

TEACHING AND BUILDING MIDDLE RANGE
INDUSTRIAL RELATIONS THEORY

Thomas A. Kochan

February 1992

SSM WP # 3380-92-BPS

Teaching and Building Middle Range Industrial Relations Theory

by

Thomas A. Kochan

January 1992¹

Thomas A. Kochan is the Leaders for Manufacturing and George Maverick Bunker Professor of Management of the Sloan School of Management, MIT

Prepared for inclusion in Industrial Relations Theory: Its Nature, Scope and Pedagogy, eds Roy J. Adams & Noah Meltz, Melbourne Australia, Longman-Cheshire, forthcoming 1992.

¹This paper is dedicated to Jack Barbash, the person who introduced me to this field and who taught several generations of students about the enduring values that should guide scholarship in industrial relations. I also wish to thank my MIT students who provided helpful comments on an earlier draft: Rose Batt, Ann Frost, Larry Hunter, Saul Rubenstein, and Marc Weinstein.

Teaching and Building Middle Range

Industrial Relations Theory

Thomas A. Kochan

Frustrated with what he perceived to be an impasse in sociological research, Merton (1949) formulated the notion of **middle range theory** to promote the development of logically interconnected explanatory models that are intermediate to minor working hypotheses of day-to-day research and the grandiose attempts to formulate integrated conceptual structures. Merton defined what he meant by middle range theory in his own work. He showed that by generating relatively specialized theories applicable to limited ranges of social problems and data, researchers could test hypotheses that would contribute to the consolidation and progress of social science theory and research. This contrasted with the broad, all encompassing efforts at social theory illustrated in the works of Marx, Weber, or Durkhiem. Indeed, Kurt Lewin could very well have had theories of the middle range in mind when he expressed his oft quoted adage that "there is nothing so useful as good theory."

Merton's definition and Lewin's description of useful theory are especially appropriate for the field of industrial relations. Ever since John R. Commons established the role model for the practitioner/academic, the goal of industrial relations researchers

has been to produce and empirically test theories that are not only useful to policy makers and the labor and management communities but are also valuable to researchers concerned with understanding complex social processes. While the twin goals of producing socially relevant and academically substantive research can conflict, this need not be the case. By summarizing the approach I use in teaching and developing theories of the middle range, this paper illustrates how the divergent requirements of industrial relations research can be constructively reconciled to further both public policy and academic inquiry.

Industrial Relations' Niche in the Social Sciences

Middle range theories cannot stand alone in a field of inquiry. Instead, they must be embedded in some unifying theoretic orientation that provides the defining features of the field. Thus, in this section I suggest what I see as the secondary and primary features of industrial relations research. These analytic foundations provide us a way to proceed with the development of middle range theory without an all-encompassing integrated framework and, at the same time, distinguish industrial relations from the competing disciplines of law, economics, the behavioral sciences, history, and political science which all share an interest in and offer alternative perspectives on various aspects of the employment relationship.

Secondary Features of Industrial Relations

The most enduring and prominent feature of industrial relations research has been its problem centered orientation. Indeed, although as Adams' paper in this volume points out, the

origins of industrial relations can be traced back to the late nineteenth century, Kaufman (1991) argues that the field established itself as an area of scholarly inquiry and teaching by addressing the key "labor problem" of the early part of this century. Following the bombing of the Los Angeles Times office, concern for violent labor conflict led to the creation of the 1911-13 Commission on Industrial Relations. John R. Commons, the father of U.S. industrial relations research, was a member of that Commission, and Kaufman lists as research assistants to the Commission a veritable "Who's Who of early industrial relations scholarship in the U.S.: Selig Perlman, William Leiserson, Sumner Slichter, Leo Wolman, David McCabe, and Edwin Witte (Kaufman, 1991, 14, footnote 3). The exposure of these early scholars to the first hand issues of the day left an indelible imprint on the field in the U.S. It established the scholar-practitioner as the model for future researchers in industrial relations, or as Commons called it, a researcher capable of producing practical theory.

This tradition carried over to the next generation as well. Witness, for example a similar list of "Who's Who" in post World War II industrial relations research from those involved in one way or another in the War Labor Board: George Taylor, Clark Kerr, John Dunlop, Robert Livernash, Sumner Slichter, Richard Lester, Charles Myers, Douglas Brown, Nathan Feinsinger, Arthur Ross, Milton Derber, and many others. The emergence of labor problems in the public sector in the 1960s and 1970s saw a repeat performance with governors and state legislatures from New York and Pennsylvania to

Illinois, Michigan, and Wisconsin, to California turn to ideas and experience of industrial relations researchers in designing their public sector collective bargaining statutes. Thus, the problem centered nature of industrial relations theory and research is clearly one of the distinguishing features of our field.

A second feature of industrial relations research is that it draws on multiple disciplines in an effort to conceptualize the problem under study in its holistic dimension. Because of the problem focus, industrial relations researchers do not have the luxury of pursuing a narrow piece of a labor or employment problem.

This leads industrial relations theory and research to be more holistic in definition of the research question and multi-disciplinary in perspective.

Unlike colleagues who define their primary intellectual mission as the deepening of a discipline, there is little opportunity for exploring in depth what a discipline such as economics has to offer to the understanding of a complex phenomenon. A disciplinary perspective can offer sharp, deep, and rich insights into a problem but seldom can provide a complete or practical solution or approach to solving the problem. This does not, however, imply that industrial relations researchers should not be well grounded in some established discipline. Recall that most of the leading scholars of this early generation came from a strong disciplinary training either in economics (Commons, Dunlop, Kerr, etc) or history (Perlman, Brody, Gutman, Taft, etc) or one of the behavioral sciences (McGregor, Whyte, etc.) Yet each of these

scholars tended to go beyond the boundaries of their discipline to examine the broader contours of the problems of interest to them and borrowed from other disciplines and from their own experiences. Thus the value of the multi-disciplinary perspective found in the best industrial relations research is not that it denies or minimizes the contributions and insights of the various disciplines that also speak to the issue, but that it builds on and integrates prior and current work from these fields and does so at a sufficient depth to gain the respect of those working on the same issues within the discipline. This is a tall order, especially for graduate students in our field, but one that is the price of admission.

One implication of this multi-disciplinary perspective is that the best industrial relations teaching and research programs are ones that mix scholars trained in multiple disciplines with those trained directly in industrial relations. In this way the diverse theories and insights from the disciplines are brought to bear on research problems, teaching, and intellectual debates along with the specific perspectives of those trained directly in industrial relations. What should bind this diversity together is not a single effort to homogenize their research interests or perspectives but a shared interest in employment problems and an interest in enriching their own disciplinary and theoretical perspectives from interaction with colleagues from other approaches.

A third feature of industrial relations research is its

reverence for and appreciation of history. Commons and the Webbs, not to mention Karl Marx, all demonstrated through their work the importance of putting any contemporary problem or theoretical insight in its proper historical perspective. Indeed perhaps Commons's most enduring theoretical work--the paper in which he develops his proposition about the effects of the expansion of the market on employment conditions--is his essay on the history of shoemakers' (Commons, 1919). Moreover, the multi-volume history of labor produced by Commons and his students and colleagues between 1918 and 1935 is a lasting tribute to the importance attached to the study of history for its insight into the problems of the day.

The value of history provides another important lesson to industrial relations researchers. It suggests that the problems that we study are enduring, and not simply transitory features of either an early stage of industrial development or something so new that there is nothing to learn from a look at prior experience.

A fourth feature of industrial relations theory follows from its multidisciplinary character--it must be multi-method as well. Being trained in a social sciences in the late 1960s at Wisconsin meant that we were expected to become competent in social science theory, quantitative methods, and experimental designs. Campbell and Stanley's (1963) primer on quasi experimental designs was treated as a standard to evaluate the quality of the design of any research project or published paper; methodological questions on PhD preliminary examinations could be expected to range from critiques of the Coleman report to questions about times series

versus longitudinal designs for studying labor force participation rates to questions about construct validity, alternative tests for reliability of behavioral measures to the relative merits of path analysis verses two stage least squares equations for testing models that were amenable to structural equations. The emphasis on methodology was designed to bring home two central points: (1) It was time (indeed overdue) for industrial relations researchers to enter the realm of the quantitative social sciences, and (2) neither econometricians nor psychometricians had a monopoly on the best way to design and conduct quantitative analysis.

This was appropriate since industrial relations was slow to take up quantitative analysis and because of this the field lost ground to other disciplines that had added quantitative analysis to their tool kit at an earlier date. Indeed, it became quite obvious that one could not effectively study many of the most interesting and important issues of the day such as the effects of unions on wages, the determinants of inequality at the workplace and in society, or the effects of alternative employment and training policies and institutions on labor market outcomes without a sound preparation in research design and quantitative methods. It was concern over the latter issue--again a public policy concern--that led researchers like Glen Cain, Lee Hansen, and Gerald Somers--very different labor economists--one a Chicago trained econometrician, another a policy oriented economist who approached labor market policies from human capital and cost benefit analysis perspectives, and the other a Berkeley trained institutional economist interested

in "manpower" problems--to cooperate in the training of a cohort of students capable of evaluating the costs, benefits, and policy implications of various employment and training policies of the 1960s and early 1970s.

Yet, while recognizing the indispensable value and necessity of quantitative analysis, the respect for the insights of institutions, history, and case study research was never and should never be lost on industrial relations. Unlike some of our more pure disciplinary colleagues, we cannot afford to dismiss as "pseudo science" those who choose to use exclusively qualitative or exclusively quantitative methods. Both, and indeed variations of both, such as case studies and ethnographies or econometric analysis of large scale surveys and narrowly focused but tightly designed laboratory experiments with student subjects all can offer insights to industrial relations problems and need to be taken seriously rather than dismissed as lacking scientific rigor. Again this places a greater methodological burden on students of industrial relations than it does on their colleagues within disciplines such as economics, psychology, etc. but, again, this is the price of admission to the field.

Primary Feature of Industrial Relations

While the above features all capture important distinguishing features of industrial relations research, I believe the primary feature that distinguishes the field from its counterparts lies in the normative assumptions and perspectives that underlie our conceptualization of the employment relationship. Ever since the work of Marx, the Webbs, and Commons we have accepted what later (Walton and McKersie, 1965) became known as the mixed motive nature of employment relationships. That is, the parties to the employment relationship are tied together in an enduring web of partially conflicting and partially common interests or objectives. The task of industrial relations theory and research therefore is to deal with this phenomenon. Carried to the its logical conclusion, this means that industrial relations scholars are equally concerned with goals of equity and efficiency in employment relations (Barbash, 1987; 1989; Meltz, 1989).

This normative assumption sets the work of industrial relations scholars apart from neo-classical economics models, and much of the managerialist organizational behavior or human resource management research. In neo-classical economics, competitive markets are assumed to eliminate conflicting interests by producing optimal labor market outcomes whereas ever since the Webbs used the concept of the "higgling of the market" industrial relations researchers recognized that perfect competition poses a competitive menace to worker interests. This is why strategies for "taking

wages out of competition" have played such an important role in industrial relations models. While few organizational behavior theorists would argue with the general concept that conflict is a feature in employment relationships, most theories in this field take a managerialist perspective; that is, it is management's job to coordinate and manage these divergent interests. Conflict is viewed more as a pathological or undesirable state of affairs than as a natural feature of employment relationships. Thus, the task of organization theory is to explain why conflict occurs so that managers can resolve, reduce, or eliminate it. Organization theorists therefore tend to look at the employment relationship from the standpoint of those who control and manage it. Thus both neo-classical economics and most organization theory either deny or minimize the legitimacy and enduring nature of conflicting interests in employment or assume market forces or appropriate managerial behavior will obviate the need for institutional (legal or collective representation) regulation in employment relationships.

While I see the acceptance of conflicting interests in employment relationships as the primary defining characteristic of our field, there remain lively, and to some extent unresolvable, normative debates among different schools of thought regarding the sources of this conflict of interests and the prescriptions for dealing with it that flow from these different schools. Marxist or labor process scholars, for example, (Hyman, 1975) view the structure of capitalist society and modes of production as the

basic source of conflict and thus feel any policy prescriptions short of fundamental replacement of capitalism with a socialist state and ownership structure as failing to address the root cause of the problem. Others (Fox, 1974; 1990) agree with the theoretical perspectives of modern labor process theory but not its ultimate prescriptions. In contrast, those operating within what some label a pluralist perspective see the conflict as endemic to the structure of all employment relationships regardless of who owns and/or controls the means of production (Barbash, 1984). These scholars see the primary task of industrial relations as contributing to an understanding how conflicting interests can be resolved periodically and how the parties can expand the frontier of joint problem solving (Cutcher-Gershenfeld, 1991).

Since these different views reflect deep normative assumptions about the nature of society and economic relations, there is little likelihood that some common ground can be found between them--see for example the exchange between Richard Hyman and myself over the approach taken in Collective Bargaining and Industrial Relations (Strauss, 1982). Thus, rather than attempt to force students to accept a single normative perspective regarding the sources of conflict in employment relationships or the appropriate policy prescriptions to advocate for addressing these conflicts, we need to encourage each individual student to come to grips with this issue for him or herself. This objective should feature prominently in the teaching of industrial relations theory and the training of graduate students. Unfortunately, as our students are

quick to point out, sensitivity to the norms implicit in one's research often gets ignored or suppressed in published empirical studies.

Implications for the Teaching of Industrial Relations Theory

The defining features of our field outlined above have influenced how I try to teach industrial relations theory as well as the types of theory and empirical research projects I've engaged in to date. In this section I will review how these considerations inform my teaching of industrial relations theory and in the next section I will review how these perspectives have informed some of my past, current, and future research.

Industrial relations theory cannot be taught in a semester or even a year long seminar or course. Instead, what we seek to do in a formal semester course is to start the long process of acquainting students with the history and basic theoretical and methodological traditions and perspectives in our field and encourage students to explore these works in more depth on their own in ways that help them formulate their own perspective on these issues and identify their own conceptual, disciplinary, and methodological niche from which they will choose to work in the field. For this reason, the teaching of industrial relations theory works best when we have a mixture of students from mainstream industrial relations and students from different disciplines such as political science, economics, and organizational theory. The multi-disciplinary knowledge and perspectives create the debates that provide an important part of

the learning and training for debates that students will encounter over their work in the future. Diversity within the student body also reinforces the expectation that works from these different disciplines cannot and should not be ignored, devaluated, or discounted as irrelevant to industrial relations. We start the course in a traditional fashion by reading samples of the classics or the works that might pass for grand rather than middle range theories in our field from Marx, to the Webbs, to Commons, Perlman, Barbash to Dunlop. But these are counterpoised with the work of Milton Friedman, March and Simon, Gary Becker, Douglas McGreger, and others who take fundamentally different normative, theoretical, and disciplinary approaches to the study of employment and labor issues. Thus, the first task of an industrial relations theory course is to provide a rich appreciation of the classics in the field and the historical controversies over how to study employment problems, and the different disciplinary approaches to the field.

One device that I've used to gain a historical perspective on the field is to ask students to write a short comparative book review of an "old" and a "new" classic that addresses a similar set of questions or problems in order to examine differences in theoretical and methodological perspectives. An example of this comparison would be Slichter's 1941 book on Union Policies and Industrial Management or the 1960 classic Slichter, Healy, and Livernash The Impact of Collective Bargaining on Management with the 1984 Freeman and Medoff What do Unions Do? The goal of this exercise is to get students to appreciate both the enduring nature

of basic questions in our field and to critique the extent to which recent research has made progress in methodology, conceptualization, and insight into the problem compared to the earlier work.

Another approach used is to emphasize the social context in which the theory and research we read was generated. Students not only, for example read a sampling of Commons' work but we also read Kenneth Parson's (1963) insightful essay reviewing Commons' progressive perspectives on the social and labor problems of his time. Students are also encouraged to read Commons' entertaining autobiography Myself. Likewise, we read Antonio Gramsci's Selections from a Prison Notebook to understand the political context of the debates over Marx, Lenin, Luxemburg, and other socialist thinkers in the early part of the century. Students tend to take great fascination in learning more about the personalities and careers that lie behind the works of more recent scholars such as Dunlop, Whyte, Kerr, Shultz, McKersie, etc. I believe this serves an important purpose since as Ronald Schatz recent essay (1992) and Kaufman's (1991) historical treatise on the field point out--the framing of the problem and the intellectual debates that evolve cannot be entirely separated from the environment of the times and the personal experiences of the authors. This is a lesson again in introspection that bears repeating for all of us--we are influenced by our environment and this is both inevitable and positive. But at the same time we need to insure that we are not unconscious victims of events so that we bounce from issue to

issue or embrace the "politically correct" thinking of the moment at the expense of a longer run perspective and set of values. Recall that Commons and his students labored on their research for more than twenty years before state and eventually national legislators would take their ideas seriously and translate them into policy and then only because of a social and political crisis rather than because of the pure power of their theories and empirical evidence.

Throughout the course we seek to move across levels of theory and research--from the grand ideas and theory of Marx to the middle range models of Walton and McKersie (1965) to the empirical tests of theoretical ideas such as the work of Freeman and Medoff or more recently our own colleagues and former students involved in the Transformation project (Cappelli, 1983; Ichinoiski, 1986; Verma, 1983; Cutcher-Gershenfeld, 1991). This is one way to demonstrate that each of these levels of theory and empirical research contributes to our cumulative body of knowledge and that different people, at different career stages, have comparative advantages at different levels of theorizing and empirical analysis. The point to be emphasized is that one need not produce another Communist Manifesto or Industrial Democracy or Industrial Relations System or Behavioral Theory of Labor Negotiations to contribute to industrial relations theory!

The final objective of the industrial relations theory course (sometimes we don't get this far in a single semester) is to acquaint students with the current debates and theoretical

challenges facing the field. Given the problem centered tradition of the field, there is no shortage of contemporary topics and debates to fill up this part of the course. But it is interesting to see how the topics have evolved over the twenty years of studying and/or teaching industrial relations theory.

As a graduate student the major debates in industrial relations theory centered over grand questions such as "what's the appropriate definition, scope, and focus of the field? Is there a single dependent variable that brings focus to the field? Is Dunlop's system's model a theory or simply a useful collection of concepts tied together in an analytical framework (cf. Somers, 1969)?" The problem with these debates is that they made little headway in advancing theory or speaking to critical issues of the day. The critical issues were how can we end the Viet Nam War or what can be done to deal with the racial conflicts in the cities and at the workplace? Thus, there was a backlash against grand theory or broad definitional issues about the nature of our field as most of us in the U.S. turned to more narrow empirical research pursuits that, we hoped, could produce more tangible and concrete insights into tractable problems.

By the time I began teaching the pendulum slowly began to swing back to linking theory and empirical research, largely with the help of the laboratory of problems and empirical opportunities offered up by the growth of public sector collective bargaining and its attendant problems and debates. Thus, in the mid 1970s we spent considerable time reviewing and critiqueing various studies

of the effects of public sector bargaining laws and impasse procedures on strikes and bargaining outcomes. This was the most important policy debate of the time and many of us were deeply emersed in empirical research on this topic.

In the late 1970s and early 1980s research on the rise of non-union personnel practices, the role of employee participation, and the relationship between unions and workplace innovations became a central topic that allowed us to debate deep normative questions as well as critique the adequacy of the various experiments, case studies, and behavioral science surveys and models for improving the quality of work. This took us to a more micro level of theory and research and away from some of the deeper and grander theoretical debates of the past.

It was not until the tumultuous events of the early 1980s that most of us in industrial relations became reconnected to debates over basic theory and current events. The conditions for a paradigm shift suggested by Thomas Kuhn (1962) existed. There were too many anomalies between what we were observing in practice and the explanations offered by our received theories and empirical evidence. Union membership had been declining for a long time but had yet to be taken seriously by industrial relations scholars. Nonunion employment systems had grown up but continued to be viewed by industrial relations researchers as exceptions to the traditional collective bargaining relationships. Efforts to reform collective bargaining by introducing various forms of employee participation were seen as interesting (or perhaps naive)

behavioral science fads that failed to adequately understand the mixed motive nature of employment relationships and collective bargaining institutions. But in a series of works involving colleagues and students at MIT (see for example a collection of papers in Challenges and Choices Facing American Labor (1984), Piore and Sabel's, The Second Industrial Divide (1984), Katz's (1985) Shifting Gears, Cappelli's (1982; 1983) early empirical studies of concession bargaining) the shape of a reinterpretation of industrial relations theory and events began to unfold and to spark a debate over whether we were in fact experiencing a set of fundamental changes that required an equally fundamental rethinking of our analytical frameworks and models or simply were experiencing another in a long history of cyclical or transitory losses of union power that would result in a rebound of labor's influence in mirror image of the past. Nothing so invigorated the study of industrial relations theory and research as the power of this debate, fueled and reinforced by the fact that unions and companies themselves were engaging simultaneously in pitched debates over the same issues! Theory and practice indeed came together in this debate. One could get an audience of practitioners and researchers to take great interest in both the broad theoretical and the specific practical issues at stake in this work. The Transformation of American Industrial Relations (1986) represented an effort to bring together the various studies we and our colleagues and students had conducted on these issues.

While the specific features of this debate have shifted--there

is now less interest in the debate over whether changes are fundamental and structural in nature or merely incremental and/or cyclical and more concern over how to cope with the changes that have occurred--the adequacy of our interpretations and the utility of our "strategic choice" model remains subject to sharp debate (Lewin, 1987; Chelius and Dworkin, 1990; Chaykowski and Verma, 1992). We believe the next phase of this debate should take place through a comparative international context--an issue I will return to below.

Developing Industrial Relations Theory: Some Personal Examples

How does one go about developing theories of the middle range in industrial relations? There are probably as many answers to this question as there researchers in the field. I can only offer several personal examples and attempt to use these to illustrate what I believe are some generic features.

As emphasized above, the best opportunities for developing new theory in our field are found in the critical problems or issues of the day. This is what gave birth to the field in the early part of this century and it is what will sustain the field in the future. Thus, since the explosion of bargaining in the public sector was the dominant collective bargaining problem of the 1960s and early 1970s, it is not surprising that this topic captured the attention of many of us who began our careers during that time period. The initial problem to be confronted was a very basic one: Just what was different about public sector bargaining than collective bargaining as it was traditionally practiced in the private sector?

Many experienced practitioners and scholars were offering advice on how to "improve" the conduct of public sector bargaining based on their private sector experiences yet it was not clear that these insights generalized well to this new environment.

Interest in this basic question led me to conduct two case studies of city government bargaining (Kochan, 1972) that provided an in depth description of how the various parties to public sector negotiations behaved. From these case studies and a reading of various theoretical and empirical descriptions of private sector bargaining (particularly Stevens (1963) and Walton and McKersie (1965)) emerged the concept that bargaining in the public sector was distinguished by its multilateral nature. That is, instead of a bilateral (labor versus management) process in which internal differences were largely reconciled internally prior to the bargaining deadline, bargaining in the public sector was inherently multilateral in nature since the employer was composed of multiple interests and organized based on the governmental principle of separation of powers. To turn this finding derived inductively from case study research into a formal testable model or theory required an excursion into the relevant organizational and political science theories of intraorganizational conflict and political decision-making. From this a formal model with testable propositions was proposed and survey research design and data analysis plan was constructed.

Note the sequence: The labor problem of the day helped identify and define the research question; the initial case studies

provided the institutional understanding or foundation on which the question could be framed in a fashion that captured the actual practices involved; the relevant social sciences helped place the problem in a broader perspective and compare it to similar questions found in those literatures; and social science research techniques were employed to develop a formal set of propositions, a research design and measurement strategy, and a set of statistical procedures appropriate to test the model. Out of this came a rather modest "theory" of multilateral bargaining in city governments (Kochan, 1974;1975).

A second example involves a study of impasse resolution procedures I conducted with a group of students and colleagues at Cornell in the mid 1970s (Kochan, Mironi, Baderschneider, Ehrenberg, and Jick, 1979). Shortly after arriving at Cornell I discovered that the State of New York passed, on a three year experimental basis, an amendment to its Taylor Law governing impasse resolution for police and firefighters. The new amendment that was to take effect in July, 1974 and "expire" in July, 1976 added compulsory arbitration to impasse resolution process that had previously provided only factfinding with recommendations. Thus, it appeared that a natural "quasi-experiment" was about to be created. The question, therefore, was how could we evaluate the effectiveness of the alternative dispute resolution regimes?

This project illustrates the difficulty of developing and testing theories and conducting research that speaks to public policy debates. The first problem to be encountered was the lack

of any real theory to guide the research. Even identifying the key questions or criteria to use to evaluate the "success" or "effectiveness" of the alterative dispute resolution systems was uncharted territory. A review of the collective bargaining literature and especially the report of the panel of experts (John Dunlop, E. Wright Bakke, Frederick Harbison, and Chairman George Taylor) that recommended the provisions of the Taylor Law suggested that an effective dispute settlement system would avoid work stoppages, encourage the parties to settle their disputes without undue reliance on the procedures, and would not "bias" the outcomes of the process from what would have been negotiated by the parties themselves. Thus, the effectiveness criteria chosen for evaluating the procedures reflected the norms underlying the sanctity of "free collective bargaining" that had been espoused by industrial relations scholars for years.

To assess the net or independent effect of the alterative procedures on these process and outcome criteria required developing theories of the other factors shaping the probability of negotiations going to impasse and identifying other factors that influence the outcomes of public sector bargaining. This too proved difficult since at the time there were few theoretical or empirical studies of determinants of impasses or the effectiveness of mediation processes (mediation was embedded in both procedures as an intermediate step in the dispute resolution process). What had been written about arbitration was mainly warnings by neutrals that its presence would invariably produce a chilling or narcotic

effect on the parties that would reduce the incentive or ability to negotiate settlements without dependence on the procedure.

Finally, there was a singular lack of data. It became clear that if we were to do an adequate job of assessing the two procedures, we would need to collect data from the parties themselves at the level of the individual bargaining units. Thus, a massive two year data collection effort was initiated with the help of a National Science Foundation grant and the support of an advisory committee composed of representatives of the state Public Employment Relations Board, the governor's office, the state police and firefighter unions, and New York League of Cities and several respected and experienced neutrals. To the credit of these interested parties, they helped provide access to their colleagues for data collection and a venue for eventually discussing and debating the results of our work and our recommendations, and left the technical research design and analysis decisions to the research team.

Note again the sequence of this project: It began with a policy question and opportunity--a change in a key law and a defined timetable for the next political debate over the law; it required hard thinking about the appropriate research design and a mix of qualitative and quantitative techniques; it required development of models of mediation and negotiations that fit the specific context of public sector bargaining but that also drew on research from a broader array of social and behavioral sciences; it required the use of econometric techniques--indeed some that ended

up being the subject of considerable debate after the fact among research team members (see Butler and Ehrenberg, 1981; Kochan and Baderschneider, 1981); and the results of the work ended up feeding into a public policy debate.

In the process a lot was learned about the effects of different types of impasse procedures, some of which could be generalized to contexts outside of New York and to procedures other than the specific ones embedded in this particular law (Kochan, 1976). But whether any new fundamental theoretical breakthroughs were achieved is more questionable. We were able to offer and test a new theory of the labor mediation process (Kochan and Jick, 1976) but perhaps the broader lesson of this project was that public policy evaluation studies such as this one will only generate new theory if the researchers build this objective into the design of the project on their own initiative and as a separate agenda from the policy makers and practitioners who have significant stakes in the outcomes of the research. This project also illustrates one of the strengths and limitations of our field--the involvement of the practitioners and policy representatives made for exciting and highly relevant research that could serve as input to an important public policy decision but at the same time required such intensive detailed analysis that abstract and lasting theoretical contributions were hard to produce. Moreover, in some respects, the project was premature--since its completion considerable theoretical research on arbitration, mediation, and dispute resolution, and negotiations has been produced which would now be

available to any industrial relations researcher who takes on a similar task.

The final example of theory development--the process that led to the publication of The Transformation of American Industrial Relations -- was already alluded to in an earlier section of this paper. That effort again involved multiple colleagues and students and extended (indeed continues) over six years prior to the publication of the Transformation book. Like the other projects it started from a puzzle about changing practices in collective bargaining and industrial relations. Soon after arriving at MIT it became apparent to many of us on the faculty (Robert McKersie, Harry, Katz, Michael Piore, Charles Sabel, and myself) that something important was changing or likely to change in the way companies were approaching labor relations. Moreover, it had been clear to many of us that the pressures for change on collective bargaining were building up for a number of years without a significant response from the parties (cf Kochan, 1980, pp. 506-11).

But it was not until we went into the field to conduct a number of informal interviews and case studies of contemporary management policies that we came away with a deeper hunch or grounded hypothesis that the change process was already underway in many companies. What we observed was a break with the past--the acceptance of the traditional norms of collective bargaining had given way to a more aggressive managerial posture toward unions and toward an effort to bring individual and small groups of employees

more directly into the problem solving process at the workplace. Moreover, we observed significant power shifts within the management structure--industrial relations professionals had lost power, line managers had taken control of what had previously been industrial relations policy decisions, and human resource professionals without deep knowledge of or appreciation for unions and collective bargaining were ascending in influence. These initial exploratory interviews led to a series of sub-studies conducted over the next several years by a our students and colleagues. The Transformation book served as an interim summary of the strategic choice framework that is still under development. Whether this amounts to a new "theory" of industrial relations will have to be judged by others, presumably at some time in the future.

What is clear is that this project shares several features with those described above and with predecessor projects in our field: It seeks to address the critical questions of our time--namely, is the U.S. industrial relations system able to transform itself in ways that can meet the efficiency and equity interests (Barbash, 1987; 1989; Meltz, 1989) and requirements of the parties in world that has changed in significant ways. It uses multiple methods--historical analysis, case studies, and quantitative analysis of published and new survey data. It involves close interactions with the parties themselves ranging from panels of management, union, and government representatives who supported our case studies and surveys, to leaders of the AFL-CIO who shared data and included us and some of our ideas in their deliberations over

future directions and strategies, to debates with our research colleagues over our interpretations and the utility of the theoretical framework that emerged out of the project. Moreover, the framework itself is a joint product that was influenced greatly by those involved directly in the research and the work of close colleagues such as Piore and Sabel's The Second Industrial Divide.

Thus these three different attempts to develop theories of the "middle range" in industrial relations illustrate the diversity of approaches to theory construction and different levels of abstraction and generality that theory can take in our field. Each, however, was grounded in what was felt to be a critical problem; each required drawing on insights from different disciplines; each required multiple methods and more than a one-shot study; each started with case study and historical analysis to provide the institutional detail needed to speak to the basic issues, and each attempted to generate results that spoke both to theory and to the needs and interests of policy makers and/or practitioners. Such, I believe, are the defining features of middle range industrial relations theories and empirical research.

Future Challenges for Industrial Relations Theory

While it is often common for researchers to claim there is a crisis at hand only so they can propose a solution, I believe the field of industrial relations is indeed under siege if not in a state of crisis that will test once again the viability of our paradigm. This time the crisis revolves around the very normative premises that I argued above provide the field its primary identity

or niche in the social sciences. Stated most directly: Is the mixed motive perspective still viable?

Alan Fox (1974; 1990) is perhaps one of the most influential yet under recognized industrial relations theorists of our generation. His 1974 book Beyond Contract: Work, Trust, and Authority in Industry, while somewhat dense and ambiguous, continues to pose one of the most basic challenges to contemporary industrial relations theory and research. Fox raises the question of whether a pluralist industrial relations system, one based on institutions that legitimate and institutionalize conflicting interests at the workplace, is capable of developing and sustaining a high trust relationship. He appears to be rather pessimistic about this and hints at his gradual disillusionment with the pluralism built into British and Anglo-Saxon industrial relations institutions. Recently this same theme has been developed, albeit in very different ways, by Charles Sabel (1991) and by Wolfgang Streeck (1991). Sabel argues that developing and sustaining trust relations is essential to rebuilding local and macro-economic institutions capable of managing the industrial restructuring that needs to occur to implement economic development programs. Streeck (1991) argues unions that continue to be built on an assumption of adversarial workplace relationships are doomed to experience further declines in membership and inhibit economic progress in their societies. Instead he advocates union strategies that seek to improve worker welfare not through simply distributive bargaining but through strategies that promote and enhance the full

development and utilization of skills in organizations and across the economy.

The concept of trust is also central to the authority relations in Asian economies that grow out of Confucianist cultures. Industrial relations systems such as found in Singapore, Korea, Japan, Taiwan, Hong Kong have combined in different ways both authoritarianism and personal trust in ways that create room for considerable debate, analysis, and eventually, cross national learning that Western scholars would be wise to neither ignore nor interpret solely through our traditional pluralist or social democratic lenses. Something is different about the trust and authority relations in these countries that has yet to be fully understood by those of us who look in from the outside. Likewise, these relationships have yet to be explained satisfactorily by those who experience and write about them from within the cultures. Thus, there may continue to be a cultural gap in industrial relations scholarship that needs to be closed if we are to fully exploit the opportunity to learn about how trust is developed and maintained in different cultural, legal, and institutional settings.

What seems to bind together all those interested in this concept is the proposition that high trust (or avoidance of what Fox described as a high conflict/low trust syndrome) is essential to achieving high levels of economic performance and worker welfare--or the twin goals of efficiency with equity at the workplace and in society. If, as I believe, these continue to be

the critical objectives of an industrial relations system and therefore serve as the ultimate normative goals of industrial relations theory, then the study of trust relationships from the level of informal work groups to the interactions of labor, government, and business at the macro levels of society deserves a prominent role on our theoretical and empirical agenda in the years ahead.

The 1980s were very hard on labor organizations around the world for a very simple reason that goes back to the Commons' proposition on the expansion of the market. Unions gained power in national industrial relations systems as they developed structures and institutions for "taking wages out of competition." With the increase international competition, the ability of unions within any country to take wages out of competition by developing national institutions weakened.

While this has posed significant challenges to unions it likewise has challenged industrial relations theorists to understand what the equivalent of Commons proposition is for economies where low wage competition from outside if not inside the country is a constant threat. This has sparked a surge in theoretical debate in our field that has yet to be resolved but offers considerable room for more focused model building and empirical research. One broad theoretical answer to this debate is found in the theories of flexible specialization put forward by Piore and Sabel (1984) and Kern and Schumann (1985): Workers and unions will gain greater leverage as markets become more

specialized and technologies demand greater flexibility thereby creating an environment where skills need to be upgraded and worker trust and motivation maintained. The result is a high value added, high wage economy.

In our own work (Kochan, Katz, and McKersie, 1986) we have modified this view somewhat by offering a strategic choice perspective. This perspective accepts and builds on the basic premise that a high value added competitive strategy for individual firms and nations is necessary if workers and unions are to avoid the type of wage competition that leads to a deterioration of working conditions and living standards. But it goes on to argue that there is no natural set of market or technical forces that will automatically produce the high value added, high skill, high wage outcomes. Instead we offer the hypothesis that the strategic choices of business, government, and labor influence the outcomes. Some firms will stay committed to low cost competitive strategies and while others may move more quickly and fully to the high value added strategies. We argue that the parties at the individual firm and perhaps (although this is not well developed in our original work) at the industry or national levels have some discretion over how they choose to compete. Thus we emphasize the need to look at the interactions of market and technical forces with the strategic choices of business, labor, and government.

A third argument critiques both of the above views for failing to adequately consider the role of the state and national institutions in shaping the environment in which firms and worker

organizations compete and labor. The regulation school (Boyer, 1988), the neo-corporatists (Goldthorpe, 1984), and other models argue that in Europe, for example, state policy, and in the case of the Economic Community, perhaps eventually regional trading blocs, policies will influence the social conditions of work that firms must meet and this is eventually what will provide the counterpart to Commons' expansion of the market hypothesis. This school of thought would argue that the Webbs foresaw correctly the rise of "legal enactment" as the key regulatory force in industrial relations, following the era of mutual insurance and common rule through collective bargaining.

This is more than a small debate over the appropriate analytical model to use to reinterpret contemporary industrial relations. The different models have significantly different implications for industrial relations theory and policy analysis and institutional development. The flexible specialization models suggest that market and technological forces will force firms to take worker interests into consideration regardless of whether workers are represented by effective unions or other institutions that provide voice in strategic decisions. The strategic choice models suggest that the key decisions lie in how firms respond to conflicting market pressures--niche markets may not be big enough to go around for all firms, technology is not deterministic in how firms deploy it or its impacts on skills and employee control, and other competing environmental pressures such as pressures from financial markets and institutions, political factors and the

values and traditional strategies of the parties all play important roles in shaping the response and the results of efforts to transform tradition practices. Thus, organizational governance arrangements and employee voice in strategic decision-making become important in these models. The regulation or state-institutions' view elevates the level of analysis farther by arguing that we need to examine the role of state policy, culture, and values as determinants of the response to global competition and differentiated markets and new technologies.

What all three of these models have in common is a view that the traditional institutional lens of industrial relations research that focused on personnel policies and/or collective bargaining needs to expand and look more closely at developments at higher levels of the management, economic and political system in order to understand contemporary events. Moreover, all three of these models point to the need for more comparative-international research that provides us with a wider variety of institutional and political responses to changing markets and technologies. Thus, this set of theoretical challenges should serve to rekindle interest in the field of comparative industrial relations research.

As Adams (1991) has stated industrial relations has laid claim to the study of "all aspects of the employment relationship." Yet over time, too much of industrial relations research (including my own) has focused on the narrower set of topics and issues associated with collective bargaining and formal institutions of worker representation. This left the field of personnel, now human

resource management, to others who often operate within a managerialist or what Fox called a unitary perspective or set of normative premises and that take the individual organization or firm as the boundary for their analytical models. Human resource management research has exploded in recent years as management became a more dynamic actor or catalyst for change in employment relations. Yet, there are significant intellectual limitations to the current human resource management literature that industrial relations researchers could fruitfully address.

The first major limitation stems from the firm level focus of attention. Human resource management theory has yet to move beyond its individual firm boundaries. This limits its utility as an analytic device in settings where the probability of adopting and sustaining investments in human resource practices depends on the whether other firms in one's product and/or labor markets adopt complementary innovations. Moreover, the movement to strategic human resource management research called for by both academics and practitioners in recent years has yet to bear fruit in terms of significant theory or evidence on the extent to which human resource considerations influence strategic decision-making within the firm.

Despite these limitations, the separation of human resource management research from industrial relations poses an important intellectual and practical challenge to industrial relations researchers.

One of the most obvious labor market developments of recent

years has been the increase in the diversity of the work forces found in modern employment relationships. Labor force participation rates of women have steadily increased in most industrialized countries and, perhaps more importantly, the career orientations of women have correspondingly broadened and risen making all issue of equal opportunity and gender relations at the workplace more prominent part of workplace relations. Part-time work, immigration, the growth of the internationalization of management in transnational corporations, the increased use of contract and temporary workers, all contribute to greater diversity in employment relationships. This increased diversity challenges traditional institutions and views of the employment relationship and institutional arrangements that seek to conceptualize or manage employment as a bilateral, employee versus employers or a bilateral partnership between collective agents of workers and employers. Whether one focuses on the distributive or the integrative dimensions of the employment relationship diversity challenges the utility of bilateral models. This, perhaps, is one reason that unions and collective bargaining have experienced difficulty in small establishments and in the faster growing service, white collar, and nontraditional employment relationships. To continue to be relevant as a field of study that encompasses "all aspects of the employment relationship" will require that we devote more attention to the study and the design of institutions capable of capturing and addressing the critical features of these more diverse employment settings. Again, collective bargaining as

it has been traditionally structured seems ill suited to the task of addressing this diversity. In the absence of significant body of theoretical or empirical work, little regulation and even less direct or indirect employee representation has been brought into these relationships.

In summary, industrial relations researchers need to carry on the tradition of addressing the critical problems facing the parties to contemporary employment relationships. As in the past this will require us to conduct historically well grounded multidisciplinary-multi-method research that conceptualizes the problem in its full complexity or holistic dimensions at multiple levels of analysis. It will also require giving a prominent place on our agenda to the study of issues such as diversity in the workforce and in employment settings, developing and sustaining trust, the role of human resource policy and employee voice in organizational governance, and the comparative analysis of industrial relations and human resource management institutions and policies.

Finally, perhaps we should revisit the point made at the outset of this paper, namely, has the problem centered, scholar-practitioner role model for industrial relations researchers served us well or held us back in the task of developing theory? Schatz (1992) recently suggested that this orientation has both deepened and limited the intellectual development of the field. Such arguments have been made before. For example, one of the most prolific of the institutional labor historians of the Wisconsin

School, Phillip Taft, was often criticized by other labor historians for being too close to the labor movement. This closeness to practice, and to the practitioners, in his critics eyes, caused Taft to lose his objectivity; more importantly, it led him and others in the Wisconsin School of labor history to focus too narrowly on the study of the official institutions of labor rather than to examine workers's cultural, social, and political environments and behavior (Gutman, 1976; Grossman and Hoyer, 1982). Derber, likewise criticized industrial relations researchers of the 1960s for being too willing to "follow the headlines" rather than staying committed to a more enduring set of problems and issues over time. Dunlop once criticized those who sought to project the future of unionism based more on their wishes or hopes than on hard analytical thinking and evidence. His predictions turned out to be more correct than those who either predicted labors demise in the 1960s or a major wave of union growth fed largely by expected gains among white collar workers. And, in a personal bit of advice Clark Kerr--one of the most preeminent scholar-practitioner-national figures of our time -- cautioned against efforts to be all things to all people. He noted that the development of industrial relations and labor economics theory suffered in the 1960s as leading industrial relations scholars of the day--Dunlop, Kerr, Shultz, Weber, Seigel, Fleming, McKersie, etc., were called upon to put their considerable policy and administrative skills to work in various public service or policy positions. This has been an important legacy of our field and, I believe, it has both deepened

and limited theoretical development. But I believe industrial relations is addicted to this affliction, perhaps by the very self-selection of those who find the field attractive. For I can think of no other field that provides more opportunities and challenges to put the Wisconsin Idea that helped attract Commons to the Madison campus, namely the task of a true scholar is to combine theoretical research with commitment to teaching and public service. This is what the field has been about in the past, and I believe what will continue to be one of its most attractive traits in the future. Whether it proves in the long run to be a liability or an asset will have to be judged by those whom we seek to serve through our work.

REFERENCES

- Adams, Roy, "All Aspects of People at Work," Unpublished manuscript, McMaster University, 1991.
- Barbash, Jack, The Elements of Industrial Relations. Madison, WI: University of Wisconsin Press, 1984.
- Barbash, Jack, "Like Nature Industrial Relations Abhors a Vacuum, Relations Industrielles vol. 42, 1987, 168-179.
- Barbash, Jack, "Equity as Function: Its Rise and Attribution," in Jack Barbash and Kate Barbash (eds.), Theories and Concepts in Comparative Industrial Relations. Columbia, S.C.: University of South Carolina Press, 1989, 113-22.
- Boyer, Robert, (ed.) The Search for Labor Market Flexibility: The European Economic Community in Transition. Oxford: Clarendon Press, 1988.
- Butler, Richard J., and Ronald G. Ehrenberg, "Estimating the Narcotic Effect of Public Sector Impasse Procedures, "Industrial and Labor Relations Review, 35 1981, 3-20.
- Campbell, Donald and Julian Stanley, Experimental and Quasi-Experimental Design for Research. Chicago: Rand McNally, 1963.
- Cappelli, Peter, "Concession Bargaining and the National Economy," in Proceedings of the Thirty-fifth Annual Meetings of the Industrial Relations Research Association. Madison, WI: Industrial Relations Research Association, 1983, 362-71.
- Chaykowski, Richard and Anil Verma, Canadian Industrial Relations in Transition. Toronto: Holt, Reinhart, and Winston, forthcoming, 1992.
- Chelius, James and James Dworkin (eds.) Reflections on the Transformation of American Industrial Relations. New Brunswick: IMLR Press, 1990.
- Commons, John R., "American Shoemakers, 1648-1895: A Sketch of Industrial Evolution," The Quarterly Journal of Economics, 1919.
- Cutcher-Gershenfeld, Joel, "The Impact on Economic Performance of a Transformation in Workplace Relations," Industrial and Labor Relations Review, (January, 1991),

Dunlop, John T. Industrial Relations Systems. New York: Holt, 1958.

Fox, Alan, Beyond Contract: Work, Authority, and Trust Relations. London: MacMillan, 1974.

Fox, Alan, A Very Late Development: An Autobiography. Coventry: Industrial Relations Research Unit, University of Warwick, 1990.

Freeman, Richard B. and James L. Medoff, What Do Unions Do? New York: Basic Books, 1985.

Goldthorpe, John (ed.) Order and Conflict in Contemporary Capitalism. London: Oxford University Press, 1985.

Grossman, Jonathan and Hoyer, "Labor History," in Thomas A. Kochan, Daniel J.B. Mitchell, and Lee Dyer (eds.) Industrial Relations Research in the 1970s: Review and Appraisal. Madison: Industrial Relations Research Association, 1982.

Gutman, Herbert, (ed), Work, Culture, and Society in Industrializing America. New York: Vintage Books, 1976.

Hyman, Richard, Industrial Relations: A Marxist Introduction. London: MacMillan, 1975.

Ichinoiski, Casey, "The Effects of Grievance Activity on Productivity," Industrial and Labor Relations Review 40, 1986, 75-89.

Katz, Harry C., Shifting Gears. Cambridge: MIT Press, 1985.

Kern, Horst and Michael Schumann, The End of the Division of Labor. Munich: Beck Publishers, 1985.

Kochan, Thomas A. City Employee Bargaining with a Divided Management. Madison: Industrial Relations Research Institute, University of Wisconsin, 1972.

Kochan, Thomas A. "A Theory of Multilateral Collective Bargaining in City Governments," Industrial and Labor Relations Review, 27, 1974, 525-42.

Kochan, Thomas A., "City Government Bargaining: A Path Analysis," Industrial Relations, 14, 1975, 90-101.

Kochan, Thomas A. Collective Bargaining and Industrial Relations. Homewood, Ill: Irwin, 1980.

Kochan, Thomas A. (ed) Challenges and Choices Facing American Labor. Cambridge, MA: MIT Press, 1984.

Kochan, Thomas A. and Jean Baderschneider, "Dependence on Impasse Procedures: Police and Firefighters in New York State," Industrial and Labor Relations Review, 31, 1978, 431-49.

Kochan, Thomas A. and Jean Baderschneider, "Estimating the Narcotic Effect: Choosing Techniques that Fit the Problem," Industrial and Labor Relations Review, 35, 1981, 21-28.

Kochan, Thomas And Todd Jick, "The Public Sector Mediation Process: A Theory and Empirical Examination," Journal of Conflict Resolution, 22, 1978, 209-40.

Kochan, Thomas A., Harry C. Katz, and Robert B. McKersie, The Transformation of American Industrial Relations. New York: Basic Books, 1986.

Kochan, Thomas A., Mordechai Mironi, Ronald G. Ehrenberg, Jean Baderschneider and todd Jick, Dispute Resolution under Factfinding and Arbitration. New York: American Arbitration Association, 1979.

Kaufman, Bruce, The Rise and Decline of the Field of Industrial Relations in the United States. Unpublished manuscript, Georgia State University, 1991.

Meltz, Noah, M. "Industrial Relations: Balancing Efficiency and Equity" in Jack Barbash and Kate Barbash eds., Theories and Concepts in Comparative Industrial Relations, Columbia, South Carolina: University of South Carolina Press, 1989, pp. 109 - 113.

Lewin, David, "Industrial Relations as a Strategic Variable," in Morris Kleiner, Richard Block, and Myron Roomkin (eds) Human Resources Performance of the Firm. Madison: Industrial Relations Research Association, 1987, 1-41.

Merton, Robert, Social Theory and Social Structure: Toward the Codification of Research. Glencoe, Ill: The Free Press, 1949.

Parsons, Kenneth H., "The Basis of Commons' Progressive Approach to Labor Policy," in Gerald G. Somers (ed) Labor, Management, and Social Policy. Madison: University of Wisconsin Press, 1963, 3-24.

Piore, Michael and Charles Sabel, the Second Industrial Divide. New York: Basic Books, 1984.

Sabel, Charles, "Studied Trust." Unpublished manuscript, MIT Department of Political Science, 1991.

Schatz, Ronald W., "From Commons to Dunlop: Rethinking the Field and Theory of Industrial Relations," in Howell Harris and Nelson Lichtenstein, (eds.), Defining Industrial Democracy: Work Relations in Twentieth Century America. Cambridge: Cambridge University Press, forthcoming, 1991.

Slichter, Sumner, Union Policies and Industrial Management. Washington, D.C.: The Brookings Institution, 1941

Slichter, Sumner, James J. Healy, and E. Robert Livernash, The Impact of Collective Bargaining on Management. Washington, D.C.: The Brookings Institution, 1960.

Somers, Gerald G. (ed.), Essays in Industrial Relations Theory. Iowa City: Iowa University Press, 1969.

Stevens, Carl, Strategy and Collective Bargaining Negotiations. New York: McGraw Hill, 1963.

Strauss, George (ed), "Review Symposium," Industrial Relations 21, 1982.

Streeck, Wolfgang, "More Uncertainties: German Unions Facing 1992," Industrial Relations 30 (Fall, 1991), 317-49.

Verma, Anil, "Union and Nonunion Industrial Relations at the Plant Level," PhD Dissertation, MIT Sloan School of Management, 1983.

Walton, Richard E. and Robert B. McKersie, A Behavioral Theory of Labor Negotiations. New York: McGraw Hill, 1965.