

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578 Sir Leslie Martin’s influences spanned architectural practice, education, research, and avant-garde artistic currents—domains that he “once bestrode […] like a Colossus” (“A Model for Us All.” 2000. *Arq: Architectural Research Quarterly* 4 (4): 291–92). To list some of the highlights of his long and multifarious trajectory, Martin co-edited with Naum Gabo and Ben Nicholson the 1937 single-issue journal *Circle: International Survey of Constructivist Art*, a pioneering compendium of constructivist currents in painting, sculpture, and architecture bringing together figures like Piet Mondrian, El Lissitzky, Malevich, László Moholy-Nagy, Richard Neutra, Lewis Mumford, Constantin Brâncuși, and others. His personal ties with some of these émigré artists during their passage from London appealed to a central figure of this chapter, Lionel March (March, Lionel. 3 May 2016 Interview by Theodora Vardouli). For March, Martin’s appointment as the first professor of architecture at the University of Cambridge and head of the department of architecture in 1956 was a key attractor for deciding to pursue architecture himself. March cited Nikolaus Pevsner’s enthusiastic appraisal of Martin’s presence in the department (Pevsner, Nikolaus. 1956. “Welcome to Professor Martin.” *Cambridge Review* 10: 136–37) as a key turning point for his personal academic pursuits (March, Interview by Vardouli). One of the few architects holding a PhD at his time (University of Manchester, 1936), Martin played a decisive role in reforming architectural education in Britain and promoting agendas of research and interdisciplinary engagement. For a more extensive summary of his work see his multi-authored obituary in the *Architectural Research Quarterly* (Levin, Bernard. 2000. “Leslie Martin.” *Arq: Architectural Research Quarterly* 4 (4): 295–308).


580 As discussed in the previous chapter, Christopher Alexander—one of Martin’s first students at Cambridge—was a vocal herald of Modernism’s fetish of newness. Alexander’s corrective to Modernism’s discontents was an embrace of tradition, which he saw as embodying long processes of form-environment adaptation. Unlike Alexander, who claimed that these processes of adaptation could be made explicit, rationalized, and used in design, Martin did not see “organic growth” as an essential element of a successful architecture. Both “artificial” and “organic” systems could function equally well. The defining factor for this well-functioning was the relationship between building geometry, spatial organization, and patterns of use—a relationship that should, instead, come under the scrutiny of rational analysis in order to expose “ranges of choices and opportunities.” (See Martin, Leslie. 1972. “The Grid as Generator.” In *Urban Space and Structures*, edited by Leslie Martin and Lionel March, 6–27. Cambridge Urban and Architectural Studies. New York: Cambridge University Press, 9).
attitude countered what Martin saw as an essential element of the “shift of architectural intention and process” and the “shift of attitude” that modernism initially heralded: the constant re-assessment of architectural problems through rationality and technical innovation. He argued that despite common belief, practical reason need not result in sterility, nor was the return to intuitive processes, to “feeling,” the path forward for architectural design. Embracing the modernist principle that buildings should be “‘built to a purpose’ and thought out rather than drawn,” but also condemning the opposition between thought and intuition, Martin laid out an ambitious program of “thinking afresh” the relationships between “patterns of use” and “pattern of [physical] form” through analysis, experiment, and testing.

A few months after his RIBA presentation, Martin put these ambitions to action by founding the Land Use and Built Form Studies (LUBFS) Centre [Figure 30] as the research center of the Cambridge University Department of Architecture, where he had been appointed chair in 1956. As was reflected by its name, the LUBFS Centre’s research focus was the correlation of building form and land use efficiency. Interest in this relationship had grown out of a series of

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582 Ibid., 192.
583 Ibid., 193.
584 Martin criticized the separation of “rational assessment” from “form-making,” which he saw as relic of the nineteenth-century fear “that ‘intuition’ might in some way be weakened by knowledge” (Ibid., 193). Drawing from philosopher A.N. Whitehead’s remarks on the reconciliation of rational and speculative thought as the source of the modern world’s rapid technological progress, Martin wrote: “[...] the rational understanding of a problem and the extension of it into speculative (intuitive) thought is one single process: that is, that thought and intuition are not opposed but complementary” (Ibid.).
585 Ibid., 191.
586 Ibid., 194.
587 Ibid., 192.
588 The LUBFS Centre was established in October 1967 by Leslie Martin and was active until 1974, when it was formally renamed The Martin Centre for Architectural and Urban Studies.
studies on the redesign of Whitehall, London’s old government district, a large and controversial project commissioned to Martin in 1964 [Figure 31]. In response to the commission, Martin launched a series of abstract morphological studies driven by the conviction that “the normal plot ratio measure (which is generally applied in London) is quite ineffective in relation to built form.” Going against the “swinging sixties” currents that emblematized high-rise building and in keeping with the mandate of continually putting “conventional wisdom” under rational scrutiny, Martin recruited a small team of researchers to build a mathematical model that would investigate the effects that “the form of a building had […] on the efficiency of land use” and explore more possibilities than the voguish building forms of the tower or the slab [Figure 32, Figure 33].

589 The Whitehall district was named after the palace of White Hall, a stupendous royal establishment between the Thames River and St. James Park, which was mostly destroyed by fire in 1698. The palace gave its name to a road running from Trafalgar Square toward Parliament Square, on which lay several U.K. administrative functions. Because of their density, Whitehall came to be considered London’s “government center.” Until the 1960s, these functions were housed in eighteenth- and nineteenth-century buildings. In Demolishing Whitehall: Leslie Martin, Harold Wilson and the Architecture of White Heat (London: Routledge, 2016), historians Adam Sharr and Stephen Thornton expound the governmental deliberations and cultural imperatives that led to the decision to replace the old fabric of the area with new buildings, aligned with the aesthetic, functional, and ideological values of a galloping British architectural modernity. They detail a line of debates among British political figures, also involving allied architects and planners, around the pressing need for a long-term plan to accommodate government departments in the Whitehall area. The story implicates figures such as minister of housing and local government Henry Brooke, who initially assembled a committee to discuss future government accommodation in Whitehall, and the conservative Geoffrey Rippon, first minister of public building and works, who commissioned the development of an overall scheme for Whitehall to Leslie Martin after arduous negotiations on the scope and execution of the study. Martin insisted on the need to conduct the study collaboratively with a small team of researchers, among which he enlisted transportation expert Colin Buchanan. In 1965 Martin submitted the report Whitehall: A Plan for the National and Government Centre (London: HMSO, 1965), co-authored with Buchanan to Charles Pannell, minister of public building and works in Harold Wilson’s Labour government. The plan was never executed, but, as Sharr and Thornton demonstrate, it set a directive tone for several of the realized proposals and contributed significantly to architectural thought—the Land Use and Built Form Studies Centre arguably being a reverberation of ideas that were, at a basal level, developed in the Whitehall study.


594 “Form,” as used by Martin in the context of the Whitehall study, pointed to types of building geometries such as “towers,” “slabs,” “courts,” “crosses,” and other. In the LUBFS Centre, “built form” was construed more broadly and more abstractly as a mathematical model of a building.
As “geometrical and mathematical explanation” of the relationship between built forms and land use became clearer, Martin began to envision applying the same principles at the environmental scale. These research pursuits went hand in hand with a broader agenda that Martin was vigorously promoting during his chairmanship at Cambridge: to establish architecture as an academic discipline by developing “theory”—a body of principles organizing the discipline’s knowledge base and informing practice through architectural and environmental research. Martin found an avid supporter of this quest in his former student and Whitehall study collaborator Lionel March, whom he appointed LUBFS Centre director in 1969. Trained in architecture and mathematics in Cambridge, March shared Martin’s conviction that research and rational analysis would provide an expanding bedrock of knowledge in support of architectural education and “establish the foundations of an academic discipline.”

March elaborated on these views in two seminal opinion articles, one published in the *RIBA Journal* in 1972 and one in the *Cambridge Review* in 1973. By the time the two articles appeared, the LUBFS Centre had published numerous working papers and articles presenting mathematical models for all scales of the built environment. The purview of the Centre’s

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595 Ibid.
596 Ibid.
597 Martin believed that the development of knowledge about built form and the extension of that knowledge by speculative thought—in other words, the development of what he called “theory”—was essential in order for architecture to take “its proper and central place in setting out alternative choices and methods of attack on our environment” (Ibid., 193).
598 As mentioned in chapter 2, Martin gave the chairman’s address at the 1958 RIBA Conference on Architectural Education, a milestone event for U.K. architectural education. Martin advocated for the transition of architecture from a guild-based profession to an academic subject, with an organized base of knowledge and theory. Among other suggestions, Martin argued for stricter entry requirements to architecture school.
599 Lionel March was one of three students with a background in mathematics to join Leslie Martin’s first class, along with Christopher Alexander and William (Bill) Newman, a renowned computer scientist. See interview with March in the Appendix for a compelling narration of his transition from mathematics to architecture.
600 March, “Research and Environmental Studies,” 90.
602 This article was an edited version of a talk that Lionel March gave at the RIBA in January 1972.
603 March, “Research and Environmental Studies.”
research ranged from representing spatial structure in cities,\(^{603}\) to allocating activities and circulation in offices and universities,\(^{604}\) to generating architectural floor plans on the basis of dimensional and functional requirements.\(^{605}\) Almost invariably, these projects had either a footing or an application in real-world scenarios, a condition stemming in part from the Centre’s sponsorship by external philanthropic, national, or governmental organizations.\(^{606}\) However, rather than being the final goal, the “applied” side of the LUBFS research projects served as illustrations, examples, or tests of the Centre’s main product: new “theories.”\(^{607}\) This was by no


\(^{606}\) By 1973, the LUBFS Centre had received nearly £250,000 in external funds for research and consisted of eighteen full-time researchers from several disciplines with a similar number of post-graduates, doctoral candidates and international visiting scholars (March, “Research and Environmental Studies,” 90).

The inaugural funding came from the Centre for Environmental Studies, an independent charitable trust established by the U.K. Ministry of Housing and Local Government “for the purpose of advancing education and research in the planning and design of the physical environment.” The Ford Foundation made a grant of $750,000 to the Centre, which was channeled in part to other research initiatives, including the LUBFS Centre. The Centre for Environmental Studies continued to support the Urban Systems Study (1967–1973) led by Marcial Echenique. Other research projects were funded by other external resources: the Gulbekian Foundation and the U.K. Department of Education and Science funded the Universities Study (1965–66, 1967–68, 1968–71) by Steadman, Dickens, and Bullock under Leslie Martin’s guidance; and the Ministry of Public Building and Works for the Office Study (1967–1970) and its supporting Computer Aided Design Study (1969–1970), which included Philip Tabor, Dean Hawkes and others. LUBFS Centre studies also received funding from the Department of Health and Social Security, the Housing Research Foundation, the Ministry of Housing and Local Government, the Royal Institute of British Architects, the University Grants Committee, and Wates Limited.

\(^{607}\) In personal conversation, LUBFS Centre founding member Philip Steadman spoke of the co-existence and interdependence between “applied” and “basic” research in the LUBFS Centre. He explained that because of the Centre’s initial financial reliance on the central British government (with grants from scientific research councils not available to the Centre until later), “…a great deal of the work at LUBFS was ‘applied’” in the sense that “it was paid for by government and its direction was specified by civil servants.” However, he continued “decisions about what directions in research to pursue […] were guided by individuals’ interests, but steered by pragmatic considerations about how that work could be funded.” Mathematical explorations that needed “just pencil and paper” were mostly performed unfunded whereas projects requiring the purchase of computers, conducting surveys, hiring research assistants, and so on “needed external money.” (Steadman, Philip. 4 May 2016. Interview by Theodora Vardouli).

Leader of the LUBFS Centre Urban Systems Study Marcial Echenique painted a similar picture of complementarity between “applied” and “basic” research, highlighting, however, pressures from the university that “encouraged … to set an independent outfit as they were not keen in ‘applied’ as opposed to ‘basic’ research.” Echenique pointed out that the Applied Research of Cambridge (ARC), a private offshoot of the LUBFS Centre founded in 1969, was initially set up “to apply some of the research to practical projects” and “helped to employ researchers who didn’t
means a withdrawal from the world of practice, although March variously celebrated academia’s fortuitous distance from the tumult of professional decisions and compromises⁶⁰⁸ and the “unique opportunity” for a “long, cool view”⁶⁰⁹ that this offered. LUBFS Centre researchers believed that “there is nothing so practical as a good theory.”⁶¹⁰ “Good” or “true” theories needed to contain “a body of ideas set down in measurable terms,” to be “open to rational argument” and to be strengthened when being “challenged successfully.”⁶¹¹ There was a shared belief, also echoed in March’s two articles, that such theories would create a veritably modern architectural discipline and implant architecture at the epicenter of the environmental sciences.⁶¹² In order to assume its new position, however, architecture needed to undergo a double reformative move of, first, disciplinary consolidation, and second, interdisciplinary outreach. In his RIBA Journal and Cambridge Review articles, March declared mathematical modeling a harbinger of disciplinary fortification and interdisciplinary bridging, and positioned the LUBFS Centre among the “many

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have further funding, charge clients for the consultancy and buy equipment outside the University.” (Echenique, Marcial. 15 January 2017. Interview by Theodora Vardouli).


⁶⁰⁹ Ibid.

⁶¹⁰ In a manifesto-like introduction to a collection of LUBFS Centre articles, published in the Architectural Design magazine, March and his co-editors asserted: “There is nothing so practical as a good theory: among other things it removes the need for trial and error. Under some conditions, for example, we may be certain that two rooms cannot be placed next to one another; the arrangement contravenes a theorem in graph theory. With true theory—not the manifestos of the second machine age—our knowledge can be more certain, our predictions more reasonable, our assumptions more explicit, and our understanding more aware of its shortcomings.” (March, Lionel, Peter Dickens, and Marcial Echenique. 1971. “Models of Environment: Polemic for a Structural Revolution.” Architectural Design 71 (5): 275). The motto “there is nothing so practical as a good theory” was taken from famed social psychologist Kurt Lewin, who we previously encountered as founder of the Research Center for Group Dynamics at MIT where Alexander’s consultant R. D. Luce had produced some of his early mathematical work in graph theory. In personal conversation, Steadman recalled him and his fellow researchers misattributing this phrase to philosopher Bertrand Russell. (Steadman, Interview by Vardouli).


In the same chapter, the authors claimed theory essential for widening choice and possibility: “But to know what is theoretically possible is to allow wider scope for decisions and objectives. We can choose.” (Ibid., 26)

⁶¹² The “environmental sciences” was a broad term that encompassed all fields studying aspects of the human-made and natural environment—its physical, social, anthropological, and other dimensions.
skirmishes” that had, for a decade or so, been establishing a “beachhead for [...] the 'mathematization' of the human sciences.”

The military metaphor was not a first for March, who frequently deployed combative language to advocate for an architectural discipline founded on mathematical rigor. For example, in a 1971 Special Issue of the Architectural Design magazine dedicated to the work of the LUBFS Centre March and his co-authors famously launched a “Polemic for a Structural Revolution” denouncing “the prattle of pricey prima-donatas” and calling for a “professional disarmament” in the service of knowledge unification across disciplines. This vigorous rhetoric stirred reactions from March’s contemporaries and is currently inviting critical scrutiny from historians who revisit the LUBFS Centre’s activity in efforts to trace institutional and intellectual lineages of computational design and digital culture in architecture. In 1976, for example, British philosopher Sir Roger Scruton issued venomous critiques against the LUBFS Centre for promoting mathematical reductionism and a “totalitarian” attitude to architecture, which he characterized “architectural Stalinism.” Subsequent architecture-historical accounts of Centre have adopted a similar analytic angle, discussing LUBFS production as a symptom

614 Architectural Design editor Monica Pidgeon—an important participant of Modern architectural movements including the Union International des Architectes (UIA), the Congrès International d'Architecture Moderne (CIAM) and the MARS (Modern Architectural Research) Group—approached Echenique and “allowed … to publish the special issue on the LUBFS work” (Echenique, Interview by Vardouli). Echenique discussed this with Lionel March and Peter Dickens, with whom he divided the work. Lionel March was the main force behind the editorial (Ibid.) In personal conversation, Echenique explained that the editorial was deliberately written in the language of a 1930s architectural manifesto. (Echenique, Interview by Vardouli).
615 In their Architectural Design editorial, March et al. wrote: “Environmental studies must, at a profoundly structural level, be related to other disciplines. If this is done, it should not then surprise us to find scientists of all kinds working together, sharing the language of mathematics in common, on environmental problems. Only the professions stand in the way. At root they divide responsibilities. […] This is not to criticize individual professions, but to condemn the institutionalization of the piecemeal approach to environmental problems which the organized professions represent. Such fragmentation was expedient when complex systems could not be looked at as a whole. But the very demarcations have increased our problems. The time has come for professional disarmament” (March, Dickens, and Echenique, “Polemic for a Structural Revolution,” 275).
of technocratic thought and postwar scientism. In these accounts, the search for mathematical exactitude in architecture is invariably seen as ostracizing intuition from architectural practice. Such perspectives have invited rebuttals from LUBFS Centre veterans, with the most recent being a corrective history of LUBFS published in the *Journal of Architecture* in 2016 by LUBFS charter member Philip Steadman.  

Most architecture historical accounts of the LUBFS Centre’s work have construed “mathematics” in generic terms, and associated its deployment in architecture with a search for quantitative certitude, or what historian of science Theodore Porter has famously called “trust in numbers.” However, as I have argued in chapter 2, the specific mathematics that participated in architecture’s mathematization matter: They provide concrete objects of inquiry that can be followed in their trajectory from mathematics into architecture, thus revealing historical connections hitherto unseen. As March noted in the *RIBA Journal* and *Cambridge Review* articles, the mathematics to deliver his visions for modernizing the architectural discipline was not the traditional mathematics of “counting, ordering, comparing, and measuring” but novel mathematical varieties: “groups, rings, fields, vector spaces, linear and boolean algebras; topology, graph theory, and varieties of algebraic geometry; linear, non-linear, dynamic and boolean programming” that had brought about “many of the significant advances in modern science and technology.”

From the panoply of “modern” mathematics listed by March, graph theory held a privileged role in discussions about layout and activities in the LUBFS Centre. Graph theory was used in a

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wide variety of studies ranging from timetabling of university student activities to the generation of house plans, and from the taxonomy of operational components in organizations to pedestrian circulation analyses. A “systems laboratory,” pursuing research projects diverse in scale and focus despite their claimed interconnections, the LUBFS Centre was characterized by a polyphony of mathematical techniques. Probability, statistics, and some “traditional” mathematical methods also had a place in the Centre’s research activity. However, when it came to representing the organization of space, time, or activities and enumerating formal or configurational possibilities in a design, graph theory figured center stage. It is not the frequency of its implementation that makes graph theory stand out in the LUBFS Centre work. Instead, it is the optimistic rhetoric of disciplinary reform and of new possibilities to look at age-old architectural problems in a new light that accompanied its application. These visions associated with the graph make it a productive lens through which to discuss the Centre’s research activity. These aspects, conversely, render the LUBFS Centre a fruitful case study for the broader query of how graph theory participated in the making of design discourse in the 1960s and 1970s.

In their writings, LUBFS Centre researchers repeatedly declared their choice of technique as stemming from the architectural problem at hand (“we use or reject methods [...] according to whether they are appropriate or not”; “widely different studies have called for the same mathematical model”; “[graphs] were the appropriate tools for what we wanted to do in architectural science”). This vocabulary assumes various mathematical techniques as generally available instruments that passively lend themselves to their users’ intellectual and practical agendas. Here I argue that the “appropriateness” of the graph as a mathematical modeling tool

621 Ibid., 103.
622 March, “Research and Environmental Studies,” 89.
624 March, “Modern Movement to Vitruvius,” 103.
625 Steadman, Interview by Vardouli.
was contingent on the avenues by which the graph became available to the LUBFS Centre researchers and the meanings that the graph carried in the communities of mathematicians and researchers that developed it and used it. I also claim that the operational properties and symbolic meanings of the graph inflected these agendas and produced new theoretical meanings of seeing, choice, and possibility in architectural design that have reached the present moment.

I begin by unpacking the vernacular meanings of mathematization in the LUBFS Centre—a term explicitly invoked by March to describe both the Centre’s goal and activity. Within the Centre, mathematization was construed as a one-to-one mapping of architectural objects, relations, and operations to mathematical ones.\textsuperscript{626} I highlight the contingency and creativity of these mappings between architecture and mathematics, but also their conditioning by broader intellectual currents and deliberations about the position of architectural knowledge among the environmental sciences. “Picking oddments from a mathematical shipwreck,”\textsuperscript{627} to use Philip Steadman’s incisive metaphor, LUBFS Centre researchers were tentative and pluralistic about their choice of mathematical technique. I discuss how a turn toward structural (i.e. consisting of objects and relations) understandings of architectural concepts paralleling a boost of structural and relational mathematical ideas promoted graph theory as a valuable mathematical “oddment” to be collected by the LUBFS Centre’s “Robin Crusoes of mathematics.”\textsuperscript{628} Then, I turn to specific mathematical models developed on the basis of graph theory in the scale of institutional clusters or individual buildings and examine how isomorphism, combinatorics, and predictivity participated in redefining architectural representation, choice, and possibility in design. The LUBFS Centre espoused science and rationality at a time when most other proponents of design’s mathematization were declaring a change of heart. LUBFS Centre researchers were

\textsuperscript{626}March, “Research and Environmental Studies,” 87.
\textsuperscript{627}Ibid.
\textsuperscript{628}Ibid.
aware that the “environment will have to be built by people” and that models were neither substitutes of reality, nor did they “offer goals or solutions.” Through a closer reading of specific research projects, I will demonstrate that although the LUBFS Centre never underwent a “participatory turn,” in the sense of the design methods movement or Alexander’s theoretical activity, its uses of graph theory encapsulate many of the ideas on the basis of which the turn from the “scientization” to the “democratization” of design unfolded. The positioning of intellectual vision as substructure to subjective seeing and the recasting of design as choosing from a “space” of comparable possibilities were two salient products of the Centre’s mathematical modeling activity that, as I will ultimately argue, share much conceptual ground with contemporaneous methods for participatory design.

4.2. The Geometry of Environment and the New Mathematics of Architecture

Beyond the Old Geometry

During its first years of operation, the LUBFS Centre’s main publication output was technical working papers and journal articles. By 1971, the Centre’s different study groups had produced a large enough body of work to warrant consolidation and presentation as a cohesive new attitude toward architectural and urban design. LUBFS Centre director Lionel March channeled the Centre’s research product to wider architectural audiences through two impactful publications that he co-edited with members of the Centre. The one was the aforementioned Special Issue in the widely circulated magazine Architectural Design, known for announcing new architectural tendencies and an overall avant-gardist outlook. The other was a book, the Centre’s first, initially

630 Ibid.
published by the RIBA with the title *The Geometry of Environment: An Introduction to Spatial Organization in Design*[^631] [Figure 34]. The *Architectural Design* Special Issue showcased the Centre’s research projects on different scales of the built environment along with statements about this work’s larger implications for the disciplines of architecture and planning[^632]. *The Geometry of Environment* had a different flavor. Although it incorporated some of the Centre’s projects, the book’s content was organized neither by scale of operation nor by underlying intention. Rather, as a quick inspection of its table of contents reveals, it was organized by mathematical concepts[^633]. More than a publication about architectural research, the book resembled a mathematical textbook: Each of the book’s fourteen chapters began with a presentation of a mathematical concept and then demonstrated some of its usages through architectural examples. Some of these examples were taken from research projects conducted within the LUBFS Centre.

The mathematical topics covered in the book were all generally related to shape and space. Even though “geometry” was featured in the book’s title, familiar ideas from Euclidean geometry that architects typically used to describe shapes, their appearance, and their metric properties, were absent. Instead, the book presented readers with more exotic subjects such as mappings,...


[^633]: Along with a preface and a guide to further reading, the *Geometry of Environment* included fourteen main chapters, covering the following mathematical topics reflected in their headings: 1 Mappings and transformations; 2 Translations, rotations and reflections; 3 Symmetry groups in the plane; 4 Matrices and vectors; 5 Point sets and modular spaces; 6 Stacking, nesting and fitting; 7 Irregular polygons and convexity; 8 Modules and numbers; 9 Proportions and series; 10 Planar graphs and relations; 11 Electrical networks and mosaics of rectangles; 12 Locations and associations; 13 Spatial allocation procedures; 14 Networks, distances and routes.
transformations, symmetry groups, matrices, modules, planar graphs, and networks. In the book’s introduction, co-authors March and Steadman advocated for the benefits of the architectural reader’s exposure to this new kind of geometry that pertained, not to measure, but to structure and organization. They wrote:

Perhaps the chief difference between the traditional treatment of geometry in architecture and the one presented here, is that, previously, geometry was employed to *measure* [emphasis in the original] properties of space such as area, volume, angle, whereas the new mathematical theories of sets, groups, and graphs —to name but a few— enable us to describe *structural relationships* [emphasis in the original] which cannot be expressed inmetrical forms, for example, ‘adjacent to’, ‘in the neighborhood of’, ‘contained by’.  

In the decade preceding the book’s publication, the subjects covered in *The Geometry of Environment* had come to the foreground of U.K. mathematical education under the new math reforms. As elaborated in chapter 2, during the 1960s the School Mathematics Project—a collaboration of eight English schools coordinated by the University of Southampton and directed by mathematician and educator Bryan Thwaites—pioneered new syllabi, methods, and examinations for secondary-level mathematics courses to “adequately reflect the trends and usages of mathematics.”  

[Figure 35, Figure 36] The new syllabi integrated twentieth-century developments in mathematics, such as set theory, graph theory, combinatorics, matrix algebra, and symmetry—in short, the mathematical techniques that March and Steadman presented in their book.

Introduced by Martin as “a book about the new mathematics and architecture,” *The Geometry of Environment* aimed to establish both generational and disciplinary bridges. By

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635 Thwaites, *The School Mathematics Project: The First Ten Years*.
637 Ibid., 6.
virtue of its topic, the book aspired to attract readers with mathematical interests and instill excitement about architecture as an intellectually stimulating subject. In the preface, March and Steadman wrote:

Our aim is twofold: one, to help bridge the gap between the new mathematics and the older generation; and two, to suggest to the youthful reader, perhaps with a science and mathematics background, that architecture is an exciting subject — it is neither wholly looking at old churches, nor laboriously calculating stresses in beams and loads with columns.  

March and Steadman contemplated uses of the book in the educational level of “sixth-form”—a year of advanced high school study that preceded university entry—as “an introduction to modern ideas of architectural form and spatial organization.” Primarily, however, the book aimed to bridge architects’ knowledge gap between traditional concepts in geometry and arithmetic—the “old mathematics”—with the “new mathematics” of structures and relationships. March and Steadman wrote:

Our specific concern is to introduce the student of architecture, whatever his age or experience, to some of the concepts of new mathematics which seem to us to have potential value in describing and helping us to understand some of the geometrical relationships that arise when we organize shape within buildings.

This concern relates to the conditions of the book’s inception. The Geometry of Environment was written by invitation. The book was, in a sense, commissioned to March by the RIBA Library Committee following ongoing conversations about the relationship of mathematics and architecture in the mid-twentieth century. These conversations were fueled by late 1940s influential architecture historical works, such as those of Rudolf Wittkower and Colin Rowe,

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638 Ibid., 7
639 Ibid.
640 Ibid.
which reappraised the architectural thought of the Renaissance by unearthing its mathematical (numerical) underpinnings. Stimulated by such works, famed British architects and Team 10 leaders Alison and Peter Smithson “expressed an interest in the relationship of mathematics to architecture at mid-twentieth century as a parallel to the Renaissance.”

Alongside this query, the Smithsons brought to the RIBA Library Committee’s attention a “generational gap” with the mathematics taught to young British students, including their son, which left them “at a loss.” Because of his mathematical training and close links to the RIBA March was invited to “write a book for architects that would illustrate the potential of the ‘new maths’ in their field.” March enlisted LUBFS Centre member Philip Steadman for the task, because of his mathematical inclinations and perhaps because Steadman had engaged the architectural scale of the building more than other members of the Centre.

The Geometry of Environment’s origin story has several layers that are worth delaminating. One, it conveys a wider desire of avant-garde architectural groups in the 1960s to be up-to-date with developments in mathematics. Two, it speaks to the palatability of this idea in professional organizations such as the RIBA. This is in continuum with a claim that pervaded architectural modernism in the interwar and postwar period, namely that architectural thought could be revived by establishing an intimate rapport with mathematical thought—an idea that we also saw Martin and March explicitly promoting in their RIBA talks and elsewhere. Three, and most

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642 Ibid.
643 Ibid.
644 The potentials of mathematical thought for architecture were, for example, celebrated by Le Corbusier, who viewed it in almost metaphysical terms: “For the artist, mathematics does not consist of the various branches of mathematics. It is not necessarily a matter of calculation but rather of the presence of a sovereign power; a law of infinite resonance, consonance, organisation. […] Chance has no place in nature. Once one has understood what mathematics is – in the philosophical sense – thereafter one can discern it in all its works. Rigour, and exactness, are the means behind achieving solutions, the cause behind character, the rationale behind harmony” (Le Corbusier. 1948. “L’architecture et l'esprit mathematique.” In Les Grands Courants de la Pensee Mathematique. Paris. 490 cited in Loach, Judi. 1998. “Le Corbusier and the Creative Use of Mathematics” The British Journal for the History of Science 31 (2): 185). Or elsewhere, “Mathematics is the majestic structure conceived by man to grant him comprehension of the universe” (Le Corbusier. 1950. Le Modulor I. Boulogne. 71).
importantly for our interests, it speaks to a hidden collusion and coalescence between the structural and relational focus of new mathematics and ideas of structures and relations that architects such as the Smithsons were grappling with in the 1950s and 1960s. In their preface to *The Geometry of Environment*, March and Steadman pointed to the alliance between structuralism and the new math, and the new math’s promise to furnish qualitative fields with a firm mathematical basis. They quoted Lévi-Strauss as herald of this potential:

> Lévi-Strauss has drawn attention to a similar trend in the social sciences where the growth of structural studies is seen to be ‘the direct outcome of modern developments in mathematics which have given increasing importance to the qualitative point of view in contradistinction to the quantitative point of view of traditional mathematics’.

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**School Mathematics and Structural Inculcation**

As I discussed earlier, the new math held a contested and geographically varied relationship with the phenomenon that is known as mathematical modernism—the discipline of mathematics’ turn toward the restructuring of its knowledge and concepts on the basis of abstract structures and axioms. Mathematical modernism pioneered a structuralist approach to disciplinary knowledge, which became a model for similar disciplinary reforms in the social sciences and architecture in the 1960s. It also provided the technical devices for representing and reasoning with relations and structures. The new math textbooks incorporated the output of mathematical modernity, such as sets, matrices, and various branches of topology. The applicability of the new math and its link with the world of everyday experience was given particular emphasis in the U.K. context. Opposite to the American new math movement that declared abstract reasoning a fundamental intellectual trait of the new citizen it aspired to create, the U.K. new math embraced the concrete,

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experiential, and intuitive. British new math textbooks were inundated with examples, many of which were drawn from the students’ everyday surroundings.

For example, SMP’s Book 5 for O-level candidates, which included most of the subjects covered in *The Geometry of Environment*, contained numerous illustrations of new mathematical concepts via “architectural or quasi-architectural problems” such as “Plans and Elevations” or “Heating a House.” At the elementary school level, a Nuffield publication used floor-plan drawings, models, and house-building exercises, to introduce students to geometry and topology [Figure 37, Figure 38]. Produced with the aid of a group of first-year architecture students at the Bartlett, led by faculty member and former BASA spokesperson George Kasabov, the Nuffield Mathematics Project book was titled *Environmental Geometry.* The *Geometry of Environment* was written in the context of such precedents, with which it overlapped both in the subjects it covered and in the method of illustration: new mathematical ideas conveyed through examples from the built environment [Figure 39, Figure 40].

March and Steadman were not merely observers and emulators of this drastic turn in British mathematical education, but had been on its receiving end before joining the LUBFS Centre. Although there was no explicit requirement that LUBFS Centre researchers should be mathematically proficient, the Centre attracted architecture students with an “abstract mind and an interest in theoretical questions” and who were “naturally inclined to quantification and rational thinking.” These were inclinations mostly developed at the high school level. Several

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646 March and Steadman, *The Geometry of Environment*, 341
647 Initiated in 1964, the Newfield Mathematics Project aimed to devise a “contemporary approach for children from 5 to 13.” The teachers’ guides focused on new active learning and discovery-oriented teaching approaches rather than entirely new syllabi. The goal was to achieve “understanding” instead of “learning off mysterious drills,” ultimately shifting the “whole attitude to the subject [of maths] so that ‘Ugh, no, I didn't like maths’ will be heard no more” (Nuffield Mathematics Project. 1969. *Environmental Geometry*. Chambers, iii).
648 Ibid.
649 Steadman, Interview by Vardoulia.
650 Echenique, Interview by Vardoulia.
of the Centre’s members entered architecture with ample classroom training in the “new mathematics” of relations and structures. Steadman, in fact, reports being Thwaites’s student in Winchester College\textsuperscript{651} and acknowledges that his high school experiences “shaped [his] ideas about how mathematics might be applied in architecture, once [he] got to Cambridge.”\textsuperscript{652} For March, the school exposure to the kinds of mathematics later consolidated as the new math was what pushed him towards mathematical studies in Cambridge and, one year later, away from them, to pursue stage design and ultimately architecture. March attributed his change of heart to an “immense boredom” with university mathematics, which “was high school all over again.”\textsuperscript{653}

The LUBFS Centre members’ inculcation in the new math, which has so far evaded historical attention, opens new analytical avenues for discussing the Centre’s research activity. On the one hand, it accounts for the curious phenomenon of architects\textsuperscript{654} suddenly exhibiting such a high level of mathematical aptitude, which was a prerequisite for the kind of research output that they produced. On the other hand, it illuminates the cultural meanings of the mathematical techniques that they put to work.

\textsuperscript{651} Steadman, Interview by Vardouli. \\
\textsuperscript{652} Ibid. \\
\textsuperscript{653} March, Interview by Vardouli. \\
\textsuperscript{654} Despite the LUBFS Centre’s efforts to recruit from mathematicians, computer scientists, economists, and engineers, the large majority of LUBFS staff consisted of architects (Steadman, Philip, William J Mitchell, and Dean Hawkes. 1977. “The Architecture of Stalinism: A Reply to Dr. Scruton.” The Cambridge Review 99 (2237): 106). The Architecture Department’s curriculum remained rather self-contained, an amalgam of Modernist and Beaux Arts traditions (Steadman, “What Really Happened,” 291). In order to obtain a bachelor’s degree, undergraduate students at Cambridge went through what was known as triposes: examinations in broad subject areas such as Natural Sciences, Mathematics, Law, Architecture and others, divided in several parts. Cambridge awarded a general bachelor’s degree without specific disciplinary designation. In personal conversation, March remarked that Cambridge University’s institutional reputation subsumed individual departments or specializations: “the important thing was having been educated at Cambridge.” (March, Interview by Vardouli). Despite a fair amount of communication among students taking different triposes (Ibid.), institutionally established bridges among different departments at Cambridge were sparse and most architecture students were not being exposed to courses outside the department (Steadman, Interview by Vardouli). Given the self-contained nature of their university instruction, it is striking how the LUBFS Centre researchers acquired the level of mathematical sophistication exhibited in their working papers. This question is explained through recourse to their high school training.
In relation to the new math, graph theory held a remarkable position. Although it was never a central subject in the new math, graph theory shared intimate links with two of its core domains: set theory and matrix algebra. Compared to these topics, graph theory emitted a seductive freshness. Comprehensive textbooks dedicated to the subject, which started proliferating in the 1960s, presented the applicability of graphs to various human endeavors and were accessible in content to the non-mathematician [Figure 41]. Furthermore, graph theory’s association with recreational mathematics made it even more attractive as a form of intellectual gymnastics. In personal conversation, Steadman reported learning graph theory through self-instruction, a process vocally encouraged in the U.K. new math culture. Far from being a marginal topic for the mathematically curious, graph theory encountered a true surge of interest in the 1960s. Mathematicians who promoted graph theory emphasized graph theory’s novelty, its accessibility, and its potentials for providing a mathematical device capable of delivering one of the key intellectual mandates of the period: the unification of all sciences under structural descriptions. Graph theory rendered set theory and matrix algebra—

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655 As discussed in chapter 2, a graph could be represented as a table of numbers (a matrix), while several set theoretic concepts could be translated in graph theoretic terms and vice versa. An example of the close relationship of set and graph theory can be seen in Christopher Alexander’s work in chapter 3.

656 In the Geometry of Environment March and Steadman present Øystein Ore’s book Graphs and Their Uses as self-consciously one of the first on the subject of graph theory. Steadman also remarked that graph theory “was a new subject which at that time had not gone much beyond the capacity of an amateur like myself to understand” (Steadman, Interview by Vardouli).

657 Ibid.

658 In discussing university mathematics, Thwaites highlighted the importance of self-education as a skill learned in school: “So far as the student is concerned, the treatment should begin in the schools. It is there that he should first learn that the more he can discover for himself, the greater will be the mastery that he achieves over the knowledge he has won.” (Thwaites, On Teaching Mathematics, 51).

659 For example, in the preface to the popular book Structural Models: An Introduction to the Theory of Directed Graphs (Harary, Frank, Robert Z. Norman, and Dorwin Cartwright. 1965, New York: John Wiley & Sons) the authors remarked that “high school graduates trained in modern mathematics’ should find the book not at all difficult” (vi).

660 Graph theorist Frank Harary, for example, fervently preached the usefulness of graph theory for practically every field of human knowledge. He claimed: “When these terms (graphs) are given concrete referents, digraphs serve as mathematical models of empirical structures, and properties of digraphs reflect structural properties of the empirical world” (Harary, Norman, and Cartwright, Structural Models, v). Later, he put this statement to prose and posited
pinnacles of structural studies—visible, applicable, concrete, and teachable. It gave structures and systems an image.

*The Image of the System*

Graphs soon came to symbolize, visually and operationally, a widespread intellectual move toward structures and systems as a way of organizing disciplinary knowledge and pursuing interdisciplinary communication. While breathing the structuralist air that permeated Cambridge University in fields as diverse as anthropology, economy, geography, sociology, archaeology, and others, LUBFS Centre researchers were espousing systems theory. General systems theory was being advertised as a remedy to the scientific community’s disciplinary balkanization—a way out of a “desert of mutual unintelligibility” with “walled-in hermits [...] each mumbling to himself words in a private language that only he can understand.” Systems were seen as theoretical models that lay “somewhere between the highly generalized constructions of pure mathematics and the specific theories of the specialized disciplines.” As such, they provided a “skeleton” for science—featureless structural mathematical entities from which to “hang the flesh and blood of particular disciplines and particular subject matters” thus revealing “similarities in the theoretical constructions of different disciplines.”

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that “for every field that has a structure, graph theory yields material for a lecture” (Harary, Frank. 1991. “On the ABC of Graph Applications.” *Le Matematiche* 45 (1): 75).
663 Ibid., 197.
664 Ibid., 208.
665 Ibid., 197.
With structuralism and systems theory floating “in the Fenland air” March called for a “structural revolution” within architecture itself. Harbinger of this revolution would be mathematical models of architecture’s “essential structures.” In opening *The Geometry of Environment* Martin declared such mappings between architecture and mathematics to be natural, given the structural nature of both fields. Martin defined mathematics as a “logical pattern of entities and relationships” and continued to remark “these [the entities and relationships] may perhaps be seen to be reflected in the physical and spatial arrangement of buildings.” Using this definition of mathematics, he extrapolated “a generalized definition of architecture” as a “logical pattern of entities and relationships’ built around activities.”

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666 In enumerating conceptual influences, March referred to work in mathematical modeling in anthropology (Edmund Leach, 1961), philosophy (Mary Hesse, 1963), planning (*American Institute of Planners Journal*, 1965), social sciences (Richard Stone, 1967), geography (Chorley and Haggett, 1967), archaeology (David Clarke, 1968). Many of these works were endogenous to Cambridge, and despite having access to these published texts irrespective of geographic location, the “Fenland air” smelled of “models, quantitative, techniques, structuralism” (March, “Research and Environmental Studies,” 87).

In interview with the author, Marcial Echenique positioned the LUBFS Centre work in “the wider currents of rationalism, structuralism by Lévi-Strauss, system analysis by Von Bertalanffy and positivism by 19th Century philosophers from Compte onwards” (Echenique, Interview by Vardouli). The work of Lévi-Strauss, that we have already seen quoted in *The Geometry of Environment* and the *Architectural Design* editorial had caught the interest of many LUBFS Centre members who, according to Echenique “believed that (they) could discover the deep structure that determine the properties of building and city forms (geometrical laws) and, probably more naively, believed that human behavior was also determined by deep structures which were discernable (through probabilities)” (Ibid.) Steadman also reminisced being interested “in continental structuralism in particular the work of Lévi-Strauss” but expressed doubts about whether other Centre members shared this interest (Steadman, Interview by Vardouli). He seemed more hesitant to enlist the Centre in any particular lineage of thought “other than the tradition of architectural Modernism, and in the new ‘systems thinking’ of the post-War sciences, amplified of course by the use of computers” (Ibid.). Echenique agreed that the LUBFS work was continuous with interwar Modernism — “Le Corbusier, Hannes Mayer, Russian Disurbanists, etc.” (Echenique, Interview by Vardouli).

667 Ibid., 102.

668 March was a key advocate and agent in architecture’s “academic drift.” He repeatedly argued for the separation of research aimed at building architecture’s knowledge base from research directly linked to building and architectural practice. He stated: “To mix practice with research within the university is to dilute the unique opportunity of the academic to take a long, cool view away from the hurly-burly of day-to-day decision-making and compromise” (March, “Research and Environmental Studies,” 91). Or elsewhere: “I think it is useful to distinguish between architecture as a process of design and creation, and architecture as a scientific study of the built environment - or, to be tough minded, to distinguish between architectural engineering and architectural science (what the Elizabethans called 'architectonics')” (March, “From Modern Movement to Vitruvius,” 106).

669 March wrote: “[...]the creative aspect of research” he wrote “requires recognition of an appropriate abstraction or idealisation of our subject by which we may represent it in order to reveal its essential structure.” (March, “Research and Environmental Studies,” 89).


671 Ibid.
“generalized definition of architecture” afforded by the development on the basis of the field’s presumed structural nature, promised furnishing architecture with a mathematical basis, thus helping it achieve academic respectability. But it heralded more than that. Representing architecture’s “essential structures” in mathematical terms also fueled hopes for finding equalities of form (isomorphisms) among problems manifesting in different scales of the built environment and among different knowledge domains despite their surface dissimilarities.

Relating architecture’s focus with “spatial organization” instead of solely physical building, as *The Geometry of Environment*’s subtitle suggested, enabled a broadening of the discipline’s academic authority to areas that had until then been within the purview of urban studies and planning. As Martin stated in his supportive foreword to the book, the extension of a study starting with a building “right through the whole environmental field” was a “fundamental contribution” of the book and offered “a base from which to build a more systematic design theory and indicates a direction which this might follow with advantage.”

Discourse on “scales of association” and the “structure of the human habitat” was already being articulated in the 1950s from avant-gardist architectural groups such as Team 10, while the image of the “architect-planner” having been installed even earlier within the CIAM meetings. However, the establishment of “urban design” as an academic field and the negotiation of architecture’s role in producing knowledge related to urban and environmental issues did not become institutionalized in the U.K. and the U.S. until the 1960s. March had been exposed to the contested position, and also to the potentials, of architecture for catalyzing urban research shortly

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672 Ibid., 6.
Leslie Martin’s position of architecture at the center of environmental studies was also echoed in Lionel March’s 1973 *Cambridge Review* article “Research and Environmental Studies.”
before starting the LUBFS Centre. During a two-year visiting research appointment at the Harvard-MIT Joint Center for Urban Studies in Cambridge, Massachusetts, March found himself surrounded by widely interdisciplinary teams of researchers who tackled issues at multiple scales of the built environment.

During his directorship at the LUBFS Centre, March aspired to consolidate architecture’s role within similar multi-scale and inter-disciplinary endeavors. The systemic idea of “spatial organization,” along with the representational device of the graph, was crucial in this effort. Recast as issues of “spatial organization,” problems vastly different in scale (for example, generating minimum housing layouts and developing a street network for a city) would be brought to the architectural researcher’s working table and become commensurable through the deployment of a mathematical representation that revealed their “essential structure.” As noted in a 1972 book compilation of LUBFS Centre’s research projects, the Centre’s work spanned three different scales:

The individual building, at which level it might be possible to examine the internal and external relationships which affect the built form; the scale of the urban sub-system or for example, the grouping of activities and buildings that are built up around the developing universities; and the major one of the urban system itself.

These different scales and contexts were integrated as “aspects of the main problem of relationships [...] and by looking at a question in this way the old barriers created by zoning are immediately removed.” Despite the unifying lens through which mathematics allowed to view

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675 In the afterword to the 1972 publication Urban Space and Structures Martin and March discussed the importance of new techniques for “establishing relationships between separate fields of investigation,” hence, he argued, the establishment of so many Joint Research Centers (“Afterword,” 263).
different environmental scales and problems, the LUBFS Centre did not strive for a unique or universal method of describing form. March asserted that “there is no great virtue in having a universal system when it is not required” and was a vocal advocate of a mathematical polyphony:

The theorist who has a wide range of mathematical models will be far more likely to recognise appropriate isomorphisms than the man without. It is for this reason that I believe a positive effort to transplant high level mathematical thought into our research must be encouraged. It is not a matter of being part of the normal work of academic architects as they exist today: if it is, we shall waste much time bungling our efforts and squandering our all too rare research resources.

**A New Visuality**

For March the discovery of isomorphisms among different problems and knowledge domains was a fundamentally aesthetic and creative endeavor. “This is at root an aesthetic activity which the arts and sciences hold in common,” he contended. The cultivation of a new vision sensitive to such structural mappings is what March famously termed, and historians subsequently scrutinized, the “systems aesthetic.” This aesthetic suggested a new kind of seeing in architecture, appreciative of deep, invisible relationships between things instead of their sense-perceptible surface appearances – a seeing that was about discerning essential structural equivalences (isomorphisms) among things (be it problems, theories, entities or other) that appeared dissimilar at first sight. In their 1971 “Polemic for a Structural Revolution,” March,

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Arguing for the effacing of boundaries between architecture and planning, Martin and March wrote: “Once some means are formulated to grasp more closely the relatedness of activities and the physical structure of the city, and once the urban system is comprehensible as a set of interacting and changing elements it becomes impossible to isolate from the system the interdependent parts. Within the totality of the urban system there are many restraints and there must be a range of choices.” (“Afterword,” 264).

679 March, “Research and Environmental Studies,” 88.
680 March, “Research and Environmental Studies,” 89.
Bullock, and Echenique polemically supported the potentials of this new kind of vision for emancipating designers from the precarious “drug” of architectural draughtsmanship and allowing them to “see” their decisions “in a hard intellectual light.”

Opposite to common interpretation, March et al. did not advocate for architectural typhlosis. Instead of interpreting the LUBFS Centre’s mathematizing work as iconoclastic, I propose viewing it as both symptom and instigator of a new visuality whose symbol and facilitator was the skeletal entity of the graph. The graph pictured systems and structures, thus rendering visible and operational what would otherwise be intangible and conceptual. “Intellectual vision” promoted the cultivation of an eye for aspects of architecture that were until then invisible and intangible and operationalization of these aspects through new mathematical representations.

The shift from “interest in appearance of the object” to “belief in an underlying structure [...] that the mathematics tried to capture” has been recognized by architectural historian Sean Keller as characteristic of the Cambridge University 1960s architectural theory culture. Keller identified this aesthetic as underpinning March’s and Alexander’s invocation of “form” and traced links with Martin’s engagement with constructivist art. Keller sees the “systems aesthetic” as an aesthetic of abstraction that replaces architectural drawing with logical, mathematical structures. For him, the mathematical rendition of the new aesthetic is boolean algebra — the kind of mathematics that enabled March to transcode Le Corbusier’s Maison Minimum as an array of letters and numbers. Here, I put forward a different reading. First, I propose that March’s systems aesthetic has more to do with structural mappings, with recognizing equalities of form,

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682 March, Dickens and Bullock, “Polemic for a Structural Revolution,” 275.
683 As Echenique argued in his Architectural Design paper on urban studies: “It is not enough to merely consider the city as a system; we need to be able to represent this system” (Echenique, Marcial. 1971. “An Approach to Urban Studies” In Models of Environment, Architectural Design 71 (5): 276).
685 Ibid., 176.
more so than simply with making structural abstractions. Second, I suggest that the matematizing work of the LUBFS Centre was not one of stripping architecture from its materiality but one of making concrete and workable aspects of architectural design and planning, which seemed palpably real in the 1960s but were not amenable to conventional representation. I will return to this point in my discussion of the Centre’s research projects. Third, and following from the two previous points, I suggest that graph theory provides a more productive entry point into the matematizing work of the LUBFS Centre than boolean algebra because of its dual nature as a part abstract/logical, part concrete/visible representation.

Keller reads March’s argument for an equivalence between the structure of “a physical column grid” and the structure of “mathematical lattice” as a “rhetorical ploy,” “the abuse of an extended metaphor,” or a means by which to “create a semi-mystical air” around the term “structure” as one connecting the domains in which it is used. He argues that “the representation and the represented entry need not have identical qualities — in fact it might be a rule that they must not, otherwise it would not be a representation but a repetition.” However, these remarks miss an operation that I see as the most crucial in understanding the LUBFS Centre’s work and the role of graph theory as structural representation: isomorphism. The recognition and matching of structures was the Centre’s modus operandi, driven by aesthetic, disciplinary, and pragmatic aspirations.

*The Geometry of Environment* is a telling example of the new visuality promoted through graph theoretic isomorphisms. The book featured plan, section, and axonometric representations of renowned architectural projects alongside mainly set theoretic and graph theoretic models of

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686 Ibid.
687 Ibid., 178.
688 Ibid.
689 Ibid.
690 Ibid.
these projects. This side-by-side juxtaposition was an argument for the benefits of representing and designing with the underlying structures of building as opposed to its surface appearances. The juxtaposition was not a claim for a complete replacement of architectural drawing from mathematical models. It was, however, a clear subjugation of the surface vision of geometric representations to the intellectual vision of structural representations. Elsewhere, LUBFS Centre members wrote:

Now it is certainly true of a set of architectural drawings as a symbolic description that they represent an inadequate means for a rigorous testing of the design against the programme requirements […] But, as we have suggested before, the technique of simulating a design, either partial or complete, in the form of a computable model whose structure is mathematical, is exactly the way to evaluate its predicted performance with a high degree of realism […] Not only can external environmental conditions be simulated, but also the anticipated pattern of use of the building. […] The process allows the intervention of taste, judgment and invention at a series of intermediate stages; but with the cardinal advantage that these are exercised in relation to a clear statement of the structure of the problem and the issues at stake at each stage.⁶⁹¹

With the new representational practice of mathematical modeling at hand, The Geometry of Environment chapters cast a new look at seminal works and driving debates in the history of the architectural discipline in order to reformulate, understand, and potentially revise them. In the chapter on “Planar Graphs and Relations,” prepared by Steadman, for example, the reader is presented with plan views and an axonometric section cut of Frank Lloyd Wright’s 1986 Devin House in Chicago [Figure 42] alongside an “adjacency graph” showing connected rooms [Figure 43]. Wright’s project was renowned for the ingenious way in which the architect had managed to preserve the independence of rooms (the spatial footing of individualism and democracy) within a symmetrical plan — a requirement, however, that had pushed Wright to a

two-storey arrangement. Steadman explained that the purpose of the project’s graph theoretic re-
representation is to ask the question “does the structure of the problem require such a
complicated solution” instead of “sitting back and enjoying the solution, in the same way that the
architect clearly enjoyed at arriving at it.” After a series of isomorphic transformations and
after applying theorems of graph planarity Steadman concluded that “Wright’s architectural
dexterity is certainly fun, but it is not functionally necessary.”

Similar unexpected insights revealed by graph theoretic representations of architectural plans
are also provided in the book’s first chapter “Mappings and Transformations,” where March and
Steadman compare Wright’s House for a Family of $5000-$6000 income (1938), the Ralph Jester
House in California (1938), and the Vigo Sundt House in Wisconsin (1938) [Figure 44]. Despite
their “extremely marked individuality” in that “each looks completely different from the other,”
the introduction of “a mapping known as a graph” shows that “whilst they may look different,
they are in fact topologically equivalent” [Figure 45]. Reading these chapters, another
aspiration behind the graph theoretic representation of Wright’s work emerges: not only to
understand the ingenuity of his architectural solutions or critique their necessity, but also to
locate his works within a design “space” of enumerable alternatives.

If a traditional plan or axonometric drawing represented a unique architectural solution
derived from the workings of an artistic genius, the graph presented a range of solutions
(alternative point-line connections) from which a designer could potentially choose depending on
various criteria. In their publications, LUBFS Centre researchers repeatedly argued that, by
enhancing understanding, mathematical models unlocked possibility. Despite common historical
interpretation that locates the Centre’s work in a lineage of thought that used mathematics in

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693 Ibid., 262.
694 Ibid., 28.
order to “optimize” design or predict its social and environmental outcomes, the Centre was driven by the “more modest” motivation to “understand [emphasis mine] the relationships” underlying the physical world not in an attempt “to achieve some imagined or desirable end result” but to “indicate a wider range of choice and a greater opportunity for a variety of patterns of living to develop.”

Not only did graph theory herald a particular kind of visuality within the LUBFS Centre, intimately connected with isomorphic mappings between structures, but it also signaled a particular construal of choice and possibility in design closely tied to ideas of combination between atomic units. The graph, a concrete visual entity that made structures and relations visible and workable, promised a new level of operational accessibility to topological aspects of architecture – aspects that, until then, could be only talked about in loose verbal terms. By making verbal topological statements mathematically concrete, it would become possible to finally relate function and physical form – not one singular form, but a variety of forms meeting specific functional constraints. The representation of a design as points and lines, entities and relationships enables counting possible combinations among these entities and relationships and evaluating them for meeting certain requirements. In the following section I discuss how functions of description and prediction delegated to mathematical modeling went hand in hand with new understandings of visuality and choice – understandings produced on the basis of the structural and combinatorial properties of graph theory.

4.3. Models of Environment and Ranges of Choice

Mathematical Modeling and Its Uses

In 1965 Ira Lowry of the RAND Corporation Logistics Department published a review article in the *Journal of the American Institute of Planners* that functioned as a key reference point for LUBFS Centre researchers.697 Titled “A Short Course in Model Design,”698 the article presented three classes of models arranged by “ascending order of difficulty.”699 First came “descriptive” models, which were translations of the observed world into the “coherent and rigorous language of mathematical relationships.”700 Then, came “predictive” models, where causality was superimposed in the relationships among a descriptive model’s variables. Predictive models allowed comparing the effects of alternative courses of action, as “knowledge of the future value of the ‘cause’ enable[d] one to predict the future value of the ‘effect’.701 Finally, “planning” models featured the additional layers of comparative evaluation between alternative courses of action, scoring of their effects according to some chosen goals, and selection of the highest scoring alternative.702 Lowry’s taxonomy sought to “provide some orientation to the model-builder’s way of thinking, interpret the jargon of his trade, and suggest a few standards for the evaluation of his product.”703 Such orientation appeared expedient, with modeling “tides […] flowing strong”704 in “a brave new world of computers, automation and space technology, and to

697 Bullock, Dickens and Steadman used Lowry’s taxonomy in “The Use of Models in Planning and the Architectural Design Process.”
699 Ibid., 159.
700 Ibid.
701 Ibid.
702 Ibid.
703 Ibid.
the astonishing status suddenly accorded to the scientist in government, industry and the military.’’

Intimate awareness of different types, functions, and purposes of models, alongside their constraints and limitations [Figure 46, Figure 47] was also a shared mandate among LUBFS Centre researchers, who repeatedly acknowledged modeling as the Centre’s defining activity. A nuanced positioning within the ever-growing and confounded field of model-related research in architecture and planning was not only essential for purposes of self-awareness, but also of bounding from adjacent activity in design and mathematics. The 1960s had been marked by a proliferation of mathematical models for architecture and planning, frequently developed for purposes of subsequent computer implementation. Most research in design and mathematics grappled with the question of how to create form and was polarized between two opposing fronts. On the one side, represented by operations research and design methods, lay the prospect of automatically generating building layouts that optimized specific economy and efficiency variables — much in the spirit of Lowry’s “planning” models. On the other side, most saliently represented by Alexander, was the critique that until “the inner relationships which

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705 Ibid.
706 A seminal attempt to synthesize and distill thought about models and modeling as it had been articulated systems theory, the philosophy of science, American planning, operations research, and other domains, was Echenique’s 1968 paper “Models: A Discussion” (LUBFS Working Paper 6). The paper listed and compared different functions, purposes, and types of models, one of which were mathematical models. An important influence for Echenique was Cambridge philosopher of science Mary Hesse’s book Models and Analogies in Science (1963. London: Sheed and Ward), where she discussed the role of models in scientific discovery and their relationships to the observed world.
707 Philip Steadman traced this work back to Alphonse Chapanis’s late 1940s work, which sought to match the layout of what with human activity. Most of these programs generated building plans with the goal to minimize circulation cost and used hospitals or factories as the building types on which they developed tested their methods. The use of graph theory was pervasive in computer aided design programs working on activity allocation, with Whitehead and Elders’ 1964 and Armour and Buffa’s 1963 paper being among the first.
go to make a form are better understood” and some clarity is achieved about “the complex nature of form and the complex nature of function” the use of mathematical modeling and mechanical computation will remain trivial at best, and distortedly reductionist at worst, for the purposes of generating physical form.

The LUBFS Centre researchers kept critical distance from both poles. They evaded this debate by arguing that the usefulness of mathematical models and mechanical computation lied not in the generation of physical form, but in the description and evaluation of various aspects of the design process. As Martin and March contended, “a mathematical formulation can take [a central and powerful place] when it is used for its proper function of descriptive analysis and evaluation [emphasis mine] within the total design process.” Most of the LUBFS Centre models therefore fell under Lowry’s “descriptive” or “predictive” class, with “planning” exceptions accompanied by disclaimers on the untenability and contingency of optimality. In describing the Centre’s unifying characteristics, Martin and March wrote:

The common background rests in the attempt to build up a substantial body of data from the real world and to illustrate all its complexities in some general representation or model so that the interaction and

Notably, Alexander’s critique focused on automatic building layout programs, taking as an example a computer program that produced hospital layouts on the basis of circulation cost minimization. Alexander highlighted the absurdity of circulation efficiency being considered the prime criterion for evaluating a hospital design’s success, as opposed to, for example, the patients’ wellbeing. He diagnosed that this reductive criterion was chosen in the first place because it could be “measured and encoded” (Ibid., 6), leading to the “ironic (situation) that the very tool which has been invented to unravel complexities imposes such severe restrictions on the design problems it can solve that the real source of complexity has to be eliminated before the tool can even get to it.” (Ibid.)

According to Philip Steadman “there were only two pieces of work that sought to optimise forms or designs in the whole varied output of LUBFS, and in one of these the word was used in a very different, specialised sense” (Steadman, What Really Happened,” 299). The first was Lionel March’s 1972 “Elementary Models of Built Forms” (In Urban Space and Structures, edited by Leslie Martin and Lionel March, 55–96. Cambridge Urban and Architectural Studies. New York: Cambridge University Press) where March sought to illustrate the contingency of optimal design solutions on the model’s initial assumptions for “didactic purposes” (Steadman, “What Really Happened,” 299). The other work concerned with optimization was Tom Willoughby’s extension of computer aids for automatically generating building plans that minimized circulation cost. This work was “peripheral” (Ibid.) to the Centre’s activity. As I discuss later in this chapter other LUBFS Centre papers outlined the several conceptual and practical problems of automatic plan generation on the basis of circulation efficiency.
interrelationship can be understood. From this first general presentation the development of the theoretical explanation of the process is a natural step.  

Viewed as a bridge between the empirical and the theoretical realm, a model was broadly construed as “a representation of a reality” expressing by “physical or conceptual means” the “relevant characteristics of reality” as “framed by the intention of the model-maker.” A mathematical model, in turn, was a type of model where the “description of reality is represented by the use of symbols and the relationships expressed in terms of operations.” “Built forms,” in that sense, were mathematical models of buildings that helped architect-researchers grapple with the complexity and uniqueness of actual buildings by isolating the aspects of building that were relevant for a given study. A particular mathematical model could “reproduce suitably chosen features of a physical situation” provided that “rules of correspondence between specific environmental situations and certain kinds of action and suitably chosen mathematical elements and operations” were established. The reproduction of physical situations turned mathematical models into useful tools for experimental testing of relationships between “environmental situations” and “kinds of actions,” “as full-scale experiments with real buildings” were “plainly limited since mistakes will be costly, and […] identical circumstances […] rarely repeated.”  

The explorative potential of mathematical modeling also figured center stage in the LUBFS Centre’s Architectural Design editorial-manifesto [Figure 48]. March et al. wrote:

715 Ibid.
716 Referring to the meaning of “built forms” March wrote: “Buildings are complex artifacts. Most are unique. Generalizations about buildings are not easy to make. It may help to look instead at built forms which are not buildings. Built forms are mathematical or quasi-mathematical models which are used to represent buildings to any required degree of complexity in theoretical studies. Built forms, then, are designed and defined specifically for each study.”(March, “Elementary Models of Built Forms,” 56).
Our common method is to formulate mathematical models which enable us to explore, experimentally, ranges of spatial patterns which accommodate various activities. This approach is shared by studies ranging over the continuum of physical scales from the individual building to the urban region. On the one hand, the work requires us to find appropriate mathematical representations which are isomorphic to the spatial and physical form of the building, site, or urban area; and on the other the modeling of patterns of activities at these scales.\textsuperscript{719}

Apart from declaring mathematical modeling LUBFS Centre’s unifying modus operandi and a response to the social and environmental impasses of the time, this excerpt identifies the two primary domains that came under mathematical scrutiny: “spatial and physical form” and “patterns of activities.” Construing space and activity as distinct but interrelated systems, LUBFS Centre researchers sought to model structures of space and structures of activity. In several LUBFS Centre models, graph theory was used to describe both spatial structure and activity patterns and establish correspondences between these otherwise incommensurable domains. The models discussed in this section taken from the Universities Study, the Offices Study, and an unfunded self-driven research by Steadman show how graph-theoretically enabled isomorphisms rendered time, space, and activity as mutually interacting structures open to description, manipulation, and matching. The three cases not only convey how LUBFS Centre researchers used graph theory in their models, but also how the properties of graph theory influenced their theoretical understandings of seeing, choice, and possibility in design.

*The Combinatorics of University Timetables*

As evident from their talks and publications, Martin and March were actively thinking about the ways that university structures and policies influenced the status of architecture and environmental research. The LUBFS Centre’s engagement with the institution of the university

\textsuperscript{719} March, Dickens, and Echenique, “Models of Environment,” 275.
went beyond educational philosophy and academic agendas to encompass its physical aspects. This was in the context of the Centre’s general orientation toward aspects of the built environment that fell under national programs of the postwar British welfare state, with educational buildings being one of them. Impelled by intense interest from national and philanthropic organizations, general changes in curricula and university education, growing numbers of students, and voices of concern being raised against the need for new university facilities, Martin initiated a group in 1965 to “study of the relationship between university student numbers, amounts of building and the use of land.” Staffed by Steadman, Nicholas Bullock, and Peter Dickens, the group was integrated in the LUBFS Centre upon its foundation in 1967, where it continued its operation as the Universities Study group with funding from the Council of Education and Science. The group’s work from 1965 to 1967 set a precedent and

720 In personal conversation Philip Steadman remarked “… the majority of the early financial support (to the LUBFS Centre) came from central government. You should appreciate that this was in the context of the post-War settlement in Britain, the creation of the Welfare State after 1945, and a general belief that architecture and planning could contribute to the common good […] So in this sense a great deal of the work at LUBFS was ‘applied’. It was paid for by government and its direction was specified by civil servants” (Steadman, Interview by Vardouli).

721 Since the establishment of the British Welfare State after the end of the second world war, educational building became a national priority. In his 1968 article “Education Without Walls” (RIBA Journal, 356–61), Leslie Martin reiterated a point that economist of education John Vaizey made his 1968 book Resources for Education, co-authored with John Sheehan. Martin admitted that “the educational programme since the War has involved great sums of money, and it has formed a considerable part of […] building output” (Ibid., 356). The prioritizing of educational building had invited scrutiny from Vaizey who argued for the necessity to examine the building of new university buildings in relationship to competing demands of housing and health facilities. Martin agreed with Vaizey’s observation that universities could not be seen in isolation from the rest of the built environment. However, for Martin, the answer did not lie in equally allocating funds national funds among competing kinds of social architecture, such as housing or hospitals. Rather, it was part of a broader research agenda on the relationship between built form and land use that he had already launched in the LUBFS Centre. “The theoretical study of physical form (of the effectiveness with which we use the land by buildings),” he argued, “is part of the consideration of the total decision-making process behind the way in which we spend our money on special categories of building” (Ibid.).


723 Prior to receiving funding support from the Council of Education and Science, the Universities Study was funded from the Gulbenkian Foundation. In personal conversation, Steadman saw Martin’s interest in university buildings and campuses as stemming from a “very large expansion in student numbers” and the founding of several new universities in the U.K. throughout the 1960s. According to Steadman, Martin himself had been involved in
provided testing ground for concepts, tools, evaluation methods, and presentation techniques of architectural research, which were later adopted by the LUBFS Centre.

As a “self-contained and well defined institution,” the university was an interesting case for architectural research in its own right. However, as Bullock, Dickens, and Steadman remarked, it could also be seen as a “subsystem” of the urban environment, whose study had the potential of more general insights about the relationship of land use and built form. The university was a case of what LUBFS Centre publications termed “the intermediate scale”: spatially or operationally defined clusters of buildings and activities that were more complex than an individual building and smaller than the urban whole. Modeling the university as subsystem of the environment was seen as bearing twofold potential. On the one hand, it would increase the efficiency of university planning decisions by linking together partial studies on the university that had been performed by separate agencies and organizations while connecting these studies with the broader urban context. On the other hand, it would “assist in understanding designing several university buildings, and had contacts with the University Grants Committee and the Gulbenkian Foundation. Before the founding of the LUBFS Centre, Martin invited Steadman, and his fellow students Nicholas Bullock and Peter Dickens, to work on the subject, possibly because he saw them as students that “might have a bent for research.” Steadman remarked that this “intuition [...] turned out to be correct” as all three became “career academics” (Steadman, Interview by Vardouli).

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Referring to “intermediate scale” building clusters, such as universities, offices, industrial plants and others, Martin and March wrote: “Such developing clusters can no longer be thought of as a series of individual buildings. They are ‘systems’ within which we need to understand the basic framework of movement by which they are related, the interaction of the parts with thus, the response of the building forms to the overlapping patterns of use that are constantly developing and changing. Cities are the main systems within which these sub-systems exist and operate” (Martin and March, “Introduction to Part 2: Activities, Space and Location,” 112).

The idea of an environmental system containing multiple subsystems figured prominently in Christopher Alexander’s thinking and was the driving idea behind his “Ten Year Program for Research on Environmental Design” — the initial inspiration for what would ultimately result in the A Pattern Language. For a discussion of the Program’s claims and aspirations, refer back to chapter 3.

The “intermediate scale” ultimately became the purview of the field known as “urban design” (Steadman, Interview by Vardouli).
the apparent complexity of the larger urban scene” by providing “a more detailed knowledge of how [...] sub-systems operate.” “A Theoretical Basis for University Planning” appeared in 1968 as the first LUBFS Centre Report, making the Universities Study group the first “to elaborate a method of work in Report form.” In this paper, the group defined the university as a system that comprised built forms occupying a specific amount of land and were animated by scheduled and unscheduled everyday activities. The university system’s model was “not to be confused with some kind of automatic design method for producing plans” or with an “effort to optimize designs or layouts in terms of some function or cost, efficiency or satisfaction of any kind”. The goal was to “increase [...] understanding and knowledge of that system” by making visible the interconnections of spatial and temporal variables.

LUBFS Centre researchers espoused the belief that “without a theoretical framework of reference factual information does not have any relevance.” The university model aimed at providing an integrative framework for the extensive statistical data that had been accumulated and the laborious cost analyses that had been performed to develop standards for various university building kinds. It aspired to bring together “hitherto isolated pieces of research and which, at the same time, would allow decisions made in one aspect of university planning, for

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728 Ibid.
731 Ibid.
732 Bullock, Dickens, and Steadman distinguished between making the university model “by defining the framework of the parameters and their interrelationships” and “formulating a plan to solve a particular problem in which the parameters take particular values.” The first was a “generalized” whereas the second a “specific” process (Bullock, Dickens, and Steadman, “A Theoretical Model for University Planning,” 115). The university model was not designed “to optimize in terms of particular constraints” but “to demonstrate the effect of one variable upon another, and to outline the consequences of different decisions at each stage” (Ibid., 116).
734 The Architects and Surveyors Group in the University Grants Committee had accumulated extensive statistical data and performed cost analyses to develop standards for various university building kinds. Pertinent data was also being made available by the Committee of University Building Officers, while Arup, and JRB Taylor, along with individual contractors also had relevant information. However, this information remained scattered and, as a result, difficult to apply in the design of university buildings and campuses.
instance numbers of students, to be seen in relation to the many other factors involved, for instance space.”735 By providing such understanding of what Martin and March called the “relatedness of things,”736 the model would offer university planners or policy makers a better sense of “the likely effect” of “different decisions.”737 As a model of an environmental subsystem “studying detailed patterns of behavior [sic] over relatively short time periods and occurring within a limited spatial context,”738 the university model would also contribute insights to the understanding of the environment as a whole. The goal was to explain “how detailed patterns of activity are arranged in time and space.”739

For that purpose, the university model was built in two distinct parts: a description of the university’s “physical context” (floor space, its distribution over land areas, its relationship with surrounding urban areas) and a description of university activities (“which students are where when”)740 [Figure 49]. Bullock, Dickens, and Steadman analogized these two parts with the “demand” and “supply” sectors of an economic system: university activities created a demand for space and kinds of buildings, which was met with a supply of different types of space.741 In terms of representing the building “stock,” the model focused first, on relating the university population to the floor area they needed for various teaching, research, and residential activities and second, on calculating the land used by relating the total floor area to built volume. The floor area calculation was a problem of describing activities in locations and movement between different locations742 for the student population. In that respect, this part of the model connected

739 Ibid.
740 Ibid.
741 Ibid., 123.
with a broader question about “the modelling [sic] of day to day activity patterns.”\textsuperscript{743} Scheduled activities were mediated by a regulating device, namely the university timetable. The timetable responded to the question “which students are where when.”\textsuperscript{744} The problem of modeling scheduled activities was therefore translated as a timetabling problem. Making student timetables was no easy feat, especially in cases such as Cambridge University, where the students could choose from a vast array of subjects. To address the combinatorial complexities of this problem, Bullock, Dickens, and Steadman deployed graph theory.\textsuperscript{745}

The graph’s vertices indicated “teaching events,” points fixed in time and space, and the links indicated conflicts between events. The first step was to enumerate all the combinations of teaching events in which a student was allowed to participate, based on university regulations. The next step was to define “teaching groups,” that is, students attending the exact same combination of teaching events. In order to produce the weekly timetable, the teaching events needed to be distributed in such a way that no teaching group was scheduled to attend two events at the same time. Constructing a timetable therefore became a problem of “packing” the graph of a teaching group’s teaching events combination “in the two-dimensional space of the timetable in a way that no conflicts occur”\textsuperscript{746} [Figure 50]. The dimensions of this “space” were the number of

\textsuperscript{743} Since the early 1960s, the modeling of activities had been an active and controversial research topic among design researchers. Several “mathematical tools” such as statistical analysis and game theory were already available from operations research, management science, and design research, but there was little agreement about their appropriateness for simulating human behavior. Once the model left the population scale of large numbers and zoomed in the erratic individual, statistical approximations of behavioral norms failed. As for game theory, the “possible moves” available to each individual in the university setting were too many and their “payoffs” too subjective and complex to make for a useful model of decisions and actions.


\textsuperscript{745} P. N. Toye of the Cambridge Mathematical Laboratory had developed an automatic timetabling program that enumerated options (not optima) of timetables and evaluated them. This program was published as the second appendix in the LUBFS Report No 1 (“A Theoretical Basis for University Planning”).

hours in the teaching week set against the range of rooms available.\textsuperscript{747} Opposite to statistical or probabilistic methods, graph theory provided definitive results:

Unlike most of the techniques for modeling activities discussed here, the timetabling algorithm produces results that are either valid or invalid: a particular timetable either satisfies the constraints or it does not — there is no source of statistical error, nor any need for calibration. […] The timetable provides a very complete model of “scheduled” activities: it not only tells us who will be where when, but also what they will be doing.\textsuperscript{748}

From this definitive model of scheduled activities, it was then possible to extrapolate floor area needs — how many, what size, and what types of rooms were needed to satisfy a specific pattern of teaching. Sometimes, only one distribution of teaching events was possible. Most times, however, because of the combinatorial nature of the timetabling problem,\textsuperscript{749} there was more than one valid solution in how teaching events could be distributed in time and space. In such cases it was not possible to directly relate a particular teaching pattern with the number of rooms needed to satisfy it. Graph theory then allowed one “to explore the extreme limits of the ‘solution space’ within which all possible timetables for a given set of constraints must fall”\textsuperscript{750} and to relate these possible solutions with the numbers and kinds of rooms required to satisfy a particular teaching pattern.\textsuperscript{751}

In the university study, the graph was exploited for its combinatorial capabilities and its capacity to establish correspondence between distinctive domains. In this case, the graph bridged the otherwise incommensurable domains of time, space, and university curriculum by collapsing

\textsuperscript{747}Ibid.
\textsuperscript{748}Ibid.
\textsuperscript{750}Ibid.
\textsuperscript{751}Graphs did not only represent the combinations of subjects allowed by different academic policies, but also described the “complexity and overlapping of courses in abstract mathematical form.” This made possible to contrast the “differing effects on the use of floor space through the allocation of teaching facilities at the level of the department, the faculty or the whole university.” (Bullock, Dickens, and Steadman, “The Modelling of Day to Day Activities,” 150).
them in a single structural representation. A convenient mathematical representation for encoding in a computer, the graph fostered optimism for exposing ranges of possible combinations between subjects, their distribution in time, and the spatial implications of such distribution. Changes in the timetable were seen as bearing the possibility to alleviate space crisis without modifying the number of students or the available “building stock.” Another implicit argument of the timetable model was that it could operate in multiple dimensions, allowing for seamless transitions between academic policy, day-to-day schedule, and floor space. Essentially, each “teaching event” collapsed a unit of time (hour/day designation), a unit of space (room), and a unit of the curriculum (subject) into one single point. As such, the graph oscillated between a temporal, a spatial, and a symbolic representation.

However, in practice the use of the graph forced the reduction of the dimensionality of these three domains in points: subjects were reduced to symbolic labels, dynamic temporality to time chunks, and three-dimensional space to non-dimensional points. Bullock, Dickens, and Steadman admitted that the truly interesting aspects of the university activities were the ones that happened between the points; the times that the students did not follow the schedule, the impromptu uses of space, the implicit coalitions of academic subjects. This was in the spirit of a broader ethos of modesty that permeated the LUBFS Centre and frequently made it in the various working papers as an awareness of their mathematical model’s inability to capture the fine grain of reality. The delegation of mathematics to the provinces of the abstract and the ideal is a debatable move, to which I will return in subsequent sections. The interest of graph theory’s use in the university study is the effort to establish isomorphisms with the dimension of time. This stands out against a backdrop of more explicit and empirically palatable isomorphisms of graph theory with spatial organization, room layouts, or routes in buildings taken up in the Offices Study.

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Isomorphisms Between Operational and Spatial Organization in Offices

The LUBFS Centre Offices Study group started its operation in 1967 with funding from the MPBW. The group’s task was to investigate “the internal and external determinants of the built form in order to develop computer aids for the design and spatial and physical evaluation of office buildings.”\textsuperscript{753} Around the same time, the MPBW had instituted a standing Committee on the Application of Computers in the Construction Industry (CACCI), with the goal to “keep under review,” “identify […] need for coordination of efforts,” “take action,” and “make recommendations”\textsuperscript{754} to consolidate and advance computer applications in the construction industry. CACCI’s activity revolved around sub-committee reports,\textsuperscript{755} conferences,\textsuperscript{756} and bibliographic compendia,\textsuperscript{757} which, despite not outputting significant applications for architectural design,\textsuperscript{758} helped consolidate and promote research on computer aided architectural design. After 1969, the MPBW sponsored a Computer Aided Design Study group within the LUBFS Centre in support of the Offices Study. The Offices Study group divided its contract from the MPBW in two sub-studies: one concerned with “physical evaluation” (lighting, sound, air circulation etcetera) of the office building, led by Dean Hawkes, and one concerned with the building’s “design and spatial evaluation,” led by Philip Tabor. The process of making decisions on the design of large office buildings involved many different parties and was notoriously

\textsuperscript{753} The description of the Offices Study Group is taken from the list of all LUBFS Centre study groups, their sponsors and dates of operation at the end of the 1971 Architectural Design special issue (322).
\textsuperscript{758} Maver, “The Computer as an Aid to Architectural Design,” 15.
inflexible, with “experiments [being] time-consuming and decisions expensive to unmake.” Tabor set out to “lubricate” the process by developing “both requirements and built solutions simultaneously and interactively.” In order to achieve this, it became necessary to forecast quickly and accurately “the effects of decisions […] on both the workings of the office function and its accommodation.”

The end goal of these decisions was efficiency, a pervasive yet elusive value in modern architecture and design. Tabor defined efficiency as “the weighing of its capital, maintenance, and running costs against the accuracy and lack of friction with which it accommodates its function.” His interest gravitated toward the second part of the definition, the office’s “operational efficiency,” which was neglected by most architects concerned with “building efficiency.” Concerned with “quantity-based systematic design,” he focused on simulating “the interactions of the more quantifiable and predictable aspects of building performance within a symbolic model.” With the argument that “a central relationship between the office function and its accommodation is the effect of built form and layout on communication patterns,” Tabor linked building performance (operational efficiency) with pedestrian circulation.

Throughout the 1960s, work in operations research and design methods had produced several mathematical techniques for modeling pedestrian traffic in buildings such as hospitals or

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760 Ibid.
761 Ibid.
762 Ibid., 2.
763 Ibid., 6.
764 Ibid., 60.
765 Ibid., 1.
766 The importance of communication in offices became paramount after the introduction of Automatic Data Processing. In the mid-1960s, Ministry of Labour reports were speaking of “executives” and “machine operators” as the new two classes of employees, and were projecting executives outnumbering machine operators. Drawing from such reports, Tabor anticipated a probable increase in “the volume of information to be acted upon, and the speed with which decisions must be taken,” this making “communication […] a more frequent, urgent and delicate function of the office” (Ibid., 4).
factories\textsuperscript{767} [Figure 51]. Tabor discussed the potentials and shortcomings of adapting these techniques in office buildings in a “practical comparative review.”\textsuperscript{768} A distinctive feature of Tabor’s papers was the mandate of applicability in design situations: the techniques under consideration were evaluated not only for their adequacy as mathematical models (such as, for example, in the Universities Study), but also for their value and ease of use as design aids. In four consecutive working papers,\textsuperscript{769} Tabor reviewed techniques for modeling pedestrian circulation, locating activities in space,\textsuperscript{770} analyzing communication patterns in an organization,\textsuperscript{771} and evaluating routes in a building.\textsuperscript{772} Among these techniques, graph theory figured prominently as a tool for describing, and mapping between, the office’s operational and spatial structure.

In keeping with his precedents, Tabor suggested to model pedestrian traffic as a connection between “activities.” These were groupings of operational components that occupied “continuous, ‘compact’ space”\textsuperscript{773} and functioned as a start or end point for pedestrian traffic. The office components, in turn, were groupings of the office’s “indivisible atoms” (individual employees, pieces of equipment, furniture etcetera) nesting to form larger components or subsystems (workstations, office departments, various facilities etcetera). The office system was essentially activities (groupings of components) connected to each other with some type of relationship. As such, modeling the office system fell under the purview of graph theory. Tabor contrasted three types of linear graphs [Figure 52] commonly used to diagram office

\textsuperscript{767} A key precedent for Tabor’s study was Whitehead and Eldars’ 1964 paper “An Approach to the Optimum Layout of Single-Storey Buildings” (Architect’s Journal, 1373-80), where they used the case of an operating theater suite to present a general method for matching the structure of a building’s layout and the activities it accommodates.
\textsuperscript{769} Tabor, “Pedestrian Circulation in Offices.”
\textsuperscript{770} Tabor, “Traffic in Buildings 2: Systematic Activity Location.”
\textsuperscript{773} Tabor, “Pedestrian Circulation in Offices,” 10.
organization in order to identify “how the principles governing their grouping differ from each other” and the “subtle dissimilarities” in the objects and relations they represent. Graphs could represent relationships of subordination and responsibility (“organizational” graphs), containment (“classificatory” graphs), or connections between origin and destination points in the office (“operational” graphs). Each graph had a different structure. The organizational and the classificatory graphs were, respectively, what we previously saw Alexander calling “trees” and “semi-lattices.” They were conceptual representations of the subsystems that built up the office system. The operational graph was topological, it did not represent hierarchy or containment, but the end points of routes and communications with the links weighed according to the strength of their connection.774

Tabor evaluated the operational graph as “the most useful to the physical planner.”775 “The connections are described more comprehensively and in more detail than in any of the others,” he explained, “they are real and not conceptual [emphasis mine], and they are those whose efficiency is directly affected by the building.”776 By appraising the “realistic” aspects of the operational graph, Tabor was pointing to the one-to-one correspondence of its vertices with empirically apprehensible entities with distinct locations in space. The trade-off for the operational graph’s isomorphism to the “real world” office atoms was its unordered nature. “Whether simply to aggregate activity atoms for their manipulation […] to sense groupings of these larger aggregates in manual design, or to generate plans automatically […],” Tabor wrote, “— this information needs ordering.”777 This was a problem of classification [Figure 53].

774 The strength of the connection between end points of routes was calculated based on how frequently trips happened between them and how costly these were. Tabor called this measure “association.”
775 Tabor, “Pedestrian Circulation in Offices,” 19.
776 Ibid.
777 Ibid.
Tabor dedicated one of his four Offices Study working papers to a comparative review of “taxonomic approaches.” Although Tabor’s specific interest was in design for pedestrian communication, taxonomic techniques had “far wider relevance wherever a complicated mesh of requirements and relationships must be untangled by designers”\textsuperscript{778} often faced with the problem of “reducing of complex data to intelligible order.”\textsuperscript{779} Ordering the operational graph was a problem similar to the one that Alexander and Manheim tried to resolve through their HIDECS 2 and HIDECS 3 programs: taking a linear graph as “input” and “outputting” a hierarchical structure, be it a tree or a semi-lattice. Despite the popularity of the decomposition approach in operations research and design methods, Tabor rejected it because of several weaknesses “inevitable when forcing a fundamentally non-hierarchic pattern into a hierarchic network of disjoint subsets.”\textsuperscript{780} Instead, he proposed a “scaling” algorithm that arranged points (activities) in a multidimensional space with distance being a measure of their connection’s strength. Points close to each other were then grouped and identified as office subsystems. Such scalings managed to “depict relationships practically unobtainable by hand.”\textsuperscript{781} However, in order to be of use to designers “visual presentation”\textsuperscript{782} was key. As a visual representation of structure, the graph could “considerably strengthen the diagram’s informative power.”\textsuperscript{783} Derived through manual or automatic modification of the scaling diagram by inserting edges between the ordered points, a tree-like graph (dendrogram) showing the resultant activity clusters and their relationships afforded “intuitive comprehension”\textsuperscript{784} and contributed toward the goal of a “lucid,

\textsuperscript{779} Ibid.
\textsuperscript{780} Tabor, “Pedestrian Circulation in Offices,” 23.
\textsuperscript{783} Another visually palatable presentation complementary to the graph was the set theoretic Venn diagram that showed groupings and containment.
\textsuperscript{784} Tabor, “Structures of Organisation,” 319.
flexible and suggestive depiction” by being “visually digestible.” The use of the diagram in architectural design encouraged a “pragmatic” attitude that sacrificed mathematical accuracy in favor of the qualities that were arguably aesthetic: with non-hierarchic graphs of the activity groupings being “difficult if not impossible to construct and grasp,” Tabor expressed preference for hierarchic representations that depicted the office system “accurately, comprehensively, and with visual economy.”

Apart from participating in the office operational organization, activities also had a “spatial identity,” a “unique and unambiguous” location representable by a dimensionless point. Because of this one-to-one matching between activity and location, ordering activities by measure of circulation bore promises of also “grouping, clustering, classifying” the spaces they occupy. Once activities had been assigned to locations, it was possible to evaluate the traffic between them by multiplying their “distance” and their “association” (the strength of their connection, a product of the frequency of trips between them and their cost by traveler’s salary) and also use similar measures to generate built forms or layouts that minimized travel costs. Route evaluation was a problem common in geography, 

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785 Ibid., 316.  
786 Ibid., 319.  
787 In their seminal 1967 work Models in Geography, a book that decisively influenced the LUBFS Centre researchers’ ideas on mathematical modeling, editors Chorley and Haggett emphasized the crucial importance of a model’s “intuitive grasp.” They wrote: “The intuitive grasp (Gestalt knowledge) of the capacities and implications of a model is thus the key to the exploitation of its suggestive character” (23). This is a crucial point, especially as it connects with the graph’s visuality and empirical accessibility as one of the alleged potencies that sanctioned it as an a mathematical modeling tool.  
789 Ibid., 61.  
790 Tabor listed several different ways of measuring distance between two activities. On the one hand, there were spatial measures that took distance to be radial along a diagonal connecting the points, rectangular turning a right angle on a corridor, or rectangular with detours. On the other hand, there were temporal measures that took into account speed altering situations such as congestion, vertical movement and so on. (Tabor, “Pedestrian Circulation in Offices,” 36-41).  
791 Tabor distinguished “layout” from “built form.” He wrote: “Layout occurs often, both before and during a building’s life, and although it may be the architect’s task, to is properly the province of management. … Built form decisions are executed just once, on the other hand, and are the architect’s concern.” (Tabor, “Traffic in Buildings 4: Evaluation of Routes,” 4.1).
sociology and management science: how to determine the shortest paths between pairs of points in a network. Graph theoretic techniques were in good currency for this problem. However, their topological focus (expressing connectivity only) had several shortcomings when it came to accounting for the geometric aspects of a building or “dealing with real dimensions.” Problems also arose in defining the graphs vertices, both because three-dimensional spaces had to be reduced to zero dimensional points and because, in defining the route graph, it was difficult to differentiate between endpoints and junctions along a path [Figure 54].

Graph theory also held a prominent position when it came to assigning activities to locations so as to minimize the cost of communications between them. Known as “systematic activity location,” “relative location of facilities,” “relationship layout,” “assignment of facilities to locations” or more simply as “circulation,” this problem had been the topic of several publications architectural and operations research journals in the U.K. and the U.S. Techniques for grappling with this “unsolved, perhaps insoluble” problem of optimizing circulation were either “additive”, where a built form was produced from zero, or “permutational,” where activities were rearranged within a given building envelope [Figure 55]. Tabor explained that because of the combinatorial complexity of the problem (the immense number of possible

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792 Ibid., 4.16.
793 Ibid., 4.17.
795 Ibid.
796 Additive activity location techniques operated on a predefined spatial framework (a generic grid or a more defined framework informed by the activities and their distances), where activities were placed and evaluated in relation to already placed activities. As the evaluation of all possibilities was computationally intractable, the evaluation proceeded with suboptimal techniques (evaluating activity pairs within every possible plan or considering only some plans but every activity pair within each of the chosen ones). After the completion of this process, manual modification to acknowledge constraints (organizational, equipment, environmental, psychological, geographical, structural, legal, financial, etc.) (Tabor, “Pedestrian Circulation in Offices,” 51).
797 Tabor characterized permutational techniques more managerial than architectural (as activities were simply being rearranged in space without changing the form of the building), albeit more realistic as most office buildings were organized and reorganized within a specific perimeter. An initial layout (location-activity assignment) was chosen temporarily (“to restrict the area of search in face of the combinatorial galaxy”) and was then modified gradually by exchanging activities and evaluating circulatory efficiency for every new assignment. (Ibid., 57).
activity combinations), the absolute best could not be guaranteed and designers had to resort to suboptimal methods, either manual (“needing only the equipment and skills of the design office”) or automatic (performed by a computer). Although manual methods, “loosely graph theoretic in origin,” could not handle more than a dozen activities without becoming unmanageably difficult, they presented a “simplicity rightly discouraging rigidity in their use.”

Automatic techniques could handle three times as many activities, but usually required to “pre specify of building shape and making impossible the flexibility of manual procedures.”

Despite practical difficulties having to do with the size of the program that could be calculated through manual uses of graphs, representing a circulation system in graph-theoretical terms carried the highest potential, because of its ability to establish isomorphic and therefore undistorted mappings between two different systems: the layout or building form and the operational organization of the office [Figure 56, Figure 57]. It is no coincidence that Tabor’s working papers were used as illustrations of graph theory and network techniques in The Geometry of Environment. In discussing the potentials of graph theory Tabor wrote:

The possibility therefore arises of projecting a given building from three into multidimensional space, and comparing its configuration with the association network similarly projected. Not only might specific activities in this way be assigned locations, but a degree of fit derived between characteristic building forms and typical operational patterns [emphasis mine].

In a 1964 condemnation of CAD directed to some of the precedents that Tabor cited, Christopher Alexander had argued against the usefulness of examining large numbers of alternative layouts. He wrote: “in theory” examining more alternatives “is a reasonable objective […] But in practice, although the number of alternatives the computer can examine is large, the range of

798 Tabor, “Traffic in Buildings 2: Systematic Activity Location,” 2.32.
799 Ibid.
800 Tabor, “Pedestrian Circulation in Offices,” 29.
these alternatives is small, because the computer can, at present, only examine a very restricted type of solution.\textsuperscript{801} Because of its inability to produce “truly unexpected”\textsuperscript{802} alternatives, the computer outputted design options that were different only with respect to trivial measurements but not qualitatively different. Alexander likened this process with comparing millimeter differences in the dimension of a block of wood intended to block a car from sliding or “measuring the size of a cooking apple with a micrometer.”\textsuperscript{803} Tabor’s remark about fitting characteristic building forms and typical operational patterns, speaks to a more ambitious use of graph theory to expose structurally distinct spatial organizations, represented in a form that could allow for comparisons with the operational structures that they were designed to accommodate. Once the mathematical model’s goal shifted from outputting one optimal solution to presenting possibility, possible layout variations and building forms became “totally enumerable”\textsuperscript{804} using graph theory. Such exploration of the extents and limits of a designer’s layout choices was the topic of Steadman’s research on minimum-standard house plans.

\textit{The Combinatorics of Possibility in Minimum Standard House Plans}

In the first half of the 1970s, the realm of housing became a central experimentation site for graph theoretic and topological thinking, coupled with possibilities of automatic layout generation afforded by computers. The small number of rooms in a house circumvented the problems of combinatorial explosion that Tabor identified in his review of activity location techniques. Also, in the case of the house, instead of reducing circulation cost, the goal of topological representation was to introduce certain kinds of adjacency and proximity constraints,

\textsuperscript{801} Alexander, “A Much Asked Question About Computers in Design,” 5.
\textsuperscript{802} Ibid.
\textsuperscript{803} Ibid., 4.
\textsuperscript{804} Tabor, “Traffic in Buildings 2: Systematic Activity Location,” 2.8.
ensure their satisfaction, and consequently fit rooms of particular sizes into a specifically sized shell. Steadman developed a method for deriving such layout variations in his 1970 work “The Automatic Generation of Minimum-Standard House Plans” that appeared as the LUBFS Working Paper #23. This was one of the few LUBFS Centre papers that addressed the residential scale – a privileged site for architectural theory because of its intricate relationship with individual habitation. The home was also a thriving topic of interest for governments and industry in the postwar period in relation to social housing.

Steadman worked on this project unfunded and motivated by theoretical concerns about architectural planning. Yet in the spirit of most LUBFS Centre working papers, he accompanied his mathematical expeditions with a problem and data drawn from U.K. architectural realities. In the late 1960s, the National Building Agency (NBA) had put forward a “Generic Plans” and a “Metric House Shells” catalogue, which listed 159 shells allowing 400 distinct possible plan types. These were based on minimum dimensional requirements developed by the Parker Morris Committee and presented in their 1961 Report *Homes for Today and Tomorrow*, and on activity requirements from the 1968 Ministry of Housing and Local Government pamphlet *Space in the Home* [Figure 58].

The question that the NBA was asking and that Steadman aspired to address, was one of central concern for postwar social housing: how to adopt a cost-effective set of (dimensional) standards for industrialized building construction, “without any significant reduction of choice in

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layout or design.” Following boisterous critiques on the cookie-cutter solutions and functional determinism of interwar and early postwar social housing, choice and adjustment to the needs of individual users was a bone of contention. In his Working Paper, an edited version of which also appeared in The Geometry of Environment, Steadman walked the reader through a step-by-step process in which discrete rooms, each dedicated to a specific activity, were mapped to points in a graph, while the graph’s lines corresponded to relationships of adjacency or proximity. Steadman pointed out that “the plan itself can be regarded as a graph,” with the walls or room boundaries being the graph’s lines and their intersections being the graph’s points. This graph, describing the physical elements of the architectural plan, could be automatically translated into a topological description of the plan (types of rooms and their connections) that it accommodated. This was done by taking the graph’s “dual” — another graph that had a point for each area of the initial graph bounded by lines. The graph of the floor plan and the adjacency graph (the topological description of how rooms connect with each other) were linked with the dual relationship — an observation that proved crucial for automatic layout programs as it allowed the transition from a description of an architectural programme to a description of physical form [Figure 59].

The graph model of an architectural plan allowed Steadman to enumerate all permissible topological variations for the same number of points (rooms) and to evaluate each variation in relation to a set of adjacency or proximity constraints. Prior to Steadman, graph theory had been used in the context of architectural layout design. Most of this work was concerned with

810 The NBA’s selection of the twenty-two preferred metric shells has met with fierce opposition, and architects see the proposed mandatory restriction to these few sizes as unreasonably limiting. Ibid., 26.
811 Ibid., 12.
812 P.H. Levin (1964) used graph theory to optimize layouts, Casalaina and Rittel (1967) to generate layouts from adjacency matrices. The year that Steadman issued his working paper further publications appeared by J. Cousin and L.C. Teague, who used graphs to compute three-dimensional configurations.
ordering the complex requirements of an architectural program. Steadman acknowledged the usefulness of graph theory for disentangling complex architectural programmes. “Particularly when the requirements for adjacency in some given layout problem are complex,” he wrote, “it may well help the architect to sit down with paper and pencil and draw out the graph of these requirements.” His method’s main contribution was not in programmatic reasoning, but in bridging it with the dimensional, geometric aspects of an architectural plan. Steadman used graph theory to fit rooms of specific dimensions within a given shell, while preserving a desired topological relationship of adjacencies and proximities among these rooms [Figure 60].

To produce such dimensional alternatives, Steadman applied a method for dissecting rectangles into squares developed in 1937 by Tutte—who we heard in chapter 2 reminiscing on graph theory’s pre-1950s status in his commentary to König book’s English translation— along with his “Trinity Four” collaborators Brooks, Smith, and Stone. The method was popularized in the 1960s by mathematics and science writer Martin Gardner. Following this method, Steadman put forward a “curious analogy” between the problem of fitting rooms within a given shell and the flow of electricity in a closed circuit. When represented as directed graphs,
both electrical circuits and architectural floor plans were guided by the same rules. In the electrical circuit there are amounts of current, while in the floor plan there are sizes of rooms. In both cases, the amount (current or size) entering a node of the graph needs to equal the amount leaving that node. With that in mind, it is possible to calculate how rooms of specific sizes can be distributed within a shell of given dimension. Apart from its technical value, the analogy also supported March’s contention that mathematics can reveal correspondences among disparate knowledge domains. Steadman’s graphs revealed that Kirchhoff’s second law held true for architectural floor planning [Figure 61, Figure 62].

The graph theoretic isomorphism of an architectural floor plan did not only build bridges between architecture and physics, but also fostered alliances with the rising field of computer research. Although computers were not yet readily available at Cambridge, there was work being done in translating mathematical models into “data structures.” Referencing Cambridge mathematician, computer scientist, and computer aided design pioneer Crispin Gray, Steadman explained that data structures were also graph-like: they consisted of data-containing blocks, interconnected by relation-representing pointers so as to be optimally efficient for specific transformations of a mathematical model. The relational structure of the graph was easily representable in a machine by an “incidence matrix”: a tabular equivalent of a graph representing the graph’s vertices and their connections in binary form. This dual nature of the graph as both

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817 The Cambridge Mathematical Laboratory, later renamed as the Computer Laboratory only had one computer, the Titan, an epigone of the experimental EDSACs (Steadman, “What Really Happened,” 291).

818 In discussing the transcription from graph to computer code Steadman appeared wary of conflicts between the dynamic nature of the graph computation done by hand, which he described in his paper, and the static representation of the graph in a computer. The nodes, he explained, needed to be updated dynamically. Elsewhere, Steadman also described problems of combinatorial explosion for a large number of rooms. In personal conversation, he referenced a system that he built with Bill Mitchell and Robin Liggett, which could produce all plans with up to about 10 rooms. A rectangular dissection system developed by Ulrich Flemming’s (DIS) pushed this limit, without, however, escaping limitations in the number of rooms that the program could ultimately handle. Because of these issues, Bill Hillier of the University College London —founding figure of “space syntax,” a structural method for analyzing spatial patterns— drew the conclusion that architectural design is not an ‘ars
a visual entity that could be intuitively grasped in a singular act of seeing and as a table of numbers ("incidence matrix") captures the meaning of "intellectual vision," as articulated by March et al., and was a source of seductiveness for architects. The graph was viewed as both numbers and image, both reason and vision, both rational abstraction and empirical concreteness thus bridging long disciplinary traditions preoccupied with seeing with a modern discipline relying on structural abstraction.

By representing a house layout as a graph and applying theorems of graph theory, Steadman aspired to provide a mathematical answer to the extents and limits of choice, to "show designers how the ranges of possible options available to them are limited by the laws of geometry and topology." In his 1973 paper "Graph Theoretic Representation of Architectural Arrangement" Steadman contemplated uses of graph theory as a design tool. He distinguished between a "heuristic" and an "enumeration" approach. The "heuristic" approach was manual and it operated by trial and error, using the graph to devise a acceptable solution for a set of adjacency or proximity requirements. This solution was usually singular and the graph operated as an aid for expediting and "mechanizing" a process that the architect would traditionally perform (producing a layout with some constraints in mind). The "enumeration" approach was required a computer and was "perhaps intellectually more interesting and elegant" by virtue of being exhaustive. Steadman posited: "The program need only be run successfully once for any

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particular problem, and then the results are known [emphasis in the original]." He also spoke of the possibility to use an exhaustive method to identify “all feasible solutions [emphasis in the original],” once and for all,” and entertained the idea of circulating to designers the record of these solutions, possibly in book form [Figure 63].

Mapping an architectural plan on a graph and exhaustively listing different combinations between the graph’s vertices would enable the development of complete “menus” of abstract layout choices whose architectural details would be rendered by the designer. Steadman argued that such enumeration could prove not only practically useful but also motivate research experiments ranging from establishing a “precise” correlation of governmentally set housing standards (dimensional or otherwise) with the “variety of allowable plan types” to exploring relationship between prefabrication systems and spatial arrangement possibilities. To be sure, Steadman did not argue that such menus would contain fully-fledged architectural designs, but rather layout suggestions whose details would then be decided by the individual architect. The architect would thus become empowered with a complete knowledge of an architectural problem’s possible solutions and the implications of each in terms of efficiency, cost, relationship with occupant’s activities etcetera. The idea of lists of architectural options generated this way played a key role in issuing correctives to the frequently prescriptive functionalist theories of Modern architecture. As March described it elsewhere:

This kind of modelling [sic] avoids criticism of 'tight fit' functionalism while ensuring that a 'loose fit' approach is not sloppy but, on the contrary, as well tailored as a good off-the-peg suit.  

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823 Ibid.  
824 Ibid.  
825 Ibid.  
826 Ibid., 105.  
827 March, “Modern Movement to Vitruvius,” 103.
In LUBFS, the combinatorial thinking afforded by graph theoretic representations would become a synonym for choice and possibility in architectural theory, and would ultimately become installed as a natural way of thinking about freedom and constraint in design. This will become evident as I next examine the ways in which such connotations validated the use of graph theory in participatory design.

4.4. To See in a Hard Intellectual Light
The previous chapter ended with Harary lamenting Alexander’s missed opportunity to exploit the intuitiveness of the graph’s “geometric representation.”828 Harary contended that, by virtue of being visually presentable, graph theory “enable[d] the nonmathematician to make more effective use of the model”829 and offered “enhanced understanding of […] abstract mathematic concepts.”830 More so than to inspect their mathematical accuracy, Harary’s look at Alexander’s theories aimed to lobby for graph theory’s advantages as mathematical modeling device. The generalized surge of interest in mathematical modeling in which the LUBFS Centre willfully participated, fueled competition between mathematical techniques and their respective mathematician advocates. A vocal campaigner of graph theory’s modeling potentials across disciplines, a campaign on which he ultimately built his reputation, Harary capitalized on structuralist and system theoretic intellectual currents. Alluding to the isomorphism between graph theory and structural concepts in different fields, he persistently highlighted the value of “knowledge of the mathematics of abstract structures” for “investigators invested in various kinds of empirical structures.”831 Claims of graph theory’s value were endorsed by famed

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829 Ibid.
830 Ibid.
831 Harary, Norman, and Cartwright, Structural Models, v.
representatives of such “investigators” —from anthropologists Claude Lévi-Strauss and Edmund Leach to geographer Peter Haggett, to list some frequently referenced names in the LUBFS Centre— who spoke of a fortuitous alignment between the “mathematics of abstract structures” and their efforts to establish a structural understanding of their respective domains. These figures variously expressed optimism about the new possibility to mathematically represent structures without distorting their intuitive reference to the empirical world, as was the case with the old mathematics of number.

In purposeful alignment with some of these voices, at times, and in unintentional assimilation of a pervasive discourse, at other times, the LUBFS Centre researchers embraced the mathematics of abstract structures as a new form of architectural representation. By repurposing graph theory from making design programs (prescribing design moves so as to achieve a better “fit” between form and activities, à la Alexander) to making models (representing relationships between form and activities and deriving spaces of combinatorial design possibilities) the LUBFS Centre’s activity aligned with Harary’s corrective to Alexander’s use of graph theory. In their publications, LUBFS Centre researchers became themselves advocates of graphs’ realism and intuitiveness, but also of its digestibility and manual flexibility within design processes. This is not to be seen as a parroting of the pervasive rhetoric about the mathematics du jour, although the adjectives “modern” or “new” were certainly attractive qualifiers for the new “attitude of mind” toward architecture and planning that the Centre promoted. LUBFS Centre researchers were careful not to “incant” concepts and methods, but to self-consciously adopt them — something evident from the many “practical comparative reviews” and taxonomies of types and techniques of modeling that they put forward. Such self-consciousness, however, did not shield them from the cultural allure of the graph nor made them aware of all tensions underlying its

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adoption. The graph’s entry into design was reinforced by preassembled meanings of modernization that it carried from mathematics and the broader cultivation of a structural imagination of the world. These symbolic meanings were not always in concert with the graph’s operational properties, thus opening space for critiquing tensions within the LUBFS Centre’s mathematical modeling activity.

The truth claims associated with the isomorphism between designs and graphs presupposed what I call “structural realism”: the idea that empirical phenomena presented themselves with a distinct structure that could be mapped and rendered workable. So far, I have discussed how this idea was reinforced through the mutual bootstrapping among the academic vogue of structuralism and systems theory, a structuralist turn in mathematical education, and the amenability of structural models for computer encoding and analysis. However, structural realism was also validated by the realities of postwar architectural production. March traced his interest in architecture’s mathematization back to “a thought planted” by British architect and industrial designer Bruce Martin during his teaching appointment at Cambridge in the mid-1950s. An avid explorer of pre-fabrication and modular coordination, mostly remembered for his design of the British red telephone box, Martin had suggested to a young March “that the elements of architecture might be set out like Lavoisier’s chemical table, and by a further analogy, that with such limited means architectural works of the imaginative power of a Beethoven symphony might be constructed.” While at Cambridge, Bruce Martin intersected not only with March, but also with another mathematically inclined architect in his class: Christopher Alexander. Alexander wrote his first published paper on perception and modular coordination in discussion with Bruce Martin, a collaboration that, as March conjectured, was a

834 Bruce Martin designed the last model of the British cast-iron telephone boxes, known as the K8.
formative influence for “the atomistic approach of Christopher Alexander’s early writings and his pattern language concept.”

The argument about the architectural origins of Alexander’s atomistic logic is of particular interest when read in conjunction with March’s analysis of other influences of his mathematical approach. In critically examining the “audacious programme” of the pattern language, March detected philosophical influences from Wittgenstein’s logical atomism: the belief that there are indivisible objects that combine to form complexes, that these objects are “unalterable and subsistent” “independent” of their configuration, and that each configuration has a “structure” that is “the determinate way in which objects are connected” in it. March’s placement of Wittgenstein alongside Bruce Martin speaks to a key analytical argument that I make in the current chapter: that conceptual, intellectual forces colluded, in a sense, with the pragmatics of architectural practice in support of structural realism. This “realism,” in turn, was formative both for selecting the mathematical techniques that supported new architectural theories and for claims made in the theories themselves. This argument also applies to March’s theoretical activity. Bruce Martin’s reference to “elements of architecture” and their combinations, was reinforced by a serendipitous corridor conversation at Cambridge with Sandy Wilson, London British Library designer and Martin’s collaborator. Wilson’s haphazard remark on “the future

836 Ibid.
838 March’s description of logical atomism, as posited by Wittgenstein and, according to March, adopted by Christopher Alexander is worthwhile quoting at length. March wrote: “For Wittgenstein there are simple indivisible objects in the world which combine together to form complexes. These atomic objects hold the possibilities of all their potential configurations and they are independent in so far as they can occur in all possible complexes. Such atoms are what is unalterable and subsistent, their configurations are what is changing and unstable. The determinate way in which objects are connected in a configuration is its structure. Every statement about complexes can be resolved into a statement about their atomic constituents and into propositions, or pictures or models of reality, that describe the complexes completely. The sum total of depictions is a language. Such pictures are facts, and a picture agrees with reality or fails to agree, it is correct or incorrect, true or false. However, all propositions (models, pictures) have equal value. ‘In the world everything is as it is, and everything happens as it does happen: in it no value exists — ’” (Ibid).
possibility of architecture being notated as mathematical code” “rang bells” for March, who initially took up Bruce Martin’s idea of combining elements in “related series under various transformations and permutations” in painting and set design.

Because of their common educational background and impactful mathematical approaches to architecture, Alexander and March have stood in comparison in several accounts of 1960s design-theoretical activity. On the one hand lie interpretations of allegiance and continuity, with historian Kenneth Frampton’s remarks on *The Geometry of Environment* being a telling example. Frampton characterized *The Geometry of Environment* as “an elaboration of a design methodology pioneered by the mathematician Christopher Alexander in his *Notes on the Synthesis of Form*.” An inspection of the books’ contents leads one to question Frampton’s claim, which mistook March and Steadman’s positive references to the *Notes* (“the first notable application of the new mathematics in architectural design”) as a pledge of agreement to Alexander’s overall project. In other writings, March vocally distanced the LUBFS Centre’s work from Alexander’s approach through extensive correctives of his theories’ philosophical shortcomings. Such critiques, which March deemed necessary given Alexander’s frequent presence “in bibliographies appended to design-theory literature,” have led several scholars to advance interpretations of dissent.

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840 Ibid.
844 For example, in her 2014 dissertation, Steenson highlights March’s critical distance from Alexander through a close reading of March’s “The Logic of Design and the Question of Value.” Sean Keller performs a similar reading of March’s text in his 2005 doctoral work positing that March took Alexander to “stand as the source of naive scientism in architecture” (Keller, “Systems Aesthetic,” 165). Such narrative of disjunction is also supported by LUBFS Centre members’ remarks. Interviewed by Steenson, Philip Tabor characterized March and Alexander as “intellectual twins and sparring partners” (Steenson, “Architectures of Information,” 66-67). Philip Steadman also spoke of the Centre’s distancing from Alexander’s ideas: “Rather quickly those of us who had been initially intrigued by Alexander’s proposed method came to see the
Despite disagreement on the role and meaning of rationality in design, Alexander and March enlisted the same new mathematical techniques to represent and compute with relations and structures. Acknowledging such technical convergence opens the path for recognizing other non-intentional parallels between their research activities. Impelling their mathematical explorations of architecture was, to use March’s words, a similar “aesthetic motivation.”\(^{845}\) Both Alexander and March were harbingers of intellectual vision in architecture. They both used “the mathematics of abstract structures,” graph theory, to depict and thus reveal structures that underlay the sense perceptible appearances of conventional architectural representations. Implicit in their claims was that underneath such appearances existed a layer of causal necessity, of truth, about the workings of an architectural proposition.

The use of graphs in the LUBFS Centre is tied to such ideas of realism and veracity. Oscillating between the perceptual and the mathematical, the empirical and the abstract, the graph fostered reconciliatory aspirations between architecture’s visual and intuitive traditions and the rationalizing and systematizing mandates of the 1960s. However, within the LUBFS Centre, the “intellectual vision” was not a complete abolishment of architecture’s visual traditions. It remained a kind of vision. The visual attributes of the graph rendered it an intuitive and accessible device, and gave it a comparative advantage in relation to other mathematical varieties also available to architects during the period. Echoing Harary and Tabor, because of their visual nature (i.e. the fact that they could be drawn like images of structures) graphs were especially handy for visually trained architects.

However, the graph’s visual allure was deceptive. Despite its visual appearance, the graph abolished the operational possibilities available when working with visual material. Visual phenomena are continuous and foster ambiguity and perceptual reformulation. Yet the graph discretized the working scene and structured the visual material in a way that only allowed for a finite number of combinations and recombinations of these discrete entities (be it rooms, activities, or other). Instead of enabling visual restructuring, the graph claimed to reveal some hidden and unchangeable essence in a design proposition. As Steadman acknowledged in his “Graph-theoretic Representation of Architectural Arrangement”:

It is usual to represent a graph with a diagram, showing the points joined by the appropriate lines; and to refer to this diagram itself as the graph. It is quite immaterial from the point of view of theory how the diagram is drawn — whether the lines are straight or curved, whether they cross or not, or where the points are placed in the plane. What is important is the number of points; and which lines join which pairs of points.

Despite deliberately polemical arguments against the “drug” of “draughtsmanship,” graphs, like other mathematical models advanced by the LUBFS Centre, were not meant as replacements of the creative, intuitive, or “artistic” aspects of designing. The intention behind mathematical modeling was to foster “understanding,” to assist designers with “describing, exploring or plan[ning] a possible reality.” LUBFS Centre researchers were aware of the grave difficulties that mathematical models encountered when it came to the “creation of new forms.” There, “even models of the […] planning type offer no direct help — for their role is in evaluation only.

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846 In the context of the computational theory of shape grammars, on which I dwell in the dissertation’s concluding chapter, George Stiny has highlighted the structureless nature of shapes as necessary for creativity and emergence in design. In Stiny’s theory, structure only emerges after the designer transformatively acts on a shape. Design, in this sense, is a process of perpetual perceptual restructuring. Nothing is assumed to be hidden under the appearance of the shape. Instead, the appearance is the driver of the designer’s actions (see: Stiny, “Introduction to Shape and Shape Grammars”; “What Is a Design?”; Shape: Talking about Seeing and Doing).

847 Steadman, “Graph-theoretic Representation of Architectural Arrangement,” 94.

848 Echenique, Interview by Vardouli.

and alternative plans must be specified in advance of the model’s operation."\textsuperscript{850} Cognizant of the impossibility “to generate a range of feasible, complete and finished solutions by a […] single, uninterrupted, predetermined procedure of a mechanical nature,"\textsuperscript{851} they nonetheless supported the possibility of a “strategy involving many successive alternate steps of design formulation and evaluation […] conducted in a perfectly logical, if not initially predetermined manner."\textsuperscript{852}

Implicit in the separation between the “creative” and “logical” aspects of design\textsuperscript{853} was a crucial claim about the relationship of architecture and mathematics: mathematics resided in the layer of reason and served as an objective infrastructure for the subjective, intuitive aspects of architecture. This separation was latent in the way that LUBFS Centre members construed the relationship of graphs with architectural drawing: a mathematical structure underlying many possible visual appearances. The graph captured an ethos of objective neutrality in support of subjective choice, characteristic of the LUBFS Centre attitude toward planning in all environmental scales. As Martin prefaced the Centre’s work: “[…] the attitude implicit […] is that planning is not concerned with visual images. Neither should it be an attempt to predict future uses or to outline desirable goals. The objective is the discovery of those \textit{neutral guidelines that set out the least restrictive framework} [emphasis mine] and allow the maximum elaboration by use.”\textsuperscript{854} The formulation of a reasoned neutral infrastructure –reminiscent of Alexander and Doshi’s “main structure concept”— will recur center stage in the following chapter, where I delve into the technical underpinnings of methods and programs developed

\begin{footnotesize}
\begin{itemize}
\item \textsuperscript{850} Ibid., 98-99.
\item \textsuperscript{851} Martin and March, “Introduction to Part 2: Activities, Space and Location,” 107.
\item \textsuperscript{852} Ibid., 108.
\item \textsuperscript{853} The separation between the creative and the rational is discernible in Philip Steadman’s riposte to historians’ pigeonholing of LUBFS Centre members as uninterested in visual expression; a misconception due to the fact that “they cannot understand that someone could be an architectural scientist on a Monday and a painter on a Tuesday.” The creative, artistic, intuitive and the rational, scientific, systematic coexisted but in separate realms. They were “two hats” worn in different days of the week. (Steadman, “What Really Happened,” 279).
\item \textsuperscript{854} Martin and March, “Introduction to Part 1: Explorations,” 3.
\end{itemize}
\end{footnotesize}
explicitly for participatory design. I will show how ideas of visuality, choice, and prediction associated with architectural science were repurposed in a rhetoric of user participation and design democratization, and how the graph participated in this transition.
Figures for Chapter 4

Figure 30: Land Use and Built Form Studies Centre logo. The black region in both squares has a different shape but the same surface area.

Figure 32: Leslie Martin’s Whitehall study. Schematic drawing of Whitehall’s existing building (left) and the same floor space allocated respectively (from left to right) to a 18, 12, and 8 storey building. (Source: Martin, “Architect’s Approach to Architecture,” 195).

Figure 33: Comparison of “court” and “slab-tower” built forms of the same built volume. Mathematical analysis shows that the slab-tower “antiform” (the court) places the same built space in the same area of land in one third of the height. (Source: Source: Martin, “Architect’s Approach to Architecture,” 198).
Figure 35: Covers of textbooks and teacher guides of the School Mathematics Project (SMP). The computer paper tape motif featured on the covers reads "THE SCHOOL MATHEMATICS PROJECT DIRECTED BY BRYAN THWAITES."


Figure 38: Photographs of children’s constructions with poles and ropes, including a pyramid and a house plan. (Source: Nuffield Mathematics Project. *Environmental Geometry*, 41).
Figure 39: Use of everyday objects to explain the mathematical concept of mappings. (Source: Nuffield Mathematics Project. *Environmental Geometry*, 57-61).

Figure 40: Use of a famous architectural example (Le Corbusier’s Maison Minimum) to explain the mathematical concept of mappings. (Source: Match and Steadman, *The Geometry of Environment*, 20).
Figure 41: Exercise aiming to advance understanding of the concepts of connectivity, orientation, and position, featured in the Nuffield Mathematics Project *Environmental Geometry*. The students made a model of their school out of paper clip-joined cardboard walls. The model showed rooms and doorways. Students were asked to use the black thread to answer questions such as: “Which is the shortest route? Which is the longest route you could take without walking along the same part twice? Could you walk along all these paths without covering any part twice?” Design researchers, methodologists, and CAD developers, used the mathematical analog of the black thread (the graph) to answer similar questions. The student exercise was meant as an intuitive introduction to the new mathematical ideas of graph theory and topology, which the authors viewed as naturally expressed in the layout of a building. (Source: Nuffield Mathematics Project. *Environmental Geometry*, 38).
Figure 42: Floor plans and axonometric section cut of Frank Lloyd Wright’s Devin House in Chicago (1896) revealing the building’s intricate circulation system. (Source: March and Steadman, The Geometry of Environment, 258-259).

Figure 43: Left: The Devin House represented by its corresponding adjacency graph (rooms and connections between them). The graph is not planar and therefore cannot be resolved in a single floor layer. (Source: March and Steadman, The Geometry of Environment, 261). Right: New adjacency graph merging the alleyways a1 and a2. The graph then becomes planar and a second floor is not necessary (Ibid., 262).
Figure 44: Floor plans of three Frank Lloyd Wright buildings. (a. House for a family of $5000-$6000 income, 1938; b. Ralph Jester House, Palos Verdes, California, 1938; Vigo Sundt House, near Madison Wisconsin, 1941). The houses “appear to be very dissimilar” (Source: March and Steadman, The Geometry of Environment, 27).

Figure 45: Graph of space and room linkages of the three Lloyd houses. The graph mapping reveals them to be “topologically equivalent” despite their dissimilar appearance; with a minor deviation of the Sundt House having an additional room. (Source: March and Steadman, The Geometry of Environment, 28).

Figure 47: Diagram of how models made for different purposes relate to each other and to “reality’s” temporal scales. (Source: Echenique, Marcial. 1968. “Models: A Discussion.” LUBFS Working Paper 6 (March), n.p.).
Figure 49: Representation of one student’s activities and journeys for one day. The graph shows the different facilities visited (points) and their order (number labels for each point). The grid is a schematic abstraction of the university campus plan. (Source: Bullock, Nicholas, Peter Dickens, and Philip Steadman. 1972. “The Modelling of Day to Day Activities.” In Urban Space and Structures, edited by Leslie Martin and Lionel March, 129–58. Cambridge Urban and Architectural Studies. New York: Cambridge University Press, 139).

Figure 51: Graph showing a nurse’s daily movements around a hospital operating theater suite, reproduced by Philip Tabor from Whitehead and Eldars’ 1964 article “An Approach to the Optimum Layout of Single-Storey Buildings.” (Source: Tabor, Philip. 1969. “Pedestrian Circulation in Offices.” LUBFS Working Paper 17, 11).

Figure 52: “Organizational” (top), “classificatory” (bottom left) and “operational” (bottom right) graphs representing different aspects of office structure. (Source: Tabor, “Pedestrian Circulation in Offices,” 11, 16, 17 respectively).
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Figure 54: Distance network between activities, linking only directly accessible points. Distances are calculated by inserting intermediate points t,u,v,w (left) or calculating a proxy distance directly between activities. (Source: Tabor, Philip. 1976. “Analysing Route Patterns.” In *The Architecture of Form*, edited by Lionel March, 352–379. Cambridge Urban and Architectural Studies. London: Cambridge University Press, 354).

Figure 55: Explanation of additive and permutational methods of activity allocation featured in the *Geometry of Environment*. Left: Additive method of activity allocation adapted from Whitehead and Eldars (1964). Each row corresponds the placement of one new activity in an “infrastructure” of empty rectangular cells. The heavy outline shows possible positions at each step, alongside the cost of the activity addition and the overall cost of the layout (Source: March and Steadman, *The Geometry of Environment*, 312). Right: Permutational method of activity allocation adapted from Armour and Buffa (1963), starting with an initial layout and swapping activities. “Branches” show possible activity swaps at each step alongside a calculation of the overall layout cost. The heavy outline shows rooms actually swapped at each step and the resulting overall cost. (Source: Ibid., 307)
Figure 56: Examples of association and structure of command graphs in the case of a research institute. Left: Total number of daily journeys from a coworker to another (Source: March and Steadman, *The Geometry of Environment*, 291); Middle: “association graph” showing total number of daily journeys between each coworker pair (Ibid., 293); Right: Structure of command graph (bold line) as a partial graph of the association graph (Ibid., 298).

Figure 57: Buffa’s (1963) activity allocation method, reproduced by Tabor. Left: Spatial layout derived “intuitively,” by overlaying rectangles corresponding to floor space required by activity on an “association graph,” also made “by hand.” (Tabor, Philip. 1970. “Traffic in Buildings 2: Systematic Activity Location.” *LUBFS Working Paper* 18 (September), 7); Right: Manual rearrangement of initial spatial layout with “slight variation of the shapes of the work areas” to fit the rooms in a given plan outline. (Ibid., 7-8).
Figure 59: Right: A house plan represented as a graph. Left: The house plan’s graph is the “dual” of the “adjacency graph” which represents the architectural programme (types of rooms and connections between rooms). The adjacency graph also includes site orientation by linking rooms to North, West, South, and East (N,W,S,E in the drawing). (Source: Steadman, “Automatic Generation of Minimum-Standard House Plans,” 12).

Figure 60: Investigation of possible room connections. Connections between rooms that cause lines in the graph to cross are rejected because they are impossible to accommodate in a single storey building. (Source: Steadman, “Automatic Generation of Minimum-Standard House Plans,” n.p.).
Figure 61: Demonstration that Kirchhoff’s second law “holds true” for the dimensions of architectural floor plans. The dimension “entering” a node in the graph, equals the one “leaving” the vertex. The direction of the graph’s lines follow the conventional direction of the flow of electricity. The “current” (dimension) of 20 entering the network at the top (overall plan width) must equal the current leaving at the bottom. (Source: Steadman, “Automatic Generation of Minimum-Standard House Plans,” 31).

Figure 62: Left: Implementation of “Trinity Four’s” (Brooks, Smith, Stone, and Tutte) 1937 graph-theoretic method for dissecting rectangles in order to fit rooms of specific sizes in a shell of given dimensions (Source: Steadman, “Automatic Generation of Minimum-Standard House Plans,” 50). Right: Reaching an acceptable solution (Ibid., 52).
Chapter 5: Yona Friedman, the Architecture Machine, and Design Participation

5.1. Your Own Native Architect

In July 1976, the audience of the 3rd Annual Conference on Computer Graphics, Interactive Techniques, and Image Processing\textsuperscript{855} (SIGGRAPH 1976) was presented with screenshots\textsuperscript{856} of a CAD system named YONA.\textsuperscript{857} Reversed polaroids, the screenshots showed graphs generated during the system’s operation and visualized on a touch-sensitized IMLAC PDS-1D. One screenshot depicted five crosshair-drawn points, labeled with names of domestic architectural spaces, and connected with lines, to shape a graph that whimsically looked like a child’s drawing of a house [Figure 64]. Another screenshot showed osculating b-spline curves fitted between the graph’s dual and its offset boundary to form a “bubble diagram” — the architectural parlance for a rough sketch of a plan indicating the locations and interconnections of spatial enclosures [Figure 65]. A third screenshot pictured a schematic drawing of an architectural plan, where the “bubbles” were rationalized into straight lines denoting wall boundaries [Figure 66]. The screenshots visually summarized a graph-theory based computer aided design process for ‘do-it-yourself’ designers.”\textsuperscript{858} Sitting in front of the computer screen, the “unpracticed designer”\textsuperscript{859}

\textsuperscript{855} The 3rd Annual Conference on Computer Graphics, Interactive Techniques, and Image Processing was sponsored by the Association of Computing Machinery’s Special Interest Group on Computer Graphics (SIGGRAPH), a professional organization formed in 1969 by Sam Matsa and Andries van Dam to “promote the generation and dissemination of information on computer graphics and interactive techniques.” SIGGRAPH was the outgrowth of a Special Interest Committee on graphics (SICGRAPH) formed in 1963. Until its first general conference in 1974, the group had organized several computer graphics specific sections in national computing conferences and was publishing the quarterly Computer Graphics journal and the SIGRAFFITI newsletter. Before 1977, when strict paper refereeing began, a selection of conference papers was published in the Communications of the ACM. For a more extensive presentation of SIGGRAPH’s early years and subsequent development see: Brown, Judy, and Steve Cunningham. 2007. “A History of ACM SIGGRAPH.” Communications of the ACM 50 (5): 54–61.

\textsuperscript{856} YONA systems’s screenshots shown at SIGGRAPH ’76 were photostat reductions and reversals of 4X5 polaroids of the computer display while the system was performing each of its six functions. For a compelling discussion of the screenshot as representation of computer aided design systems see Allen, Matthew. 2016. “Representing Computer-Aided Design: Screenshots and the Interactive Computer Circa 1960.” Perspectives on Science 24 (6): 637–668.


\textsuperscript{858} Weinzapfel and Negroponte, “Architecture-By-Yourself,” 74.
started by selecting a desired architectural programme (types and sizes of spaces) and their connections. The user-specified spaces became the points (“nodes”) of a graph, and the links became the lines. A planarity test checked if a one-level arrangement of the architectural programme was possible or if the user needed to make modifications, and outputted an image of a labeled planar graph with a preliminary site positioning that the user could rearrange by moving nodes on the screen. Once a desirable arrangement was reached, the system presented the corresponding “bubble diagram” to the user, who could then shape it into rooms by snapping straight lines and curves on a grid projected on top of the “bubbles” [Figure 67].

Written in PL/1,860 YONA was running on the Interdata861 mini-computers that equipped a computing facility in the MIT Department of Architecture that went by the name “The Architecture Machine” (ArcMac). Founded in 1968 by MIT Architecture graduate and freshly appointed assistant professor862 Nicholas Negroponte, MIT’s ArcMac functioned as “a teaching operation, a computer-aided design facility, and a computer science research unit.”863 The unit

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859 Ibid.
860 PL/1 standing for Programming Language 1 (initially known as NPL, New Programming Language) was developed in 1964 by George Radin and Paul Rogoway of the IBM Corporation, after the Advance Language Development Committee of the SHARE FORTRAN project and IBM requested a language “to serve the needs of an unusually large group of programmers … scientific, commercial, real-time, and systems programmers” and “to allow both the novice and the expert to find facilities at his own level” (Radin, George, H. Paul Rogoway, and Jr. Cheatham T. E. 1965. “NPL: Highlights of A New Programming Language.” Communications of the ACM 8 (1): 9). The language was designed to be relatively machine independent (especially in relation to input output channels), cater to the novice by not overburdening the user with learning irrelevant notations, and be error resistant.
861 MIT’s Architecture Machine, the satellite computing facility where YONA was developed, was equipped with several mini-computer donated from the Interdata Corporation, Oceanport, New Jersey. The first donation of an 8K Interdata Model 3 was received in July 1969. Another Interdata Model 3 and a Model 5 were received by 1971.
862 Nicholas Negroponte, digital media frontiersman and founder of MIT’s Media Lab, became assistant professor in the MIT Department of Architecture in 1968, after one year of teaching in the MIT Department of Mechanical Engineering. Negroponte earned his Bachelors from MIT Architecture with a thesis on “Systems of Urban Growth” and his Masters in 1966 with a thesis on “The Computer Simulation of Perception During Motion in the Urban Environment,” under the advisoship of his future collaborator Leon Groisser and the consultation of seminal computer graphics figure Steven Coons.
benefited from MIT’s MULTICS and IBM S/360-67 timesharing systems, of minicomputers and peripherals from industrial donors, and custom sensors and actuators made in-house by student researchers of varying academic levels and disciplines who passed through ArcMac [Figure 68, Figure 69]. With these facilities, and ample funding from sources such as the National Science Foundation (NSF) Division of Computer Research, the U.S. Department of Defense’s Advanced Research Projects Agency (ARPA), and the Office for Naval Research (ONR), ArcMac performed experiments to “assist, augment, and replicate design activities” by propelling “both the state-of-the-art of understanding design and by developing better hardware and software.” YONA was the computer implementation component of a larger “experiment in computer aided design” titled Architecture-By-Yourself (shortened as ABY), performed with the support of a research grant from the NSF Division of Computer Research. An homage to eminent Hungarian-born French architect Yona Friedman who furnished the system with its “underlying philosophy” and an acronym encapsulating the system’s aspirations, “Your Own

864 Project MAC (Project in Mathematics and Computation) was initiated in 1963 at MIT under sponsorship from the U.S. Department of Defense Advanced Research Projects Agency (ARPA) and the National Science Foundation (NSF). The project was aimed at developing the first working timesharing system — a configuration where users from different locations working on separate terminals could access and use the same computer. The project initially used IBM System/360 Model 67 (S/360-67) mainframe computer. In 1965 MULTICS (Multiplexed Information and Computing Service) time-sharing system was jointly developed by MIT, General Electric, and Bell Labs.

865 By 1971, Architecture Machine industrial donors included Interdata Corporation, Badger Company, Bolt, Beranek and Newman (BBN), Bell Telephone Laboratories, Sylvania, Bright Industries, Foxboro Company, General Instrument Corporation and Tektronix. (Grosset, Leon Bennett, and Nicholas Peter Negroponte. 1971. Computer Aids to Participatory Architecture. Cambridge, Mass.: Massachusetts Institute of Technology, 65.) These donation equipped ArcMac with state-of-the-art mini-computers and peripherals that enabled students to experiment with input/output devices, general purpose interfaces, and graphical devices without needing to have extensive programming experience. Negroponte found such hands on experimentation to be crucial for introducing architecture students to computing because of them being “tactile people” (Negroponte, “Research in Progress: The Architecture Machine,” 191).


867 Ibid.


869 The Architecture-By-Yourself Project was performed under the NSF grant Grant Number DCR74-20974-A01, Machine Recognition and Inference Making in Computer Aids to Design.

Native Architect” or “YONA” strove “to allow people to design their own homes without either a middleman or a middle machine creating whole solutions for them.”\(^871\)

Developed in consultation with Friedman, who visited ArcMac and monitored ABY’s experiments, the YONA system built on a speculative machine that Friedman conceived but did not execute\(^872\) for the 1970 Osaka Expo – a Word Fair remembered for its emphasis on multimedia and information technologies. Responding to the Expo theme “Progress and Harmony for Mankind,” Friedman proposed a machine that he called the “FLATWRITER” as the implementation of a graph-theory based mathematical method of do-it-yourself design that he delivered as seminar at the University of Michigan, Harvard University and the Université de Montreal in the fall of 1967.\(^873\) In 1971, Friedman published what he promoted as a theory of “democratization” in architecture and urbanism under the title *Pour Une Architecture Scientifique*. In his theory, graphs were tasked with “remodeling”\(^874\) the process of architectural design, reforming the designers’ professional identity on the basis of scientific ideals, and granting the “future users” of domestic and urban spaces the power to choose and change their living environments. The theory was the result of a mathematical asksesis that Friedman initiated in 1964\(^875\), striving to imbue with scientific credibility his architectural visions of “spatial urbanism” — levitated infrastructural mega-grids filled with ephemeral dwellings designed and perpetually rearranged by their inhabitants [Figure 70]. Friedman’s appeal to scientific sobriety responded to the proliferation of rational design methods in British and U.S. universities and the foundation of international organizations such as the DRS in the U.K. and the DMG in the U.S.

\(^{871}\) Ibid.
\(^{873}\) Ibid., 27.
\(^{874}\) Friedman, *Toward a Scientific Architecture*, 9.
Diligent observer of upcoming tendencies in design and frequent visitor of North American academic institutions, Friedman decided to refrain from architectural drawing and use mathematics to clarify the theoretical value of his ideas. In *Pour Une Architecture Scientifique*, he invited the readers to leave aside the skeletal infrastructure imagery that had earned him his international reputation and indulge in depictions of an also skeletal, but this time abstract, mathematical entity that revealed the reasoning behind them: the graph.

Besides sharing lines of kinship with Friedman’s theory, the YONA system was also in conversation with, and capitalized on, developments in CAD and computer graphics. Inside MIT and internationally, graph theory-based methods for architectural layout generation were burgeoning. Much of this work circulated through the Anglo-American network of rational design methods, where growing focus on CAD forged the disciplinary anxieties, scientific values, and ethical quivering of the movement’s first years into a new kind of technological pragmatism. At the same time, the rapid rise of timesharing technologies in the mid-1960s boosted research in human-machine interaction and interfaces, while promising accessibility of computer resources to wider social groups. The prospect of making computer aids to architectural design accessible to the general population grew from extrapolations of such technological possibilities while also absorbing social, political, and intellectual demands for the “democratization” of architectural design. One of the intellectual agendas that instigated research and methodological activity in design in the early 1960s was the articulation of explicit, communicable, and ultimately teachable design processes through adherence to scientific

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876 Nigel Cross, J.C. Jones’s student and a leading figure of design methods and research after the 1970s, asserted the computer aided design emphasis as characteristic of American design methods. He wrote “Perhaps the most significant feature of the conferences from 1968 onwards was of papers on computer-aided design. This work had its roots in the earlier approaches to systematic design but seemed to develop on a pragmatic basis despite the doubts and waverings of the erstwhile protagonists of “design method”” (Cross, Nigel. 1977. *The Automated Architect*. London: Pion Ltd., 17).
principles and rationality. The end of the decade found the democratizing ethos associated with design rationality molding with the politically charged genre of participatory design. The participation of wider social groups in making decisions about the domestic and urban environments in which they dwelled was a demand that rose rapidly amidst international sociopolitical fermentations that questioned the designers’ professional expertise and decision-making legitimacy.

The YONA system was a product of this intricate manifold. Although it did not amount to much more than an academic experiment, it exemplifies how graphs facilitated and impacted the merging of a design method with the pragmatics of computer research under a participatory and democratizing rhetoric. It also helps tell a story of transactions among the distinct contexts that vested graph theory with the symbolic meanings and operational conveniences that it presents in YONA. These contexts range from the realities of French town planning to the visions of French “radical” architecture. They extend from the deliberations of a frontrunner U.S. computer research group seeking to repurpose existing computer aided techniques under new interactional possibilities to the changing status of Anglo-American design methods and architectural research at the start of the 1970s. This chapter delves into these contexts, tracing the forces and events that led to the “happy correspondence,”877 to use YONA’s authors words, between the “capabilities”878 of graph theory, ideas about what a design aid for “unpracticed designers” should entail, and ArcMac’s “interests in graphic communication and nonpaternalistic guidance.”879 By looking into the making of Friedman’s graph-based theory of “scientific” architecture and ArcMac’s graph-based work on computer aided participatory architecture, the chapter highlights translations and transformations of the graph’s symbolic and operational

878 Ibid.
879 Ibid.
properties as it crossed geographic lines and research contexts. It also elucidates how these properties facilitated such crossings, ultimately shifting the cultural sign of computation and computers from technocracy to democracy.

In the first section, I discuss how Friedman used graph theory to recast his theories of spatial urbanism, which he initially expressed through drawing, in mathematical terms. I begin by following the graph’s appearances within Friedman’s writings from 1964 until the publication of *Pour Une Architecture Scientifique*. Then, I delve into the contexts in relation to which Friedman undertook his mathematizing activity. I use combinatorics, isomorphism, and predictivity to frame debates about what architectural design is and who should be performing it, as they unfolded in these contexts. I also examine how the specific meanings, both cultural and practical, that these properties took on within these debates, validated graph theory as a harbinger of “science” and “democratization” in architectural design. First, I discuss how French postwar planning cast combinations of standardized elements as a natural model for delivering emerging demands of user choice and democratization of architecture. I also address French State decisions that boosted a culture of “research” in French planning and architecture, and interrogate how the graph enabled translations and isomorphisms with the human sciences — themselves under the influences of structuralism and information theory. Second, I zoom into two futurological groups in which Friedman was an active participant in the 1960s: the Groupe d'Études d'Architecture Mobile (GEAM) (Group for Studies of Mobile Architecture) and the Groupe International d’Architecture Prospective (GIAP) (International Group of Prospective Architecture). I investigate how visual and conceptual engagements with structure through the groups’ space frame explorations, cultivated a version of what I have previously referred to as structural realism — in this case expressed as the idea that space consists of atomic, discrete
units that can be composed to match social or behavioral structures. I also discuss how ideas of “prospectiveness” in architecture advanced in these groups, related to the values of certainty and prediction that initially motivated design research and methods in the U.K. and the U.S. This leads to the third context: Friedman’s teaching and research appointments in North American Institutions, where he developed his design method in collaboration with Frank Harary. I examine how ideas about the graph’s intuitiveness and accessibility relayed by Harary, as well claims about its intellectual value as instrument of disciplinary unification, met its more pragmatic uses within the rising tide of design methods and computer research.

In the second section, I use YONA as entry point to ArcMac’s research activity. I begin by delaminating the different roles, symbolic and operational, that the graph played in the system. This unveils changes and constants as the graph shifted from being a device for mathematization in Friedman’s theory, to an instrument for computerization of architectural design. YONA also initiates a discussion of shifts and continuities with ArcMac’s early trajectory. Inquiry into three research projects, URBAN5, Intelligent Environments, and Computer Aids to Participatory Architecture (the first preceding and the two other inaugurating ArcMac’s operation) shed light on the transition from developing computer aids for professional architects to embracing an anti-professional, participatory agenda. I discuss these projects alongside a parallel history of the Design Methods Group and the Design Research Society, in whose conferences ArcMac’s projects were presented. In doing so, I show how ArcMac absorbed existing techniques from CAD and responsive environments and repurposed them through new human-machine interfaces and interaction possibilities pioneered by ArcMac. Such repurposing resulted into what Negroponte marketed as a “soft […] computational paradigm”\textsuperscript{880} — computation as enabling, and not anymore inhibiting, personal meanings and idiosyncrasies. In the case of computer aided

participatory architecture, one of the directions in which ArcMac pursued such “soft” paradigm, ArcMac fused graph-based methods for automatic floor planning with research on sketch recognition: translating sketches of floor plans in graph theoretic form and using these as the specification of an architectural brief. The graph there served as a translator between the visual language of the sketch and the mathematical language of the computer, enabling what Negroponte variously described as a congenial human-machine conversation.

I conclude by circling back to the YONA system. A cross-pollination of pervasive CAD techniques, ArcMac’s endeavors in graphical interfaces, and Friedman’s approach to do-it-yourself design, YONA lacked the sketch recognition component. Instead, it visualized the graph on screen, as a directly manipulable entity. In keeping with Friedman’s claims about the intuitiveness of the graph, claims also echoing more widely in the cycles of design research and methods, YONA declared the graph an object apprehensible both by humans and by machines. The revealing of the graph also bears ethical implications pertaining to demands of “nonpaternalism” and transparency. The relevance of such issues expands beyond participatory design, to the transition from “glass box” design methods to “black box” computer implementations — to use the words of design methods frontiersman J. C. Jones. 881 It relates to much work on computer aided design post-1975 that proceeded through concealment of techniques behind interfaces. Ultimately, the mathematizing endeavors of Friedman and the computerizing efforts of ArcMac illuminate the workings of the graph-theory instigated intellectual vision —the visualization of abstract structures underpinning empirical phenomena— in establishing continuities among design methods, computer-related research, and participatory design.

5.2. A Scientific Architecture and the Building of Structural Realism

Yona Friedman entered the international architectural scene with a boisterous critique of architects for using “pseudo-theories” to enforce their own values and ideas about architecture, thus defining and oppressing the lives of its future users. In 1956 Friedman traveled from Haifa, where he was working as an architect, to Dubrovnik in order to participate in the 10th CIAM. In the event that came to mark CIAM’s demise, Friedman first voiced his critique against the functional determinism advanced in the Congresses and presented ideas of modular temporary dwelling, self-planning, and social mobility. Soon after, Friedman published his manifesto for a “mobile architecture,” a theory “stemming from the public domain” and accommodating “all personal hypotheses.” From 1958-1963 Friedman developed and avidly promoted the architectural expression of his theories, the so-called “Ville Spatiale” (“Spatial City”), through publications, exhibitions, teaching, and lectures internationally. Drawings and collages of a

882 Yona Friedman’s “Manifesto Architecture Mobile” first circulated in 1958 as a cyclostyled edition consisting of 300 copies. The manifesto was based on a pamphlet that Friedman used in CIAM 10, outlining the principles of mobile dwelling and self-construction. A second cyclostyled edition of also 300 copies circulated in 1960, with the added subtitle “Ten Principles of L’Architecture Mobile.” In 1970 the Mobile Architecture manifesto was published by Casterman, also in pocket form (Friedman, Yona. 1970. L’Architecture Mobile: Vers Une Cite Concue Par Ses Habitants. Paris: Casterman). Friedman had circulated his Mobile Architecture principles through articles published in European journals. One of the earliest ones was the 1957 presentation of the Mobile Architecture idea in the German journal Bauwelt (Friedman Yona and Günther Kühne. 1957. Ein Architektur Versuch. Bauwelt, 48 (16)).


884 Ibid.

885 The “Ville Spatiale,” as Yona Friedman called it, was a systematic mega-grid supported by pillars 200-250ft apart. The pillars contained circulation, electric, and water installations. The space-frame, in turn, contained voids of 300–400 sq. ft. that the inhabitants could fill up to 50% with ephemeral “dwellings or offices,” as Friedman specified, positioned in an unconstrained manner. Friedman produced drawings, models and collages of the Ville Spatiale from 1958-1963, which he published internationally. An indicative example was the proposal for a “Spatial Paris,” illustrated with numerous photo-collages of a three-dimensional space-frame suspended over various Parisian landmarks, such as the Arc de Triomphe, the Champs-Élysées, the Place de la Concorde and others, leaving the existing city fabric untouched. The proposal for a Spatial Paris was published in Architecture d’Aujourd’hui in 1962 (Source: Friedman, Yona. 1962. “Paris Spatial.” Architecture d’Aujourd’hui 33: xxxvii).

systematic mega-grid filled with ephemeral dwellings and suspended over the English Channel, the Place de la Concorde, or African hinterland had a significant impact in international architecture, and their formal and structural ideas influenced many actors in what is often referred to as the “radical” architecture of the 1960s [Figure 71]. In the turn of the 1970s, however, Friedman found the visual aspects of his work widely imitated, but the underlying theoretical ideas poorly understood. Pour Une Architecture Scientifique intended to remedy this situation by presenting, as Friedman wrote in the book’s “Perspective,” “nothing but reasoning, without wandering off into visualizations of the results which this reasoning has produced.”

Despite Friedman’s introductory iconoclastic declarations the book is heavily illustrated. The 63 hand-drawn figures that appear in the main body of the book are, however, of a different kind. The reader does not encounter the familiar collages or perspectival drawings of crystalline inhabitable megastructures hovering over the built environment, but drawings of another, mathematical, structure: a planar labeled graph. Friedman placed the graph alongside sketches of plan layouts [Figure 72], schematic drawings of space frames [Figure 73], theoretical articulations [Figure 74], charts of an individual’s living habits [Figure 75], and interface sketches of the FLATWRITER machine [Figure 76]. Friedman declared the graph essential for a “democratized” “method” of architectural and urban design. This “remodeled architectural process,” as Friedman termed it, suggested a refashioned designer, who did not make decisions on behalf of the user in a “paternalistic” manner, but presented the future user with “all physically possible” configurational choices of floor plans for a given architectural

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887 Friedman, Toward a Scientific Architecture, xi.
888 Ibid., 15.
889 Ibid., 9.
891 Ibid., 10.
programme (number and sizes of rooms) along with an “objective” personalized evaluation of these choices, and the risks that they entail. This method, he argued, necessitated the separation of the so-called “objective” aspects of architecture and planning from the “intuitive” ones. Friedman assigned the “objective” realm to architects and planners and the “intuitive” aspects to the future users of architecture [Figure 77].

Throughout the book, Friedman described in full length the advantages of the graph for enabling an “objective,” “axiomatic” theory of architecture that could form the substructure of all idioms and personal meanings. Pour Une Architecture Scientifique did not include references. However, Friedman’s insistence on the necessity of an “axiomatic” theory of architecture was a Bourbakist move. Friedman used the concept of a planar directed graph to identify the maximum number of axioms (statements) that a theory of architecture and planning should have in order to be consistent (no contradictions among postulates), non-redundant (each statement used only once) and complete (no “unenunciated” external statements). Friedman took the graph’s points to represent statements and the directions of the lines the order by which these were sequenced. With this representation, Friedman purported to prove that the correct number of axioms was three. Connecting three statements was straightforward; there was only one way to connect them (they made a triangle) and sequencing them was easy. It sufficed to use a rule that wanted one arrow “entering” a point and one “leaving” it. Four statements made finding a correct order more difficult, and five (or above) resulted in crossings between the graph’s lines (the graph was no longer planar) [See again Figure 11]. Crossings created a new relationship between statements that needed another implicit statement to be resolved.

892 Ibid., 15.
893 Ibid., 13.
894 Ibid., 10-11.
895 Ibid., 23.
The Bourbakist air that Friedman’s exposition about the “axiomatic” theory of architecture dissolved when it came to specifying the particular axioms. Friedman defied the claim that axioms should have no empirical significance and, instead, chose axioms that had an obvious correspondence to real architectural objects. In fact, his axiomatic theory could be visually summarized as a sketch of a room enclosed by four walls and one door in one of them.896 Architecture and planning, Friedman explained next to the sketch, is about three things: “1. [architects and planners] make enclosures in pre-existing space; 2. For each enclosure there is at least one path leading to every other enclosure; 3. There are at least two different kinds of enclosures.”897 Friedman’s axiomatic theory enabled the production of mathematical structures for architecture. Yet these were not abstract constructions without real-world referents; they were mathematical models of spatial structure (enclosures connected with accesses). To represent and reason with these models, Friedman also used graph theory. The graph’s points, Friedman argued, had a “one-to-one” correspondence with the real spatial structure898 of an architectural plan thus enabling immutable translations between reality and its representation, regardless of context and cultural convention. Apart from universal representations, the graph also enabled disinterested evaluations. The “warnings” for the local and global efficiency of each design option, were not a product of the designer’s external values, but were derived from the structure of a design itself, and the habits of the future user. Friedman used the graph to justify the necessity of the Ville Spatiale’s standardized spatial framework for the theory and method that he put forward. He explained that a spatial infrastructure was the equivalent of a “saturated graph,” capable of containing as its subgraph any substructure that the future user chose.

896 Ibid., 28.
897 Ibid.
898 Ibid., 9.
899 Ibid.
It was not the first time that Friedman had argued that the Ville Spatiale was not an artifact of architectural creation driven by formal and aesthetic choices, but a product of reason. The argument for an “ideal infrastructure” that would come to form the basis of Pour Une Architecture Scientifique, first appeared in a transcript of a lecture that Friedman delivered at Harvard University and at the Carnegie Institute of Technology in March 1964. The transcript was featured in Architectural Design the same year under the title “Toward a Coherent System of Planning,” next to an order form for Alexander and Chermayeff’s just published Community and Privacy. In his lecture, Friedman set out to address what he saw as a fundamental contradiction: planning, construed as a projective, anticipatory activity, and the inability to anticipate, to predict a world in constant flux. “The only way that appears to be open for such a prevision,” Friedman claimed, “would be to reduce planning criteria to a few, obviously unchanging facts: In other terms, to fix the axioms of town living.” Friedman construed “axioms” as what remained invariant amidst a fast-changing urban surround. He sought such invariants in the “content of towns”: i.e. urban society. Using the set theoretic idea of a “commutative group” (a term that he defined as a system that is complete, coherent, and non-contradictory) Friedman developed a combinatorial table of “all possibly conceivable patterns” of social organization. The city, he then argued, would need to be planned so as to permit a seamless shift between such different social structures – hence the need of an “ideal infrastructure” able to accommodate them.

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902 Ibid.
903 Ibid.
904 In his 1964 article “Towards a Coherent System of Planning” Yona Friedman argued for the necessity of an “ideal infrastructure” as a means to support a seamless transition between different spatial configurations, which would in turn correspond to different social organizations. Mid-way in his article Friedman explicitly made mention of the Ville Spatiale (or the “space town” to use the English term employed in the paper) as fulfilling the requirements for a “technical basis of the town,” requirements that he claimed to have identified through logical
Three years later *Architectural Design* featured an article titled “A Research Program for a Scientific Method of Planning.”905 There, Friedman presented a fully worked method of “urban mechanisms,”906 which he later also included in *Pour Une Architecture Scientifique*. The method was a way for describing and intervening in the urban settlements potentially developed within his proposed “ideal infrastructure.” In this article, Friedman likened the city to a machine907 whose input was the inhabitants’ activities and output were configurations of elementary volumes corresponding to these activities. Friedman’s idea of “effort”908 first appeared in this paper as the measure of efficiency for conducting each of these activities. Using the graph theoretic concept of a network,909 Friedman strove to mathematically describe not only the static three-dimensional infrastructure of the Ville Spatiale, but also the mobile fillings within the infrastructure (a network of departure and arrival points labeled with different functions).910 Empirical observation of the frequency911 of movements between departure and arrival points would provide the behavior pattern of the city’s inhabitants. The efficiency of a specific urban

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906 Ibid., 379.

907 Ibid.

908 Ibid., 380.

909 Ibid., 379.

910 In his 1967 article “A Research Program for A Scientific Method of Planning,” Friedman argued that it is possible to track the behavior of the city’s inhabitants by measuring the weekly frequency of movement between departure and arrival points with different functions. These functions were construed as labels corresponding to various nodes of the city, which Friedman represented using a three-dimensional network. It is worthwhile noting that the different functions overlaid as labels in the network were the four functions of the city as they had been articulated in the Charter of Athens, the product of the CIAM IV under the theme of "the functional city." Indeed, the positioning of “effort” as a key metric for evaluating an “urban mechanism” can be viewed as speaking the lingua franca of Modern urbanism. The difference with the Modernist dogma of economy and efficiency is that Friedman repurposed “effort” as a “heuristic,” a way for “informing” decision making processes, rather than a definitive factor for the form of the city.

911 Ibid, 380.
configuration within the Ville Spatiale for conducting these activities constituted what Friedman called the “effort” of a particular “urban mechanism”\textsuperscript{912} \textbf{[Figure 78]}.

Friedman argued that “effort” belonged to a dimension that did not have to do with the physical form of the city but with its underlying topology; the relations between the various departures and arrivals. In essence, Friedman’s formulation was a version of the activity-location problem and the circulation studies that pervaded design methods and operations research. As we saw in Tabor’s review of such work in chapter 4, graph theory was a popular technique for generating plan layouts and evaluating circulation efficiency for a given activity pattern. Friedman recast such methods in three dimensions and also shifted their goal from striving for optimum or sub-optimum solutions to informing the decision making process. He envisioned using a computer \textbf{[Figure 79]} to enumerate the “full range of possibilities”\textsuperscript{913} of urban configurations along with corresponding “warnings” for the efficiency, “effort,” for its inhabitants. Apart from a “warning device for the planner, urbanist, and sociologist,”\textsuperscript{914} Friedman’s system would also serve as a “research tool”\textsuperscript{915} that would keep track of different activity patterns and physical configurations, possibly revealing underlying mathematical regularities.

In 1968 Friedman published his “scientific” system for architecture and planning in the French journal \textit{Techniques et Architecture} under the title “Recherche d’une Méthode”\textsuperscript{916} (“Research of a Method”) This was the first francophone edition of his mathematical explorations, which Friedman had mostly been developing during his visits in the United States.

\textsuperscript{912} To model an “urban mechanism,” Friedman wrote, one needed “a list of a list of possible configurations of a set of obstacles in a limited field; a list of possible distributions of frequencies of movements between couples of such obstacles (here meaning departure points and targets); a calculation of the total overall efforts deployed by the inhabitants (individuals or groups) in their movements.” (Ibid.).
\textsuperscript{913} Ibid., 381.
\textsuperscript{914} Ibid.
\textsuperscript{915} Ibid.
\textsuperscript{916} Friedman, Yona. 1968. Recherche d’Une Méthode. \textit{Techniques et Architecture}. 29(2): 76-82
This method was published the same year in *Arch*+\(^{917}\) and was introduced by Friedman as a “condensed version of the seminars given at various universities in the U.S. between 1964-1967, the outlay following the one [offered] at the University of Michigan, Harvard University and the Université de Montreal in the fall of 1967.”\(^{918}\) The method closed in on the domestic scale and addressed questions specific to architectural design. Friedman polemically presented an “ordering process”\(^{919}\) aiming to transform architecture from a “form of witch-doctorship, a set of uncoordinated kitchen recipe-type knowledge into a well-ordered discipline.”\(^{920}\) Delegating the material aspects of architecture to the artisan or the manufacturer, Friedman defined the disciplinary task of the architect as one of “assembling catalog elements”\(^{921}\) (industrialized architectural components, prefabricated houses and so on) to produce spatial configurations. Invoking Gropius’s characterization of architecture as “sociology’s hardware,”\(^{922}\) Friedman described a system for correlating the habits of the future user with the structure of an architectural layout. Friedman wrote:

> A diagram of points and links (I will call it a network instead of its correct mathematical name: a graph), can give a simplified but understandable image of any architectural plan.\(^{923}\)

Friedman associated the graph with a new ethical imperative: by enabling an objective presentation of all physically possible configurational choices [Figure 80] and their associated

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\(^{918}\) Ibid., 27.

\(^{919}\) Ibid., 34.

\(^{920}\) Ibid., 27.

\(^{921}\) Ibid., 27.

\(^{922}\) Ibid., 28.

\(^{923}\) Ibid.
risks, the graph would deliver the moral and political necessity to “minimize choice [by the professional architect] in the name of other persons, or imposing things on other persons.”

In a subsequent publication in *Techniques et Architecture*, Friedman attached to his method the label of “Démocratisation.” One year later *Pour Une Architecture Scientifique* circulated as part of the “Art-Action-Architecture” collection of Pierre and Franca Belfond’s newly established publishing house in Paris.

**State Planning in France and the Combinatoires of Choice and Change**

When *Pour Une Architecture Scientifique* was published, the French architectural audience was “finally” – as Friedman exclaimed in the introduction- receptive to “a book that is nothing but reasoning - without worrying about pictures.” The proclamation “finally” is significant here. While Friedman’s mathematical explorations of architecture and planning were being well received in North America throughout the 1960s, leading to a proliferation of teaching appointments and lecture invitations, the French contingent approached them with skepticism.

For example, in reporting on Friedman’s 1966 presentation at the International Dialogue of Experimental Architecture (IDEA), École Nationale Supérieure des Beaux-Arts (ENSBA) student league member Bernard Huet accused Friedman of using a “pseudo-mathematical

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924 Ibid., 34.
926 Friedman, *Toward a Scientific Architecture,* xii.
927 Ibid., xii.
vocabulary of a disarming naïveté in order to justify several formal ideas, which in the end are quite limited.” By the end of the 1960s, however, French architects and planners seemed to “finally” be ready to understand Friedman’s mathematical vocabulary not as a post-rationalization of the architectural forms he had been producing, but as the foundation on which he based their inception.

At the end of the 1960s French architecture and planning underwent a significant shift in professional identity. In architecture, the Beaux Arts ideal of the architect as “demiurge,” gave way to the architect as “researcher”: a devotee to science, method, and their associated values. Intensified by the events of May 1968, the demand for scientificity had become synonymous with the demand for “critique,” a rational basis for evaluating the decisions of architects in relation to the social impact of their projects. Following similar developments in the U.K. and the United States that I discussed in previous chapters, “research” in France played a role in restoring a problematic professionalism and upgrading the academic status of architecture. In France, the emergence of the term “research” specifically stood for the demand of interdisciplinary collaboration between sociology and the disciplines concerned with the design of the built environment. Interdisciplinary collaboration in order to provide “rigorous methods and efficacious results,” did not only stem from dissatisfaction with the Beaux-Arts system, but was also a dominant concern of the French State. The integration of architecture and

931 Cupers, “In Search of the User,” 226.
932 Ibid., 8.
933 Ibid., 7.
934 French historian Jean-Louis Voileau indicates that the founding moment for the initiation of “research” in French architecture was the formation of CODRA (the Committee for Research and Development in Architecture) in February 10 1972, following French Minister of Cultural Affairs Jacques Duhamel’s directive that architecture “embrace research which presents rigor in its methods, wholesomeness in its demands and is efficacious in its results.” (Violeau, “Why and How ‘To Do Science’,” 8).
urbanism with the human sciences grew out of the discontents of postwar planning of the “grands ensembles”\(^{935}\) (large complexes), the “backwards” state of the French housing industry, and the aspiration to design the “villes nouvelles”\(^{936}\) (new towns) in ways that would allow them to respond to changing and unpredictable future. Launched in 1965 as part of the Schéma Directeur d'Aménagement et d'Urbanisme de la Région de Paris (Masterplan for the Layout and Urbanism of the Paris Region), a new plan for the city region of Paris, the “villes nouvelles” were at once an ambitious projection into the future and a corrective to the pathogenies of the state-aided, collective housing of the “grands ensembles” that reduced their “users” to universal norms derived from statistical analysis. The State saw the success of the new towns endeavor’s as contingent on the organization of planning expertise.

This change of attitude also instigated a change in funding, not any more to State commissioned planning projects but now to open calls for research into urbanization. Such calls were issued in the mid-1960s by government research organizations such as the Délégation Générale de la Recherche Scientifique et Technique (DGRST) (General Delegation of Scientific and Technical Research). The open calls signaled a shift from applied to basic, curiosity-driven research. This development made space for speculation and criticality\(^{937}\), opened channels of communication between academia and practice, and generated a “market” for urban research.

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\(^{935}\) The planning and design of the *grands ensembles* was formed on the basis of norms and standards for the user, established by the French state as well as national civil society organizations like the Union international des organismes familiaux or International Union of Familial Organizations (Cupers. “In Search of the User,” 76). The production of statistical norms, at times including the more nuanced yet statistically derived notion of user types parsed by age or gender, was also sanctioned by sociologists such as Robert Auzelle and Chombar de Lauwe. Cupers provides a compelling analysis of the elusive figure of the “user” as a controversial site of knowledge production. After the Masterplan for the Paris Region, the passive, functionalist, statistical model of the user would come to be seen as a deliberate construction of an oppressive State apparatus (Ibid., 270).

\(^{936}\) As stated by the Institut d'Urbanisme et de l'Aménagement de la Region Parisienne (IAURP), the organization responsible for the planning of the new towns, the *villes nouvelles* would materialize a “will to transcend” the planning attitude that had generated the “dormitory towns” and the grands ensembles, with their perceived “uniformity and monotony” (IAURP. 1969. “Note Concernant la Conception et la Réalisation des Centres Urbains des Villes Nouvelles de la Région Parisienne.” 199110585/011. CAC, cited in Ibid., 231).

\(^{937}\) Ibid., 262.
engaging offices and semi-public institutions. A common theme in many independent research initiatives was the critique of the universalist construal of the “user,” a key category in the interfacing of sociology, architecture, and urban planning since the making of the “grands ensembles.” These organizations were, in part, inspired by U.S. urban research center models.

For example, leading DGRST figure Michel Conan was an arduous student of Christopher Alexander and highly critical of quantitative models. In 1969 Minister for Cultural Affairs Edmond Michelet founded an architectural research sector, which commissioned French new math pioneer André Lichnerowicz to develop proposals on programs for architectural research. A 1970 proposal resulted in the foundation of the Comité de la Recherche et du Développement en Architecture (CODRA) (Committee of Research and Development in Architecture) in 1972.

The program was geared toward architectural experimentation and its evaluation and fostered visits of foreign experts, exchanges, translation of international works, and a specialist journal.

The idea that permeated these initiatives was that a new kind of expertise, founded upon open interdisciplinary research, would both predict the social consequences of planning and urbanization, and address the highly complex and diversified needs and aspirations of the “users.”

These demands crossed from the planning to the architectural scale through the Plan Construction, initiated in May 1971 under the direction of the “villes nouvelles” key figure Paul Delouvrier. Aspiring to adapt housing to society’s present and future needs and to make housing more efficient and technologically informed, the Plan advanced a form of scientific experimentation where each project was seen as testing a “system of hypotheses” and evaluating

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938 Research bodies that received state funding, in parallel with the initiation of research project funding by governmental organizations, were for example the Centre d’Etudes, de Recherches et Déformation Institutionnelle (CERFI) led by Felix Guattari, teams around Baudrillard or the Institut d’Urbanisme de Grenoble (Ibid., 263).
939 Ibid., 270.
these hypothesis in relation to the satisfaction of user needs. Based on experimentation and evaluation the Plan strongly promoted the informational exchange among various architectural-production related disciplines about recent social-scientific developments and the training and participation of lay users. Laypersons came to be assumed key actors of housing innovation. The Plan recast the user as an active participant in planning and architecture either in French sociologist and philosopher Henri Lefebvre’s sense of “appropriation” or as a maker of choices in the stages of design. A keyword that grew out of this line of experimentation was the so-called “habitat évolutif” (evolutionary dwelling) [Figure 81], “a dwelling capable to assure at once the different aspirations of the clients and the structural modifications of their family.”

The habitat évolutif absorbed a user-oriented rhetoric that aligned with the new ethos of socio-architectural research and market rejuvenation concerns in late 1960s France. It also gave rise to the idea that a remodeled architectural production on the basis of personalized assemblages of prefabricated elements carried liberating potential.

A common belief that grew out of Plan-sponsored projects and studies was that a “combinatoire” (combinatory) of standardized elements could enable users to both directly choose and change their living environments. Despite its ambitions, the applications of the habitat évolutif were limited. Actual implementations usually involved professional architects operating as “assistants,” who informed users about the system of choices, sketched possible layouts according to their needs and aspirations, and finally finished the final apartment plan. Because of technical difficulties the principle of change was also often abandoned and replaced.

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941 Cupers, “In Search of the User,” 392.
942 Lefebvre described “appropriation” as the claiming of space in the course of everyday life, in ways that were not planned or anticipated by the authorities that produced it. The term “appropriated” space was often presented as the opposite of “dominated” space. (Lefebvre, Henri. 1968. *Le Droit a La Ville*. Paris: Anthropos, 155).
943 Ibid., 406.
944 Ibid., 407.
by a fixed layout. The combinatoire ultimately came to be viewed as a way to reclaim the human and the social within the pervasive conditions of standardization and prefabrication in post-war France. In the individualized structuring of industrialized elements, the user’s particular choice of combination, French architects and urban planners saw a remedy to the monotony and repetition of mass production. Apart from reclaiming individuality and self-expression, the combinatorial view of design was also viewed as carrying intellectual benefits. Undergirded by intelligible structures, particular choices of combinations of spatial units or building elements could be compared with structures that fell under the purview of social scientists (behavioral structures, activity patterns, social structures etcetera). Such comparison could potentially reveal relationships between space, society, and individuals and help compare decisions made on the drawing board with the afterlife of the building and its inhabitants.

In *Pour Une Architecture Scientifique*, Friedman presented the FLATWRITER machine in the abstract as an “application of the repertoire”\(^\text{945}\) (the process of making “menus”\(^\text{946}\) of all possible architectural plans for a number of rooms). Yet the FLATWRITER was modeled after, and spoke to, the conditions of the *Plan*. Conceived as a design typewriter of sorts, the FLATWRITER automated the combinatorial process that Friedman saw as giving shape to the Ville Spatiale – Friedman’s speculative ville nouvelle. The FLATWRITER presented the user with two keyboards, one consisting of abstract geometric shapes and equipment types (assigning function to the shape), and one consisting of “weights” (denoting the frequency that the user visited each space type in his everyday life). The machine “predicated a framework of existing stocks of prefabricated elements, service units, bathroom and kitchen units”\(^\text{947}\) [Figure 82]. With

\(^{945}\) Friedman, *Toward A Scientific Architecture*, 53
\(^{946}\) Ibid., 33.
\(^{947}\) Friedman, “The Flatwriter,” 130.
these keyboards, the user could “type” and “print out” preferred plan configurations within a “repertory of several million plans,” while the machine fed back “warnings concerning the consequences implied by any projected use pattern.” The FLATWRITER calculated such “warnings” by implementing the graph theoretic method that Friedman had developed for his North American teaching tour to assess the congruence of “use patterns” and layout “configuration.” The machine also performed a “second loop,” where Friedman deployed the graph-theory based method of “urban mechanisms” to inform the user about the urban scale consequences of positioning the selected configuration in a specific location of the Ville Spatiale.

In contrast to the habitat évolutif experiments, the FLATWRITER claimed to remove the expert architect, urbanist, or sociologist from the process of design decision-making and evaluation of their results. Friedman promoted the machine as eliminating intervention from the “architect assistant,” and allowing users to both self-operate the system of configurational choices and evaluate the consequences of a given configuration for their particular living habits (or track the evolution thereof).

GEAM, GIAP and the Image of the Infrastructure

In 1974 French writer, journalist, historian, and critic Michel Ragon published an article in the French journal Urbanisme applauding Friedman’s abstinence from architectural production. The article, titled “Yona Friedman: De L’Habitat Evolutif A L’Autoplanification” (“From Evolutionary Dwelling to Self-Planning”) positioned the work of the dissolved GEAM, and in particular its founder Yona Friedman, as the basis of the proliferating habitat évolutif

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948 Ibid., 129.
949 Ibid., 130.
950 Ibid.
951 Cupers, “In Search of the User,” 407.
experiments. Ragon lamented that Friedman’s ideas had been diffused to such an extent that they appeared “normal,” despite the fact that they “ha[d] only been partially applied and always severely denaturing the initial point of view.”\textsuperscript{952} In its short lifespan from 1958 to 1962, the GEAM\textsuperscript{953} published manifestos bringing together ideas of transformability, change, and participation, and bundled them with a recognizable architectural expression: space frames and modular structures.\textsuperscript{954} In the imagery of the GEAM’s gigantic space frames and their principles of mobility and transformability Ragon saw a solution to the impasse of French State planning and architecture. In his 1962 book \textit{Où Vivrons-Nous Demain} (Where Will We Live Tomorrow), Ragon connected the infrastructural projects of the GEAM members with the emerging and pressing question of how to plan for an unpredictable future.

In 1963, Ragon coined the term “spatial urbanism” to describe ongoing experiments with space frames and megastructures.\textsuperscript{955} Soon after, Ragon enlisted architects and artists experimenting on the subject, including Friedman in the Groupe International d’Architecture Prospective (GIAP) (International Group of Prospective Architecture). As architectural historian Larry Busbea discusses, “prospectiveness” in this context stood for “technical forecasting of what is likely […] in a more or less scientific manner” as opposed to the “utopian imagining of the possible.”\textsuperscript{956} In \textit{Les Visionnaires de l’Architecture}\textsuperscript{957} (The Visionaries of Architecture) \textbf{[Figure 83]}, the 1965 catalog that showcased GIAP members’ work, Ragon admitted that the

\begin{footnotesize}
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\item \textsuperscript{952} Ragon, “De l’Habitat Evolutif a l’Autoplanification,” 75.
\item \textsuperscript{953} After his participation in CIAM X in 1956, Friedman reached out to various prominent figures and groups in the international architectural scene, including Team 10, Le Corbusier, and Buckminster Fuller to encounter “violent opposition” from the first and “favorable responses” from the two latter (Busbea, Larry. 2007. \textit{Topologies: The Urban Utopia in France, 1960--1970}. Cambridge, Mass.; London: The MIT Press, 62). In 1957 he published the principles of his Mobile Architecture in an article co-authored with Günther Kühne that appeared in the journal \textit{Bauwelt}. One year later, having relocated from Haifa to Paris, he founded the GEAM, including like-minded architects Georges Emmerich, Werner Runhau, Günther Gunschel, Frei Otto, Paul Maymont, Eckhard Schulze-Fielitz and others.
\item \textsuperscript{954} Ibid., 64.
\item \textsuperscript{955} Ibid., 82.
\item \textsuperscript{956} Busbea, \textit{Topologies}, 96.
\end{itemize}
\end{footnotesize}
projects were “visionary” and “fantastic,” albeit of a particular kind. Countering “free” fantastic (aesthetically driven imaginative follies), the catalog presented a version of a “prospective” fantastic, soberly conceived and potentially realizable. In the context of constant social and technological change in postwar France, “prospective” architecture set out to accommodate an unpredictable future by replacing fixed-layout buildings with infrastructures that could allow many configurational possibilities.

Despite the daring imagery of their proposals, the ethos of the GIAP was one of reasoning and self-abstinence – of “lone research” as GIAP member Marc Gaillard would later describe it. Friedman was a leading presence in GIAP and front-runner of its prospective culture. A keen observer of new technological and scientific currents, Friedman brought together the architectural expression of space-frames with a mathematical formulation that professed to clarify their principles. This mathematical formulation, in turn, allowed Friedman to engage with the computer; scarcely available in 1960s France and inspiring speculation as the epitome of socially transformative technological progress. These endeavors resonated with Ragon, who in his 1974 Urbanisme article Ragon commended Friedman’s “abandonment of graphic visualization” in favor of the “mental and the speculative” and advertised his three-year collaboration with MIT in order to implement his theories on a computer. Ragon was referring to Friedman’s work with MIT’s ArcMac circa 1971-1973 – the culmination of his 1964-initiated theoretical and methodological investigations in North American institutions.

959 Ibid.
960 Ragon referred to the three-year participation of Yona Friedman as a visiting researcher in the MIT Architecture Machine Group, under an NSF funded proposal titled “Machine Recognition and Inference Making in Computer Aids to Design.” Friedman collaborated closely with Guy Weinzapfel of the Architecture Machine Group for the “Architecture-By-Yourself” project, aiming to implement Yona Friedman’s graph theoretic participatory method in a computer. The computer program was called YONA, standing for “Your Own Native Architect” and acknowledging the program’s intellectual fatherhood.
Indeed, as his publications between 1964 and the release of *Pour Use Architecture Scientifique* indicate, the bulk of his mathematical techniques drew from work in the new field of design methods or from operations research. Friedman absorbed and repurposed activity-location techniques, circulatory efficiency evaluations, and layout generation methods, many of which relied on graph theory. At an operational level Friedman enlisted the graph to do what it was already doing in the context of rational design methods. However, he decisively shifted its symbolic meaning. Moving beyond technical pragmatism, Friedman associated the graph with intellectual, ethical, and aesthetic values of science, democracy, and realism. The interest in Friedman’s mathematizing work is not as much as how he imported and repurposed existing techniques under the rubric of a new rhetoric, as in the forces that made graph theory appear a congenial and natural device for delivering his architectural and theoretical agendas. The story is more about metaphor and association that about import and appropriation. The graph’s perceived naturalness and congeniality grew from the confluence of visual and intellectual cultures that Friedman engaged with in France and the U.S.

In the late 1950s structural engineer Robert Le Ricolais, commonly acknowledged as the father of spatial structures and precursor of spatial urbanism, defined architecture as the “science of combinatorial arrangements of space.” In an unpublished 1958 manuscript titled “Topology, the Cornerstone of Structures” Le Ricolais remarked the affinity of topology with the system of the space-grid, a novel structural system pioneered by Konrad Wachsmann and Buckminster Fuller. A dynamic, expandable system, the space-grid did not only constitute engineering innovation, but also carried programmatic and spiritual implications of wholeness,

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961 Busbea, *Topologies*, 152.
962 Ibid.
universality, comprehensiveness, and dynamism. Topology was not only a way for calculating the structure of a space-frame, but also for manipulating the material that it contained: space. The space-frame partitioned continuous space in a number of discrete spatial units, cultivating an understanding of design as a process of configuring this discrete material. The idea that space was discrete material manipulable through a kind of spatial combinatorics also prevailed the work of GEAM member and Friedman’s close collaborator, Eckhard Schulze-Fielitz. In his 1962 article “Une Théorie Pour l’Occupation de l’Espace” published in the French journal L'Architecture d'aujourd'hui, Schulze-Fielitz proposed to view space as a “macro-material,” which can be reduced to a “few elementary particles.” He then argued that the characteristics of the material could be determined by the “combinatorial possibilities” of these spatial atoms [Figure 84]. GEAM member Georges Emmerich developed Le Ricolais’s ideas emphasizing the manipulability of topologies by the users of architecture. Similar to Eckhard Schulze-Fielitz, Emmerich articulated an idea of “autoconstruction” and a system of “construction games” based on combining and recombining modules within crystalline structures made of platonic solids.

These space-frame explorations were crucial in supporting Friedman’s graph realism; the claim that graphs represented the real world in a non-distortive manner. Visually, the graph’s points and lines evoked the space frame’s rods and nodes [Figure 85]. In Pour Une Architecture Scientifique, Friedman drew graphs in the same skeletal visual language that he sketched the Ville Spatiale’s infrastructure. Apart from this visual resemblance, space-frames also supported the graph’s validity as representation of architectural space, as well as processes of architectural design. The graph’s discrete points matched the discrete units in which space-frames segmented

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905 Busbea, Topologies, 155.
three-dimensional space. Its lines represented the act of combining and configuring these units, which space-frame proponents defined as the act of architectural design. Notably, in the experiments of “spatial urbanism” the visual language of the space-frames and the combinatorial process of arranging their units was coupled with ideas of participation, mobility, and agency. Space-frames provided a neutral and featureless infrastructure, which derived from physical and mathematical laws of spatial subdivision. This infrastructure could accommodate many possible combinations, thus affording its inhabitants the possibility to choose and change their living setting by configuring and reconfiguring discrete units. Friedman’s immersion in space-frame experiments, promoted a combinatorial understanding of choice and change. Friedman recognized the same principles in the spatial combinatorics of graphs.

North American Expeditions and a Serendipitous Encounter

Upon venturing to the U.S. to promote his architectural theories, Friedman came across implementations of graph theory in layout generation and evaluation methods. These methods generated different configurational possibilities of a defined number of spatial units and evaluated their efficiency for a given or anticipated pattern of user activities. Similarly, in his mathematical method Friedman invoked isomorphisms between activity patterns and spatial structure and used the graph’s combinatorial properties. However, instead of striving to maximize efficiency, he repurposed the graph to inform users about choices available to them and their consequences. Friedman broke from the more application-oriented use of graphs in U.S. design methods, by declaring the graph a harbinger of a transparent architectural science. By Friedman’s moral code, the “paternalistic” attitude of deciding what is best for the users was inherently immoral. “Any system that does not give the right of choice to those who bear the
consequences of bad choice,” he aphorized, “is an immoral system.” Friedman’s “science” limited the role of architects and planners to the enumeration of all possible configurations for a user-specified architectural programme (number and types of spaces) and calculating “warnings” for each configuration based on the user’s personal lifestyle (daily use patterns). In this new architectural process, the expert designer’s role was reduced to that of a mechanical executor, replaceable by a typewriter-like machine like the FLATWRITER. The process relied on the fundamental separation of the “objective” aspects of architecture and planning (delegated to experts, or to machines) from the “intuitive,” “subjective” aspects (preserved as the users’ exclusive domain).

In personal conversation Friedman revealed that he developed his theory after meeting mathematician Frank Harary at the University of Michigan. Harary was best known for applying graph theoretic concepts to the human sciences and for his mathematical contributions to structural anthropology. For promoting graphs, Harary capitalized on the dovetailing between graph theory and the human sciences in the 1950s and 1960s, which had qualified structural abstraction with both humanizing and scientizing values. Human scientists and mathematicians shared optimism about the unification of the “soft sciences” under the rubric of mathematical descriptions that were at time qualitative and mathematical, “soft” and “hard.” A telling example was Lévi-Strauss’s elegy to graph theory in his famous 1954 paper “The Mathematics of Man.” Levi Strauss criticized quantitative reasoning oversimplifying qualitative phenomena, while being based on obscure calculations, impossible to intuitively evaluate by the common

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967 Yona Friedman, 31 March 2012, Interview by Theodora Vardouli.
968 Lévi-Strauss, “Mathematics of Man.”
individual. “Qualitative mathematics,” he continued, would come to remedy these problems, furnishing the “soft sciences” with a hard, mathematical core. Enabling different fields to “speak the same language [emphasis mine],” would open a pathway for “the co-ordination of methods of thought.”

Lévi-Strauss remarked that the use of this “qualitative mathematics” required an intellectual shift, similar to the one that had taken place in early twentieth century structural linguistics: the identification and separation, in each discipline, of an objective substructure that could be represented as atomic elements linked with relationships, and of a subjective surface that could be described on the basis of combinatorial operation on the ordered elements of the substructure. Despite Friedman’s contentious relationship with the French vogue of structuralism, he absorbed its tenets in separating an “objective” substructure from its “intuitive” surface both in his architectural proposals (the Ville Spatiale) and his theoretico-mathematical articulations [Figure 86]. Echoing Lévi-Strauss’s formulation, Friedman construed freedom of choice as a combinatorial operation on an invariant and objective order. In graph theory, Friedman saw the possibility to first separate, and subsequently bridge, the domain of necessity (an objective substructure that can be scientifically known and designed) with the domain of freedom and

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969 Lévi-Strauss stated: “A school of what might almost be called qualitative mathematics [emphasis mine] paradoxical as the term may seem, because a rigorous treatment no longer necessarily means recourse to measurement. This new mathematics (which incidentally simply gives backing to, and expands on, earlier speculative thought), teaches us that the domain of necessity is not necessarily the same as that of quantity [emphasis mine].” (Ibid., 585).
970 Ibid., 582.
971 Ibid., 590.
972 The salient ideas that Levi Strauss extracted from structural linguistics, was first that the language consisted of distinct, separate and identifiable elements (phonemes) and second, that these elements were linked with relationships. If language was atomic, and all social communications were based on language, then Levi Strauss contended that the social could also be understood in atomic terms. After all, he continued, the atomic nature of language was also validated by independent research in the field of communication theory, which identified communication on the basis of combinatorial operation on ordered elements. (Ibid.).
personal choice (a combinatorial operation on the substructure, that cannot be predicted but can be supported and absorbed).

Intellectual advantages aside, Friedman’s embracing of graph theory crucially relied on its intuitive graspability. “Graph theory,” Friedman remarked, “for certain Mathematicians is not Mathematics, it is not Logic, but it is a very useful tool. It is useful for people to experiment.” Such claim was not only a statement of the visual associations, naturalness and congeniality that the graph evoked for Friedman, but also a persistent argument in Harary’s work. In 1971, Harary published an article titled “Aesthetic Tree Patterns in Graph Theory,” in which he discussed the findings of an experiment that he performed on the thirty-eight students that attended a course he was teaching at the University of Waterloo in the fall of 1970. The experiment replicated an experiment on the psychological aspects of pattern recognition performed by Claude Faucheux at the University of Paris. The experiment involved asking students to draw all tree graphs corresponding to 8 points, revealing the number of options to the first group and concealing it from the second. The goal was to see which graphs were bound to be omitted, and correlate the omission with mathematical properties such as symmetry. Upon replicating the experiment, Harary added a step where the students were asked to rank the different tree graphs by aesthetic preference and assign to them labels such as “prettiest,” “ugliest,” “funniest,” “sexiest” [Figure 87]. Implicit in this experiment, was the claim that graphs, were ultimately, aesthetic entities that engaged the eye as much as they did the mind.

The construal of the graph as a purely mathematical and not a visual (aesthetic) object was saliently conveyed by Friedman’s claim that despite its many illustrations, Pour Une Architecture

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974 Friedman, Interview by Vardouli.
976 Ibid., 229.
Scientifique has “no pictures.” However, as I have attempted to illustrate in this section, aesthetics was a powerful force for attaching values of architectural democracy and science to the graph. Its potency was precisely its oscillation between the visual and the computable, the soft and the hard, the subjective and the objective, enabling the reconciliation of rationalizing and scientizing with humanizing and democratizing ideals. After passing through Friedman’s texts, the graph continued to perform its reconciliatory functions between anticipation and unpredictability, control and open-endedness. In introducing the MIT Press-published English translation of Pour Une Architecture Scientifique ArcMac leader Nicholas Negroponte, declared his affinity to Friedman and his ideologies. “[...] Friedman contradicts the scholastic insinuation,” he wrote, “that if you can understand graph theory, for example, you probably lack the human compassion and breadth of thought necessary to appreciate social goals.” As I now move on to discuss, the graph, compatible with the discrete and combinatorial operations of digital computers, would come to be invoked by ArcMac as a representation of space, shape, and of the user, ultimately forming the technical basis of ideas of interactivity and participation in design.

5.3. Architecture-By-Yourself and Computer Aided Participatory Design

YONA on an Interdata

Circa 1975, Friedman’s theory was incarnated in a computer system for architecture-by-yourself. In SIGGRAPH 1976, ArcMac researcher and YONA system developer Guy Weinzapfel, presented the system with a mix of arguments on professional morality ("unpracticed designers

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977 Friedman, Toward A Scientific Architecture, xii.
979 Guy Weinzapfel completed his Master’s at the MIT Department of Architecture in January 1971. Before enrolling to MIT as a graduate student, Weinzapfel studied architecture in the University of Arizona and worked professionally for Walter Gropius’s Boston-based architectural firm The Architecture Collaborative (TAC).
[... ] unlike architects, bear a risk!”  

and fertility for computer research (“the demands of the unpracticed will [...] accelerate development of graphical input techniques, display capabilities and design strategy systems”)  

Weinzapfel suggested that because of their “demands to visualization” and “strategic assistance” computer aids for non-professional designers provided a “relentless setting” that could motivate further developments in computer graphics and CAD. This was in agreement with the aims of the project’s computer research sponsor (NSF’s Division of Computer Research), targeting technical developments in computer research more so than participatory architectural agendas. In a research-in-progress report that appeared in the July 1975 issue of the Computer-Aided Design journal, ArcMac founder and director Nicholas Negroponte was more unapologetic about cashing in on do-it-yourself and participatory design philosophies to push computer research opportunities. He presented the Architecture-By-Yourself experiment, of which YONA was part, as “an application of principles in computer-aided design and of techniques in computer science which taxed each to their utmost” and continued to reassure the readers that “while the philosophy of architecture-by-yourself may be distasteful to some, the techniques and systems have very real application to more pragmatic and professional views of architecture.” An example of ABY’s productive output was the reuse and testing of “major parts” of other professionally-oriented CAD systems developed in the MIT School of Architecture and Planning at the end of the 1960s under NSF support.

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981 Ibid.
982 Ibid.
983 Weinzapfel located the relentlessness of developing computer aids for unpracticed designers in three domains: the demands of the user, the difficulty of the problem, and the personalized, one-of-a-kind nature of the required design solution (Ibid.)
985 Ibid.
986 Ibid.
While in the first half of the 1960s most CAD research at MIT transpired under the umbrella of the “Computer-Aided Design Project” (CAD Project) — a project mainly affiliated with the Departments of Mechanical and Electrical Engineering — the end of the decade found the Department of Architecture establishing its presence in computer aids development. In December 1968 School of Architecture and Planning Dean Lawrence Anderson was reporting that despite the comparatively slow adoption of “the powerful tools of computation” in architecture and planning “the trend is now in full swing.” The wake of the 1970s found the Department of Architecture harnessing a tenfold increase in computing power and storage capacity of “in-house” computing facilities within a year and a dramatic rise in research funding. Research activity in architecture and planning at MIT was also boosted by the School’s participation in new institute-wide interdisciplinary initiatives such as the Urban Systems Laboratory (USL), founded in 1968 with the goal to connect, support, and amplify urban research efforts by faculty members and students via computer, information, and other resources. The turn of the 1970s found computer research in the MIT School of Architecture and Planning gathered around three


989 Negroponte and Groisser, Computer Aids to Participatory Architecture, 64.

990 Sponsored research in the MIT Department of Architecture rose from a cash flow of $256 per year in 1965 to $198,255 in 1970, mostly as a result of new computer-related work (Ibid., 71).

991 The Urban Systems Laboratory (USL) was founded in early 1968 with Ford Foundation funding as a collaboration of the Schools of Architecture and Planning, Engineering, Humanities and Social Science, and the Sloan School of Management. The USL responded to the recommendation of the Ad Hoc Faculty Committee on Urban Affairs, which encouraged MIT to capitalize on the “quality and quantity” of “systems research and computer methods of problem solving” at MIT to fulfill the institute’s “long term … major commitment to urban affairs” and the “formulation and solution of the complex problems of the city” that were the “broadest and more complex systems problems faced by the Institute” (Miller, Charles L. 1968. “Urban Systems Laboratory.” Massachusetts Institute of Technology Bulletin: Report of the President 104 (3): 490). The USL was construed as an open community of people rather than a separate organization (Ibid., 491).

992 Ibid.
research initiatives: IMAGE [Figure 88], an automatic space-planning system that arranged rectangular volumes in three dimensional space so as to reduce violations of designer-specified criteria and constraints led by Timothy Johnson; DISCOURSE, a programming language specific to problems of architecture and planning led by William Porter, and the Architecture Machine, led by Negroponte. Notably, YONA’s author Guy Weinzapfel pursued IMAGE-related research as graduate student in the MIT Department of Architecture, and integrated parts of IMAGE in the ABY project.

While all three initiatives variously benefited from the School’s newly acquired resources and funding channels, including the NSF, ArcMac was especially adept at appealing to computer research funders. It achieved such a feat by using architectural design, construed as a meaning

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993 IMAGE was developed under NSF funding from June 1961 to September 1970. Despite the visual connotations of its title, IMAGE’s title referred not to the visual appearances of a design, but to the cognitive, structural “perception of the problem” (Weinzapfel, Guy, Timothy E. Johnson, and John Perkins. 1971. “IMAGE: An Interactive Computer System for Multi-Constrained Spatial Synthesis.” In Proceedings of the 8th Design Automation Workshop, 101–108. DAC ’71. New York, N.Y., USA: ACM, 108) IMAGE followed the typical automatic space arrangement type programs: “modeling a selected architectural problem” (Ibid., 101) on the basis of points (in IMAGE’s case rectangular units of space) and lines (relationships), generating possible configurations, and circling through them to evaluate how they meet a number of objectives such as visual access, alignment, and enclosure (as opposed to only distance or circulation criteria). Its goal was to assist “in exploring the possible consequences of his perception of the problem” (Ibid., 106) by allowing for exploration of “a wide range of problem formulations and a large set of possible arrangements” (Ibid.) instead of settling with a “final or optimum” (Ibid.) design.

994 Timothy Johnson was Assistant Professor in the MIT Department of Architecture and leader of the NSF-funded IMAGE research project. He was mostly well known for his 1963 masters thesis in the MIT Department of Mechanical Engineering, where he developed a three-dimensional extension of SKETCHPAD — the first graphical computer aided design program that came out of Ivan Sutherland’s dissertation. See, respectively: Johnson, Timothy. 1963. “SKETCHPAD III, Three Dimensional Graphical Communication With a Digital Computer.” Cambridge, Mass.: Massachusetts Institute of Technology, and Sutherland, Ivan Edward. 1963. “Sketchpad, a Man-Machine Graphical Communication System.” Thesis, Massachusetts Institute of Technology.

995 Guy Weinzapfel submitted his Master’s thesis to the MIT Department of Architecture in January 1971 with the title “The Function of Testing During Architectural Design” (Weinzapfel, Guy Edward. 1971. “The Function of Testing during Architectural Design.” Thesis, Massachusetts Institute of Technology). The thesis was supervised by William Porter and Timothy Johnson, leaders of DISCOURSE and IMAGE respectively: the two other large NSF-funded computer-related research projects taking place within the MIT School of Architecture and Planning aside from the Architecture Machine. Weinzapfel’s thesis was the outcome of work he conducted on the IMAGE system from 1968-1971. In 1971, Johnson left IMAGE to the responsibility of Weinzapfel, who managed to solicit further support from the NSF. With the supervision of a faculty member being a requirement for performing funded research, Weinzapfel turned to Negroponte, who took Weinzapfel in ArcMac. Before pursuing graduate studies at MIT and turning to computer aided design research, Weinzapfel studied architecture in the University of Arizona and worked professionally for Walter Gropius’s Boston-based architectural firm The Architecture Collaborative (TAC) (Weinzapfel, Guy. 22 March 2012. Interview by Vardouli).
rich, contextually embedded activity, as a frontier for computers, viewed as notoriously poor in handling context and meaning. By the mid 1970s Negroponte was celebrating the freedoms of securing funding “entirely from computer science sources, not the equivalents of the RIBA or Department of the Environment in the U.K.”\(^{996}\) “Basic research [funding] to further understanding of computer aided design”\(^{997}\) enabled ArcMac to direct its research “at the forefronts of computer hardware, systems programming, artificial intelligence, and programming languages.”\(^{998}\) Although ArcMac was geared toward education as “primary product” and not “tools for practice”\(^{999}\) these technologies were applied to design in the context of “thesis topics, course work, and student-initiated research.”\(^{1000}\) Architecture-By-Yourself was such an example of bringing together a popular academic topic of the early 1970s (participatory architecture) with technological challenges (input and output devices, graph theory algorithms) and existing tendencies in CAD (automatic layout generation and space planning, as exemplified by IMAGE).

Having pursued his graduate thesis on theoretical formulations of “testing” in architectural design\(^{1001}\) and its implementations on the IMAGE system, Weinzapfel was versed in the

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\(^{997}\) Ibid.

In 1975, Negroponte estimated that the Architecture Machine received about $300,000 per year. This contrasted an earlier report in *Computer Aided Design* by Charles Eastman, a key figure in the development of computer aided design, known for his seminal contributions to Building Information Modeling (BIM). In appraising “computer applications to architecture in the USA,” Eastman calculated computer aided design funding to a yearly $300,000, which Negroponte found too low: “in that case, it’s all spent on The Architecture Machine” (Ibid.) For Eastman’s article see Eastman, Charles. 1974. “Through the Looking Glass — Why No Wonderland?: Computer Applications to Architecture in the USA.” *Computer Aided Design* 6 (3): 119–24.

\(^{998}\) Ibid.


\(^{1000}\) Ibid., 190.

\(^{1001}\) Weinzapfel’s MIT Masters thesis was an effort to formalize the testing of architectural form and examine its role in the design process, driven by the “realization that testing was a very important function of design and that it could have a ‘life of its own’ separate from generation” (Weinzapfel, “The Function of Testing during Architectural Design,” 11). Weinzapfel used IMAGE to generate design alternatives from a set of constraints (mainly circulation related) and tested these by both “conventional and computer aided processes” (Ibid., 39). Because IMAGE did not yet incorporate testing capabilities, tests were performed “separate from the generation of the alternatives” thus allowing to “further isolate the testing function and prevent its confusion with other operations” (Ibid., 40).
possibilities and discontents of automatic constraint-based space planning, alongside the customary technique for pursuing it: graph theory [Figure 89]. Such practiced understanding of designing as a process of evaluating architectural arrangements on the basis of how they perform in relation to objectives led “quite naturally,” as Weinzapfel put it, to the computer implementation of Friedman’s “graph theory approach” in the YONA system. A computer aided design system for the “unpracticed,” Weinzapfel contended, required a representation that could describe both the design itself (the physical space) and the designer’s objectives. By representing “the linkages of the house - the connections (doorways) between its spaces,” Friedman’s graph theory method “provide(d) both the backbone of the design and the largest payoff in terms of describing the designer's objectives.”

In the previous chapter, we saw Philip Tabor and Philip Steadman discussing the use of graph theoretic techniques to generate architectural layouts, either manually, by drawing graphs with pencil and paper, or automatically in a computer program. In YONA, Weinzapfel proposed the graph as both a malleable visualization of architectural arrangement and a computational device. Instead of hiding it as background computation, YONA visualized the graph on a touch-

1002 Weinzapfel presented a critique of IMAGE in the second International Conference of the Design Research Society (DRS) that took place in London, in August 1973 with The Design Activity as theme. The DRS was an outgrowth of early 1960s design methodological endeavors in the U.K., which in the wake of the 1970s distanced itself from rationalist and scientific rhetoric and embraced ideas of social engagement and user participation in design. In The Design Activity conference, Weinzapfel presented a critique of IMAGE’s impediments for creative design problem solving, highlighted from independent observational studies of designer behavior. Weinzapfel claimed that although IMAGE was a definite advancement in computer aided design, it compromised the designer’s “overall success” by its “rigid syntax” and the entrapment in quantitative tasks that “prevent (the designer) associating freely with his own stored experiences.” He attributed this to the removal from IMAGE from the designers’ “real needs” to the “self-induced curiosity of what design activities the computer could be made capable to performing.” Such phenomenon led Weinzapfel to ask a troubling question for computer aided design, also featured as his presentation’s title: “It Might Work But Will It Help?” Implicitly, Weinzapfel was asking for an attunement of the field that would come to be known as design research — a scrutiny of design activity across different disciplines and domains— and computer aided design research. The context for such quest will be become clearer as the chapter progresses.
1003 Weinzapfel and Negroponte “Architecture-By-Yourself,” 75.
1004 Ibid.
1005 Ibid.
1006 Ibid.
sensitive computer screen and allowed the users to manipulate it using their fingers (instead of the customary light pen) [Figure 90, Figure 91]. In his SIGGRAPH presentation, Weinzapfel eulogized the graph’s many benefits: its versatility in capturing under one singular mathematical representation different stages of the design process (“the graph theory approach […] incorporates the notion of using different representations, each of which is uniquely appropriate to a specific function”); its simplicity (“[the graph] form[s] a procedural strategy for the inexperienced designer to follow […] [which] introduces completeness to the design without undue complexity”); its informativeness in revealing otherwise unseen conflicts of connectivity, access, or enclosure (“The graph enables the designers to see simple difficulties in his linkages […] permits an observer to monitor the designer’s progress and […] spot troubles early on”).

Most salient among these many traits, and specific to the way that Friedman construed the graph, was its intuitiveness, its “tangibility.” Weinzapfel wrote:

Friedman’s approach decomposes the design problem, not by abstract concepts of form and function, or by service spaces and areas served, but by the more tangible factors of room placement, connectivity, size, shape and 3D form.

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1007 ArcMac acquired a touch sensitive digitizer from the Canadian company Instronics Ltd. in April 1976, on which they performed “fingerpainting” experiments. This work was conducted by Richard Bolt under ARPA funding (contract number MDA-903-76-C-0261) from April 1, 1976, to September 30, 1976. Work on touch sensitive displays continued in 1977 under Army Research Institute funding (DAHC19-77-G-0014) with Negroponte as Principal Investigator. For an overview, see Herot, Christopher F., and Guy Weinzapfel. 1978. “One-Point Touch Input of Vector Information for Computer Displays.” In Proceedings of the 5th Annual Conference on Computer Graphics and Interactive Techniques, 210–216. SIGGRAPH ’78. New York, N.Y. , USA: ACM.

1008 In order to extract the demands of a computer aided design system for unpracticed designers, Weinzapfel observed the “design ‘actions’” of a couple of inexperienced designers, the Falcos, who he observed over the course of eight weeks. According to Weinzapfel, these observations confirmed visualization and strategic guidance as crucial requirements. (Ibid., 74).

1009 Ibid., 75.

1010 Ibid.

1011 Ibid.

1012 Ibid.
The YONA system did away with the “lifestyle” self-tracking that was integral to the FLATWRITER. It added, however, missing semblances of a drawing board process (bubbles, grids, shapes), with the aspiration to engage the user in an activity that looked less like typing in a typewriter and more like what would traditionally be understood as designing. Ultimately, the YONA system was a redux of FLATWRITER’s graph theory based operation, enhanced through planarity testing and dual formation algorithms that were developed in mainstream automatic layout generation research, and rehashed through the possibilities of computer graphics. The system was also the epigone of a growing agenda against architectural professionalism that ArcMac invoked both as motivation and frontier of its computer-related research. Below, I trace these lineages, as they intimately intertwined with broader developments in design methods and CAD research.

**URBAN5’s Cubes and DMG’68 Conference’s Boxes**

In the U.K., the surge in design methodological activity throughout the 1960s was fueled mainly by anxieties about design as an intellectual discipline and questions of professional responsibility. British design methods spanned engineering and architectural domains and were mainly concerned with finding a rational basis to explain and justify professional design decisions. Although eventually just as prolific, the U.S. design methods cohort did not formally assemble until the late 1960s. It was mostly oriented toward architecture and environmental design and was from its onset closely intertwined with computer implementations. Telling of such intertwining was the topic of the 1966 International Conference on “Design and Planning” at the University of Waterloo that motivated the foundation of the DMG. Themed “Computers in Design and Communication” the Waterloo conference was portrayed by its organizer Martin
Krampen\textsuperscript{1013} as “the first design conference in which the speakers were not mainly designers but computer specialists.”\textsuperscript{1014} The foundational filiation of U.S. design methodology with computer research was also reflected in the Waterloo Conference proceedings, co-edited by Krampen and graphic designer Peter Seitz,\textsuperscript{1015} and published both as a book\textsuperscript{1016} and the impactful double issue of Design Quarterly “Design and the Computer”\textsuperscript{1017} [Figure 92]. Seitz introduced the “more technical than usual”\textsuperscript{1018} issue of Design Quarterly with a polemic for the adaptation to, and adoption of, new computer tools and methods in design. He framed the embracing of computer technologies by means of a serendipitous analogy:

> Very much like the children who are caught between the old math and the new, today’s designers have to face the computer age, turn away from the security of the familiar and learn to adapt to the new methods.\textsuperscript{1019}

One can only hypothesize Seitz being a recipient of the new mathematics proponents’ rhetoric, which frequently linked its content with a new age driven by computation and technology. Be it a

\textsuperscript{1013} Martin Krampen was Professor of Design and Psychology in the University of Waterloo and foundational member of the DMG. At the time of the Design and Planning '66 conference, he was also teaching in the Hochschule für Gestaltung in Ulm. Krampen was amongst a group of researchers in various universities, pursuing further work on Alexander’s HIDECS 2 and 3 systems (Moore, Gary T., ed. 1967. “Conceptual Design Problems -- HIDECS.” DMG Newsletter 1 (1): 4–5). Krampen compared “intuitive” with rational decomposition of complex design problems in Krampen, Martin. 1967. “Design Analysis on the Basis of ‘Experience’ vs. Design Analysis by Computer.” In Design and Planning, edited by Martin Krampen and Peter Seitz. Vol. 3. New York, N.Y. : Hastings House. He also presented a method for generating a semi-lattice by running HIDECS’s program SIMPX, iteratively on its own output.


\textsuperscript{1015} Seitz studied under Argentinian designer Tomás Maldonado in the Ulm Hochschule für Gestaltung and continued to pursue graduate studies at the Yale School of Art, where he was awarded a Master’s of Fine Arts degree in Graphic Design and Photography in 1961. After professional work for the architectural firm I.M. Pei & Associates in New York and a short teaching appointment at the Maryland Institute College of Art, he was hired as design curator of the Walker Art Center in Minneapolis and editor of Design Quarterly. For a fuller account of Seitz’s trajectory see: Pitner, Kolean. 2012. “The Life and Work of Designer Peter Seitz.” Design Observer. May 23. http://designobserver.com/feature/peter-seitz/28258.

\textsuperscript{1016} Design and Planning 2 was published as a companion to Design and Planning 1965 as “a book of readings in the field of design … geared to answer specific questions on the possibilities of using computers in the design of environments, buildings, products, and communications” (Seitz and Krampen, Design and Planning 2, 3).


\textsuperscript{1018} Seitz, “Introduction,” 2.

\textsuperscript{1019} Ibid., 4.
deliberate statement or a suggestive analogy, Seitz was advocating for an active embarking of designers on computer research. He argued this both on the basis of the familiar diatribe about the inadequacy of ossified intuitive design methods, as well as the imperative to jump on the research bandwagon so as to “avoid the computer specialist solving the designer’s problems.”

DMG’s computer orientation was also evidenced in its first International Conference that took place in June 1968 at MIT. Around the time of the conference, design methodology in the U.K. was nettled by ethical and philosophical questions on its early 1960s efforts to recast design as a rational, scientific process. The 1968 DMG Conference did not show to be affected by the internal crisis of British design methods. The event was affluently funded.

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1020 Ibid., 4.
1021 In the DMG Newsletter, Moore placed the conference in conversation with a series of events that considered the role of computer aids and computer graphics in architecture and design: A meeting of 30-40 people at MIT in September 1966 to discuss common work on computer aided building design, the outcome of which was the launch of the Newsletter on Computer Aided Building Design in December 1966 with funding from the National Bureau of Standards; the Portsmouth Symposium on Design Methods in Architecture in 1967 that signaled an internal crisis in the rationalist orientation of early design methodology and the embrace of perception, intuition, and experience as integral parts of design — at least on a philosophical and rhetorical level; the American Institute of Architect’s (AIA) Research Conference in Gatlinburg, Tennessee, in October 1967 on how to keep architectural education up to date with new techniques and technologies necessary for the betterment of professional practice; and finally the Yale Conference on Computer Graphics in Architecture and Design, organized by Murray Milne in April 1968, to foster better understanding of the architect-graphic console-computer relationship and explore how computer graphics can move beyond producing “super draftsmen” to effecting a qualitative change in the design process. The DMG conference was also in conversation with articles and special issues on design and computer technologies that appeared in Design Quarterly, Progressive Architecture, Architectural and Engineering News and others.

1022 The 1968 DMG conference included 30 papers divided in 10 sections, refereed by a committee of DMG Newsletter editorial advisory board members. The committee was led by Newsletter editor Gary Moore and board chairman Marvin Manheim. A second committee comprised of Stuart Silverstone, Marvin Manheim, Donlyn Lyndon, and Allen Bernholtz was responsible for local arrangements.


1024 As soon-to-be leading figure of British design research and figure in the current story Nigel Cross reported in his retrospective on design methods: “despite the apparent change of evidence of the success of systematic design, conferences on essentially that topic continued to flourish; expect now the conferences were almost exclusively concerned with architecture or environmental design and held in the United States” (Cross, The Automated Architect, 16).

1025 The conference was sponsored by the School of Architecture of the Boston Architectural Center (whose first conference was the landmark event “Architecture and the Computer,” organized in 1964 by Serge Chermayeff), the MIT Departments of Architecture and Civil Engineering, the MIT USL, and the Department of Architecture and Laboratory for Computer Graphics of the Harvard Graduate School of Design.
by esteemed organizations, widely attended, and ardently reported. In a conference report published in the July 1968 issue of *Architectural Record* architect and urban designer Jonathan Barnett characterized the event as a long overdue consolidation of design methods specific to architecture:

Researchers interested in architectural design methodology began meeting around the fringes of conferences devoted to engineering research and computer technology. Now, at last, this subject has attained the status of a conference of its own. It was held at M.I.T. In early June, under the auspices of the Design Methods Group, a low-key organization that seems to include a high proportion of the researchers doing this kind of work. The conference [...] provided encouraging evidence that design methodology is becoming both more sophisticated and more practical.

The conference included thirty papers divided in ten sections. The section topics reflected major research areas in design methodology and computer research ranging from “Identification of Problem Structure” (mainly correctives of Alexander’s HIDECS method) to “Building Layout Models” (concerned with automatic layout generation on the basis of constraints) and to “Major Comprehensive Approaches to Design and Planning” (attempting to describe general problem solving processes, or theories of design). Among the participants were seminal figures in British design methods such as Jones, who spoke of a new design program that he set up in the University of Manchester Institute of Science and Technology (UMIST), and Bruce Archer, who

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1026 The audience of the 1968 DMG conference was reported as attracting more than 200 participants, from the U.S., Canada, Great Britain, Argentina, Australia. Among the audience were computer scientists, designers, and psychologists.

1027 The conference papers appeared in several venues, such as *General Systems Yearbook, Transaction, Architectural Science Review*, including a Special Issue of the *DMG Newsletter* which featured 25 of the papers. The full proceedings of the 1968 DMG conference, edited by Gary Moore, were published by the in 1970 with the title *Emerging Methods in Environmental Design and Planning*. The book gained Design Award from the Type Directors Club of New York and was translated into Japanese and partially in French. The book received several reviews commenting on the status of architectural science or the role of computer aids in design, to which Moore responded in introducing the 1973 Paperback Edition. Among the reviewers (and absent from the 1968 conference) was Lionel March, writing around the time that he assumed the directorship of the LUBFS Center. March synopsized the conference as indicating three foci in the state-of-the art of design research: the psychology of the design act, the structure of design problems, and the application of scientific and technological developments to design.

was working toward defining a new discipline of design through educational experiments in the Royal College of Art (RCA). DMG founding members and Design and Planning ’66 participants were also there. Alexander and Poyner’s “Atoms of Environmental Structure” appeared in the conference proceedings, but Alexander did not participate in the conference. 1029

In his Architectural Record report, Barnett reiterated the distinction between “glass box” and “black box” approaches to design that Jones had invoked in the 1967 Portsmouth Symposium in efforts to classify different attitudes in the design research and methods community. The “glass box” approaches were concerned with an analytico-mathematical rendition of the design process and its redesign, in the fashion of early Alexander. Historically, they were linked with deliberations that instigated design methods research: to bring design out in the open and study it in a systematic manner. “Black box” approaches, mainly associated by Barnett with computer graphics research, accepted the indescribability of the designer’s operations. They strove to enhance partial tasks that designers faced in a traditional process with the aid of new graphical and interactive technologies.

This apparent rift did not seem the least disconcerting to Barnett who saw in it a “formidable combination” of “computer technology applied to design problems in a subtle way, plus more rigorous methods of using the intricate design abilities of the human mind” 1030 and advised architects to “keep an eye on the Design Methods Group.” 1031 If nothing else, Barnett explained, the DMG conference indicated the existence of “a significant group of people, many of them still

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1029 In his review, Barnett explained Alexander's absence from the 1968 DMG conference, as indicating “his dissatisfaction with some aspects of his earlier work” and commented on the staying power of his ideas: “At the same time, these early ideas cannot be put aside simply because they may be criticized and modified” (Ibid., 128).
1030 Ibid.
1031 Ibid.
in their 20’s, who possess a sufficient knowledge of both architecture and of the mathematics needed to work with computers that they can deal intelligently with either one.”

Despite Barnett’s hopes for productive synthesis between the “glass box” and “black box” approaches, the pragmatics of computer technology dominated U.S. design methodological activity. This provided fertile ground for Negroponte and the ArcMac who not only intersected with DMG’s events in multiple occasions, but also used architectural and environmental research debates that emerged in these events as context for computer research. Negroponte participated in the 1968 DMG Conference with a computer aided design project out of whose “serious scrutinzation” grew the Architecture Machine: URBAN5. Presented at the conference along with MIT Architecture faculty member Leon Groisser, URBAN5 was part of a series of computer aids for urban design (the URBAN series), developed at the IBM Cambridge Scientific Center. Negroponte described the system as “focusing on the direct design process usually associated with yellow tracing paper,” thus soliciting Barnett’s endorsement as the DMG conference’s “most spectacular example of blackboxmanship.”

URBAN5 was a computer graphics system intended to “study the desirability and feasibility of providing a man-machine conversation about a specific environmental design problem.” Such conversation was visual (URBAN5 worked with ten-by-ten foot cubes — “the simplest

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1032 Ibid.
1033 Negroponte and Groisser, Computer Aids to Participatory Architecture, 60.
1037 Barnett, “Glass Box and Black Box,” 127.
1038 Ibid.
geometry known”\textsuperscript{1039} – that the designer could move on the IBM 2250 graphic display unit using a light pen) and linguistic (the user could make assignments of material, environmental, or functional attributes to the cubes by selecting from pre-existing, user- expandable,\textsuperscript{1040} menus and perform operations)\textsuperscript{1041} [Figure 93]. The cubes were selected for their “few architectural or urban design impositions and [...] many research conveniences”\textsuperscript{1042}, creating what Negroponte famously called a “frictionless vacuum” of abstraction.\textsuperscript{1043} Despite having volume the cubes were essentially discrete points corresponding to the end of the tip of the light pen. The computations proceeded on the basis of labels assigned to these points and computations on the basis of their relations. Conceptually, URBAN5 represented design operations as manipulations of graphs (points and relations). Technically, it also deployed graph theory methods already pervasive in automatic activity location or automatic layout generation to “monitor”\textsuperscript{1044} the designer’s operations.

In terms of developments in CAD research URBAN5 was merely a collage of existing techniques, or as Negroponte later characterized it “a barrage of special-purpose (little) architecture machines.”\textsuperscript{1045} The system’s purported originality lay in its conversational interface

\textsuperscript{1039} Negroponte, “URBAN 5, An On-Line Urban Design Partner,” 289.
\textsuperscript{1040} URBAN5 featured six SYMbol buttons, each holding sixteen symbols that described activities, functions, or formal qualifications. The user populated each SYM button with existing symbols or defined new ones, by typing a word and relating it to one of sixteen predefined generic categories (for example, daily, commercial, education, service, private etcetera). URBAN5 could accommodate up to 64 symbols assigned to cube units, with each cube accepting only one assignment (Ibid., 291). In the 1968 DMG Conference, Negroponte presented such expandability as a first step toward personalization: “In just these examples of word-building, the designer is beginning to construct his own machine partner out of the aboriginal framework of URBAN5. This transformation occurs in the machine: the user is allowed to penetrate the surface of URBAN5, getting deeper and deeper into its assumptions and definitions” (Negroponte and Groisser, “URBAN5: A Machine that Discusses Urban Design,” 114).
\textsuperscript{1041} “OPERate,” “QUALIFY,” and “ASSIGN” were the buttons that put URBAN5 in operational mode. Each needed to be specified with a second button push that described the particular interest area, for example SURFACE, CIRculation, ENVIRonment, ACTivities, SITE, USER — the latter describing a neighborhood’s population by density, income, ethnicity, sex, age etcetera.
\textsuperscript{1042} Ibid.
\textsuperscript{1043} Ibid., 107.
\textsuperscript{1044} Negroponte, “URBAN 5, An On-Line Urban Design Partner,” 289.
and the attitude toward computer aided design that it promoted. The system was different from such already existing CAD systems in that it didn’t “optimize” for cost efficiency or “generate” solutions for easiness and speed. While the designer was moving cubes around, URBAN5 was taking the role of “an urban design clerk,” keeping an eye out for “conflicts” (when the physical form produced did not satisfy a designer-specified criterion) and “incompatibilities” (objective criteria that a building type should meet). Negroponte marketed the system as “an objective mirror of the user’s design criteria and form decisions.” By overlooking processes of environmental design and comparing the designer’s decisions with “a larger information base than the user’s personal experience,” URBAN5 aspired to installing a more “responsible architecture” by enabling the designer to consider “more than one or two salient relationships.”

Perhaps precisely because of the system’s “blackboxmanship,” not only as an attitude toward design but also as a decision to not disclose information about the “special purpose little architecture machines” that generated the warnings, the audience of the DMG Conference focused on URBAN5’s interfacing between users and machine. This was in agreement with Negroponte, who viewed “the form of the interaction […] as important as the substance.”

Computer artist, Bell Labs researcher, and Waterloo Conference participant Michael Noll, who was a respondent for the “Computer-Aided Design” section of the DMG Conference,

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1047 In URBAN5’s INITIALIZE mode, the user could specify up to one hundred general criteria (goals that the designer strove to achieve) in natural language. URBAN5 analyzed the sentences with a one hundred-word dictionary specific to urban design. (Negroponte, “URBAN 5, An On-Line Urban Design Partner,” 290).


1049 Ibid.

1050 Ibid.

1051 Negroponte and Groisser, *Computer Aids to Participatory Architecture*, 58.
characterized URBAN5 as “extremely sophisticated” touching upon “the truly creative aspects of urban design” and “probably represent[ing] the prime direction of future research into computer-aided design”.

Noll was captivated by Negroponte’s suggestive discussion of mutual learning and evolutionary personalization between the human designer and a “self-teaching system” such as URBAN5. The prospect of such personalization tapped into another theme that emerged in the discussions and workshop following the DMG Conference’s Computer-Aided Design section: the possibility to develop computer aids not for professional architects but to give “those affected by city planning and architectural environments” “a stronger role and perhaps even the opportunity to do some of the designing themselves.”

Although computer aided participatory design was only tentatively discussed at the DMG Conference, Negroponte swiftly embarked on the idea and embraced it as one of ArcMac’s inaugural pursuits.

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1053 Ibid.
1054 Ibid.
1055 Ibid.
1056 “Learning” from, and adapting to, the each individual user through “growth” and “self-improvement” was URBAN5’s distinguishing element from its predecessor URBAN2. Negroponte URBAN2 to be “a rigid system” developed upon the misguided belief “that all the embedded assumptions about the design process, were true (because many designers agreed), were fixed (because computer programs are that way, so we thought) and were universal (because that would be nice) (Negroponte, The Architecture Machine, 95). Upon encounter with a user, URBAN5 stored a new personal version of the system on magnetic tape, which it restored every time the user’s name was entered in the system. “Inklings of evolution” or “pseudoevolution,” as Negroponte and Groisser characterized it at the DMG conference, occurred by URBAN5 keeping track of which routines the user deployed the most and letting unused routines “self-destruct.” In this way, a personalized version of URBAN5 grew out of the “virgin system” (Negroponte and Groisser, “URBAN5: A Machine that Discusses Urban Design,” 114). URBAN5 also adapted to the user’s work rhythm by “timing” the evaluation of criteria according to the user’s decision making speed. “In theory” Negroponte and Groisser speculated, “after some time the designer’s system would bear little semblance to the original URBAN5. The authors of URBAN5 might not recognize the transformed version.” (Ibid.)
1057 Ibid., 113.
1059 Ibid.
'Soft' as Computational Paradigm

In the late 1960s, participatory design was becoming a matter of mounting concern, motivated by political and social fermentations and growing disbelief toward a fraught design professionalism. The rise of the Civil Rights Movement and the 1968 Fair Housing Act on a national level, as well as MIT-specific events such as the establishment of the Black Student Union, surging student activism, urban poverty and community housing studies by faculty members and visitors such as Robert Goodman, John Turner, and Giancarlo di Carlo, and the release of Stanford Anderson’s seminal *Planning for Diversity and Choice*, paint some of the context in which Negroponte’s attitude toward computer aids shifted toward ideas of participation. Negroponte later described this shift as a turn from “emulating [architects] in computers” in order to produce a “super architect, a surrogate architect” to “doing away with architects (in housing)” and developing “computer aided instruction system and mediator for the goals of the group and the desires of the individual.” The possibilities of such a system were already latent in URBAN5.

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1060 Edited by MIT historian Stanford Anderson and sponsored by The Graham Foundation, the American Institute of Architects-Princeton Educational Research Project, and the Department of Architecture, MIT, *Planning for Diversity and Choice: Possible Futures and Their Relations to the Man-Controlled Environment* (1968, Cambridge, Mass.: MIT Press) was documentation of a conference held at Endicott House, Dedham, Massachusetts, October 13-16, 1966 with the title *Inventing the Future Environment*. Futurology, along with debates on ethics, power, and professional responsibility on the one hand and modeling, prediction and anticipation on the other, was a growing topic of concern among designers in the late 1960s. This was evidenced by several committees and research projects on the topic mainly in the social sciences that had an impact on architectural and environmental design research. Such examples were the Ford Foundation funded *Futuribles* project initiated under the directorship of Bertrand de Jouvenel in 1963; The *Committee for the Next Thirty Years* directed by Michael Young and Mark Abrams, as part of the English Social Science Research Council, the Commission on the Year 2000 of the American Academy of Arts and Sciences, initiated in 1965 with Daniel Bell as director. Bell was author of the impactful critical review “Twelve Models of Prediction— A Preliminary Sorting of Approaches in the Social Sciences” (*Daedalus* 93 (3): 845–80). For a more complete list of initiatives organized around the category of the “future” see Huber, Bettina J. 1971. “Studies of the Future: A Selected and Annotated Bibliography.” In *Sociology of the Future: Theory, Cases and Annotated Bibliography*, edited by Wendell Bell and James Wau, 339–469. New York, N.Y. : Russell Sage Foundation.


1062 Ibid.

1063 Ibid.

1064 Ibid.
Although the system required its user to be an “architect, a good architect,” it advanced principles of personalization and evolutionary learning that Negroponte recast for computer aided participatory design purposes. URBAN5 was designed to be a “self-teaching system […] nursing the user deeper and deeper into the system, first teaching him, then learning from him, and eventually carrying a dialogue with him.” Negroponte put these principles to work in order “make machines sensitive to and understanding of the individual needs of a person designing his own home, presumably in a high density, conceivably in a low income, physical and cultural setting.” This line of research also built on “a series of experiments in linguistics, self-organizing controllers, and machine vision” that took place in summer 1968 under Ford Foundation and USL funding, followed by further research in machine vision and heuristics in the fall 1968 using the MIT Artificial Intelligence Group’s facilities, and experiments with sensors and effectors. These experiments cultivated the prospect of a machine getting input directly from the physical environment and its inhabitants without the mediation of a specialist, be it a human programmer feeding the computer models of the world or a professional architect providing the machine with descriptions of design requirements.

ArcMac used architectural questions to push new computer developments. Such a stance, in tandem with anti-professional attitudes drawing from participatory design rhetoric, materialized
in two concurrent research projects that formed an umbrella for ArcMac’s diverse research and educational experiments in the first half of the 1970s. The first was “intelligent environments,” a project that ArcMac pursued under the support of the Graham Foundation for Advanced Study in the Fine Arts and Ford Foundation funding through MIT. Intelligent Environments relinquished drawing board (or computer console) design and the descriptions of physical environments it produced. Instead, it speculated on a physical environment embedded with sensors and effectors and capable of computation that could change in real time according to its inhabitants’ behavior. The project continued, and aspired to correct, ArcMac’s 1969-1970 experiments with behavioral modeling and environmental adaptation, exemplified by the famously failed experiment SEEK.\footnote{Some of the text in this paragraph has been adapted from the forthcoming (2017) commentary on SEEK in \textit{Closed Worlds}, edited by Lydia Kallipoliti. New York, N.Y. : Lars Muller/ Storefront Editions.} Shown at the landmark exhibition \textit{Software Information Technology: Its New Meaning for Art}, curated by Jack Burnham, SEEK was a plexiglass-encased blocks world of four hundred eighty metal-finished two-inch cubes, rearranged by an overhead pressure sensing and electromagnetic robotic arm in response to the behavior of a colony of highly active Mongolian gerbils \footnote{Negroponte and Groisser, \textit{Computer Aids to Participatory Architecture}, 140.} [Figure 95]. The Interdata Model 3 appended to the plexiglass display, evaluated discrepancies between its model of the block’s configuration and gerbil-induced displacements. Depending on the displacement’s magnitude, it ruled block dislocations either as accidents or “gerbil desired moves”\footnote{“The Architecture Machine Group, M.I.T. SEEK 1969-70,” In \textit{Software Information Technology Its New Meaning for Art}. New York, N.Y. : Jewish Museum, 23.} and used these moves to devise a probabilistic model for where to place the next block. Despite reportedly exhibiting “inklings of a responsive behavior,”\footnote{Paul Pangaro, November 27, 2006, telephone interview with Molly Steenson, cited in Steenson, “Architectures of Information,” 206.} no fortuitous equilibrium between the gerbils and their environment was achieved. Gerbil casualties,\footnote{Paul Pangaro, November 27, 2006, telephone interview with Molly Steenson, cited in Steenson, “Architectures of Information,” 206.} patent emotional distress,\footnote{Paul Pangaro, November 27, 2006, telephone interview with Molly Steenson, cited in Steenson, “Architectures of Information,” 206.} and technical mishaps, famously lambasted by
exhibition critic Thom Hess, did not discourage Negroponte. Instead, Negroponte technically and rhetorically exploited SEEK to both enlist in the ranks of, and carve a distinctive niche within, concurrent research on “responsive environments.”

In the cycles of architectural and environmental research, the subject sat in the interstices of work on flexible building systems and “form-behavior relationship” studies, a proliferating yet controversial topic in the cycles of Anglo-American design methods. Extrapolating from existing sensor/actuator-enhanced physical environments that monitored and regulated heat, light, or other conditions, responsive systems were conceptualized as action-reaction systems, adapting physical form to “the behavior of statistical groups.” Negroponte used SEEK to argue for the irreducibility of behavior to static models and the necessity of a mutual “learning” and adaptation among inhabitants and their environment. ArcMac’s project on “Intelligent environments” advanced ideas of conversation and evolutionary learning nascent in URBAN5, by enlisting the work of figures such as British cybernetician Gordon Pask or MIT-trained

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1075 Hess used SEEK as a cautionary tale against the negative effects of technology: “Artists who become seriously engaged in technological processes might remember what happened to four charming gerbils.” (Ibid.)


1077 The relationship of form and behavior occupied much of the conversation in the 1967 Portsmouth Symposium, with critical voices being heard against behaviorist attitudes and environmental determinism. “Form-behavior Relationships” was also the theme of a session in the 1968 DMG Conference. Among the session participants were Portsmouth Symposium co-organizer Anthony Ward, British architect and pattern language appraiser Frank Duffy, and DMG charter member Charles Rusch. Rusch continued on the subject with a critical article on attitudes toward responsiveness published in Design Research and Methods. In this article, also invoking Warren Brodey’s ideas of “soft architectures,” Rusch questioned the need for changing, adaptable physical forms as requisites of responsiveness (Rusch, Charles W. 1972. “On Responsive Environments.” DMG-DRS Journal Design Research and Methods 6 (1): 14–16.)


1079 Pask, Gordon. 1969. “Architectural Relevance of Cybernetics.” Architectural Design 39: 494–96. Pask was interested with questions of architecture and design, with his participation in the 1962 Conference on Design Methods — the inaugural event of design methodological activity — being a notable example. Pask was consultant and collaborator for many of ArcMac’s projects in the first half of the 1970s.
cybernetician Warren Brodey, from whose writings Negroponte readily appropriated the monikers “soft architectures” and “intelligent environments.”

Intelligent environments combined these ideas with AI experiments in natural language processing, face recognition, motion tracking, and semantic processing to imagine mutual modeling between two sentient, intentional beings developing an intimate relationship. Intelligent environments remained in the sphere of speculation, mostly because of limitations in embodying “soft” as “computational paradigm” to actual physical building systems, or as Negroponte put it, the “gap between the technology of making things and the science of understanding them.” Nonetheless, the subject served as productive rhetorical vision for ArcMac. It tapped into the vogue of “softness” and ephemerality that permeated the U.S. counterculture and dominated the international “radical” architectural scene in tandem with psychedelic fantasies of environmental enhancement [Figure 95, Figure 96]. Negroponte was ardently pursuing such connections, in efforts to push computer research from the provinces

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1080 Brodey was a Canadian physician who started the Boston Environmental Ecology Laboratory, after serving as psychiatrist at U.S. National Institute of Mental Health (NIMH) and Georgetown University, studied cybernetics at MIT under Warren McCulloch and consulted the Man/Machine Interaction Group at the NASA Electronic Research Center in Boston. In his “Soft Architectures” article, Brodey used the Arturo Rosenblueth, Norbert Wiener, and Julian Bigelow’s 1943 landmark article “Behavior, Purpose and Teleology” (Philosophy of Science 10 (1): 18–24) to speculate on different degrees of responsiveness and evolutionary adaptation in the physical environment, ultimately advocating for a mutual co-evolution of human and computationally enhanced environment. Brodey also discussed the “human enhancing” implications of self-organizing and evolutionary relationship among humans and machines in a 1967 coauthored with Nilo Lindgren (“Human Enhancement through Evolutionary Technology.” IEEE Spectrum 4 (9): 87–97).


1082 Negroponte spoke of such material resistances to environmental responsiveness in Soft Architecture Machines (1975) and his talk at the Basic Questions of Design Theory symposium at Columbia University, titled “Limits to the Embodiment of Basic Design Theories.” In Soft Architecture Machines, he discussed “softs” (inflatables and pneumatic structures) and “cyclics” (continuous cycles of construction and destruction) (Negroponte, Soft Architecture Machines, 150).

1083 Ibid., 5.


of the “military-industrial complex”\textsuperscript{1085} to the front line of architectural speculation.\textsuperscript{1086} Such alliances paralleled Negroponte’s sober exposition of intelligent environments as a challenging, yet plausible, research agenda; a far-reaching goal impelling advances in “learning the user.”\textsuperscript{1087}

\textbf{DRS’71 and the Loops of Design Participation}

Negroponte presented a sketch of an intelligent environments theory\textsuperscript{1088} in the 1971 Design Participation Conference \textbf{[Figure 97]}, the first international conference of the U.K.-based DRS. Initiated in 1966 by the organizing committee of the 1962 Conference on Design Methods\textsuperscript{1089} and other researchers who had embarked the design methodology enterprise in the first half of the 1960s, the DRS was an international organization aiming to “provide facilities for the exchange of new knowledge about the design process in engineering, industrial design, the graphic arts and all other creative disciplines.”\textsuperscript{1090} British design methods instigator J. C. Jones used the Ergonomics Research Society (ERS)\textsuperscript{1091}—an interdisciplinary forum of researchers and professionals sharing common interests—as a model for the DRS, which opened its doors to “all

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\begin{itemize}
\item \textsuperscript{1085} Negroponte, \textit{Soft Architecture Machines}, 99.
\item \textsuperscript{1086} An example of such pursuits was Negroponte’s short essay contribution, alongside Yona Friedman, Kenzo Tange, and Buckminster Fuller on the topic of the “Total City.” The section was first published in \textit{Japan Architect} (no 178, December 1971) and abstracted in the French periodical \textit{2000}. There, Negroponte was presented as one of four major figures (“grands”) in the domain of architecture and urbanism. He spoke of the possibility of intelligent (“sensible”) cities. (Tange, Kenzo, Yona Friedman, Nicholas Negroponte, and R. Buckminster Fuller. 1973. “La Ville Totale.” \textit{2000 [Deuxmille]: Revue de l’Amenagement Du Territoire et Du Developpement Regional}, no. 24: 5–7).
\item \textsuperscript{1087} Negroponte and Groisser, \textit{Computer Aids to Participatory Architecture}, 32.
\item \textsuperscript{1089} The Design Research Society (DRS) was founded in September 1966 by J. K. Page (chairman), J. Christopher Jones (vice-chairman), Frank Height (treasurer) and Peter Slann (secretary), with headquarters in the Imperial College London.
\item \textsuperscript{1090} The DRS’s founding and its initial plans to launch an annual conference and a journal were announced in \textit{Design}, the journal of the British Council of Industrial Design (Page, J.K. 1966. “For Those Interested in Creative Processes.” \textit{Design} 213: 30.)
\item \textsuperscript{1091} Cross, Nigel. 22 September 2016. Interview by Theodora Vardouli.
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those who can make a contribution, regardless of their profession or professional status.”

Representing the British contingent of design methods, the DRS intimately engaged with the more architecturally oriented and pragmatically minded U.S. counterpart, the DMG, with which they eventually published a joint journal and shared conferences. However, while at the end of the 1960s the DMG was harnessing the success of its 1968 Conference, the end of the decade found the DRS in limbo. The philosophical and ethical challenges to the rationalist roots of design methods voiced in the 1967 Portsmouth Symposium, caused a stalling self-scrutiny about identity and theoretical position.

The DRS’s impasse was recognized by Nigel Cross, a lecturer and part-time doctoral researcher at UMIST researching human and machine roles in CAD. Actively involved with BASA during his undergraduate studies at the University of Bath, member of the DRS since 1967, and motivated observer of international developments in CAD, Cross strove to revive the DRS by engaging it with “new topic of participation in design.” Circa 1969, he proposed to host a conference on the topic in the Design Research Laboratory (DRL), a lab that Jones

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DRS grew to be from an interdisciplinary forum to a discipline-building organization. Continuing the lineage of design methods, the DRS construed design as an activity spanning different domains and professional specializations, and eventually forming its own discipline. Examples of this view were the DRS’s second international conference on Design Activity (London, 1973) and the first issue of the DRS’s journal Design Studies, co-edited by Nigel Cross, that featured a series of articles on “Design as a Discipline.” The contributors included Bruce Archer, who in the mid-1970s pursued work on “general education” in design in the RCA, aspiring to communicate to the general population modes of knowledge, thought, and action particular to design. This work was influential for Nigel Cross, who had moved, following Jones, to the Open University (OU) — a distance-learning, “university of the air” institution established in 1969. At the OU, Cross became one of the first faculty members of the 1970-founded “Design Discipline” in the Faculty of Technology.

1093 Following the first DRS conference, the DMG and the DRS issued the joint journal Design Research and Methods, co-edited from 1972 to 1974 by Nigel Cross and Don Grant of the DMG. This was followed by the joint journal Design Methods and Theories, first launched in 1977.

1094 Cross, Interview by Vardouli.

1095 Ibid.

1096 Ibid.

1097 Ibid.

1098 Although computer aided design was not the central activity of the UMIST Design Research Laboratory (DRL), several of its students engaged with computer programming and broader questions about the role of computer systems in the design process. Students learned to program in “Atlas Autocode,” “laboriously typed in (their) little
had set up in UMIST as consolidation of a graduate course in “Industrial Design Technology” that he was running in the UMIST Department of Building since 1961.\textsuperscript{1099} The conference would broadly address the role of design methods and new technologies in enabling “wider sections of society to actively participate in the processes of planning and design.”\textsuperscript{1100} Fostering a general optimism toward the socially transformative effects of technology, a crucial one being the demise of professional powers, Jones was supportive of Cross’s proposal.\textsuperscript{1101}

The Design Participation Conference took place in the University of Manchester in September 1971 as the first International Conference of the DRS. The event was an “experiment in conference design.”\textsuperscript{1102} Through the aid of information technologies, such as the HOST system (standing for “Helping Organize Selective Togetherness”), on demand lecture access, and CCTV relay of the lectures into the lounge, the conference participants were relieved from the programs and submitted the tape reels” in a punch tape machine room above the Department of Building. These were then collected and sent off to the UMIST Atlas computer, from where students then collected their line-printed results (Cross, Interview by Vardouli). Prior to starting the DRL Jones had initiated a Computer Aided Designing Project at UMIST, along with Chris Goodwin (course lecturer in Jones’s graduate program), on the real time, on-line use of time-shared computers in building design. Two research reports on the project appeared in the \textit{DMG Newsletter} 1(8/9). In reminiscing about the DRL, Cross could only recall one student project that centered on “actual computing” work; an automatic floorpan layout program by George Stabler. Cross used this program in his PhD, where he compared the performances of human designers trying to solve layout problems with and without computers. Automatic floor planning, alongside its technique of choice; graph theory, attracted much attention in communities of researchers in design (see for example my discussion of Tabor’s and Steadman’s work in the previous chapter) and in computer aided design.

Jones ran the graduate course on design technology at UMIST with the support of Dennis Harper, known as the first architect to have been appointed Professor of Building in the U.K. and a passionate advocate of the need of scientific and technological foundations in design education (Cross, Interview by Vardouli). In personal conversation Cross described the graduate course as consisting of half a year of coursework, with lab sessions and lectures and half a year of an individual research project, culminating in a thesis. Lectures on design methods, ergonomics, computing, statistics given by Jones, Goodwin, and Reg Talbot. Other UMIST faculty gave lectures on operations research, manufacturing reliability, history of Technology. Lab sessions were “practical work in applying design methods and conducting small ergonomics-type experiments” (Ibid.).


\textsuperscript{1100} In personal conversation Cross spoke about Jones’s optimistic attitude toward the effects of computers and automation: “One of Jones’s themes was the influence of computers and automation on work and everyday life - he assumed this would be mostly good, with impacts such as the weakening of professions, intelligent machines becoming ubiquitous and ‘zero-learning’ devices (i.e. intuitive or user friendly)” Jones presented such views to his graduate students through a lecture series on futures and systems thinking. (Cross, Interview by Vardouli).

burden to “endure long and uninteresting discussions” and allowed to collaborate in workshops, or have impromptu discussions with outside parties. Participants were designers, architects and planners, convening with internationally acclaimed architectural critics, Reyner Banham being a notable example, artists, information technologists, and engineers. Recurrent themes in the conference were a suspicion against technocracy and professionalism, questions of risk, expertise, decision-making legitimacy, and explorations of social and alternative technologies. The conference participants also discussed techniques and methods of participatory design, ranging from informing citizens about design decisions, to engaging them in the decision making process, and to enabling them to shape their own environments.

Entitled “Aspects of Living in an Architecture Machine” Negroponte’s paper comparatively reviewed varying attitudes toward computationally enhanced environments, specifically in relation to how they approach “learning” the inhabitant and “responding” to the inhabitant. The conference was useful for ArcMac’s subsequent expeditions in “intelligent environments” because it helped Negroponte articulate the difference between ArcMac’s work from other early 1970s research in “responsive” and “adaptive” systems. It did so, by providing convenient counterexamples such as Charles Eastman’s “Adaptive Conditional Architecture,” whose unsophisticated thermostat-like action-reaction logics Negroponte

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1103 Ibid.
1104 For a more comprehensive discussion of the Design Participation Conference themes and contributors, see Vardouli, Theodora. 2015. “Who Designs?” In Empowering Users through Design, edited by David Bihanic, 13–41. Springer International Publishing. Some of this paragraph’s text has been adapted from this article.
1106 At the time of the Design Participation Conference, Charles Eastman was associate professor at the School of Urban and Public Affairs, at the Carnegie-Mellon University. Eastman is known not for his contributions to responsive architecture, but as pioneer of Building Information Modeling (BIM).
invoked as registers of a “hitherto unseen complacency.” The Design Participation Conference was also formative for the other project against architectural professionalism that, alongside intelligent environments, marked the first years of ArcMac’s operation. Participatory architecture as the new frontier for CAD had been in ArcMac’s radar since the 1968 DMG conference. A prominent topic also in the Design Participation Conference, computer aided participatory architecture would become the subject of a two year NSF-sponsored project that ArcMac pursued before starting the ABY experiments. Of key rhetorical and technical influence for these research pursuits was the rejuvenation of Negroponte’s relationship with another conference participant and subsequently ArcMac’s collaborator: Yona Friedman.

In the DRS Conference, Friedman performed his moral and philosophical arguments for removing the professional “middleman” from the design process and described the principles of the FLATWRITER machine. Friedman’s widespread recognition and impact in architectural cycles, his sober utopianism, and the radically anti-professional stance that he adopted with respect to participatory design aligned well with ArcMac’s frontier-setting rhetoric. At the same device” elicited information about the inhabitants’ activities through quantitative or qualitative methods, such as sensing, interviews, surveys, and observations. The “if-then” statements defined the environment’s response. The “change mechanism” modified environmental parameters accordingly. Finally, the “control setting feature” enabled users to control the system’s “set point,” to assign a desirable value to the variables that described various aspects of inhabitant-environment relations.

Negroponte, Soft Architecture Machines, 134.

Negroponte first met Friedman in 1964, during a car ride that they shared from Boston Logan Airport. In car conversation with the famed architect, Negroponte—at the time a graduate architecture student at MIT with notable graphic skills, fluency in French, and an “impressionable” demeanor—encountered a resonating architectural “ideology.” As he would recount in forewording the English translation of Pour Une Architecture Scientifique, Negroponte was “turned on” by Friedman’s “soft spoken but persuasive argument” for giving users direct and unmediated control over the built environment because they were the risk bearers of “bad design” (Friedman, Toward A Scientific Architecture, ix). Felicity Scott writes on Friedman’s impact in Negroponte’s 1965 Bachelor’s thesis on Systems of Urban Growth (Scott, Felicity D. 2016. Outlaw Territories: Environments of Insecurity/Architectures of Counterinsurgency. New York, N.Y.: Zone Books, 360).

The car ride also left an impression on Friedman, who ecstatically recognized a real life sample of his “villes ponts” (bridge cities) paper architectures in a branch of an American supermarket franchise built over the Massachusetts Turnpike (Negroponte, Nicholas. 16 March 2012. Interview by Theodora Vardouli).

time, Friedman’s 1960s flirtations with mathematics and computation imbued these theories with a level of plausibility and pragmatism appealing to ArcMac’s sponsors.

*Computers Aids to Participatory Architecture and Graph-ical Conversations*

Before inviting Friedman as a consultant in ArcMac’s ABY project, Negroponte declared alliance to his theories and methods for “architectural do-it-yourselfism”\(^\text{1111}\) — the kind of architectural process that “completely removes the architect and *his* [emphasis in the original] previous experience as intermediaries between my needs (pragmatic, emotional, whimsical, etc.) and my house.”\(^\text{1112}\) In *Soft Architecture Machines*, a retrospective of ArcMac’s first four years of operation,\(^\text{1113}\) Negroponte framed this kind of immediate control over one’s built environment as the most radical form of participatory design and contrasted it with other more timid attempts of de-professionalization presented at the Design Participation Conference.\(^\text{1114}\) The computer implementation of such process was first outlined in a proposal that Negroponte submitted to the NSF in 1971, along with Groisser, with the title “Computer Aids to Participatory Architecture.” The research proposal received a $60500 two-year award (1972-1974) from the NSF’s Institutional Support for Science and Computing Activities and formed preparatory ground for ABY. Sharing common ground with URBAN5 especially as it pertains to human-machine communication, the 1971 proposal described a “straightforward, free-wheeling, and congenial conversation”\(^\text{1115}\) between the “proverbial man-in-the-street, who is both a novice in architecture and a novice with machines”\(^\text{1116}\) and “a machine which has some ‘knowledge’ of architecture

\(^{1112}\) Ibid., 101.
\(^{1113}\) *Soft Architecture Machines* was written in 1972 but published by the MIT Press in 1975 because of production related delays. (Ibid., Author’s note (n.p.)).
\(^{1114}\) Negroponte, *Soft Architecture Machines*, 100.
\(^{1115}\) Negroponte and Groisser, *Computer Aids to Participatory Architecture*, i.
\(^{1116}\) Ibid.
not only appear[ing] to be a ‘competent’ architect, but […] a sympathetic conversant, a good model builder, graphically dextrous, and friendly.”\textsuperscript{1117} Such machine would provide “the necessary design tools” to enable “each man being his own architect or […] each man at least participating in the design of his immediate built environment.”\textsuperscript{1118}

Negroponte and Groisser conceptualized the “dweller’s plot of land” as a “three-dimensional chunk of space […] lodged within a framework of high density.”\textsuperscript{1119} Depicted in the proposal with a drawing of wireframe parallelepipeds superimposed with graphs connecting their centers of gravity, this abstraction was reminiscent of both of URBAN5’s “frictionless vacuum”\textsuperscript{1120} of cubes and Friedman’s renditions of the Ville Spatiale (an infrastructure consisting of cube-shaped spatial units). Friedman’s ideas were also operative in the phasing of the research proposal, that Negroponte and Groisser presented as taking place in two rounds one “concerned primarily with the design of (the dweller’s share of the chunk of space)”\textsuperscript{1121} and one addressing “the problem of control and mediation of the individual's needs and desires with respect to his neighborhood.”\textsuperscript{1122}

This formulation shares affinities with the FLATWRITER’s two loops of participation—one between the inhabitant and the machine and one between the inhabitant and the “others,” mediated by the machine. The “first loop” was mostly about what Negroponte and Groisser, echoing American economist and computer scientist Herbert Simon, called “criteria”: subjective and qualitative “goals to be maximized or minimized”\textsuperscript{1123} The second loop, deferred to the

\textsuperscript{1117} Ibid.
\textsuperscript{1118} Ibid.
\textsuperscript{1119} Ibid., 2.
\textsuperscript{1121} Negroponte and Groisser, Computer Aids to Participatory Architecture, 2.
\textsuperscript{1122} Ibid.
\textsuperscript{1123} Ibid.
research project’s third year and therefore unfunded, was about “constraints”: objective and quantitative “bounds and magnitudes of certain variables.”\(^{1124}\)

Unlike URBAN5, which evaluated designs on the basis of constraints and criteria, the proposed computer aided participatory design system also generated designs. To do so, it deployed automatic floor and space planning methods that pervaded CAD research in the turn of the 1970s, with the MIT IMAGE system being a state of the art example. Like URBAN5, Negroponte and Groisser’s proposed participatory design system rehashed such existing CAD methods under a new type of human-machine interface. The system built on “the extensive constraint and criteria resolution work in IMAGE and from the exhaustive data structure work of DISCOURSE,”\(^{1125}\) but aspired to a personalization and congeniality that vexed the two systems. Such fluidity was in keeping with the “thrusts of the Architecture Machine Group”:\(^{1126}\) “interfacing with a user and dealing directly with the real world and the procedures for handling problems of context and of missing information.”\(^{1127}\) Work on inference making and user modeling aimed to appeal to the prospective computer research sponsors of the project who were invited to support “developments in man-machine interaction [that] may be applicable to other disciplines”\(^{1128}\) despite the fact that “the application is ‘participatory architecture’ and while the hardware and software cater to the context of built form.”\(^{1129}\) Typical automatic floor and space planning systems took as input a well defined “architectural program” [quotes in the original]\(^{1130}\) consisting of constraints and criteria and outputted a (sub)optimum layout or a number of

\(^{1124}\) Ibid., 22.
\(^{1125}\) Ibid., 73.
\(^{1126}\) Ibid., 72.
\(^{1127}\) Ibid.
\(^{1128}\) Ibid., 21.
\(^{1129}\) Ibid.
\(^{1130}\) Amidst the anti-professional discourse that Negroponte and Groisser adopted in their NSF proposal for computer aided participatory architecture, lay pragmatic tidbits on the proposed computer systems’ professional usefulness; in this case as a “buffer” for the practicing architect, helping clients understand their own architectural program. (Ibid., 3).
possible layouts satisfying the program requirements. Negroponte and Groisser’s program was different in that it aspired to infer “stated or unstated criteria”\textsuperscript{1131} from the user’s design acts. The conceptual shift was one from “specification” to “recognition”\textsuperscript{1132} [Figure 98]. Such recognition required an intelligent machine that could go beyond the information provided and reason about its meaning on the basis of the information’s context – in this case, an evolving model of the system’s user.

The system elicited information about the user’s criteria was by “observing the relationships [emphasis mine] that the user establishes graphically and linguistically.”\textsuperscript{1133} Graphical relationships were picked up through sketch recognition, a subject that was occupying much of ArcMac’s research activity at the time of the 1971 NSF proposal. In the computer graphics community assembled around MIT’s CAD Project, sketching stood for a fluid, dynamic interaction between user and machine. A register of ambiguity and typically associated with the early stages of the design process, sketching denoted the tentative and the changing. In the context of these associations, sketching formed a slogan and impetus for new developments both in graphical interfaces (light pens, tablets, point-digitizers, and other devices) that would make drawing in a computer feel and look more like sketching, but also in computer graphics and machine vision that would enable computers to “read” and handle their user’s sketches. In 1971, ArcMac developed a sketch recognition system named HUNCH\textsuperscript{1134} that syntactically

\textsuperscript{1131} Ibid., 23.
\textsuperscript{1132} Negroponte, \textit{Soft Architecture Machines}, 119.
\textsuperscript{1133} The shift from specification to recognition in the context of computer aided participatory architecture also boosted ArcMac’s research in topics such as sketch recognition, inference making, and personalized systems.
\textsuperscript{1134} Notably, HUNCH was developed as part of MIT’s Project MAC and was the only ArcMac project taking advantage of MULTICS. Subsequent work on computer aided design and sketch recognition in the ArcMac was also sponsored by the Office of Naval Research (Contract Number N0014-67-A-0204-0074, Context Definition through Idiosyncratic Systems).
processed sketches hand-drawn on a drawing station [Figure 99]. Negroponte contrasted HUNCH with the “rubber-band pointing-and-tracking vernacular” promoted by systems such as SKETCHPAD, which “polluted the notion of ‘sketching’, in any sense of the word.” Opposite to SKETCHPAD-like systems, developed to distill the structures and geometric forms behind the drawing’s “dirty marks” and “wobbly lines,” “HUNCH,” wrote Negroponte, “takes in every nick and bump, storing a voluminous history of your tracings on both magnetic tape and storage tube.” “The wobbliness of lines, the collections of over tracings, and the darkness of inscriptions, formed the basis on which the system made inferences about the drawing’s geometric meaning, the designer’s intentions, and even preferences in architectural style.

In their 1971 NSF proposal, Negroponte and Groisser proposed the use of SQUINT— an offspring of HUNCH—that could recognize boundaries and interpenetrations of shapes. In doing so, SQUINT could elicit “positional” (direction and orientation) and “proximity” (adjacencies, connections, overlaps) relationships implicit in a hand-drawn sketch of a house plan made by the system’s architectural novice user [Figure 100]. Interpreted as unstated criteria, such relationships were topological and non-metric. They had to do with “the absolute arrangements of elements that the user […] sketched.” Despite “draw[ing] very badly” the architectural novice was “extremely adept at describing physical relations and juxtapositions”. SQUINT extracted adjacencies and linkages and represented them as graphs.

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1136 The ArcMac performed its sketch recognition experiments in a graphic station comprising a Sylvania data tablet, a storage tube, and a minicomputer. In the mid 1970s, Negroponte was reporting on three such drawing stations in the ArcMac premises.
1138 Ibid., 64.
1139 Ibid., 65.
1140 Ibid.
1142 Ibid.
Graphs provided “an initial, though crude, overview of the user’s criteria,”\textsuperscript{1144} while ensuring congruence with the generated designs. As Negroponte and Groisser wrote parenthetically, “in the reverse problem of solution generation, these two kinds of criteria (positional and proximity) provide the bulk of the variables.”\textsuperscript{1145} After eliciting an “architectural program” and representing it in the form of a graph, the system continued to generate alternative layouts in the fashion of the graph theory based generator programs proliferating in computer aided design.\textsuperscript{1146}

The system did not output an optimum or sub-optimum design, nor a full list of alternative options as in Friedman’s or Steadman’s methods. Instead, the user and the machine engaged in cyclical interactions where the machine “generat[ed] alternatives and encourage[ed] the user to alter arrangements” while “storing and retrieving ‘apparently satisfactory states’.”\textsuperscript{1147} The goal was for the user and the machine to “together […] move toward more appropriate alternatives.”\textsuperscript{1148} Through these interactions the system was “build[ing] a model of the user’s new or modified habitat” while “simultaneously building a model of the user and a model of the user’s model of it.”\textsuperscript{1149} Such a computer system was speculated to need three “significantly different kinds of data structures […] for the graphics, for the design itself, and for the models of the user.”\textsuperscript{1150} A structure of discrete geometric elements described in terms of position and size was overlaid on graphs representing adjacencies and linkages extracted from the architectural novice’s sketches. The system’s user was simultaneously being modeled through “a probabilistic

\textsuperscript{1144} Negroponte and Groisser, Computer Aids to Participatory Architecture, 28.
\textsuperscript{1145} Ibid.
\textsuperscript{1146} Negroponte and Groisser did not provide details on the layout generation algorithms. In Soft Architecture Machines, Negroponte referenced Grason’s work that we previously saw references by Steadman. For a discussion of automatic layout generation see section on Steadman in chapter 4.
\textsuperscript{1147} Ibid., 19.
\textsuperscript{1148} Ibid.
\textsuperscript{1149} Ibid., 7.
\textsuperscript{1150} These different layers of models were outlined in Gordon Pask’s “Conversation Theory.” Pask was a consultant and collaborator in ArcMac’s Computer Aids to Participatory Architecture research project.
\textsuperscript{1150} Negroponte and Groisser, Computer Aids to Participatory Architecture, 50.
chaining of functions and procedures, situations and responses.”¹¹⁵¹ This was in keeping with the
probabilistic models of the failed SEEK experiment. These data structures were “all terribly
entangled with all the other data structures at every level”¹¹⁵² — an entanglement vital for
achieving “true responsiveness” and “good knowledge” of the user.¹¹⁵³ The result of this process
was not only to produce physical form and express it graphically but also self-awareness
(“discover, understand and express his own needs and desires,” “to understand more fully his
own patterns of living and how they affect and are affected by the physical environment in which
he lives”[underlined in the original]).¹¹⁵⁴

Such understanding did not grow out of the conscious self-tracking of daily habits as
Friedman imagined in the FLATWRITER, but in conversational immersion with a machine that
elicited intentions from the user’s sketches and outputted corresponding architectural proposals.
Negroponte construed sketching as a stepwise convergence between design intentions and
graphical articulation. Although he acknowledged that in the case of architectural design
intentions were “continually changing” as a result of “the user’s viewing his own graphical
statements,”¹¹⁵⁵ his proposals suggested that computers could keep track of, and reason on the
basis of, the history of such changes. Such recording occurred by distilling the structures
underlying the user’s sketches and parsing them in all in the common mathematical language of

¹¹⁵¹ Ibid.
¹¹⁵² Ibid.
¹¹⁵³ A crucial aspect of user modeling was “backtracing” errors to their causes, not only to take remedial action but
also to benefit from them by improving the heuristics (rules of thumb) that the system employed in order to lower
the probability of a similar error. In their NSF proposal, Negroponte and Groisser remarked on the challenges of
such procedure: even the most advanced self-corrective “criticizer” programs developed in AI research usually
related errors to the way the data was examined and “do very little to change the structure of its programs” (Ibid.,
49).
¹¹⁵⁴ Ibid., 9-10.
¹¹⁵⁵ Ibid.
Negroponte later articulated sketching as a “a computational paradigm for personalized searching” (Negroponte,
22–29.)
the graph. The translation of sketches to graphs appeared natural to Negroponte, who had absorbed computer graphics basal claim that sketches and drawings actually exhibited structures.

By isomorphically mapping the user sketches’ alleged structure to graphs, Negroponte strove for an undistorted translation between the user’s graphical statements and mathematical structures with which the machine could compute. The machine’s computations, in turn, built on on the pervasive idea that an “architectural programme” can be isomorphically mapped onto a topological description of a plan, which can be used to generate layout possibilities satisfying the programme. Ultimately, ArcMac’s computer aided participatory architecture system purported to map between graphical descriptions (user-generated sketches), structures of intentions (architectural programmes that the machine elicited from users’ sketches), and structures of physical space (computer generated floor layouts). The graph as a common representation of all three domains was paramount in supporting claims of conversational fluidity between user and computer. These isomorphic mappings were also in keeping with the imperative of “non-paternalistic guidance”\textsuperscript{1156} — a requisite of ArcMac’s architectural “do-it-yourselfism”\textsuperscript{1157} that drew from Friedman’s articulations of participatory architecture. Although by hiding the computer’s workings from its users ArcMac’s conception of computer aided participatory architecture continued to champion “blackboxmanship,” graph-enabled isomorphisms ostensibly ensured an operational transparency. The black box was to be trusted; it was your own native architect.

\textsuperscript{1156} Weinzapfel and Negroponte, “Architecture-By-Yourself,” 75.
\textsuperscript{1157} Negroponte, \textit{Soft Architecture Machines}, 101.
5.4. Seeing Graphs

Despite Negroponte’s long-standing efforts to cultivate trust in the computer as a non-distortive mirror of its user, Friedman remained unconvinced. In ArcMac’s pursuit of the “general thesis […] that each individual can be his own architect [emphasis in the original]” through a “very personal computing machine” Friedman still saw traces of the “paternalist scheme” that he condemned in his theories. By learning the “future user’s” “peculiar particularities” and acting on the user’s behalf, even with “paternal benevolence,” the computer bastardized the immediacy of unmediated do-it-yourself architectural design for which Friedman was advocating.

[Figure 101]. In introducing the “Computer-Aided Participatory Design” section of Negroponte’s Soft Architecture Machines, Friedman presented his concerns as stemming not only from a “personal moral attitude” but also “principally” from the pragmatic reason that “the learning about the personality of the future user is less implementable [emphasis mine] than the learning about structural characteristics of the real world.” By “real world” Friedman meant empirically observable aspects of the built environment and its inhabitation (such as physical space and human behavior) that were out in the open as opposed to hidden in a human subject’s mind or psyche. The “implementability” of learning about the “real world,” he explained, was not because “it is less complex” than human personalities, but because “it is —by definition— more ‘structurable’.”

1158 Negroponte, Soft Architecture Machines, 100.
1159 Ibid.
1161 Ibid., 95-96.
1162 Ibid., 96.
1163 Ibid.
1164 Ibid.
1165 Ibid.
1166 Ibid.
As I have shown in this chapter, Friedman’s claim about the real world’s structurability was less of a philosophical argument and more of a truism validated by his own personal experiences. In the architectural work that he pursued individually and as part of experimental architectural groups, space frames turned physical space into structurable material. In French postwar urbanism, a structural language pervaded for building communication bridges among the human sciences and the architecture, but also for matching the habits (behavior) of occupants with configurations of physical space. At the architectural scale, prefabrication further enforced processes of structuring (combining) discrete building components. In the academic environments that Friedman frequented, structuralism was in good currency. Ambitious mathematicians exploited this climate, to promote methods that made these structural ideas workable. In pledging faith to the “structurability” of the “real world,” Friedman was documenting his immersion in a world of material, physical structures and abstract structures, made tangible through mathematical representations. Hence, his mathematizing work is an illustrative case of what I have been referring to as structural realism: the idea that the world consists of structures that can be mapped and manipulated.

Although the mandate to implement theoretical ideas on a digital computer made ArcMac researchers more self-conscious and pragmatic in their choice of mathematical technique, the agendas for which they enlisted the graph also relied on, and exhibit assumptions of, structural realism. ArcMac’s belief in the “structurability” of human idiosyncrasies, which Friedman dismissed as morally suspect and “less implementable,” stemmed from ArcMac’s intimate involvement with the growing field of Artificial Intelligence. AI researchers promoted structural renditions of human utterances (linguistic, bodily, or graphical), which they variously used to derive insights (inferences) on cognitive or intentional characteristics of a human subject. The

\^{1167} Ibid.
genres of cybernetics with which ArcMac engaged (for example Brodey’s or Pask’s work), also imagined communication among humans and machines as a construction and matching of structures – as a dance of isomorphisms.

In my discussion of ArcMac’s work, structural realism operated not only in the sense of visual and intellectual associations, like in Friedman, but also in the sense of computer-related pragmatism. In computer graphics research, drawings were being approached as structured entities. Representing them in a computer entailed breaking them down to discrete units and defining relationships among these units. In early computer graphics work, the lines of an architectural drawing were represented them either as arrays of points or sets of line vectors. In the late 1960s dual graphs were popularized as a new representation of architectural drawings (plans, specifically) in computer aided architectural design (CAAD). Dual graphs were seen as having the added advantage of being able to represent both the drawing’s geometric characteristics and its non-metric topology thus bridging geometric shape with the realm of “design objectives” (i.e. architectural programme). ArcMac’s participatory design proposals relied on the dual graph to translate between descriptions of a floor plan and the internal intentions of a designer. ArcMac posited that sketches could be represented as graphs from which the computer could elicit an “architectural programme,” also represented as a graph. Using the graph’s dual, the computer then generated a description of a floor plan that satisfied this programme. The user looked at the computer-generated plan and “responded” through another sketch. The process continued with the computer taking the dual of the user’s sketched response as a new architectural programme, comparing it with the initial programme, drawing inferences about the user based on the differences, using these inferences to generate another floor plan, and

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1169 Ibid.
so on. The process was repeated until there was convergence between the computer’s floor plan suggestion and the user’s response.

Friedman’s theories of architectural “do-it-yourselfism,” as ArcMac described his work, offered ArcMac the opportunity to position new CAAD techniques such as dual graph representations under an architectural rhetoric of participation and emancipation. Such a positioning aligned with Negroponte’s efforts to position computer research as a kind of avant-garde architectural practice. Friedman’s graph-theoretic architectural method, as it was presented in *Pour Une Architecture Scientifique*, absorbed techniques of floor layout generation that had been developed in rational design methods endeavors. Friedman removed these techniques from the rational methods context and repurposed them for agendas that resonated with his propositions of mobile architecture and user agency in the built environment. In this act of appropriation, he changed their symbolic meaning. Friedman took the pragmatic methods of automatic floor plan generation and elevated them to an axiomatic theory of a transparent architectural science; he linked evaluation methods of circulation efficiency with the moral imperative of “warning” the user; he recast the combinatorial generation of layouts as a register of open-ended possibility. More crucially however, and despite his iconoclastic statements, he strongly advocated for the graph’s intuitiveness and accessibility and imbued it with humanizing and democratizing meanings.

An offspring of ArcMac’s collaboration with Friedman, the YONA system brought the graph on to the computer screen. Instead of mapping a sketch, the graph was presented as the sketch — a malleable, visual entity, accessible well outside the cycles of a mathematical elite. The presentation of the graph as sketch highlights values of intuition and congeniality attached to the graph. These values have been a recurrent theme in this story: from Tabor’s acclaim of the
graph’s “visual digestibility,“\textsuperscript{1170} “intuitive comprehension,“\textsuperscript{1171} and “lucid, flexible and suggestive depiction,“\textsuperscript{1172} to Friedman’s embracing of the graph as an “accessible language […] for the uninitiated“\textsuperscript{1173}, to Harary’s surveying of “prettiest,” “ugliest,” “funniest,” “sexiest”\textsuperscript{1174} trees, and to Weinzapfel’s commendation of the graph’s concrete “tangibility.”\textsuperscript{1175} By featuring the graph on the screen, the YONA system revealed its internal mathematical representation to its users. This revealing of the computer’s interior to the eye did not only abide to Friedman’s moral mandate of transparency, but also implicitly declared the graph as an interface humans and machines: as an entity that spoke the language of both. An abstract mathematical object with tangibly concrete visual depictions, the graph fostered optimism that human engagement with computers need not be seen in terms of dehumanizing quantification and mechanizing systematization.

YONA’s understanding of the graph as a natural interface between humans and machines was both concrete historical outcome and conceptual offspring of architecture’s engagement with rationality after the Second World War — the overarching theme in the cases that I have discussed thus far. As I have shown, the intellectual agenda motivating such engagement was not to eliminate intuition and subjectivity altogether, but to couple them with subject-less rationality. Such reconciliatory aspiration echoes from the design methods endeavors’ foundational embrace of both “systematic” and “intuitive” processes that we saw in the discussion of the Conference on Design Methods in chapter 2, to Leslie Martin’s remarks on the complementarity of intuitive

\textsuperscript{1171} Ibid.
\textsuperscript{1172} Ibid., 316.
\textsuperscript{1173} Friedman, \textit{Pro Domo}, 40.
\textsuperscript{1174} Harary, “Aesthetic Tree Patterns in Graph Theory,” 229.
\textsuperscript{1175} Weinzapfel and Negropont, “Architecture-by-Yourself,” 75.
and rational thought in chapter 3, all the way through to Friedman’s discussion of the “objective” and “subjective” aspects of architecture.

Intellectual vision encapsulates the specific ways in which the graph performed such a reconciliatory operation: on the one hand, by casting rational structure as a causal substrate of intuitively perceptible appearance, and on the other hand, by making rational thought intuitive and graspable. In this chapter I have shown how graph-enabled intellectual vision moved beyond discussions of disciplinary consolidation and professional legitimacy to become the foundation of anti-professional debates of participatory architectural design: in theories of participatory architectural design, the graph was promoted as an objective substructure defining extents of permissible choices thus supporting superimpositions of subjective meanings and aesthetic choices. The visual intuitiveness and tangible concreteness of the graph also aligned with ideas of accessibility by a general populace of non-specialists. The graph’s presence on YONA’s screen is also a revelatory moment for intellectual vision’s persistence in the deceptively image-rich universe of computer graphics and visualization. What was on the screen now lies behind it. Although users of computer tools for architectural design see geometric shapes on their screens, these shapes are bound within a discrete, structured, and combinatorial universe of underlying abstract structures of points and relations. Computer applications for architectural design run on intellectual vision.

By delving into contexts of architectural theory in the past three chapters, I have traced some ways in which intellectual vision, the idea that physical form and shape are underpinned by abstract invariant structures, influenced the development of architectural theory and was reified in computer tools for architectural design. I have done this by focusing on a concrete entity that signaled and facilitated this particular approach to seeing and subjectivity: the graph. Exposing
graph-based intellectual vision as constructed, as opposed to natural, opens pathways for critiquing its assumptions and limitations for architectural design. Yet, as historian of science David Kaiser has observed, asserting the social construction and contingency of established knowledge or practices in a discipline is only the first step toward investigating the ways in which the construction took place.\footnote{Kaiser, David. 2000. “Stick-Figure Realism: Conventions, Reification, and the Persistence of Feynman Diagrams, 1948-1964.” Representations 70: 49–86.} In keeping with this observation, I have proposed structural realism as the legitimizing force for intellectual vision’s establishment in architectural theory, along with its symbol and facilitator, the graph. I traced structural realism at the intersection of intellectual, pragmatic, and aesthetic forces. In this chapter, I specifically showed that aesthetic forces—what graphs looked like and what they invoked—played a key role in establishing the graph in architectural theory as a way to practice structural abstraction without challenging the freedom, subjectivity, of open-endedness of architectural design.

Ironically, the kind of visual leaps and associations that allowed graphs to proliferate in architectural theory and to be repurposed under sparring agendas were precisely the aspect of architectural design that graphs abolished. By subjugating drawing under an abstract invariant structure, graphs purified it from the contingency of visual ambiguity and tamed it under the certainty of a combinatorial universe in which all possibilities of physical form are known from the start. Concealed behind the screen, graphs haunt architectural design as ghosts of an ambivalent modernism and its agonizing quest for certainty.
Figures for Chapter 5


Figure 65: Screenshot of YONA’s PRE-SHAPE SPACES function, overlaying “bubbles” on the linkages graph (the bubbles were B-spline curves fitted between the graph’s dual and an offset of its perimeter). (Source: Weinzapfel and Negroponte, “Architecture-by-Yourself,” 75).
Figure 66: Screenshot of YONA’s SKETCH SHAPE function. The user finger-sketches lines and curves on top of the “bubbles” snapping on an underlying grid (Source: Weinzapfel and Negroponte, “Architecture-by-Yourself,” 75).

Figure 67: Another example of the YONA’s SKETCH SHAPE function output (Source: Weinzapfel and Negroponte, “Architecture-by-Yourself,” 77).

Figure 70: One of the ten principles of “spatial urbanism” (Friedman, Yona. 1961. L'architecture mobile, les dix principes de l'urbanisme spatial. © Bertrand Prévost - Centre Pompidou, MNAM-CCI /Dist. RMN-GP; © Artists Rights Society (ARS), New York / ADAGP, Paris – reproduced by permission of the ARS).

Figure 71: Spatial structure on the Seine river with the Bercy bridge (Friedman, Yona. 24 April 1964 One of the ten principles of “spatial urbanism” (Friedman, Yona. 1961. Structure spatiale au-dessus de la Seine avec le pont de Bercy. © Philippe Migeat - Centre Pompidou, MNAM-CCI /Dist. RMN-GP; © Artists Rights Society (ARS), New York / ADAGP, Paris – reproduced by permission of the ARS).

Figure 72: A schematic architectural plan mapped one-to-one to a labeled planar graph, and then translated to an adjacency matrix and binary code. (Source: Friedman, Yona. 1975. Toward a Scientific Architecture. Translated by Cynthia Lang. Cambridge, Mass.: The MIT Press, 31).

Figure 73: A saturated graph describes all spaces contained in a three-dimensional physical skeleton. Specific configurations are sub-graphs of the saturated graph. (Source: Friedman, Toward A Scientific Architecture, 48).
Figure 74: Use of graph theory to identify the number of statements permissible in an axiomatic theory of architecture (Source: Friedman, *Toward A Scientific Architecture*, 23).

Figure 75: Chart of future user’s (Mr. X) daily habits derived from self-documentation of room visits per day (Source: Friedman, *Toward A Scientific Architecture*, 52).
Figure 76: The FLATWRITER’s keyboard indicating position, shape, equipment placement, and site orientation, accompanied with a sample sequence of keystrokes (Source: Friedman, Toward A Scientific Architecture, 55).

Figure 77: The role of the “future user” and the professional architect in a “remodeled” architectural process, where the “future” users of architecture choose and change their individual and collective living environment (Source: Friedman, Toward A Scientific Architecture, 10).
Figure 78: Illustrations of Yona Friedman’s “urban mechanisms” system (Source: Friedman, Yona. 1964. “Towards a Coherent System of Planning.” *Architectural Design* 34 (8): 371–72, 380).

Figure 79: Encoding the “urban mechanism” system in a computer through matrices, planar graphs (representing individual goals and movements), and three-dimensional networks (representing the urban system) (Source: Friedman, “Toward a Coherent System of Planning,” 381).
Figure 80: Configurational studies of architectural layouts using permutations of a planar graph. (Source: Friedman, Yona. 1968. “Seminar on Methods for Architects and Planners.” Arch + 2: 27–44, 40).

Figure 82: The components of Friedman’s design method, subsequently translated as FLATWRITER’s keys; type of configuration, shape, equipment placement, and building/equipment components (Source: Friedman, Yona. 1968. “Recherche D’une Methode.” *Techniques et Architecture* 29 (2): 76).
Figure 83: Cover of Michel Ragon’s edited volume *Les Visionnaires de l’Architecture* (1965. Paris: Robert Laffont)

Figure 84: Eckhard Schulze Fielitz’s space-frame explorations cast space as discrete and configurable material (Source: Schulze-Fielitz, E. 1962. "Une Theorie pour L Occupation de L'Espace." *L'Architecture D'Aujourd'hui* 102: 78-85).
Figure 85: Configurations within Friedman’s space-frame infrastructure. (Source: Friedman, “Recherche D’une Methode,” 76).
Figure 86: Yona Friedman’s diagram of architecture and planning as consisting of an “objective” and an “intuitive” system. Following the structuralist trope, Friedman argued that the “objective” aspects of architecture and planning are open to scientific scrutiny, while the “intuitive” aspects (the domain of extemporaneity and freedom) can be conceptualized as probabilistic combinations on a supporting “objective” system. (Source: Friedman, *Toward a Scientific Architecture*, 18).

Figure 87: Table showing frequency of preferences of six-point trees in Harary’s experiment on the aesthetics of tree patterns (Source: Harary, Frank. 1971. “Aesthetic Tree Patterns in Graph Theory.” *Leonardo* 4 (3): 227–31, 229).

Figure 90: Display and interface of the IMAGE system (1971). For display, the system used an IBM 2250 cathode-ray tube, driven by an IBM 1130 satellite computer, telephonically connected to the IBM 360/67 time-sharing system. The figure represents the cutting edge of a graphical human-computer interface in the turn of the 1970s, which included a screen, a light pen, a keyboard, and function key buttons that allowed the user to switch between different system modes. (Source: Weinzapfel, Johnson, and Perkins, “IMAGE,” 105).

Figure 91: Display and interface of the YONA system (1976). The system used an IMLAC dynamic display, but replaced the “cumbersome and inaccurate” light pen by replacing IMLAC’s implosion screen with a Touch Sensitive Display tablet developed by ArcMac. This allowed “naive” users with no previous computer experience to directly manipulate objects on the screen with their fingers. (Source: Weinzapfel and Negroponte, “Architecture-by-Yourself,” 76).
Figure 92: Cover of the *Design Quarterly* Special Issue “Design and the Computer” designed by Peter Seitz.
Figure 93: Left: Display and interface of URBAN5 system. Similar to IMAGE (see figure 28), the display was an IBM 2250 cathode-ray tube using the IBM 360/67 in a dedicated (not time sharing) manner. The interface included also a light pen, keyboard, and function key buttons. The function key buttons could be overlaid with programmer defined layers (Source: Negroponte, Nicholas. 1973. The Architecture Machine: Toward a More Human Environment. Cambridge, Mass.: MIT Press, 74). Right: URBAN5’s flowchart (Source: Negroponte, Nicholas. 1967. “URBAN 5, an On-Line Urban Design Partner.” Ekistics, 289–91, 290)

Figure 94: Photographs of the 1969 SEEK project. A pressure sensing electromagnetic “finger” moved the blocks in response to gerbil-induced dislocations. (Source: Groisser, Leon Bennett, and Nicholas Peter Negroponte. 1971. Computer Aids to Participatory Architecture. Cambridge, Mass.: MIT, 140-141).
Figure 95: Jean Michel Folon’s Somewhere Someone (Quelque part, Quelqu’un) poster alongside texts on the “Total City” Tange, Friedman, Fuller, and Negroponte. The poster reveals the totalitarian connotations of proposals that managed the city as system. Negroponte sought to subvert such ideas by invoking humanist discourse and emerging countercultural trends. (Source: Tange, Kenzo, Yona Friedman, Nicholas Negroponte, and R. Buckminster Fuller. 1973. “La Ville Totale.” 2000 [Deuxmille] : Revue de l’Amenagement Du Territoire et Du Developpement Regional, no. 24: 5–7).

Figure 96: Detail of Ant Farm’s 50X50 Pillow, installed at an environmental design conference organized by Berkeley professor and CES collaborator Sim Van der Ryn in Freestone, California (March, 1970). Ant Farm became known for its work on pneumatic structures, which it linked with countercultural ideas of nomadism, communalism, and ecology. The intertwining of countercultural values with inflatables is manifest in their popular do-it-yourself manual Inflatocookbook. In his discussions of “intelligent environments,” Negroponte nudged toward the intertwining of environmental design with architectural attitudes that grew out of the U.S. counterculture.
Figure 98: Sketch by “unpracticed” architect and its interpretations by a professional architect (bottom left), by the computer as gridded plan (top right) and by the computer as planar graph (bottom right). (Source: Negroponte, Soft Architecture Machines, 86).

Figure 100: Computer-generated alternative layouts using a planar graph interpretation of a novice user’s sketch. (Source: Negroponte, *Soft Architecture Machines*, 94).

Figure 101: Yona Friedman’s sketch of a “paternalist” and a “non-paternalist scheme” in the introduction of Negroponte’s *Soft Architecture Machines* chapter on Computer-Aided Participatory Design. In a non-paternalist scheme, the computer should model the “real world” and not the “user.” (Source: Negroponte, *Soft Architecture Machines*, 94).
Chapter 6: Conclusion

Ghosts of Modernism

The amphitheater was buzzing with some two-hundred first year architecture students in their first day of classes. Mathematics was on the schedule. Offered in the first semester only, the subject figured in the curriculum as a transition between the students’ math-centered high school training and the artistically inclined university education that they had selected for themselves. Because of its short duration, the course revolved around mathematical topics deemed essential for architects. The one was projective geometry; the other, graph theory. I do not remember much from that first encounter with graphs, other than it inaugurated my architectural education. My memories of the subject have faded too. With no further mention or use of graphs in the curriculum, Eulerian trails and Hamiltonian cycles joined the ranks of an unnecessarily technical first semester — one that third or fourth year students looked back at with a mixture of dread and bafflement. I do, however, distinctively recall one idiosyncrasy, possibly because of the associated student sniggers. The graph theory instructor, faculty member of the School of Applied Physical and Mathematical Sciences, talked about graphs in the first person. “I, the little graph” opened definition statements and proof expositions. This catchphrase was only part of full-blown graph anthropomorphism: the graph had legs and hands; blood-pumping arteries; a torso. Speaking through the instructor, it urged us: “Look at me!”

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Admittedly, this personal anecdote stands far apart in time, site, and context from the story I have been telling here. I do not presume it to be anything more than an autobiographical snippet — an informative one, nonetheless. Bells of recognition rang when at the end of the telephone

1177 This is a personal rendition of the graph theory class offered to first year students in the National Technical University of Athens School of Architecture, in fall 2002.
line, Lionel March fondly spoke to me about his mathematics professor, the famous algebraic geometer H. F. Baker, describing graph theory by “call[ing] these little objects [graphs] that he drew on the blackboard ‘creatures’.” I was struck by the proclaimed animism, the visualness, the objecthood. I had a similar sentiment when, in his Paris apartment a few years back, I heard Yona Friedman talk to me about graph theory’s usefulness “for people to experiment,” or, when I read Harary speaking of “sexy,” “funny” and “ugly” graphs. It was all a variation of my graph theory professor’s “I, the graph.” Arguably, a short course in graph theory within a teaching-oriented architecture school in early 2000 Greece has little to do with architects’ conscious deployment of the graph the research universities of the 1960s. Yet, encountering the graph far and beyond its point of origin, alerted me precisely to the forces of its dispersion—to its allure for architects and its stature as a mathematical subject in natural proximity with architecture.

My personal experience was probably a relic of graph theory’s “golden years” in architecture, part of which I have recounted in this dissertation. Along with the demise of the legitimizing power of mathematical language for architectural discourse, has come a decline in the enthusiastic invocations of graph theory’s intellectual and practical potentials for the field of architecture. Few architects today have heard of graph theory at all. However, talk of “systems” and “networks” pervades contemporary architectural discourse. Relational webs at all scales of the environment connecting people, spaces, or building components occupy the architectural imagination. Skeletal structures of points and lines figure in the covers of books that herald a

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1178 March, Interview by Vardouli.
1179 Friedman, Interview by Vardouli
1180 Harary, “Aesthetic Tree Patterns in Graph Theory,” 229.
1181 For an eloquent description of the pervasiveness of networks in the writing, teaching, and practicing of architecture—a phenomenon that most readers can probably verify from experience— see Wigley, “Network Fever,” 1.
new computational and algorithmic era of architecture. Beyond the level of symbolism or metaphor, structures of discrete entities define the operational universe of architects who work with computer technologies. The imagery of nodes and lines is well familiar in Visual Scripting platforms. But also behind the screen, in Building Information Modeling, Geographic Information Systems, and Computer Aided Design and Drafting systems, lie independent entities (symbols) combined and recombined based on relationships defined by the designer. Contemporary architecture, especially its computer-related varieties, is operationally and symbolically saturated by structural constructs — or as Berge, Harary, and other graph theorists have taught us to correctly name them, *graphs*.

We continue to think and operate in a graph-infused architectural universe.

**Contributions / Conversations**

The contemporary dominance of the graph may be viewed as a consequence of the digital computer’s pervasiveness. Digital computers calculate by processing discrete symbols based on relationships that connect them (data structures). It was the impetus to use digital computers, one may therefore argue, that motivated the description of architectural objects, concepts, and processes as discrete entities and relationships, as graphs. Projected onto historical analysis, this view would direct efforts to interpret graph theory’s entry into architecture to phenomena of computerization. Surely, computer implementations, ranging from Alexander’s HIDECS system to the proliferation of automatic floor-planning programs in the late 1960s and early 1970s, were fertile ground for the application and dissemination of graphs. The practical exigencies of the computer, however, are only part of the story. In this dissertation, I have advanced a view of the

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graph not as an outcome of architecture’s computerization, but as its enabler. I have ventured to encounters between architecture and mathematics in the 1960s and 1970s, to argue that graph theory entered architectural theory with intellectual stakes that surpassed technological applications. Before computerization, came mathematization.

To architectural history, I have contributed a new way of looking at transformations of architectural theory in the 1960s and 1970s; one that de-centers computer technologies and turns the analytical light on processes of mathematization. Defined as a translation of architectural concepts and operations in mathematical terms, mathematization opens architectural history to histories of mathematics and general intellectual histories. Here, I have retrieved the mathematizing transformations of 1960s architectural theory as a broad intellectual enterprise, whose understanding requires the simultaneous consideration of two scales: the large scale of disciplinary debates and the micro scale of theory-making operations. I have illustrated that the graph, as a specific mathematical entity that participated in architecture’s mathematization, performed work at both scales. It grounded large intellectual agendas in concrete and workable mathematical techniques, which in turn formed the “material” for making architectural theories. I have shown that the graph entered architecture as a symbol and facilitator of structural abstraction — an intellectual operation that in the 1960s came to be seen as a requisite of modernism. I have used the term modernism to point specifically to a project of disciplinary consolidation and interdisciplinary unification that developed after the Second World War and which was enabled, but also motivated, by particular institutional formations related to education and research.

The graph entered architecture propelled by the thrust of modernism, to ultimately furnish it with distinctive form. When I say that the graph gave form to postwar architectural modernism, I
mean it quite literally: the graph was both a visual symbol and a way of practicing structuralist attitudes that underpinned this intellectual project. Through scrutiny of particular cases, I have shown that the graph-based working of modernist attitudes transformed architecture-theoretical statements. These transformations led to the generation of participatory and emancipatory statements that appeared antithetical to the agendas for which architects initially used the graph — an antithesis that the persistence of the graph exposes as conceptual and technical continuity.

To STS, I have contributed perspectives on how architects made theory, by following the mathematical entity of the graph across research groups and laboratories. Compared to various other scientific fields that STS scholars have examined through the lens of “non-human” actors, postwar architectural theory presents challenges and opportunities. Although I have insisted on calling the graph an “entity,” the graph was far from a stable object. It was invoked for different purposes (from analysis to modeling and to the generation of physical form); it was drawn in different ways and through different media; it was called different names; it moved between being a concrete thing and an abstract concept. In this sense, this story has been about “following” a protean entity; one with as many manifestations as its contexts of use. To tackle this challenge, I have adopted a broad and forgiving definition of the graph as a depiction of a structure of relationships between entities through points and lines. It is such images of graphs in architecture-theoretical books, papers, and reports that I followed to construct this story. The graph’s visual depiction as points and lines is crucial here, as this story has been about tendencies to abstraction and rationalization as much as it has been about the architects’ ambivalence toward seeing. I have illustrated that the trajectory of the graph cannot be reconstructed by recourse to its intellectual utilities and practical instrumentalities only. The graph’s visual presentation was key for its spread in architectural theory. Despite claims to
abstraction and rationality, architecture never ceased to favor the visual realm and be deeply driven by aesthetic values and judgments.

The graph’s visual presentation instilled architectural abstraction with aesthetic judgments: the space-frame aesthetic of French “experimental” architecture is palpable in Friedman’s graph drawings, for example. Visual presentation also served purposes of symbolism: Alexander used images of graphs for rhetorical purposes, to signal changes in his theoretical commitments. Most crucially though, the graph’s visual presentation instigated a particular kind of seeing. I coined the term “intellectual vision” to refer to this new way of seeing that positioned abstract structures under sense-perceptible appearances. Learning to see the lines of an architectural plan’s drawing as lines of a graph, was practicing intellectual vision. I argued that intellectual vision was legitimized by a pervasive “structural realism” — the controversial assumption that social ensembles and the built environment were made of structures that could be mapped and rendered workable. The graph required, and gave rise to, the cultivation of a structural imagination of the world. The graph’s symbolic meanings alongside its operational properties formed, in turn, the basis on which architect-researchers developed working methods, negotiated architectural debates, and ultimately remodeled seeing, choice, and possibility in architectural design.

This legacy is one still to debate in the present, albeit reified in computer applications. Contemplating the impact of LUBFS Centre’s expeditions with the mathematics of relations and structures, March pointed out that the contemporary computer implementations that pervade the practice of architectural design run on the “modern math”: “Behind it all, it is exactly that mathematics. Even if the user quite often doesn’t know it.” Here I have lifted the curtain of computer applications to expose the contested history of the graph as one having to do with architectural modernism’s ambivalent relationship with the ambiguities and contingencies of the

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1183 March, Interview by Vardouli.
visual and empirical world. This leads to this dissertation’s critical premise. Exposing the assumptions and deliberations on which the intellectual vision was founded, challenges its naturality or neutrality and opens a way to critique its suppositions and limitations for creative design. The following paragraphs are dedicated to this end. I begin with a critique of three graph-theoretic “properties” that recur and connect the diverse contexts that I have studied so far. I use the term “properties” here to refer both to a mathematical utility, something that the graph was viewed as enabling, as well as to themes of architectural discourse. Beginning from these properties’ mathematical meanings and implications, I expose their architectural extrapolations, along with implicit questionable assumptions.

**Three Properties**

**Isomorphism:** The chapter on “Planar Graphs and Relations” in *The Geometry of Environment*, explained that two graphs are termed “isomorphic,” “if they have the same number of vertices, and wherever two vertices in $G_1$ (say $a_1$ and $b_1$) are connected by an edge, then there are two corresponding vertices $a_2$ and $b_2$ in $G_2$ also connected.”

The definition was prepended with isomorphism’s implications for discovering similarity under the appearance of difference: “Since the same graph may be drawn in a variety of ways, it follows that two graphs ($G_1$, $G_2$) which are apparently at first sight dissimilar, may prove to be identical when analysed [sic].”

Isomorphism was conceptually central for the architectural theories I examined here. It manifested in the context of three interrelated architectural debates: *mapping* across contexts and substantive media, *comparison* of dissimilar entities, and *communication* across disciplines. In his early work, Alexander used graphs to establish structural mappings among different contexts.

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1184 March and Steadman, *The Geometry of Environment*, 243-244
1185 Ibid.
of architectural operation — to ensure congruence between analysis and synthesis (the structure of “problem” requirements and the structure of physical form). Alexander was accustomed to viewing isomorphism as a structural mapping from gestalt psychology — the field that advanced correspondences between the structure of physical form, the structure of physiological configurations in the brain, and the structure of cognitive representations. This understanding of isomorphism brings to mind the Aristotelian morphê — an ideal form that exists independently from matter and is imprinted upon it. In Alexander, form appeared not as a geometric blueprint, but as an invariant structural pattern.

Isomorphism as mapping also appeared in the context of the LUBFS Centre’s mathematizing work. There, mapping occurred not among architectural contexts, but between architectural and mathematical entities (graphs, in this case). Such structural mapping, which is the basic premise of mathematical modeling, presupposed a structural rendition of architecture. Mathematization as isomorphic translation between architectural and mathematical structures both resulted from and motivated a structuralist project in architecture: the identification of independent architectural entities along with the relationships that connect them. The LUBFS Centre cast isomorphism as a representational virtue, standing for undistorted translations between concrete physical phenomena and their abstract mathematical models. This representational virtue came with ethical and democratizing claims: Friedman attributed to the one-to-one mapping between architectural and mathematical objects (an architectural plan and a graph) ethical values of transparency. At the same time, both Friedman and ArcMac viewed the immediacy and intuitiveness of this mapping — the transposition of a graph to an architectural plan — as a testament to the graph’s accessibility by a lay public.

Isomorphism as comparison of dissimilar entities, to move to the concept’s second manifestation, relates with the dethroning of visual appearance from architectural design and its replacement by intellectual vision. This meaning of isomorphism was most pronounced in the work of the LUBFS Centre. This was partially the case because the Centre’s researchers were invested in the production and organization of architectural knowledge and thus often engaged in reappraisal of architectural precedents through new mathematical devices. The comparison of visually dissimilar Wrightian plans to reveal them structurally identical is one such example in the LUBFS Centre’s work. Such comparisons were infrequent, if not absent, from the methodologically inclined explorations of Alexander, Friedman, or ArcMac. However, intellectual vision was at work in all four cases, subjugating the perceptual realm to an underlying rational order.

Finally, discussing isomorphism’s third manifestation as a model of communication across disciplines requires recourse to larger intellectual debates in which architects participated. Isomorphism was a basic tenet of structuralism, associated with ideals of disciplinary unification. In grappling with the question “What is Structuralism?”, Runciman declared the premise of “a general isomorphism between structures”\textsuperscript{1187} —“not merely that separate structures can be broken down into their components”\textsuperscript{1188} — as the only promising route toward a definition of the hazy intellectual movement. Architects enthusiastically embarked in such unifying visions enabled by universal isomorphism. March spoke of structural mathematical models as a common language among disparate disciplines —what Spillers later referred to as the “common ground.” Alexander echoed a similar belief in his 1968 essay “The Bead Game Conjecture.” Borrowing

\textsuperscript{1187} Runciman, “What is Structuralism?,” 255.
\textsuperscript{1188} Ibid.
from German author Herman Hesse’s homonymous novel. Alexander envisioned the possibility “to invent a unifying concept of structure within which all the various concepts of structure now current in different fields of art and science can be seen from a single point of view.”

Graph-enabled isomorphism signaled a move away from concrete particulars, from empirical differences, to seeing similarities of the structures beneath. Crucially, these structural similarities would not require further analysis but would be immediately graspable by the eye. Mathematical comparison of graphs in an analytical manner was there to protect the eye from being deceived from potential differences in the graphs’ visual presentation. Isomorphism was about suppressing images’ disposition to look different and the eye’s proneness to see them in different ways. Along with a belief in the unifying power of structural abstraction, came a disregard for the diversifying effects of concrete appearance. It did not matter what the graph’s points represented. The properties of things evaporated under the power of their relations.

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Combinatorics: In his 1971 paper “The Automated Generation of Architectural Form,” UCLA assistant professor William J. (Bill) Mitchell portrayed the state of the art in systematic processes of architectural design as intellectually continuous with Aristotle, 13th century mystic Ramon Lull, and 17th century philosopher Gottfried Leibniz. The connective thread, Mitchell argued, was the espousal of a combinatorial view of form generation — form as resulting from a combination of fixed elements. He explained: “we define a set of dimensions of variation, describe the alternative possibilities along each of these dimensions, then consider the set of alternative

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combinations thus generated.” The other intellectual kinship that Mitchell identified was in closer historical proximity with the works he reviewed. It was a heuristic problem-solving method called “generate and test.” “Generate and test” involved finding a convenient representation of solutions, a way of generating solutions, and a way of testing if the solutions solved a given problem.

Mitchell’s themes of a combinatorial attitude to the generation of physical form and a heuristic problem-solving approach to architectural design recur in the contexts of architectural theory that I have navigated. Alexander’s combinatorial outlook runs from Notes (where descriptions of physical form are produced through superimposition of “diagrams”) through to A Pattern Language (where descriptions of physical form are produced through sequential combination of component-like patterns). Alexander openly embraced a combinatorial, or “kit of parts” as he called it, model of generativity in his article “Systems Generating Systems.” However, the “test” component was absent from Alexander’s theory. Alexander claimed to have to have already identified the “program” or “sequence” for deriving an adequate solution by decomposing the design “problem” or studying “environmental structure” and its components. On the contrary, the automatic activity allocation processes that Tabor reviewed, the university timetabling studies of Steadman, Dickens, and Bullock, or the IMAGE system that we saw in chapter 5, adopted the “generate and test” approach.

1192 Ibid., 193.
In discussing possible architectural representations for the “generate and test” method, Mitchell referred to the potentials of the dual linear graph. The usefulness of this representation, he remarked, derived from its capacity to directly relate adjacency requirements with floor plan representations, thus allowing the generation of architectural plan variations and their evaluation (test) against specific programmatic requirements. Mitchell’s remark on graph duals captured the attitude of Steadman’s work on the automatic generation of minimum standard house plans (which Mitchell referenced), Friedman’s generation of “menus” of architectural configurations, while it anticipated ArcMac’s Architecture-By-Yourself work. However, there is one important asterisk. Steadman and Friedman purported to present the entire range of combinatorial possibilities as opposed to some optimal or suboptimal solutions. By issuing claims about the extents and limits of choice, these works advanced a combinatorial view of choice and possibility: that possibilities are preset and enumerable before any action takes place, be it during the design process or during a building’s occupation by its inhabitant.

At times, this combinatorial perspective on possibility was presented somewhat self-consciously. For example, in describing the influence of Friedman’s methods to ArcMac’s work, Negroponte cautioned readers about “the particularly French notion of a ‘banque de données’ or what he [Friedman] calls a ‘repertoire’.”* Negroponte was distinguishing Friedman’s theory from the combinatory of prefabricated elements customary in habitat évolutif examples that we saw in chapter 5. The problem with these approaches, he explained, was that “the offerings of a menu of solutions obviously cannot exceed the combinatorial product of the parts (which may be enormous).”* Negroponte argued that Friedman escaped the particular constraint tainting

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1197 Ibid.
French researchers by including in his repertoire “topologies that do not have a metric”\textsuperscript{1198} – in other words, graphs. “It is the user's adding of this metric,” he concluded, “that affords the limitless variety.”\textsuperscript{1199} Steadman also invoked the graph’s abstract nature to claim that his full enumeration of architectural possibilities did not reduce architecture to a combinatorial art\textsuperscript{1200}. The full range of combinatorial possibilities of architectural floor plans, Steadman argued, was only possible in the abstract level of a generic floor layout — specific details, materials, choice of architectural elements were left up to the architect.\textsuperscript{1201} What was left out of the abstraction was a kind of embellishment: choosing a building’s finishings on a pre-decided layout. This was similar to the approach of the YONA system. There, the geometric shapes that users were allowed to draw over the non-metric graph topologies, were rationalizations (through straight geometric lines or arcs) of bubble diagrams – duals of adjacency graph representations of programmatic requirements.

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**Predictivity:** In his 1964 article short-titled “Twelve Models of Prediction,” eminent American sociologist Daniel Bell positioned prediction as a key marker of the social condition from which he was writing. “One of the hallmarks of ‘modernity’,” he wrote, “is the awareness of change and the struggling effort to control the direction and pace of change.”\textsuperscript{1202} Despite his invocation of the dauntingly large and hazy category of “modernity,” Bell was really documenting his contemporaries’ fervent preoccupation with the “future.” From social conjectures about the

\textsuperscript{1198} Ibid.
\textsuperscript{1199} Ibid.
\textsuperscript{1200} Steadman, Interview by Vardouli.
\textsuperscript{1201} Steadman, interview by Vardouli.
\textsuperscript{1202} Bell, “Twelve Models of Prediction,” 846.
imminent future performed by newly founded, and well funded, social science committees\textsuperscript{1203} to
the predictive models of the RAND Corporation,\textsuperscript{1204} questions of who controls or should control
change, and how, were center stage. Architects were far from untouched by such a future-centric
culture or the challenges that galloping change posited to their professional role. The categories
of “certainty”\textsuperscript{1205} (as we saw in “Polemic for a Structural Revolution” by March et al.) or
“unpredictability”\textsuperscript{1206} (as we saw in Friedman’s participatory theory), spoke to such concerns. In
fact, commitment to certainty or uncertainty seems to separate architectural theories sitting at the
opposite sides of the “participatory turn.” In Alexander’s early work, or the work of the LUBFS
Centre, the question of rational choice was at stake: how to make present decisions to achieve
future goals. Through his graph-based decomposition methods, and the resultant design process
structures, Alexander sought to prescribe a correct sequencing of design decisions. Influenced by
decision theory, Alexander’s trust in rational design decisions was less nuanced that the one
exhibited by LUBFS Centre researchers. March’s essay “The Logic of Design and the Question
of Value” articulated such nuances.\textsuperscript{1207} So did the general skepticism that we heard the Centre’s

\textsuperscript{1203} As elaborated in an earlier footnote in chapter 5, such committees were the Ford Foundation funded \textit{Futuribles}
project and the Commission on the Year 2000 of the American Academy of Arts and Sciences, whose director was
Daniel Bell.

\textsuperscript{1204} The RAND Corporation is a non-profit organization existing since 1948, which was initiated as “Project RAND”
in 1945. The Corporation’s current motto “Objective Analysis. Efficient Solutions” reflects its historically persist
ent commitment on research, rationality, and analysis as the source of policy-related advice.

\textsuperscript{1205} In opening their “Polemic for a Structural Revolution” March et al. quoted Leonardo da Vinci on mathematics,
certainty, and legitimacy of knowledge: There is no certainty where one can neither apply any of the mathematical
sciences nor any of those which are connected with the mathematical sciences. Whoever condemns the supreme
certainty of mathematics feeds on confusion, and can never silence the contradictions of the sophistical sciences,
which lead to an eternal quackery. (March, Dickens, and Echenique, “Polemic for a Structural Revolution,” 275).

\textsuperscript{1206} Friedman, \textit{Toward a Scientific Architecture}, 86.

\textsuperscript{1207} In “The Logic of Design and the Question of Value” March drew from philosopher Charles Sanders Pierce’s
discussion of the logic of science to argue that design includes abduction (which March called production),
deduction, and induction. These three processes corresponded respectively to the creation of new designs, the
prediction of their performance, and the accumulation of knowledge (suppositions) about values and characteristics
of built forms, which in turn formed the basis for the production of new designs. As March famously wrote
“production creates; deduction predicts; induction evaluates” (Ibid., 18). March saw the three processes linked in a
“cyclic, iterative procedure” (Ibid., 20). Contrary to Alexander who sought to clear design from values and
judgments through objective deductions (design theory), March argued for a “critical, learning process” (Ibid.). In
members exhibit on mathematical models of the “planning” type. Where the presence of graphs truly bears critical potential, is in participatory architectural theories whose authors pledged allegiance to “unpredictability” and “uncertainty.”

I concluded chapter 5 with an allusion to architects’ continued quest for certainty, despite their claims to open-endedness. What I was pointing to was a desire to eliminate the transformative and unpredictable effects of action. As American philosopher John Dewey critiqued in his seminal 1929 The Quest for Certainty, a concern with fixities instead of change reveals uncertainty aversion. It conveys a desire to carve a secure domain by abolishing perilous action. Architects exhibited such uncertainty aversion by striving to contain both possibilities of action and its outcomes within the bounds of preset combinations permissible by an abstract mathematical structure (the graph). On a rhetorical level Friedman, ArcMac, or members of the LUBFS Centre for that matter, aligned with what American economist, decision-making theorist, and general polymath Herbert Simon later described as “designing without final goals.”

Paralleling designing with oil painting, Simon eulogized the unforeseeable effects of design interventions as opportunities for “new ideas” potentially leading to “more and richer” experiences of the world. In a similar spirit, architectural theorists preached architectures with

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1210 Ibid., 163.
1211 Ibid., 164.
open ends. The mathematical device that supported these theories, however, exposes them as fraught with contradiction: graphs reduced change to knowable (predictable) states of an invariant (infra)structure.

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These commentaries on the graph’s “properties” are meant to synopsize how large debates about architectural M/modernism were channeled, translated, and worked through particular operations that architects saw the graph as affording: mapping and matching structures, combining entities, predicting by delimiting choice. These operations ultimately amounted to a model of creative architectural design: choosing one of several combinatorial variations of an abstract structure, which can be transposed to the physical, empirical world without distortion. The same model was used to conceptualize open-endedness and “user” freedom in architectural theories that issued claims of participation and interactivity. This is what Alexander dubbed “The Main Structure Concept” or what Friedman and Negroponte alluded to when they spoke of physical and computational “infrastructures.” I have shown that this structural model of architecture has risen from the confluence of the graph’s properties with intellectual commitments and cultural beliefs that are not immune to critique, innocent of contradiction, or free of ambivalence. For this story has revealed architects’ continued, and often undisclosed, appreciation of the visual realm even in projects of modernizing rationalization and iconoclastic abstraction. Testament to this has been the role of the graph’s visual presentation as a driving force behind its rapid dissemination in architectural theory and its eventual celebration as a register of congeniality and intuition, when architects embraced participatory agendas. This story then, has been one about allure and deception. The graph’s visual presentation was only deceptively visual because it attributed no

1213 Simon also adopted a combinatorial view of open-endedness. He conceptualized open-ended “proliferation of forms” as a “combinatoric play upon the simpler” requiring an ever “larger and richer collection of building blocks” (Simon, The Sciences of the Artificial, 165).
transformative power to the perceptual realm. No matter how a graph is drawn it is always the same thing: a set of discrete entities and relations. Things with that look dissimilar being the same, after all, is the premise of isomorphism. The graph’s appearances may play a role on a psychological level of visual evocation, as Harary showed in his “Aesthetic Tree Patterns” experiment, but they make no difference in the graph’s manipulation. In working with graphs, appearances do not matter.

**Beyond the Main Structure Concept: Reclaiming Shape**

Among the presentations of “Basic Questions of Design Theory,” the 1974 symposium that Harary keynoted with an enthusiastic observation about graph theory’s pervasiveness in design, was a paper titled “Formalization of Analysis and Design in the Arts.” The paper adopted the mathematical parlance of structures and systems that characterized most symposium presentations, yet with a pivotal difference: The authors —UCLA Systems Science Department doctoral student George Stiny and Stanford Computer Science Department doctoral graduate James Gips— focused on what they called “an aesthetic viewpoint,” which they defined as a set of “interpretative conventions and evaluative criteria.” The authors explained that these conventions were applicable both in analysis (“the structure and content found in a work of art”) and design (the “production of a work of art having a given type of structure and content”) — two processes that they construed as “symmetric.” Aesthetic viewpoints were varied and relentlessly subjective, as many as a work’s creators and onlookers. Yet this variety of

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1215 Ibid., 507.
1216 Ibid.
1217 Ibid., 507.
1218 Ibid., 508.
1219 Ibid., 507.
viewpoints, argued Stiny and Gips, did not preclude the possibility of their precise (mathematical) statement.\textsuperscript{1220} The intellectual commitment to subjective multiplicity was critical: it advanced mathematical scrutiny not on the workings of intellectual vision that made different things be the same, but on the workings of perceptual vision that made it possible to see the same thing in different ways.

This required bringing shapes back into the picture. The work on aesthetic viewpoints captured a proclivity for the productively diversifying effects of perceptual appearance. Making perceptual vision operational within the analysis and generation of designs was the consequence of another mathematical formalism that Stiny and Gips had first presented three years before the conference. “Shape grammars,”\textsuperscript{1221} as this formalism was called, was a redux of MIT linguist Noam Chomsky’s transformational grammars:\textsuperscript{1222} sets of replacement rules indicated by arrows that were used to generate sentences. These rules were used to identify types of linguistic elements in a phrase (noun, verbs, adverbs etcetera) and replaced them with other types of single elements or compounds of elements. Shape grammars adopted the idea of transformation rules, but replaced linguistic elements (words) with spatial entities (shapes). In a series of papers subsequently published in \textit{Environment and Planning B},\textsuperscript{1223} a journal of design and architectural research founded by Lionel March, Stiny published a series of articles that highlighted the theoretical implications of reclaiming shape. The articles deployed modern mathematical language of sets and boolean algebras to define shape and its transformations in an algebraic manner, yet without disavowing the shape’s metric and visual properties.

\textsuperscript{1220} Ibid., 508.
\textsuperscript{1223} See for example, Stiny, “Introduction to Shape and Shape Grammars.”
Details aside, the papers approached mathematically the intuitive notion that shapes are ambiguous and that a picture—to paraphrase the colloquial saying—is not reducible to words. Consisting of infinitely many parts, shapes resist a predefined structural description. Therefore transformation rules needed to apply directly on the shape that one saw, on the surface of appearance, and not on some underlying abstract structure. To achieve this, shape grammars relied on transformation geometry—an approach to geometry that focuses on shape invariances under geometric transformations such as translation, scaling, rotation, and reflection. We saw transformation geometry in chapter 2 in the context of the British new math controversy about the status of geometry. Tracing back to Klein’s “anti-modern” endeavor to safeguard concrete experience in the realm of mathematical abstraction, transformation geometry allowed for algebraic descriptions of shapes while preserving their metric properties and their spatial and visual concreteness. In shape grammars, transformation rules took the form of $A \rightarrow B$, where $A$, $B$ were actual drawings of shapes (as opposed to symbols standing for shapes) and the $\rightarrow$ was a replacement operation. The replacement operation found an instance or a transformed (scaled, rotated, reflected) copy of a shape $A$ in a drawing and replaced it with a shape $B$. Stiny framed this as a shift from “identity,” in which a rule requires to recognize a specific symbol, to “embedding,” in which a rule requires to perceive a shape, possibly within another shape.

In a 1982 letter to the editor of *Environment and Planning B*, titled “Spatial Relations and Grammars” Stiny distinguished between “set grammars,” which consisted of rules that treated designs as “symbolic objects [emphasis in the original],” and shape grammars, which treated

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1224 Embedding includes identity. Construing shapes as discrete symbols (identity) is a special case of a broader non-symbolic operation (embedding).
1226 Ibid., 113.
designs as “spatial objects [emphasis in the original].” A new formalism that Stiny defined in this paper, set grammars used rules called “spatial relations” to position or remove discrete objects (for example building blocks) in ways specified by the grammar’s spatial relations (rules). Stiny presented set grammars as a compromise between the spatial attitude of shape grammars and the symbolic requirements of computer implementations. Operating through identity, set grammars encompass the structural and combinatorial approach of graph-based architectural theories. Yet set grammars were presented as a case of a broader approach that went beyond points (symbols) and relations. In shape grammars, the structurelessness of shapes, which we heard many of this story’s actors berating, was reframed as an asset for creative design. It was seen as providing for ongoing perceptual restructuring of the elements that one was manipulating during design, as opposed to mechanical combination of fixed entities.

As I have shown, the condemnation of drawing in 1960s architectural modernism was partially instigated by a sense that working with shapes was uninformative for the profession’s truly pressing issue, namely the rational translation of functional requirements to physical form. This critique was voiced explicitly by Chermayeff and expressed through Alexander’s scorn of “doodles at the edge of […] drawing boards.” It re-emerged in the LUBFS Centre’s dismissal of drawing as an “an inadequate means for a rigorous testing of the design against the programme requirements.” It also echoed in Sutherland’s purification of drawings’ “dirty marks” through a structural, symbol-based rendition of shapes in computer graphics; a rendition that we saw technically constraining ArcMac’s subsequent work, despite the group’s

1227 Ibid., 114.
1228 Ibid., 113.
1229 Ibid., 114.
1232 Sutherland, “Structure in Drawings and the Hidden-Surface Problem,” 75.
espousal of the value of “wobbly lines.” The consensus was—to paraphrase Chermayeff—that working with shapes could not solve problems; that physical form essentially was, or ought to be, the result of some functional or other rational order. Because of its initial focus in art, shape grammars claimed for shapes an autonomy that architects may have found from inadequate to contemptible. Yet, the emancipation of shape from external determinants did not mean a complete disregard for the programmatic or material aspects of architecture. From its onset, shape grammars included the idea of rules operating in parallel with the visual transformations, which Stiny later articulated as “description functions.” Loosely, description functions associate two parallel sets of rules, one addressing the spatial and visual elements of a design (shapes) and a set of descriptions of purpose or other characteristics of a design, such as meaning or type. Applications of shape rules have implications for the descriptions set and vice versa. This parallelism between the visual and the functional, or more broadly the rational, implies that shapes can generate possibilities of function. In other words, unexpected results that emerge by virtue of the shape’s ambiguity can lead to programmatic and functional insights.

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Writing after the 1960s, philosophers and cultural critics have variously identified focus on abstract structures behind the appearances of things as the condition of modern knowledge.

1235 Ibid.
1236 The move from appearance to its underlying orders as a signpost of modern knowledge, and its associated cultures, is a recurrent theme with articulations too numerous to list here. I resort to a widely impactful perspective on the topic that became central reference to subsequent critical appraisals of modern knowledge. In his landmark 1966 book *Les Mots et Les Choses (The Order of Things)*, French philosopher Michel Foucault famously identifies the move away from appearances to some deeper order and organization of things as the condition of possibility of modern knowledge. Modernity, in Foucault’s account, begins with the rise of the nineteenth century. The modern “episteme,” a term that Foucault took to mean a condition that grounds the
This awareness has come alongside critiques to the stifling consequences that the shunning of concreteness and derision of perception bear on creative thought and action; on the production of novelty. Encapsulating such concerns, British philosopher Owen Barfield put forward in the late 1950s a project of “saving the appearances” as a route to productive imagination. In a quasi-spiritual tone, Barfield urged immersion in the world of perceptual surface—or “participation” as he termed it—as a way not to dispel, but to enrich rational thought’s capacity for discovery. The felicitous use of “participation” to denote active involvement in processes of figuration is evocative for critiquing and rethinking a central concept in this dissertation’s stories: participatory design. To the passive selection from preset menus of possibilities, it counterpoises a perceptually active engagement either in the process of design or of inhabitation as emancipatory practices.

possibilities of knowledge in a historical epoch, dispelled similitude—the operative term for 16th century knowledge—as deceptive and idolic (Foucault, Michel. 2005. The Order of Things: An Archaeology of the Human Sciences. Taylor & Francis e-Library, 57). In the new condition, broadly demarcated as “rationalism,” analogy was substituted by analysis. With it, came trust in the possibility of “complete enumeration” as opposed to the “infinite” “interplay of similitudes” (Ibid., 61). Complete enumeration and “the possibility of assigning at each point the necessary connection with the next” relied on, and impelled, the fixed discrimination of things.

Critiques to the surface-depth divide, or concomitant concrete-abstract dichotomies have taken the form of “post-”s and “turns” of various kinds whose nuances and interconnections are too varied to do justice in one footnote. I am referring here to large and nebulous intellectual currents such as postmodernism, poststructuralism, and more recently material turns in the theoretical humanities. Several of these intellectual movements, especially recent variations with names such as “speculative realism” or “object oriented philosophy” have embraced concrete objects but de-centered objects’ appearances and perceptions by humans as symptoms of anthropocentrism. It ought to be clarified, therefore, that the critiques to which I refer in my text continue to assume a correlation between concrete things and a perceiving subject. This aligns with the context of my discussion (architectural design), which includes such perceiving subjects (designers and/or “users”).


I use the term broadly to encompass qualities such as “new,” “emergent,” “unforeseen.” The term “productive imagination” runs deep into Kantian philosophy. In loose summary, Kant construed “intuition” as a conscious, objective representation (as part of “cognition,” as opposed to the subjective “sensation”). Opposite to concepts, however, which implied analysis and classification, intuition was singular and immediate; it related directly to the perceived object. The faculty of intuition was “sensibility,” which consisted from “sense” (operating in the presence of an object) and the “power of imagination” (operating in the absence of an object). “Productive imagination,” in this context, was the ability to visually imagine something not previously experienced, something new. For a more extensive description and references, see Janiak, Andrew. 2016. “Kant’s Views on Space and Time.” In The Stanford Encyclopedia of Philosophy, edited by Edward N. Zalta, Winter 2016. Metaphysics Research Lab, Stanford University.

Barfield, Saving the Appearances.

Ibid.
The complementarity of the “rational” and the “intuitive” is a project that several actors of this story professed to embrace. A few chapters back, we heard Leslie Martin quoting Whitehead’s remarks about the confluence of rationality and intuition. Basal endeavors to systematize “design” in the 1961 Case Conference or the 1962 Conference on Design Methods sanctioned such coexistence. Their participants incanted their commitment to “imagination” and “creativity,” which they cast as the epitome of design. However, their quest was one of instilling rationality in a discipline that they saw flawed with an overabundance of intuition and an overt preoccupation with appearances. This was reflected in the conceptualization of novelty within these debates as a combinatorial operation on a preset structure. As if having anticipated mid-twentieth century architectural theory, English romantic poet and literary critic S.T. Coleridge juxtaposed “imagination” with a mechanical combinatorial operation that he labeled “fancy.” Coleridge presciently described fancy as “no other than a mode of Memory emancipated from the order of time and space” that operated through “choice” and “receive[d] all its materials ready made from the law of association.” 1242 “Imagination,” on the other hand was a power that “dissolves, diffuses, dissipates [...] in order to recreate,” 1243 a power that “shapes into one” 1244 and “conveys a new sense.” 1245 This definition implies not an appearance that occurs on top of a fixed structure, but an appearance that restructures; that defines new entities and relationships in one single act of perception.

My elliptic exposition of shape grammars, alongside affine intellectual attitudes, is meant to suggest what I think history ought to: that things could have been, and can be, otherwise. The

1243 Ibid., 304.
1244 Ibid., 168.
1245 Ibid., 169.
ostracization of shape is not mathematization’s deterministic outcome. Mathematics and computation need not signal dematerializing abstraction or purified intellectual vision. There is space for other kinds of vision, of the perceptual kind. But first, we need to make amends with shape and unabashedly embrace the surface — to be superficial, as the philosopher and poet wanted it, out of profundity.\textsuperscript{1246}

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Theodora Vardouli: I wanted to start by taking you back to the time before you started your studies in architecture at Cambridge. I am interested in the specifics of your mathematical studies in Cambridge University and in high school: the classes that you took; things that you were thinking about; mathematical problems that excited you...

Lionel March: I joined Architecture after doing one year of Mathematics at Cambridge, and I joined with Chris Alexander and Bill [William] Newman, the computer scientist. So the three of us who had basically been involved in Mathematics joined [Sir] Leslie Martin’s first class. I had gotten into Cambridge for Mathematics and then did two years of national service in the Royal Navy. I’ve always argued —well, used as an excuse— that Mathematics is a very athletic intellectual discipline and if you stop practicing for any time it kills the Mathematics off in you. You really need to keep going and keep right in the front page of what you are doing.

I had in fact done a piece of work at school, of original work, without any schoolteachers involved at all, in extending the idea of individual numbers. So whereas complex numbers are two-dimensional, I thought why not three- and four- and five- and n-dimensional. Then my original work there got, and I don’t know how, in the hands of the science correspondent of the daily express, and he then sent it on to the University College London. They looked at it and they suggested it should go up to Alan Turing in Manchester. And I then had a couple of letters from Alan Turing and it happened that Alan Turing was very close to somebody called Babbage, Dennis Babbage, not Charles of course, who was then working in Bletchley. My understanding was that Turing recommended me to Dennis Babbage who became my supervisor. So, I think I got to Cambridge partly because of the original work I did when I was at school on n-dimensional numbers. And Alan Turing did comment that it was very commendable work, so I think that’s how I got into Cambridge.

Then because my first year in Mathematics was actually just about everything I had done when I was at school, it was immensely boring because I had already done it! I had this first year doing it all over again. By that time, I was no longer the athlete that I had been. I found the mathematics classes very boring and got involved in stage designing. So my first year was doing a couple of sets for stage designing for the University’s Dramatic Club, it was called the ADC [Amateur Dramatic Club]. It was there that one of my directors asked me if I knew that the School of Architecture was having a new professor called Leslie Martin. I didn’t, but there was a long article by Sir Nikolaus Pevsner on who was then Leslie Martin. Pevsner was saying that it was a good thing that Martin came to Cambridge and somebody of his distinction was a director of Architecture. So I went and saw Martin. The thing that I remember is that he asked what books I had been reading while I was in the Navy. One of them was Gropius on the Bauhaus. Leslie Martin said “oh yes I know all about that; it won’t be repeated,” he said, “there was no repeating of the Bauhaus.” “What we can do in Cambridge,” he said, “will be completely different, it won’t be the Bauhaus; I know that for sure.” Because before the Second World War
Martin had been very involved with émigrés from Europe, including Gropius, who had gone through London and had met with him and Sadie Martin, on the way through. This included painters like Mondrian and so on. So Martin had been very involved with all these people and of course that appealed to me. He then sent me off to see the director of studies at Magdalene College, which is the college where Dennis Babbage was in as well, and I interviewed there and basically the senior student there said to me: “Well March, are you determined to do this Architecture?” I said yes. He said, “well you have a choice – there is a good chance if you want to do Architecture and this means changing your subject, that it will send you down” --that was a little ugly—“because you got into the University to do Mathematics and that’s what we expect you to do” (laughs). But everything went well. Obviously, Leslie Martin was interested in me and David Wyn Roberts who was the supervisor of Architecture at Cambridge was interested in me and after one year in Mathematics and doing all the stage design and getting involved in theater, I went into Architecture. And that’s where I was in the same path as Chris Alexander and Bill Newman. Bill Newman by the way was the son of Max Newman who had appointed Alan Turing to Bletchley and had been the professor of Mathematics after the war at Manchester where they were building the first computer. So they were all connected.

**TV:** To backtrack a little, you said that in your first year in Cambridge you were offered the same subjects in Mathematics that you were being taught at school. What Mathematics was that? Was that modern Mathematics? In your books there is a lot of discussion on set theory and graph theory. Did architects know this kind of Mathematics at the time?

**LM:** Yes, it was that kind of Mathematics. I had a wonderful professor, basically of geometry, but he was really describing graph theory and he called these little objects that he drew on the blackboard “creatures.” We had this thing, the graph, and he called it a creature. He was absolutely wonderful. He was certainly well into his sixties. He was called [Henry Frederick] Baker and he had published the principles of geometry in about seven volumes with Cambridge University Press. Maybe four volumes, I always thought it was seven. It was about geometries that went off into n-dimensions. So that was quite different from what we were taught at school. But the thing was that Mathematics was a sort of rising sport when I was at school.

**TV:** So then when you moved to architecture, was the architecture school curriculum a kind of “traditional” curriculum? Did you have connections with other departments, did you take subjects outside of architecture, or was it more of a self-contained program?

**LM:** Forget Architecture and be certain that I was at Cambridge, and Cambridge was very fluid. In fact people changed disciplines all the time at Cambridge. They took what was called triposes, which was one-year courses. And so my friends went to the Natural Sciences, to study English, and all kinds of things. People moved around. The important thing was you were being educated in Cambridge, not really in a discipline very much. This gave you a lot of freedom. It was very good. And so architecture itself because it was Leslie Martin’s first year it was basically the same old stuff as before I joined. I mean, he could not change everything. He had lecturers who were there before he was appointed and so on. So it was fairly traditional, like university architecture that was being taught elsewhere.

**TV:** You traveled at some point in the United States, was that right after your studies?
LM: That was after I finished my basic undergraduate and graduate degree as well so it must have been ... I can’t remember the date exactly. But it was a Harkness Fellowship of the Commonwealth, which was given to people in all disciplines. I happened to be the single architect that year [...] I stayed in the United States, Boston, for two years. I left Boston after one year and then traveled and then came back to Boston and then left. By that time Leslie Martin had written me a letter asking if I would come back and help him on the Whitehall proposal, which I did. I probably left about a month or two earlier than I would be normally expected.

TV: Did Whitehall start the Land Use Built Form Studies Centre?

LM: Let’s go back a little. Many Harkness fellows take degrees when they go to the States to do the Harkness Fellowship. I didn’t take a degree. I joined the Joint Center of Urban Studies between MIT and Harvard, which was the hot intellectual thing going on at the time. It was while working in the Joint Center that I saw all disciplines across the subjects of urban studies coming together. When I went back to England that was the model that I carried with me. It was the Joint Center idea.

TV: Was that a different model than from was customary in Cambridge? Was there infrastructure in Cambridge for building a lab that would bring together different disciplines?

LM: No it wasn’t actually, so much. Two things were going on. In London there was Centre for Environmental Studies, which had Alan Wilson as the assistant director and an engineer, Henry Chilver, who became Vice Chancellor at the Cranfield University. They were multidisciplinary around the idea of urban studies, and they were supported by the Ford Foundation that incidentally also supported the Joint Center at Harvard and MIT. Something was going on there in the high level of financing these groups. It was the Centre for Environmental Studies that got money from the Ford Foundation to distribute that money to various places. Leslie Martin and I went up to the Centre for Urban Studies and they decided to give money to the Land Use and Built Form Studies Centre in Cambridge, so we had financial backup there.

TV: Looking at the LUBFS Centre’s trajectory, would you say that there were major changes from its foundation, and through the years, in terms of size, the kind of research that you were undertaking, the people who were participating?

LM: When I came back from the U.S. I worked with Leslie Martin in the Whitehall proposal. In parallel to that in the School of Architecture Philip Steadman, Peter Dickens, and Nicholas Bullock were working on a university study that Leslie Martin had set up with the Gulbenkian Foundation. So that existed. I then was added, if you like to that group. There were three of them and me a fourth. I was older by a couple of years in university terms and became the director of what was going to grow into a bigger thing called Land Use and Built Form Studies. Land Use and Built Form Studies had developed out of what we did at the Whitehall project. So that was the founding group and everything developed from there.

TV: The projects that the LLUBFS Centre started with, the Whitehall project or the university study project, were in a very broad sense “applied.” But you also undertook a lot of theoretical
research in Architecture and modeling or representation. I was curious how those two streams, the applied and the theoretical work, co-existed, what their relationship was.

LM: (Pauses to think) Well, most of the work was supported by our Science Research Council, the Social Science Research Council and then the Physical -- the Research Council that mainly supported physics and Mathematics and the like. The [LUBFS Centre] staff grew rapidly until it actually exceeded the teaching staff in the School of Architecture. It was entirely supported by research grants. For example, Patricia Apps who had gone through Yale before it burned down -- all her records had been burnt and she came to Cambridge without academic records—[...] specialized on the housing market. She went on to the London School of Economics and developed her economic skills and then back to Australia and became a planning economist in the School of Law in the University of Sydney. I always thought very strongly that whatever we did was educational and went across disciplines. I was very careless about disciplines. If you were intelligent, you could work on something and then certain fields would interest you and give you some background as with Patricia. And the was another woman, Helen Couclelis who was Greek and had been short listed for the Greek equestrian team for the Olympics at the time – she didn’t in fact fully qualify for the Olympics but she was the one under consideration. She came to me when I was lab director and said that she wanted to do a PhD and she wanted to challenge all the work that we were doing on urban systems. I said “This is exactly the thing I want!” I want somebody here to criticize what the mainstream is doing, and she did precisely that. Then she went on to become Professor of Geography at the University of California in Santa Barbara and she was working with Michael Batty when he was at the University of California Santa Barbara. So there was a big sort of mixture going on all the time. I was very close friends with Michael Batty when he was running the Research Unit at the University College London.

TV: You spoke about crossing disciplines and having a common ground in speaking with people in different domains. I was wondering what you think about the role of mathematics as a kind of common language between different disciplines. Were there particular mathematical ideas that helped you establish this common ground? I would like to hear your thoughts about the process of crossing disciplines and the ways of doing that.

LM: Well, I certainly was somebody who saw mathematics as underpinning many disciplines through subjects such as anthropology, sociology, and others. There were lots of books coming out on mathematics and sociology or anthropology. Also, archaeology -- we had various people who pioneered that... And geography... Major books came out on the mathematical principles underlying these disciplines.

TV: I was reading an article by Lévi-Strauss called “The Mathematics of Man,” from 1954, where he was talking about the difference between quantitative and qualitative Mathematics. By “qualitative mathematics” he meant mathematics of structures and relations. It seems to me that graphs and sets were also coming with a different way of thinking about various disciplines and what they could do for these disciplines. It seems that structure, in general, was a key concept -- I was wondering what it meant for you both mathematically and in Architecture and who else was talking about it at the time.
LM: Oh well, at the time, in school mathematics there were books coming out with the title the “New Mathematics.” And in fact the *Geometry of Environment* was written as a result of me being called to attend a meeting of the Royal Institute of British Architects Library Committee. And the person who was asking the questions was Peter Smithson because he had a boy who was at school doing the New Mathematics and he found it all very mysterious. And he [P. Smithson] was of the age of being taught calculus or things like that rather than set theory. That was the origin of the book; that surely Architecture could do with a book explaining the new mathematics and possibly its applications to Architecture. So that’s how that book came about.

TV: What was the book’s reception? Did architects use it?

LM: (Laughs) I have no idea! It actually has survived. I know that much. And it is cited over and over again, so I presume that it is in use. And it was also the coming of computers ... things like graph theory and quantitative mathematics, Boolean algebras, which Philip Steadman made use of in the big book on building forms. You see how it is being used. And whenever architecture is being created on computers, behind it all it is exactly that Mathematics. Even if the user quite often doesn’t know it.

TV: Indeed! I was just trying to get a sense by who the book was mostly read, was it read by architects, by mathematicians, by computer engineers?

LM: I know that at the time it was used in the Physics Department in Cambridge University by Lord Hunt of Chesterton, who went on to be director of the National Meteorological Office. He was the Director of Physics at the time. He did fluid dynamics and he taught set theory using our book (laughs).

TV: Other researchers in that period were referencing people like Frank Harary or other mathematicians who were promoting the use of graph theory. I was wondering if there were eminent mathematicians that you intersected with, worked with, or had quarrels with...

LM: Off the top of my head, I know that I wrote an article with Harary. He was the leading popular graph theorist at the time. I mean if you looked at the libraries of the time for graph theory then certainly Frank Harary’s name would come up. And I wrote a paper with him. Then we had somebody up in Aberdeen who was doing chaos theory that I was involved in. Then there was [Ron] Atkin who was doing the multidimensional man. I had quite a lot of connections with him. And in fact in the end I appointed someone who he had as a research student, called Jeff Johnson, who is professor of complexity theory at the Open University now. [...] 

TV: Again along the lines of thinking about the climate of the period and people that you were in contact with, I was wondering if you had connections with design research and design methods groups. Philip Steadman seemed critical of design methods.

LM: (Laughs) Yes he is! The whole group was pretty skeptical of the so-called design methods.
TV: Did you have contacts with these groups? What was different in your approach from the work of, for example, Christopher Alexander or other people undertaking architectural research at the period.

LM: I did have contacts with design methods at the Open University. This was because the person under me, who was reader of Design, was a big enthusiast of design methods. The one article I wrote “The Logic of Design and the Question of Value,” mentions Chris Alexander and has a sort of critique. […] 

TV: I wanted to close with your reflections on the legacy of LUBFS. Looking back at the trajectory of the group were there things you would have given more emphasis to, or would have done differently, or open ideas worthwhile exploring today?

LM: We spread our interest across different scales. So the office studies that Dean Hawkes and Philip Tabor were involved in were about an individual kind of building – the office building. And then the university study was dealing with clusters of buildings, campuses and so on. Toward the end there was a very interesting study about the use of time by the students of the university -- time in traveling between different locations in the university and the university and town and so forth, which Philip was again involved with. In fact the so-called university study developed into these diaries that students kept documenting where they went and when. Then there was of course the urban scale, which Marcial Echenique was involved in. We had quite a lot of visitors at the period. We had a lot of Latin Americans, mainly Venezuelans […] We had a big conference in Caracas that was joined by Michael Batty who never joined our unit but was part of the people that the Centre for Environmental Studies was sponsoring. I went, Marcial went, and we spent a whole week giving lectures in Caracas with people from all over from Latin America […] That Latin American group was very important especially in developing the urban models, which Marcial was in charge of. We had a geographer involved in that particular group in Marcial, he was quite important.

TV: Was there a political angle to some of the urban modeling work? -- even if it wasn’t expressed in the models themselves. There were for example popular ideas such as participation in design etcetera at the time. Did these enter LUBFS?

LM: The Venezuelans would certainly be on the left. What was going on in the Joint Center of Urban Studies that I was in, with heavy governmental financing, was all very political. The Ford foundation was involved with it. That was on the side of capitalist development. And the people that came to Cambridge through Birmingham – they did their masters in Birmingham and came to us to do their PhDs were not at the time capitalists, they were definitely left wing. They spent their early time in Venezuela parachuting into the jungle! (laughs) We had a wonderful crowd of people. Alberto Feo was the leader’s name. […] 

TV: As we conclude the interview, I wanted to go back to the mathematical ideas that you used and their potentials or limitations. You spoke about sets and graphs. Are there other mathematical ideas that you would have used instead or that excite you now? I am interested in your reflections on the mathematical underpinnings of the work and what they meant for you back then, or what they can mean today.
**LM:** When I went to the United States, to UCLA, after the Open University I found that team research of the sort that I had directed in Cambridge was not existent. At UCLA, you were left on your own to do your own thing. This is very much what, for example, George Stiny felt very comfortable doing. So when I got to UCLA I toyed with the idea of working in a group and it never worked out, so I ended up doing a traditional scholar approach to Alberti and Palladio, who were both mathematical. Both had been reported on being good mathematicians by people who wrote about them at the time, and so I found that sympathetic. I studied a number of papers on Alberti and on Palladio and confronted Rudolf Wittkower who had come up with a very simplistic argument about proportions and so on in Palladio. If you look at his work he uses rather complex proportional systems -- cubic root of two, he definitely uses that. So most of the work before I retired was on Alberti and Palladio and the proportional theory of the Renaissance and the mathematics. The arithmetic of the Renaissance is often just ignored, and so the Renaissance material is studied from the idea of modern arithmetic which is very curious. There are many things in the arithmetic that have stayed the same but it is the way that they thought about numbers and used them that was very different in the Medieval and the Renaissance period. Fairly recently I wrote a paper which you may have seen, analyzing a famous book whose authors were not known and I wrote about various reasons why the author should be Alberti. Anyways, so I went from being a team research director at Cambridge based on what I had seen going on at Harvard and MIT in the Joint Center back to being an individual scholar. So sympathetic research going on, first was George Stiny’s or Terry Knight’s, even Chuck Eastman’s at Los Angeles [...]

Interview with Professor Sara Ishikawa
Interviewer: Theodora Vardouli
Date: October 13th, 2016
Medium: Email
Note: Prof. Ishikawa answered some of the questions by referring me to the histories of A Pattern Language and the Center of Environmental Structure that she and Murray Silverstein presented at the 2009 Conference of the Portland Urban Architecture Research Laboratory, at the University of Oregon. These questions have been omitted from the interview.

Theodora Vardouli: Before co-founding the CES, you had already spent several years in the Berkeley College of Environmental Design, first as an undergraduate student in Architecture and consecutively as faculty member. What did the Architecture department’s curriculum look like at the time? Did students typically take subjects outside the department (e.g. mathematics, philosophy, social sciences...) and where? Did the courses that you took as a student or taught as faculty member inform your subsequent research in the CES?

Sara Ishikawa: My degree at Berkeley was a Bachelor of Architecture - which was a professional degree - there wasn’t much of a graduate program except in history and structures. I majored in art for two years before entering the architecture program which gave me the chance to take courses in the social sciences which I was interested in - especially sociology, anthropology and psychology. The program now is a four/two program - four years Bachelor of Arts and two years Master of Architecture. This four/two program came about largely to address the need for students to have a broader background.

TV: After earning your BA of Architecture, did you transition directly to teaching or did you also work professionally as an architect? What was the relationship between teaching, practice, and research for a young professor of Architecture at Berkeley? Could you give me a sense of the meaning and status of architectural research in Berkeley in the mid 1960s and its relationship to environmental design research at that time?

SI: I was working my way though school and wanted a part time job in an architectural office - this was near impossible largely because I was a woman. After two years in the architecture program, I decided to take a break in 1957 and visit a friend in Stockholm and was offered a job there in an architectural office, I worked there for 2 1/2 years and returned to Berkeley to finish my degree and was able to get a part time job in Hans Ostwald’s office in Berkeley where I gained experience in all aspects of architectural practice (which helped once we started doing projects at CES). The experience in Stockholm was very important because it was there that I became interested in housing and community planning and was exposed to research in housing.

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and town planning for the first time. Prior to my degree in 1963 there was little research going on here that I was aware of and I had a hard time getting help in the research in housing that I was involved in for my fifth year thesis, and so I was very taken by Chris’ [Alexander] lecture at the school the summer I graduated (A city is not a tree), as it was the first time I heard of a theory that could help answer the question, how should one go about designing something. The 60’s was a time of change. The School of Architecture was going through a huge change at the time - restructuring the school into a College of Environmental Design with departments of Architecture, Landscape Architecture and Planning. Of course there was an awareness for the need for research in architecture and landscape architecture (Planning was already involved in research). There is documentation on how the College of Environmental Design came about - as it recently had its 50th anniversary.

TV: The CES was founded in order to perform a dual task: “to develop a library of environmental patterns and to construct a coordinated environmental pattern system,” or as Stephen Grabow describes, in order to develop patterns (structural descriptions of timeless environmental situations) and describe the generative structure that binds them together. Would it be fair to say that the first part (developing patterns) was more empirical/intuitive and the second (describing their structure) more rational/theoretical, even mathematical? If this is a valid distinction, did it correspond to a division of labor within the Center? Were there specific CES members that engaged with the first part and others who primarily worked on the second part?

SI: True that developing patterns was more empirical/intuitive and the second more rational/theoretical. There was no division of labor among the three of us (Chris, Murray [Silverstein] and me) although Chris was definitely the leader in both areas.

TV: Did working on the pattern language’s development require mathematical fluency? Were all the members of the Center as mathematically adept as C. Alexander? How often did the CES participants have to engage mathematical reasoning and/or calculations, and for what types of questions? What kind of mathematics, if any, did you use at the CES?

SI: Working on the pattern language’s development did not require mathematical fluency. No one at the Center was as mathematically adept as Chris.

TV: What was the role of computers and computation in pattern language research? Was there a computer at the CES? If yes, what did you use it for? In his early 1960s work, C. Alexander had used graph theoretic representations in order to encode design requirements in a computer. Was there ever an aspiration to turn the pattern language into computer program? If yes, did graph theory come into play again in these efforts?
SI: There was no role of computers or computation in pattern language research. There was no computer at CES. Before we settled on the book format for APL, we experimented with a loose leaf format for patterns and we were aware that patterns could be computerized, as a way of getting people to participate in pattern development and improvement much like Wikipedia, but none of us wanted to monitor or manage such a system so we decided on a book format calling the collection *A Pattern Language*.

TV: The Japanese Teahouse experiment is an example of computer use for finding a correct sequencing of patterns. I am interested in hearing your memories about this project. What type of computer did you use, who programmed it, what was the outcome, what conclusions did it lead to about the role of computation in the pattern language?

SI: The Japanese Teahouse is a clear example of a pattern language - most teahouse complexes in Japan have these patterns in them - we experimented how these patterns should be sequenced to give us insight in how patterns in general should be sequenced when designing. There was no computers involved in these experiments.

TV: In reading social scientific or architecture articles from the 1960s that engaged with mathematical modeling, I could not help but notice an explicit optimism about the “new” mathematics of sets and graphs. Indeed sets and graphs seem to undergird many new theories and methods in design developed during the period that the CES was active. Can you talk about the status of graph theory and set theory during the 1960s and 1970s, in the way that you perceived it back then?

SI: I was not involved in set and graph theory. As I understand it, Chris did most of this work prior to coming to Berkeley. Now, as I review Grabow’s bibliography - some of this was done with Marvin Mannheim (HIDECS 2) who was one of the people at the Inverness seminar.

TV: In the pattern language, the concept of *structure* figures center stage. Could you unpack the meaning of structure as you construed it back then? How did the concept of structure as understood in CES relate, if at all, to the vogue of continental structuralism? What about systems theory? Did you understand the Centre as participating in (a) particular lineage(s) of thought, and which?

SI: It was quite clear that the environment had a structure to it - this is something that Chris has discussed in most of his articles, and we have tried to describe in the Introduction to APL and other publications, Chris also wrote an article called “Systems Generating Systems.”

TV: A key hypothesis underpinning the CES activity was that “environment” and “society” mirror each other, or to use a mathematical term, are isomorphic. Did the notion of “structure”
help bridge these two domains and how?

SI: I would say so.

TV: Developing the pattern language involved constant evaluation of the patterns and their sequence through extensive experimentation with non-architects. Alexander has described that patterns that carried the desired “quality without a name” would immediately be perceived as familiar by a non-architect. Evaluating the pattern language was therefore, to a great extent, about this feeling. Was this idea of feeling a purely intuitive one, or were there conversations among the CES members about a possible mathematical description of such feeling? How did feeling and quality relate with structure?

SI: We were aware of the fact that good, deep patterns had a feeling of wholeness, were balanced, complete, and timeless, and that most people also saw them that way. Structurally, each pattern was part of some larger pattern, and contained other patterns and that this structure also had a feeling of wholeness.

TV: Christopher Alexander declared his break from the traditions of interwar and early postwar modernism. How do you see the CES as breaking from modernist thought in architecture? If you were to place the CES in a history of architecture and in a history of science compendium, where would you place it?

SI: There was a general admiration for traditional environments which for the most part, were not designed by architects but by the users, craftsmen, and traditional patterns about which people were knowledgeable and familiar. We had great respect for Frank Lloyd Wright, who seemed to be using a pattern language in his designs and was in touch with what would work for people and what people related to and desired in their houses and workplaces.

TV: In browsing the first issues of the Design Methods Group (DMG) Newsletter, one finds numerous references to C. Alexander’s and to the CES’s activity. An interest in systematic understandings of design was picking up during the period. What was your relationship with the DMG? What was the CES’s relationship with the DMG? How about other centers or labs performing architectural or environmental research internationally (for example the Land Use and Build Form Studies Center in the U.K., the CAD group at MIT, the Environmental Design Research Association and others)?

SI: I believe that some of the people in DMG were ex students of Chris’. There are articles about design method entities and how we were related to them - see, for example, Interview with Max Jacobson, “a Refutation of Design Methodology” in the bibliography of Grabow’s book.
TV: In the late 1960s many groups or individuals developing rational and mathematical approaches to design (such as the ones mentioned in question 19) shifted toward humanistic/participatory perspectives. Instead of empowering the designer to design better, it became about empowering the “users” to choose living arrangements more adequate for their own lifestyle, or change their living settings according to their wishes. Such “turns” occurred against a backdrop of social and civil rights struggles, the questioning of technocracy and positive science, environmental concerns etcetera. Was the participatory aspect present in the CES from the beginning or did the shifting context play a role in bringing the participatory component center stage?

SI: Participatory design was an early interest - we came up with quite an elaborate system whereby users could experience it in the difficult context of mass housing in the Peru Project - see the chapter on the Choice Process. The whole idea of the pattern language was to provide a means by which users could design their own environments.

TV: I am interested in hearing your thoughts about the CES’s relationship with American counterculture. The *Whole Earth Catalog* made several references to patterns and there was also a conceptual convergence around the pursuit of “wholeness.” Am I merely pointing out a superficial similarity or were there deeper links and concrete avenues of exchange?

SI: The *Whole Earth Catalog* and Stewart Brandt became interested in the pattern language independently. We had no ties with them except shared values.

TV: What do you see as the main legacy of the CES’s work, intellectually and technically?

SI: I think this is a question for you and others to answer.

TV: In my understanding, one of the main aspirations of the pattern language was to be generative and open-ended. How did these requirements impact the way you set up the language? Do you think that there is any alternative to the combinatorial/structural approaches developed in the CES? If you were to build a new Center for Environmental Structure today, what kind of research would you envision?

SI: APL was to be open ended and generative, and I think its success as such is exemplified by the fact that there is still enormous interest in APL - not only by designers, but laypeople, computer programmers (PLOP), and other disciplines as a problems solving method.
Interview with Professor Philip Steadman

Theodora Vardouli: I wanted to start by going back to 1960 when you enrolled in Cambridge University to study Architecture. I am interested in the type of coursework that you undertook during your architectural training and if/how this coursework informed your subsequent research, first under Leslie Martin and then in LUBFS. As a student, did you take subjects outside the Architecture Department (e.g. mathematics, philosophy, social sciences...), and where? Was extra-departmental coursework typical for students in the University of Cambridge?

Philip Steadman: The architecture course at Cambridge was almost completely self-contained, and we had very little contact as students with other parts of the University. In this respect the teaching was quite unlike say the modern American university with electives and options outside one’s main subject. The architecture course was in large part studio-based, with lecture courses in architectural history, structures and building science/construction. The Faculty embraced fine arts as well as architecture, and there was a modest amount of contact with the fine artists, and a joint library. There were occasional lectures given to architects by members of the Faculty of Engineering, on structural engineering topics. I attended public lectures elsewhere in the University on art and other subjects. But I had no contact at all with the departments of mathematics, social sciences or philosophy. My interests in mathematics and physics were formed at school, where I specialised in those subjects, along with art.

TV: You have described that upon concluding your architectural training in 1965, you transitioned immediately into research in university planning under Leslie Martin. What drove this decision? Was your decision to pursue research in architecture an uncommon trajectory for an architecture student in Cambridge, and in the U.K. more generally? Could you give me a sense of the meaning and status of architectural research in Cambridge University in the mid 1960s (was it well established, well regarded, peripheral to the discipline ...)?

PS: I would say that I had an inclination for research through having an abstract mind and an interest in theoretical questions. I had pursued these interests in my Diploma thesis (roughly equivalent to a Master’s) and in my own private reading. There was however practically no tradition of research in architecture at that time – in the sense in which I think you mean – either at Cambridge or elsewhere in Britain. There was research in architectural history, and some activity in traditional building science (the physics of heat, light and sound in buildings). But the idea that one might do research in architecture itself was very new – which was of course a great part of its attraction. I embarked on a PhD. But a PhD in Architecture was a very rare beast at that time. There was of course a lot of scepticism about the possibility and value of architectural research, both among practitioners and in other disciplines.

The reason for working with Leslie Martin on university planning, specifically, is that he was involved in designing several university buildings, and had contacts with the University Grants Committee and the Gulbenkian Foundation, who funded the research. A number of new universities were founded in Britain in the 1960s and there was a very large expansion in student
numbers. The research looked at some of the architectural and planning issues raised by this programme. Leslie Martin invited me and my colleagues Nick Bullock and Peter Dickens to work on the subject, I suppose because he saw us as students who might have a bent for research – an intuition in which he turned out to be correct. All three of us became career academics.

**TV:** You became a member of LUBFS from its foundation and stayed on to become assistant director in the Martin Centre. Did you witness changes in the LUBFS Centre’s composition and structure throughout these years? How about changes in driving debates or research foci in the Centre? Could you speak to the institutional, technological, intellectual conditions that drove these changes?

**PS:** LUBFS lasted less than a decade (1967 to 1974). The centre expanded very rapidly at first, and the main changes were organizational – a widening of the subjects being studied, and the arrival of more staff, some of them from overseas. From an initial focus on architecture and building science, the centre expanded into planning and urban studies. Leslie Martin and Lionel March saw a continuity between architecture and planning, and were interested in the intermediate scale of what would now be called urban design. There were many debates over detail, but the overarching emphasis in the work was on mathematical and later computer modelling. The purpose was NOT to mechanise or automate the design and planning processes, as I have tried to explain in my article in the *Journal of Architecture*. It was to try to pioneer an architectural science, and to contribute to the established tradition of urban and regional science; to produce knowledge and develop tools that would inform and support designers and planners. In the *Journal of Architecture* piece, I have described some of the intellectual and technological context, including the nascent state of computing in Cambridge University in the early 1960s, and interests among parallel disciplines such as geography, social sciences and archaeology in systems-theoretic and modelling approaches. These we followed mostly by reading books produced in those disciplines, although we also made some personal interdisciplinary contacts at Cambridge, especially in computing (or what was then called the Mathematical Laboratory). The staff of LUBFS was drawn in the first place from architecture, but over time we were joined by computer scientists, mathematicians, planners, geographers and statisticians.

**TV:** In your recent essay in the Journal of Architecture you talked about the co-existence of LUBFS work devoted to practical issues with more abstract mathematical modeling and theoretical activity. What was the relationship between “applied” and “basic” research in the Centre (if you think that these terms are valid)? Did each correspond to different (or competing) cultures of research or did the two approaches co-exist harmoniously? Who decided about the kinds of projects that LUBFS researchers pursued?

**PS:** There are some important points to be made here about the wider context of the work at LUBFS. There were the very beginnings of the application of computers to research, as I’ve mentioned. There was an interest - shared with many disciplines outside architecture and planning - in mathematical modelling, systems theories and structural approaches (if one can lump all those together). And then there was the fact that several departments of the British government (Housing, Health, Education, ‘Public Buildings and Works’) employed architects and planners who were receptive to the products of research and whose organisations provided
much of the funding for LUBFS. We had some grants from scientific research councils at a later stage. But the majority of the early financial support came from central government. You should appreciate that this was in the context of the post-War settlement in Britain, the creation of the Welfare State after 1945, and a general belief that architecture and planning could contribute to the common good. This was also the time when sceptical questions were only beginning to be asked about the philosophy and aspirations of architectural Modernism.

So in this sense a great deal of the work at LUBFS was ‘applied’. It was paid for by government and its direction was specified by civil servants. The original ‘land use and built form studies’ on density by Leslie Martin and Lionel March grew out of Martin’s commission to develop a plan for government buildings along Whitehall in London. Otherwise, decisions about what directions in research to pursue were made by individuals or groups of researchers, not by Martin and March alone; these decisions were guided by individuals’ interests, but steered by pragmatic considerations about how that work could be funded. It may seem contradictory, but there was nevertheless at the same time a general concern with theoretical issues and basic research. One of the phrases that was often bandied about was Kurt Lewin’s remark ‘There’s nothing so practical as a good theory’ (although we attributed it to Bertrand Russell). There were parallel activities, some of them funded externally, others that were not funded. The work on ‘built forms’, on mathematical forms of representation, the work that Lionel March and I wrote up in The Geometry of Environment – all that was done without special funding, since what was needed was just pencil and paper. It was the projects requiring the purchase of computers, survey work, taking on research assistants and so on that needed external money.

So in specific answer to your question, the ‘basic’ and ‘applied’ aspects of the work co-existed and indeed depended on each other. They did not I think compete.

TV: I would now like to transition to the content of your research. Could you provide a brief chronology (with key projects, if possible) of your personal research’s evolution within LUBFS — from university planning, to activity simulation, to rectangular dissections and plan cataloguing? Do you see connections among all the projects that you engaged with? Did your own research go through shifts/breaks or do you see it as a continuum?

PS: As I remember I was actually employed on the university planning work, and then on a couple of projects connected with ‘time budgets’ and models of people’s activities and movements during the day and week. I also went for a year to Princeton, and was employed for a year in the School of Architecture where I taught first-year studio with Lionel March. During all this time I was working in parallel on my thesis on biological analogies, which ultimately became a book, The Evolution of Designs: Biological Analogy in Architecture and the Applied Arts (Cambridge University Press 1979). And I was working (unfunded) on the mathematical questions that you list, including applications of graph theory to plan layout and the enumeration of rectangular dissections. Some of this activity found its way into the book that Lionel March and I wrote, The Geometry of Environment, published by the RIBA in 1971. That book has fourteen chapters, of which Lionel wrote chapters 1 to 7 and I wrote 8 to 14. So you will see if you look at the book that it was I who covered graphs, networks and plan layout.

You ask about connections between these areas of work. I think I saw them as relatively separate at the time, although they were connected philosophically I suppose by a set of overarching ideas about the proper role of mathematical and computer modelling [sic] in architectural research – ideas that I tried to indicate in The Journal of Architecture. The Evolution of Designs was in part a history of ideas in architectural theory, in part a polemical argument about what I thought an
architectural science should be (and what it should not be). This was the main purpose of the two chapters on Christopher Alexander, and the two concluding chapters on the role of biological concepts in architectural history and science respectively.

TV: A striking element in your publications, at least for the contemporary reader, is the mathematical fluency that they exhibit. Did you acquire this mathematical knowledge as part of your architecture studies in Cambridge or was this something that required further training? Were all LUBFS researchers as mathematically adept, and was this a prerequisite for joining the Centre? Did you collaborate or consult with mathematicians when performing your research?

PS: It is kind of you to speak of ‘mathematical fluency’. I have actually always thought of myself as something of a mathematical amateur. I collaborated at one time with an Argentinian architect friend Leonardo Combes, who said to me: “Phil, we are the Robinson Crusoes of mathematics,” picking oddments washed onto the beach from some mathematical shipwreck. My knowledge of mathematics came from my school education, where as I mentioned I specialised in the subject. I will say a bit more about this, in answer to your question about the ‘new maths’ below. I don’t think I acquired much more knowledge of maths in the Cambridge architectural course. My mathematical education beyond school was largely self-education. I found and devoured books on graph theory, of which there were then rather few. Such as there were, were quite accessible, before the subject became extremely technical.

There was no requirement for researchers at LUBFS to have mathematical skills, and levels of knowledge varied I would say. We worked with some applied mathematicians, in particular Crispin Gray, Richard Stibbs and Robin Forrest, all of whom had subsequent careers in computing and CAD.

Lionel March knows a lot more mathematics than I do. I believe he studied the subject for a year as an undergraduate at Cambridge before moving to architecture.

TV: Most of your research was based on combinatorics and graph theory. Do you recall when you first came in contact with this kind of mathematics and what spurred your interest in it? What was useful or compelling about combinatorics and graph theory? Which of the graph’s properties were useful for your research?

PS: I’ve already started to answer this question. I read books on graph theory by Øystein Ore and Claude Berge, and some books on network theory, in particular Harary, Norman and Cartwright’s Structural Models. (Much of what I was reading is listed in the bibliography of The Geometry of Environment – see pp.346-7.) I was interested in the subject in its own right, but was mainly motivated by the potential that graph theory seemed to have for describing relationships of adjacency and access in plans, and for approaching the question of enumerating plan possibility. There was a certain amount of work on graph and network theory going on elsewhere in architectural research, outside LUBFS, as for example the work of P H Levin, Jean Cousin, the Ulm School of Design, and in particular the work of John Grason at Carnegie Mellon who was doing a PhD under the supervision of Herbert Simon. Lionel March and I were also interested in some puzzles in the literature of recreational mathematics, and in the very earliest work in AI. I believe that what we did with graph theory in LUBFS was later important for the development by Bill Hillier and colleagues of ‘space syntax’. We had contacts with Hillier from the 1970s.
TV: I am putting forward the hypothesis that graph theory was not a neutral technique but was loaded with visions of epistemological reform in the fields in which it was applied. I would appreciate your reflections on this. Did you use graphs for practical reasons (because they were the right technical device for the work at hand) or were there other aspects that sanctioned them as adequate representational devices?

PS: We used graph theoretic techniques because they were the appropriate tools for what we wanted to do in architectural science. Using graphs it was possible to separate topological properties of plans (adjacency, accessibility, containment etc) from configurational and metric properties. I’m not quite sure what you mean by being ‘loaded with visions of epistemological reform’. Our ambitions were to represent architectural arrangement at a relatively abstract theoretical level, in order to say something about possibility. This didn’t mean that we were unaware of everything that was being omitted by the use of such schematic representations. Both Lionel March and I were and are very interested, wearing other hats, in many aspects of architectural aesthetics and other kinds of representational technique. I for instance have had a lifelong fascination with perspective geometry. For me it is a question of what representations are appropriate for the particular purposes in mind.

TV: In your recent essay you position your research and more generally the LUBFS activity as aiming to "show designers how the ranges of possible options available to them are limited by the laws of geometry and topology." If this is the case, would you then say that design is a combinatorial process, or a search within a pre-existing space of possibilities? How did you envision this redefining the designer’s role back then?

PS: This is a tricky question. I would want to emphasise once again the key distinction between architectural science and architectural design. I see architectural science as producing knowledge and tools that can inform designers and help them in their work. This is similar to the role of traditional building science in informing and providing tools for architectural design. By making simplifications and restrictions on the domain of interest, for example rectangular plans consisting of small numbers of rooms whose shapes are simple rectangles, it is possible to enumerate all possibilities at the level of dimensionless configurations. One can produce a catalogue. This catalogue can then be searched or sieved to find all plans that satisfy some specified set of constraints: required adjacencies, dimensional limits on rooms and on the plan as a whole, and required orientations for rooms. This is not a design method for generating unique or supposedly ‘optimal’ solutions. It is a method for showing to architects the ranges of all possible solutions. Using computer methods there are nevertheless limits imposed by the combinatorial explosion that occurs, as the number of rooms is increased. Bill Mitchell, Robin Liggett and I built a system that could produce all plans with up to about 10 rooms. Ulrich Flemming’s DIS system pushed this limit by pruning the trees of possible dissections by means of the specified constraint set. But in the end, it’s not possible to escape the fact that this kind of approach is intrinsically limited in the numbers of rooms that can be handled. Bill Hillier drew the conclusion that architecture (by which he meant architectural design) is not an ‘ars combinatoria’.
I think that Bill Hillier was only partly right. Obviously, the production of plan arrangements is only one part of architectural design. Rectangular plan diagrams are gross simplifications of real
buildings, and there are very many other features/ elements of designs that are manipulated by designers. There can be no question of building catalogues of all possibilities in that larger sense. So in answer to your question, I don’t believe that everyday architectural design is a matter of searching spaces of pre-existing possibilities, at least in any practical meaning of this idea. One might possibly dream of such spaces in a highly theoretical or abstract way, but the spaces would be so large as to be without practical interest.

In architectural science on the other hand, one can simplify and limit the domain of interest. One can have diagrammatic and schematic representations – like rectangular dissections as representations of certain properties of plans – that can then be enumerated exhaustively. There are some areas of architectural practice where arguably, the insights that this scientific work can provide could be useful to designers. As an example, I would refer you to paper that Frank Brown and I wrote, applying Flemming’s DIS system to some questions in the history of British housing: F E Brown and J P Steadman, ‘The analysis and interpretation of small house plans: some contemporary examples’ Environment and Planning B: Planning and Design, Volume 14, 1987 pp.407-438. In the paper we showed among other things how the British government set standards for the plans used in social housing that were extremely limiting and resulted in unique solutions – although the standards were only intended for guidance and were not meant to limit creativity. Knowledge of this fact might have been useful to those framing policy. (In reality, architects and policy makers have not I think made such use of the results of this kind of work – partly because they don’t believe that plans can be enumerated, or understand how this could be done.)

In my more recent work I’ve been experimenting with a different kind of representation of plans, that treats them not at the level of individual rooms, but at the level of larger zones, each of which might contain numerous rooms. The representation, that is to say, is lifted to a higher level of abstraction. The price is to sacrifice detail; but the gain is that it then becomes feasible to enumerate more complex and larger rectangular plans than was possible with the earlier methods. The combinatorial explosion still occurs. But with this approach one is not limited to small plans at the scale of houses. All this is set out in my latest book, Building Types and Built Forms (Troubador 2014). Again I would emphasise that the purpose is not to produce tools or methods for designing. The work is intended to contribute to architectural science (as well as throw some light on the geometrical aspects of architectural history).

TV: In reading social scientific or architecture articles from the 1960s that engaged with mathematical modeling, I could not help but notice an explicit optimism about the “new” mathematics of sets and graphs. In contemplating relationships between architecture and mathematics post-1960, Lionel March himself positioned modern mathematics as key for the development of new theories and methods in architecture. Can you talk about the status of graph theory and set theory during the period, and their relationship to design?

TV: In The Geometry of Environment there is mention to the educational movement of “new mathematics.” In one of his papers, Lionel March narrates an anecdote about the Smithsons being baffled by their son’s courses in “new maths” and RIBA subsequently asking Prof. March to write a book explaining how “new maths” could be applied in architecture. Could you talk about the status of “new maths” in the U.K. during the period, and their relationship to design?

PS: I’m going to answer your questions 11 and 12 together. The ‘new mathematics’ referred to by Lionel March (and which mystified the Smithsons) was a movement in the 1950s to reform
the school mathematics curriculum. This was also known as the School Mathematics Project. You can find accounts online. The purpose was to update or transform the mathematical curriculum inherited from the 19th century with some teaching of 20th century developments, especially in discrete mathematics such as set theory, graph theory, combinatorics generally, matrix algebra, symmetry and so on. The leader of the School Mathematics Project in Britain was Bryan Thwaites, who I was taught by at my school, Winchester College. (I guess Lionel March was also exposed to the new maths at school. I don’t know. You could ask him.) I don’t remember studying graph theory at school—although I might be wrong. But I can imagine, looking back, and provoked to reflect on this by your questions, that what I did learn of the ‘new maths’ at school then shaped my ideas about how mathematics might be applied in architecture, once I got to Cambridge.

Graph theory as you know had its origins with Euler’s work in the 18th century, and the Konigsberg bridges problem. But the systematic study of the subject only started in the mid-20th century I think (I’m not completely familiar with the history) and the first books only began to appear in the 1960s. Øystein Ore said in 1963: “the number of books on graph theory is very small.” I think graph theory appealed to me first because it had obvious implications for the representation of spatial arrangement, and because—as I said before—it was a new subject which at that time had not gone much beyond the capacity of an amateur like myself to understand. (At one time I thought I had solved the four-colour problem; alas my ‘proof’ was completely fallacious.)

I personally was less interested in set theory and did not myself immediately see the implications for architectural research. But these became clearer when Christopher Alexander published his Notes on the Synthesis of Form. My colleague at LUBFS Philip Tabor used set theory extensively in his studies of the structure of organisations, in connection with layout problems in architecture. There was work going in elsewhere in Cambridge in the ‘60s on automatic classification methods using set theory, which we were aware of from a distance.

**TV:** In most LUBFS publications, the concept of “structure” figures center stage. Could you unpack the meaning of “structure” as you construed it back then? How did the concept of “structure” as understood in LUBFS relate, if at all, to the vogue of continental structuralism? What was similar or different about it? Could you speak to the LUBFS Centre’s main conceptual influences? What were some of the things you were reading back then? Did you understand the Centre as participating in a particular lineage of thought (maybe one of the -isms; rationalism, structuralism, positivism …)

**TV:** How did traditions of international and U.K. modernism translate into the LUBFS research projects? Many researchers who pioneered computational design methods professed to break from the traditions of interwar and early postwar modernism. Do you see LUBFS as continuing or breaking from modernist thought in architecture? If you were to place LUBFS in a history of architecture and in a history of science compendium, where would you place it?

**PS:** Again I’m going to try to answer two questions together. These are both difficult, because they’re so general. I guess by ‘structure’ we were referring above all to spatial structure. But people (like Tabor) were also interested in the structure of organisations. In the urban work there were concerns with the structure of transport systems and the use of network theory. We read technical works in network analysis and operations research. We read books about models and
systems coming out of subjects like geography and archaeology, as I mentioned in The Journal of Architecture. I was interested myself in continental structuralism in particular the work of Lévi-Strauss (although I doubt if many others at LUBFS shared this interest). But that work was concerned with binary oppositions in culture and language, and the relationship to architecture was more tenuous. Bill Hillier with his social science and anthropological focus drew much more I think from structuralism than LUBFS did. I don’t think we saw ourselves at LUBFS as participating in any ‘particular lineage of thought’ as you put it, other than the tradition of architectural Modernism, and in the new ‘systems thinking’ of the post-War sciences, amplified of course by the use of computers. We weren’t very self-aware. Perhaps it’s for historians like you to pigeonhole us. I used to think of myself as a positivist. But one isn’t allowed to say that these days.

TV: Structuralist understandings of design and the use of graph and set theory were in good currency outside LUBFS. I am thinking about the work of Christopher Alexander, various researchers in U.K. design methods or the Design Methods Group in the USA, the Architecture Machine Group, Yona Friedman and others — with the latter two being specifically interested in plan generation and cataloguing of different choices. What was your relationship and LUBFS’s relationship with these people or organizations? Did you meet/correspond with them? Was there antagonism? Open exchange? Also, how was your work similar or different to this other work performed during the same period?

PS: I’ve already answered this question, up to a point. Christopher Alexander was an undergraduate at Cambridge, before going to Harvard to do his PhD. Lionel March knew him as a student, but I didn’t. (Later we both went to visit Alexander in Berkeley.) Of course we were well aware of Alexander’s work, and the others you mention, through their written work, but also through meeting them at conferences. I was critical in print of Yona Friedman as I remember. I devoted two chapters of my Evolution of Designs to a critical analysis of Notes on the Synthesis of Form. We at LUBFS knew about the Design Methods Movement both in the USA and the U.K. We attended their conferences as well as meetings of environmental psychologists who were interested in architecture at that time. With some exceptions there was general scepticism at LUBFS about the work in design methods, for reasons I explain in The Journal of Architecture.

TV: In the late 1960s many groups or individuals developing rational and mathematical approaches to design shifted toward humanistic/participatory perspectives. Instead of empowering the designer to design better, it became about empowering the “users” to choose living arrangements more adequate for their own lifestyle, or change their living settings according to their wishes. I am thinking about the Design Participation conference of the DRS in 1971, Christopher Alexander’s shift toward participatory approaches to design and other similar turns -- all happening against a backdrop of social and civil rights struggles, the questioning of technocracy and positive science, environmental concerns etcetera. Did this shifting context influence the work and debates at LUBFS, and how?

PS: We were certainly aware of these developments in participatory design. We were painfully aware of the criticisms that were made of ‘technocracy and positive science’, with one very harsh attack mounted on LUBFS by right-wing elements at Cambridge, including the philosopher Roger Scruton and the architectural historian David Watkin. But I wouldn’t say that we were
deflected from the centre’s main purposes, which were radically misunderstood by these critics. The misunderstandings have persisted a long time, as you know [...] 

**TV:** What do you see as the main legacy of the LUBFS Centre’s work, intellectually and technically?

**PS:** Once more I refer you to *The Journal of Architecture*, and my account towards the end of that paper. I see my own current work as continuing in the LUBFS tradition, and I think some of my erstwhile LUBFS colleagues who still continue in research would say the same, although much of course has changed in 50 years. Computer-aided design in architecture, models of the environmental performance of buildings, and integrated land use/transport models are the most influential legacy of LUBFS. There was much work in all these fields elsewhere, but LUBFS was among the pioneers.
Interview with Professor Marcial Echenique
Interviewer: Theodora Vardouli
Date: January 15th, 2017
Medium: Email

Theodora Vardouli: I wanted to start with your transition from Chile to the U.K., your architectural training, and your decision to pursue research focused on urban studies. I am interested in the type of work that you conducted during your architectural education, any activities that you undertook between completing your studies and joining LUBFS, and if/how these motivated you to join the Centre or informed your subsequent research in it.

Marcial Echenique: I completed my architectural education at the Catholic University of Santiago in Chile. I was awarded a scholarship for postgraduate study at the University of Barcelona in Spain. There, I obtained a Doctorate in Architecture (Specialized in Urbanism) in 1966. During my period of studying in Spain I was a teaching assistant in Urbanism and participated on exciting urban design projects. These projects under the direction of famous architects (Antoni Bonet and Oriol Bohigas) did materialize in 1992 when Barcelona was the seat of the Olympic Games. During my doctorate, in the summer holidays, I travelled once to Milan in Italy for visiting a well-known urbanist – Giancarlo di Carlo (Urbino fame) – and once to London and Cambridge to visit well-known architects – James Stirling (Leicester fame) and Colin St. John (Sandy) Wilson. This last one was a lecturer in architecture at the University of Cambridge and was bidding for an architectural and urban design commission to design the Liverpool Civic Centre in the U.K. He contracted me when I finished my Doctorate to work in the Liverpool project and thus I moved to Cambridge in 1966. Latter in 1967 Sandy Wilson introduced me to Sir Leslie Martin – Head of the School of Architecture at the University of Cambridge - and to Lionel March who was starting the LUBFS Centre. They invited me to join the newly formed Centre to direct the Urban System group funded by a grant from the Centre for Environmental Studies (Ford Foundation financed). I believed that I was lucky to go to Barcelona where architect’s education was heavily influenced by engineering skills (there I was introduced to computing) and to Cambridge where Christopher Alexander – an alumnus – was very influential for his design methods and Lionel March – also alumnus – was working on geometry. Being personally interested in rational approach to architecture and urban design and with some experience and skills on these subjects I was immediately attracted to join the LUBFS.

TV: Could you give me a sense of the meaning and status of urban studies research in the Department of Architecture in the mid 1960s (was the subject well established, well regarded, peripheral to the discipline ...)? Also, could you speak about the alliances that you forged and the resistances you met inside Cambridge University during your early work in the Urban Systems study group? What about outside the University (for example, your relationship with the Centre for Environmental Studies or other groups in U.K. and abroad)?

ME: There was no interest in urban studies at the School of Architecture but Sir Leslie Martin and Lionel March did published work on urban design, mainly on the geometry of built forms. This work started the interest in the field. An influential paper by March entitled “let’s build in lines” investigated the possibility of building linear cities compared with central places à la
Christaller. I got interested on the properties of transport networks at the time but soon I realized that we needed to understand human behavior in order to progress in urban modelling [sic]. The work of Alan Wilson at the Centre for Environmental Studies opened the possibility to model transport behavior using probabilities. This led me to study the pioneering work of Ira Lowry who published a report in 1964 entitled “Model of Metropolis” which modeled the locational behavior of people in Pittsburgh. The combination of economic ideas from Lowry with mathematical rigor from Wilson and physical aspects from Martin and March, led me to produce the first model of a European city: Reading in the U.K. It was possible to model a real city due to the existence of good spatial data brought by two colleagues – Crowther and Lindsay – and the joining of Richard Stibbs – a computer scientist – to our group. The work was published in 1969 under the title “A spatial model for urban stock and activity” in Regional Studies, Vol. 3, pp 281-312.

TV: Did you witness changes in the LUBFS Centre’s composition and structure throughout the years of its operation? How about changes in driving debates or research foci in the Centre? Could you speak to the institutional, technological, intellectual conditions that, in your opinion, drove these changes?

ME: LUBFS attracted architects and PhD students who were naturally inclined to quantification and rational thinking. The Centre was somewhat peripheral to the main teaching at the School but I had a group of architecture students who applied the urban model which we developed to the city of Cambridge and slowly the teaching curriculum included the research work of the Centre (March, Stedman, Hawkes, etc.). Intellectual influences on the Centre’s work were many. The Cambridge Philosopher of Science – Professor Mary Hess – who wrote a little book “Models and Analogies in Science” inspired me to write an article called “models: a discussion” which I believe helped to clarify what we were doing. Latter the Cambridge Geographers – Hagget and Chorley – were actively describing the work of German spatial economists that gave historical and theoretical substance to our work. Another influence was the work of the Cambridge Professor of Applied Economics – Richard Stone – latter awarded with the Nobel Prize, who worked on a computer model of the British economy. He was interested in our spatial work and I had some interaction with him, incorporating his input-output ideas (He called Social Accounting Matrix) into the urban models.

TV: In looking at the LUBFS Centre working papers, one finds work devoted to practical issues coexisting with more abstract mathematical modeling and theoretical activity. Where did the concept of “built form” sit in the abstract/concrete; practice/theory spectrum? What was the relationship between “applied” and “basic” research in the Centre (if you think that these terms are valid)? Did each correspond to different (or competing) cultures of research or did the two approaches co-exist harmoniously? Who decided about the kinds of projects that LUBFS researchers pursued?

ME: It is true that some of us were interested in practical applications while others were more inclined to concentrate in theoretical work. But the more abstract work by March on built form was visually compelling and certainly impacted the architectural practice, especially social housing. At the time the tower block was the common solution for social housing which changed to low-rise court development, as a product of the Centre’s research. In 1969 a group of LUBFS
researchers set up a private company – Applied Research of Cambridge (ARC) – with the purpose to apply some of the research to practical projects. This company helped to employ researchers who didn’t have further funding, charge clients for the consultancy and buy equipment outside the University. The University encouraged us to set an independent outfit as they were not keen in “applied” as opposed to “basic” research. I was of the opinion that both were complementary and needed each other to progress. ARC provided the necessary link.

Most of the research work was decided by the group leaders (Stedman, Hawkes and I) under the leadership of March and we did apply for research grants to several institutions.

**TV:** LUBFS research spanned all scales of the environment – urban, intermediate, and domestic. What was the relationship between the Centre’s different working groups and research projects? Can you identify any principles, ideas, or techniques that unified all these scales and that were distinctive of LUBFS?

**ME:** At the beginning the geometrical exploration of Martin and March were applied at the three different scales. Latter the urban scale, which I led, concentrated in modelling [sic] spatial human behavior and its relationship to physical forms of building and networks. The behavioral research connected our work with work in spatial economics in the U.S. but with a more physical bias. The work at intermediate scale, led by Steadman, explored the time dimension and branched out into new areas of the use of time and space. The work at the building scale, led latter by Hawkes, concentrated on environmental performance of built forms. In addition March and Stedman produced books and papers on geometrical properties of built forms and layouts. Each group applied to research councils and foundations for funding their research.

The principles behind the Centre’s work were a common interest in representing and rigorously assessing buildings and cities. The techniques employed were computer models of buildings and cities, using statistical information to calibrate the numerical parameters that defined human behavior and the properties of forms. This allowed the exploration of designs at different scales.

**TV:** You co-authored the editorial of the *Architectural Design* Special Issue “Models of Environment.” I would be very interested in anecdotes or memories you can share about how that Special Issue came about, how the editorial was written, and the internal deliberations that resulted in subtitling the editorial “Polemic for a Structural Revolution.” Also, could you speak about the specific meaning that the terms “models” and “environment” had within the LUBFS Centre?

**ME:** I was approached by the editor of *Architectural Design* – Monica Pidgeon – who allowed us to publish the special issue on the LUBFS work. I discussed this with Lionel March and Peter Dickens and divided the work between us. March was the main force behind the editorial. Re-reading it, it’s clearly influenced by the spirit of the time which was concerned with social and environmental questions. It was written intentionally in the language of architectural manifesto of the 1930s.

The meaning of “models” had been clarified in my earlier paper “Models: a discussion” which explained that models were representations made of different substances (physical or conceptual) for the purpose of describing, exploring or plan a possible reality. In this case the models were developed for representing the physical “environment” of building and cities and the behavior of
people within them for the purpose of exploring and better planning to achieve social and environmental goals.

TV: In many LUBFS publications, including the AD editorial, the concept of “structure” figures center stage. Could you unpack the meaning of “structure” as you construed it back then? How did the concept of “structure” as understood in LUBFS relate, if at all, to the vogue of continental structuralism? What about systems theory?

ME: In the editorial, the work of Lévi-Strauss is cited and for us, we believed that we could discover the deep structure that determine the properties of building and city forms (geometrical laws) and, probably more naively, believed that human behavior was also determined by deep structures which were discernable (through probabilities). In addition the concept of system was central to our work in the sense that any represented element in a model was related to others making the connections between them the core of our research.

TV: A striking element in your LUBFS papers, at least for the contemporary reader, is the mathematical fluency that they exhibit. Did you acquire this mathematical knowledge as part of your architecture studies or was this something that required further training? Were all LUBFS researchers as mathematically adept, and was this a prerequisite for joining the Centre? Did you collaborate or consult with mathematicians when performing your research?

ME: Certainly March was a mathematician. Stedman was also very proficient. I was introduced to advanced mathematics in Barcelona because the training of architects included structural calculations. The incorporation of Computer Scientists with mathematical training helped to develop the necessary tools for research. There were no formal requirement for researchers to be good at mathematics but attracted those who had an inclination for it. But there were notable exception such as Catherine Cook who worked in the history of building research and later specialized on Russian constructivism.

TV: In reading social scientific or architecture articles from the 1960s that engaged with mathematical modeling, I often come across discussions about a transition from the “mathematics of number” to the “qualitative mathematics” of structures and relations (set theory, graph theory etcetera). Did this distinction play a role in your thinking about urban models in LUBFS? Could you speak about the relationship of statistical theory with structural concepts (for example “urban spatial structure”) in your work? How did you decide what mathematics to use for your models?

ME: I was not as proficient in advanced mathematics as March or Stedman but I got very interested in probability theory and the interpretation by Wilson using Entropy (developed from Jaynes’ information theory). This provided the backbone for estimating the most probable state of human behavior in spatial analysis. The statistical estimation of parameters governing the relationship between element of the system such as between jobs and housing, determined the “spatial structure” of a city. Later on, the same equations were developed from Random Utility Theory (developed by Nobel Prize winner Daniel MacFadden) which allowed the models developed to be anchored on microeconomic theory.
TV: What was the role of the computer in your work? Did the technological possibilities of the time (from programming languages, to storage, to processing power) influence in any way the models that you developed?

ME: The pioneering development of computing at Cambridge was of crucial importance. We had access to a very powerful interactive but unreliable computer (TITAN which its software was similar to Microsoft Windows, developed decades latter). A couple of computer PhD students joined LUBFS who helped translating our theoretical work into practical tools. Most of us learned to program in Fortran IV and developed algorithms that still are in use today. We needed substantial computer power to iterate the models to equilibrium with a large number of spatial zones and large transport networks. The people at the Cambridge’s Computer Lab hated us as we did block the computer at night time. They call us “Lots of Useless Bloody FoolS” (LUBFS). Later, at ARC we acquired minicomputers which help us to develop commercial software for practical applications.

TV: In *The Geometry of Environment* there is mention to the educational movement of “new mathematics” as enabling a “structural” understanding of the environment and aligning with the new possibilities offered by computers. Did you come across conversations about the “new maths” during your time in LUBFS? How about “modern maths”? Do you recall any specific “new” or “modern” mathematical techniques that were formative for your research, or for LUBFS research more generally?

ME: I personally used traditional maths but colleagues at the LUBFS were also involved in “new maths.” Computer programing was a powerful tool for developing our models. It would have been impossible to find equilibrium situations for traffic or locations for large number of zones or networks in the models of real cities. It demanded iterative procedure which could only be calculated by electronic means.

TV: Much of the LUBFS work on spatial organization used graph theory (I am thinking about Philip Steadman’s work on minimum house plans, Philip Tabor’s work on office layout, Nicholas Bullock’s, Peter Dickens’s and Philip Steadman’s work on university planning etcetera). Was graph theory popular in architectural research at that time? Why do you think that this technique was so pervasive in LUBFS? Was topology and graph theory useful for your research in LUBFS?

ME: Yes. Many were interested in Graph Theory. In my case the description of transport networks used some Graph ideas but it was more important for my team to use or developed computer algorithms to find the answers for the calculations of minimum paths through networks and the probability of traffic using particular sections of networks.

TV: In your March 1968 working paper “Models: A discussion” you distinguish between descriptive, predictive, and explorative models. Which, if any one, of the three types better characterizes the LUBFS work? I am asking this with a recent essay by Philip Steadman in mind, in which he distinguished the didactic/exploratory aims of mathematical modeling in LUBFS ("showing designers how the ranges of possible options available to them are limited by the laws
of geometry and topology”) from the normative purposes of design research/design methods (prescribing how one should design”). What do you think about this distinction?

ME: March and Stedman believed that there was a limited number of combinations for generating building layouts. So by modeling them and exploring the total realm it was possible to select those layouts which achieved an externally defined optimum. I was more skeptical in optimizing alternatives at the urban scale. My work was more concerned with testing design alternatives (elucidated from any source – analogies to other phenomena, policy ideas, etc.) through predictive models and then performing an assessment – latter developed into a combination of cost-benefit analysis with social and environmental indicators – for the politician/public to decide which alternative to choose. The rationale for leaving the decision to politicians, representing the public, depended on the weights assigned to the different aspects – economic, social and environmental – which were a matter of judgement and not a precise objective measure. My argument was that in very limited and simple circumstances an optimum design can be automatically generated. Models help in understanding the limitations and, crucially, test a design. I used a typical example of an engineer who couldn’t generate a design of a bridge directly from a model but once a bridge was designed it could be tested by the use of models. In other words, models are a help to creativity but not a substitute.

TV: Structural or system theoretic understandings of architectural design (from the house to the city) were in good currency outside LUBFS. I am thinking about the work of Christopher Alexander, various researchers in U.K. design methods or the Design Methods Group in the USA, the Architecture Machine Group, Yona Friedman and others. What was your relationship and LUBFS’s relationship with these people or organizations? Did you meet/correspond with them? Was there antagonism? Open exchange? Also, how was your work similar or different to this other work performed during the same period?

ME: Certainly we were very aware of Alexander’s work and other Design Methods’ people. We had visits from them. Some of the people writing at the time either in architectural or planning research quoted our work. But there was a fundamental difference between the Design Method ideas and LUBFS work: we believed that there was a “science of the artificial” (buildings and cities) à la Simon which was possible to discover and we were less interested in procedures to optimize design. March later wrote a paper in the book “The Architecture of Form” which distinguished three logic of thinking in the design process: abductive (or productive), deductive and inductive. LUBFS was essentially concerned with deductive logic which permitted forecasting of properties of forms and also with inductive logic which permitted evaluating these properties and less interested with the productive logic which was the interest of the Design Methods people.

TV: In the late 1960s many groups or individuals developing rational and mathematical approaches to design shifted toward humanistic/participatory perspectives. I am thinking about the Design Participation conference of the DRS in 1971, Christopher Alexander’s shift toward participatory approaches to design and other similar turns -- all happening against a backdrop of social and civil rights struggles, the questioning of technocracy and positive science, environmental concerns etcetera. Did this shifting context influence the work and debates at
LUBFS, and how? Did ideological or political considerations play a role in your own work, and more broadly in the work of the Centre?

ME: In my field a critique by Lee in “requiem for large scale models” was important and papers by people who dismiss the scientific approach in favor of political advocacy did shake our confidence. But our applied work in modelling [sic] Teheran, Sao Paulo, Bilbao, etc. demonstrated that the LUBFS approach was practical and politically useful as it offered real arguments in the selection of urban policies. Again, latter on, in the Cambridge Futures work, models proved a valuable tool for public participation.

TV: Could you speak to the LUBFS Centre’s main conceptual influences? What were some of the things you were reading back then? Did you understand the Centre as participating in a particular lineage of thought (maybe one of the -isms; rationalism, structuralism, positivism …)

ME: I believe that we thought that we were part of the wider currents of rationalism, structuralism by Lévi-Strauss, system analysis by Von Bertalanffy and positivism by 19th Century philosophers from Compte onwards.

TV: How did traditions of international and U.K. modernism translate into the LUBFS research projects? Many researchers who pioneered computational design methods professed to break from the traditions of interwar and early postwar modernism. Do you see LUBFS as continuing or breaking from modernist thought in architecture?

ME: I thought that we were continuing the modernist tradition by trying to establish scientific basis for design in architecture and planning but with much better tools of mathematics and computers. For me the LUBFS work offered continuity and not a break with the interwar period of Le Corbusier, Hannes Mayer, Russian Disurbanists, etc.

TV: If you were to place LUBFS in a history of architecture and in a history of science compendium, in which hypothetical section would you place it?

ME: I would follow Herbert Simon’s section of the “science of the artificial” (as opposed to “natural sciences”) and its relation to human behavior.

TV: What do you see as the main legacy of the LUBFS Centre’s work, intellectually and technically?

ME: Probably I would say that LUBFS Centre opened a field in architecture and planning which help to understand the properties of forms and offered practical help to designers of buildings and infrastructure. Some of the ideas and techniques have proved successful in practice.

TV: If you were to build a new LUBFS today, what kind of research would you envision?

ME: I think that there has been a substantial improvement in our understanding of forms and human behavior at large scale, but there is a need to understand human behavior at building scale. Some of the statistical techniques utilized at urban scale can be adapted to model the
behavior of people within buildings (such as econometric techniques of revealed and declared preferences) but there is a substantial work to do before we can be sure that we can model it with confidence.
**Interview with Yona Friedman**

Interviewer: Theodora Vardouli

Date: March 30th, 2012

Medium: In-person conversation


**Theodora Vardouli:** I am reading Pour Une Architecture Scientifique (Towards a Scientific Architecture in the English translation), and I wanted to know more about the context in which you wrote it, your motivations, your intentions.

**Yona Friedman:** I have now time to prepare an exhibition with the MOMA and a group of museums; not a personal exhibition but a programmatic one. At the MOMA there was in the sixties “Architecture without Architects”; I am proposing now “Architecture without Building.” I will show you a booklet that I prepared which will come out in a couple of weeks from now. The idea is that we are over-building now, and it is not necessary. [...] I was doing similar things already. Uncertainty and feasibility is always a problem, so my principle was always that architecture is too preconceived, too fixed, and people, real people, cannot reorganize it, they cannot criticize it according to their life; because it is their life. Architects force people to live in a certain way. [...] So I say that the center of the habitat, of living, it is simply people. It is not the object, it is not hardware, is not software either, I will call it simply reality. With software everything is already too formalized. [...] This project at MIT, Architecture-by-Yourself was based on this principle and on how to transplant it to computer. What came out of this practical experience is that the computer is not good for it. It is too fast. People need time. If they do something manually they think more. With the computer, ok, the computer gives the best answer, yes, but what best answer? Real life has a certain speed. For certain problems these computer programs can transfer reality into a game, but even then a game is very far from reality. The models work but they are not the real thing. For example in order for a group to conceive a collective building it takes about six months: the discussion between (the members of the group) will take six months. The computer helps, but it is not the right way: people do not think about the best solution, they look for what fits them. There is a big big difference! And you cannot look for this with the computer. The computer has first to make your own profile et cetera; this becomes inefficient. This is why I am telling you that the best thing is the reality. Reality can be made easy. [...] 

**TV:** In Towards a Scientific Architecture you used graph theory and graphs as a system that could transpose reality to a representation system.

**YF:** Graph theory for certain mathematicians is not mathematics, it is not Logic, but it is a very useful tool. It is useful for people to experiment. It also helped me in doing a very funny thing: First, about the critical group size: I found it through graph theory. Now it is accepted by sociologists. The other very funny thing is when I was writing the Urban Mechanism model. I was writing this model in the early sixties and then computers were not easily available. At that time, I finally got a computer for experimentation, this is a funny thing, by the Physics Department!
TV: Here? (in Paris)

YF: In the States, Princeton. The funny thing is that I didn’t know why. They told me: “Yes, your idea is really interesting for Physicists.” Then much much later I found that Richard Feynman had also a model with path diagrams and it was the same mathematical model (as in Urban Mechanisms). I didn’t know. Simply because of this resemblance, physicists were interested! Here again the idea is similar. In the “Urban Mechanism” model you think: “I don’t know what happens in the city. I know that from this point there are people who go and at this point there are people who arrive. I don’t know what is happening in between, but somehow this movement gives an image of how the network is charged.” Feynman’s idea was very similar: I shoot an electron and an electron arrives -it is not sure it is the same! The electron could take many different paths. Through this model Feynman reads the same kind of patterns. So it is essentially a summing up of the possible itineraries and adding, calculating the probabilities. The mathematical apparatus is simple. We need the computer because it can do it for a big number of departure and arrival points. I have always been always very much interested in physics because physics is very much linked with mathematics - in a very questionable way. It is very simple to explain: what is important in events is the process not the result; the same is the case with architecture. [...] it is the process which is important. There is only one way to know (describe) the process: it is a linear sequence. It it is a story, a history of things, which does nor follow a logic; you cannot make abbreviations. You can do a statistical approach which is in many cases meaningless. It is like in architecture: when I was a student I remember everyone talking about the “Average Man.” I was thinking “But this is the only one who does not exist!” Now the funny thing is that the mathematics for handling processes is still not invented. You take for example a sequence. How are you adding another sequence? It is meaningless: they are simultaneous. What are the detail sums for example? We don’t know. I was giving as an example of a model for a potential mathematics for sequences simply a musical partition: there are the different dimensions, the different instruments and they go simultaneously. The only thing in common is the time. And nobody can know the end result before someone performs it. This is important. Let us now translate it for a moment in physics. This would mean that the only independent variable is time. You can describe space through time -you can describe obviously time elsewhere through space- but time is the only variable you cannot manipulate. All the others you can work with. In the musical example I chose, the addition of sequences have simultaneity that means that time is common. This could be perhaps very useful for conceiving real planning; a planner could look at things like a psychological transformation on a common time scale. Let us now leave physics. This would be a development for architecture and planning. The real tool for this is the computer. This is where I see the use of the computer in planning: that histories can be played on a common timescale. That is technical ready, we have no problem with it, what is necessary now is experimentation.

TV: Do you think that understanding the simultaneity of histories would help inform people to make planning decisions or would it be able to anticipate or predict the outcome of a scenario?

YF: The way that this could be is: first, have a look at the simultaneities in a real case like this (points to space chain) and the second would be to do it in imaginary scenarios and to learn the
language - because a language comes out of all this. There is something I wanted to add about sequences. For example, any text (points to my notebook) is a sequence which has a meaning. This sequence it is not mathematizable. It can be mathematized for automatic voice, but not when it comes to the meaning. How can you add several lines of text? There is no way. Take for example that it is possible that these (the lines of text) are real histories, case histories. These case histories can be anything. For example the effective path people take in a space: this can be photographically documented. What does it mean? What comes out of it? This is a very primitive thing. Another example could be how people change the disposal of furniture. These are the “real” scenarios. Now the imaginary scenarios could be “I would like to turn this room; I want to change the window; I will cancel this room.” There are many imaginary scenarios possible and if somehow these scenarios are recorded then they might show how this mathematics could look like. The simplest sequential mathematics is simply in arithmetic. You write down a number and then another number; this is a sequence we know how to operate with. Once we get through other sequences which have a meaning there is a problem. I was always of this kind of attitude: the Complicated Order; “complicated” because it cannot be mathematized, there is no way to describe it in a mathematical or logical notation, but it is an “order”! This is where I see at the moment probably the most interesting use of computer in this area. Because it is the only tool which is able to do it. This started with particle physics, but the difference is that processes in particle physics have no meaning. But then I tell you to add two lines (of text) you can put a simple mathematical rule, “adding,” but it is meaningless. The problem is how to add sequences which have some meaning. This is something to toy around. […] 

TV: Who would you say that were your major influences in these explorations?

YF: I am a non-specialist. I have a professional Architect’s Diploma but I am not working with architecture like the craftsman-architect. I simply follow my curiosity. I don’t know what to say. I had a dog; and I learned enormously from the dog. You will laugh but from the human point of view the dog is an extraterrestrial; it is completely different! So I was living for years with an extraterrestrial, observing it. It was a very intelligent extraterrestrial because it was understanding me but I didn’t understand it. One thing that dogs do, is that they don’t focus with their eyes. They have a hazy view of things and they therefore cannot see discrete things. If you don’t see discrete things you cannot invent language. Language is names for discrete things. If you don’t see discrete things you cannot invent arithmetic. So in principle a dog, let’s say as a being, has a different kind of sight and it cannot invent language or arithmetic in our sense - but it can invent another arithmetic and another language. For example dogs have an emotional language and not an information language in our sense... Ok, I don’t know about the arithmetic! You see I am curious about things and the hypothesis that things can be otherwise.

TV: In Towards a Scientific Architecture you wrote that in order to plan, to make temporary arrangements, within the infrastructure it is necessary to have a common language between all the participants or at least keep the community informed about individual decisions. I was wondering about the processes that are going on in the infrastructure and how you think they can be made possible between people.

YF: Now I am using a common language: it is the real thing! If they can modify the real thing, for example they can take containers, room size boxes with transparent walls and fix them in
structure. If they have a crane they can do the experiment, they can do it. Evidently the containers are designed in a way that allows them to be manipulated. This thing (the real scale space frame) is built in construction, simply improvised, “Put it there! Put it there!” and they did it. I am trying to get the experimentation with the thing itself. In every flat people do this experimentation with furniture: “Put the chair there! Put there the table here!” They do not need to make drawings for that.

TV: Do you think that people have to see and then they know?

YF: Yes, because then you can have several mental processes going on simultaneously. When you say “Oh, I like it better there,” this is a complex mental process that a person does automatically. This is what I am trying to arrive here and (plays video with Container structure) So it is there, improvised!
But they built it with a real space frame structure. The space frame structure does not need to be regular. The regular structure is just more typical to calculate and easier to experiment.
So this is actually everything: at real scale. You can experiment with a model but the real scale should be easy to manipulate with obtainable equipment. This is really what I am trying to propose.

TV: Would you say that this is a different approach that the graph theory approach? Graph theory was abstract.

YF: The graph theory was practically a mapping of reality, but reality itself is a much better mapping! You can use graph theory, it is no contradiction, but you can shortcut it if you want. This is a prolongation of the experience I had with real cases: it takes time for people (to think abstractly). With reality it is faster. With the abstraction, people have to translate it to their reality. People don’t make a sketch when they say “I like to put a chair there, like to put it here” They just put it and see. If you want a general rule, the best model for reality is reality itself. [...] 

TV: What was your experience from your work at MIT, when you worked with the Architecture Machine Group in the Architecture-by-Yourself Program?

YF: It was my first real experience of the non credibility of the computer. It is very funny because Nicholas (Negroponte) had a program which was based on conversation. So I was trying a simple thing: I was making voluntary mistakes that common people would make, to see how the computer would react. It got simply mad! And Nicholas was absolutely surprised! We did not know what happened because the computer did not record the process. There was no trace. It could not find the trace of what had happened, how came the definition of the problem. The other important experiment that happened at MIT is that they (the Architecture Machine Group) were mobilizing real people, real people to plan with the machine. They could not do it, because of the speed (of the machine). This is where I first realized that the process had to be slow, the program needs to be adapted to the people’s thinking speed and not to the computer’s. So for me these two experiments were very important: there could be mistakes all the time but no-one learned anything because we did not know how the mistake occurred, and the speed. For me this was an important observation. You cannot solve this problem (of self-planning) simply with the computer, the computer can solve other problems.
TV: When you were writing *Towards a Scientific Architecture* people like Levi Strauss here in Paris were also using mathematics (namely graphs) to describe the social. Were you interested in that, did you have any contact with these cycles?

YF: You know I was teaching at this time at Ann Arbor in Michigan. The book *Scientific Architecture* was essentially the course I was teaching. So this was maybe 1964–1965. Then this was printed in French in 1970 and then it was reprinted even in the Soviet Union! It was translated in many languages. Until now I agree with all this but what I am trying to do is to expand it, to go beyond the abstraction. In the same time I could say that the result of the process came from that.

My experience teaching in the United States was surely decisive for me. When I was teaching, I think it was in 1964–1965 I was first invited first to Harvard and then I went to lecture at MIT. This is the case that Nicholas [Negroponte] says that he was picking me up from the airport! This was a period they were giving grants to researchers so I had an absolute freedom with my work in the universities that invited me, which I did not have here.

TV: So when you were writing the book the influences were mostly from there, from the United States?

YF: Mathematicians were interested and helped me in my work, even if I was invited in the U.S. by architects. One of the most renowned graph theorists in the States was also teaching at Ann Arbor: Harary, Frank Harary.

TV: Did you develop your theory after meeting him? The theory was there since the Ville Spatiale, but I am referring to the specific formalization with the graphs you present in *Towards a Scientific Architecture*.

YF: It was essentially a simple departure. In the Ville Spatiale you can get all the combinations. What are these combinations? And now I could tell the same thing; I could put this (points to the Space Chain) in graph theoretic language but now I have a technique which makes it easy to go to the reality....

TV: In your model, you said that people would actually have to see all the possible combinations to be able to select one. Do you think this is a different model from the computer only giving one solution? And which is closer to your idea of non-paternalism?

YF: People are more revolted against what I was calling “paternalism.” Not completely; it is a mixture. For example, at the start, I was friends with Konrad Wachsmann and he was saying that grids had to be regular. I have a demonstration that it does not need to be like that. You can do it with completely irregular grids. This is a space frame (points to a model of “Cloud” structure). In reality you have to fix the structure everywhere the metal wires touch. This structure is improvised—I cannot draw it, I cannot read the drawings, no one can read the drawings.
TV: I was wondering if you think that by experimenting and improvising that people will learn how to think “as architects.” Is there something that an architect does that everyone can learn how to do, or are we all already architects?

YF: Intelligence starts with improvisation, as simple as that. People have always improvised. Einstein improvised! So the idea is always improvisation. Let us explain what is improvisation: that you make complicated mental processes, which are not in every point conscious, they are a mixture. You find something as automatically satisfactory exactly because your unconscious works with it. It is not always the rational. Always people do something and they say, “Oh, I like it better!” So I think architects improvise already but it is important that they also make improvisation by people really possible.