

CROSS CULTURAL COMMUNICATION
BETWEEN SCIENTISTS AND LAWYERS
IN JUDICIAL POLICY-MAKING

by

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ABSTRACT

A sociological-anthropological approach is taken in formulating and justifying a theory of how scientists and lawyers communicate in the courtroom. Science and law are depicted as being different languages, neither of which, like any other language, can be understood apart from the culture that it comes from. The study regards the philosophy of science and the philosophy of law as describing the languages spoken by "scientific natives" and "legal natives," respectively. The cross examination of the scientific expert witness is then analyzed as an interaction in which people from different cultures (that is, scientists from the scientific culture, and lawyers and judges from the legal culture) attempt to communicate with each other.

Because these people are, in general, neither "bilingual" nor "bicultural," the study finds that scientific expertise, whether evaluated favorably or unfavorably, is necessarily misunderstood in the judicial process, not just by the cross-examining lawyer, but also, more importantly, by the judge and by the lawyer with whom the scientist is collaborating.

Actual cross examinations are analyzed and annotated. They are taken from Equal Employment Opportunity Commission v. American Telephone and Telegraph Company (F.C.C. Docket No. 19143), a case involving sex discrimination. The study draws observations from the cross examinations of an economist, a social psychologist, and a physician.

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INTRODUCTION AND SUMMARY

In this study, I formulate, put forward, and justify a theory of how scientists and lawyers communicate with each other in the courtroom. I approach science and law as if they were distinct and dissimilar languages, neither of which, like any other language, can be understood apart from the culture that it comes from. I analyze actual examples of the cross examination of the scientific expert witness as being an interaction in which people from different cultures (that is, scientists from the scientific culture, and lawyers and judges from the legal culture) attempt to communicate with each other.

Because these people are, in general, neither "bilingual" nor "bicultural", I find that scientific expertise, whether evaluated favorably or unfavorably, is necessarily misunderstood in the judicial process, not just by the cross-examining lawyer, but also, more importantly, by the judge and by the lawyer with whom the scientist is collaborating.

In this study, I employ sociological and anthropological thinking to deliver a depiction of how scientists and lawyers talk to each other in the courtroom. In the way that sociologists and anthropologists enter contemporary communities or preliterate societies, and ask,

"What is the function of this artifact?",

"What is the meaning to this social activity?",

"What role does this artifact play in this social activity?",

I similarly enter the world of the scientists, the lawyers, and the judge in my case study, and ask,

"What is the function of this artifact, that these people call 'science'?",

"What is the meaning to this social activity, that these people call 'judicial policy-making'?",

"What role does this artifact, called 'science', play in this social activity, called 'judicial policy-making'?".

Taking this approach, I find that the artifact, called "science", had different meanings for the scientific natives, on the one hand, and the legal natives, on the other hand. In conversations, words with the same pronunciation signified one meaning to the scientists, but another meaning to the lawyers. It was as if the two groups were speaking different languages, reflecting the different ways of reasoning and the different value systems existing in their respective cultures.

In one sense, the task I carry out in this study is remarkably simple. I approach the philosophy of science and the philosophy of law as being ready-made descriptions of the languages that scientists and lawyers speak. Framed in this way, a good portion of my work has already been done for me: I may regard the voluminous literatures of the philosophy of science and the philosophy of law as being thorough documentations of the two languages. This is work that I may premise, rather than duplicate. What remains for me to carry out, then, is not the grandiose task of answering the questions, "What is science?" and "What is law?", for better-abled scholars have already provided thorough answers; instead, what remains is the humble (though arduous) task of (1) placing the answers to the two questions side by side, (2) performing the comparison, and (3) applying the results to explain scientist-lawyer communication in the courtroom.

As might be expected, the languages of science and law (like any other two languages being compared) show some differences. What is interesting is not that they are different, but that their ways of reasoning--in particular, their respective manners of justification--happen to proceed in symmetrically opposite ways. Please turn to Diagram 1, on the following page.

The primary concerns of science and law are in justifying different things; in science, it is the premise; in law, it is the conclusion. Also, the act of justification in science is conclusion-oriented (the premise may be accepted as valid only if the conclusion is valid), while, in law, justification is premise-oriented (the conclusion may be accepted as valid only if the premise is valid). Diagram 1 is a visual summary of this study's analysis of the philosophy of science and the philosophy of law, as these are expressed in the writings of Karl Popper, Thomas Kuhn, Alfred Schutz, Ernest Nagel, John Dewey, and Roscoe Pound.

What happens, then, when a judge or a lawyer attempts to understand scientific expertise? What happens when legal methods, not scientific methods, are used to evaluate scientific theories? An instance from my case study, in which the lawyers and the judge spoke with an economist, is illuminating. Please turn to Diagram 2, on the page following Diagram 1.

There was the curious result in which, in the eyes of the lawyers and the judge, scientific justification appeared premise-oriented. They projected their own legal meaning onto the economist's major premise, minor premise, and conclusion. This occurred in the same general way that people subconsciously and ethnocentrically project meanings from their own culture onto the artifacts, similar in appearance, of a different culture. The re-

Diagram 1

SCIENCE

THE THEORY,
whose
justification
is the
primary concern

major premise

Men
are
mortal.

minor premise

Socrates
is a
man.

conclusion

Socrates
is
mortal.

THE INITIAL
CONDITIONS,
from a concrete
situation

THE HYPOTHESIS,
a conclusion drawn
from the theory
and an indicator
of its validity.

conclusion-oriented justification

LAW

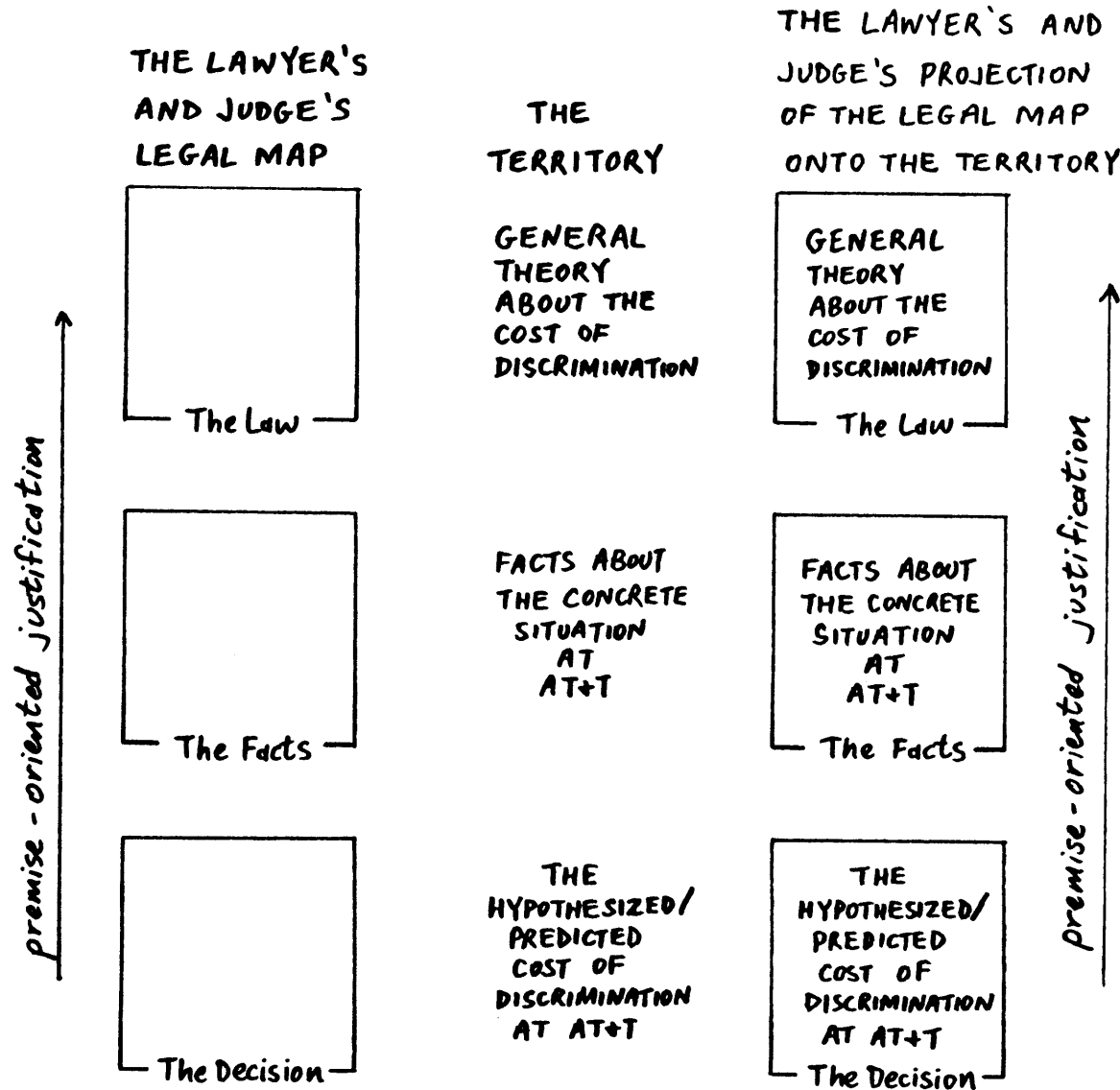
THE LAW,
a premise to
the decision
and an indicator
of its validity.

THE FACTS,
a premise to
the decision
and an indicator
of its validity.

THE DECISION,
whose
justification
is the
primary concern

premise-oriented justification

Diagram 2



sult was confusion and miscommunication. The legal natives understood and evaluated the economist's research not for what it was (a work whose justification was known through its conclusions), but for what it was not (a work whose justification was known through its premises).

The theme of this study is that scientists in the courtroom are people who are estranged from their own culture and the collegial support it provides. Although they are aliens in the legal culture, they are nonetheless subject to this other culture's rules. Therefore, because of (1) the opposing logics of science and law, and (2) the institutional forces of the courtroom, which sanction the legal logic over the scientific logic, I find that scientific expertise, whether evaluated favorably or unfavorably, is necessarily misunderstood in the judicial process.

In chapter one, the task can be simply stated, that we need merely to place the philosophy of science and the philosophy of law side by side and then to compare the two. However, the vast literatures generated by the two philosophies readily threaten to turn the task into a never-ending one. We restrict our focus, then, to just those portions of the literatures that are relevant to scientist-lawyer communication in the courtroom. Accepting the fact that justification is "the name of the game" for the activities of the courtroom, particularly the social activity called "the cross examination of the expert witness," we start off with just those portions of the two philosophies which pertain to the respective meanings that scientists and lawyers attach to the act of justification. In a figurative way, Popper, Kuhn, Schutz, Nagel, Dewey, and Pound present "thick descriptions" or "ethnographies" of the respective meanings to the social activity, called

"justification", in the respective worlds of the scientific natives and the legal natives.

In chapter two, we entertain the question about the extent to which chapter one's description of the scientific language, in being taken from the philosophy of (natural) science, applies to social science. After all, Popper and Kuhn had observed natural scientists; therefore, the judgment that the philosophy of science applies in whole or in part to social science either entails a leap of faith or, preferably, is a point that can be demonstrated. With regard to the purpose of this study, we need to demonstrate only that the philosophy of science applies "in part", namely, the part pertaining to justification. Does the scientific meaning to justification, which in chapter one we borrowed from the philosophy of (natural) science, also apply to social science?

This question is crucial because social science plays a dual role in this study. Social science is both this study's object (the testimonies which social scientists, as expert witnesses, offered in our case study) and this study's method (the sociological-anthropological manner in which we formulate and justify our theory of scientist-lawyer communication). In a reflexive way that happens to improve the rigor of this study, how we characterize social science as this study's object must apply to our practice of social science as this study's method, and at the same time, how we characterize social science as this study's method must apply to our observation of social science as this study's object.

To answer the specific question regarding the nature of justification in social science, we turn to the writings of Alfred Schutz. We make the

case for the position that the manner of justification in social-science inquiry is the same as in natural-science inquiry. We embrace Schutz's analysis in which he applies the words of Ernest Nagel, a philosopher of natural science who maintains stringent standards for scientificity. What distinguishes social science from natural science, we will demonstrate with considerable help from Schutz, is not the approach each one takes to the justification of theories, but the approach each one takes to the formulation of theories (which is a richer and more problematic challenge in social science).

Having characterized the languages of science and law and the method of social-science inquiry in the first two chapters, we are ready to inquire into an actual cross examination of a scientific expert witness in chapter three. The scientist is Dr. Orley Ashenfelter, an economist who testified on behalf of the Equal Employment Opportunity Commission, in its litigation against the American Telephone and Telegraph Company for practicing race and sex discrimination. The AT&T lawyer, Mr. Levy, cross examined Dr. Ashenfelter on (1) EEOC Exhibit 2A (consisting of "statistical evidence" that Dr. Ashenfelter had helped prepare for the EEOC), and (2) his written testimony, "Telephone Rates in the Absence of Discrimination" (an economic analysis providing predictions or hypotheses of the percentage by which AT&T's costs, and hence telephone rates, would have fallen if AT&T had employed women to the full extent that their productivity justified). Perhaps it is because economics self-consciously emulates the scientific approach, or because the mathematical form of economics lays bare what its theories do and do not say, that Dr. Ashenfelter illustrates so clearly for us the conclusion-

oriented mode of justification in science. It contrasts sharply against the premise-oriented mode of justification in law, which Mr. Levy illustrates. Chapter three looks at the following issues that arose in the Ashenfelter cross examination: the different scientific and legal meanings to the assumptions or premises in a scientific theory; the different scientific and legal meanings to statistics; and the different scientific and legal meanings to the testing of a scientific theory's prediction.

By the end of chapter three, we will have formulated and illustrated our theory of scientist-lawyer communication in the courtroom. In chapter four, we test the theory.

In the manner of a "natural experiment", the cross examination of a different scientific witness introduces variations into the circumstances surrounding the subject matter. It introduces (1) a different person in the role of scientific witness, who applied (2) a different theory to (3) a different set of facts, all of which came under the scrutiny of (4) different persons in the role of lawyer, whose attention was focused on (5) a different portion of the scientific "territory" (that is, the initial conditions, rather than the theory as in Dr. Ashenfelter's case). We justify our theory of scientist-lawyer communication by testing its hypothesis that there should be no differences in the premise-oriented behavior of the new lawyers, the conclusion-oriented behavior of the new scientist, or the miscommunication between the two in the courtroom. In observing no such changes, we would be witnessing either a remarkable coincidence, or a corroboration of our theory. The scientist in chapter four is Dr. Judith Long Laws, a social psychologist who illustrated her discipline's theories with

data from AT&T. Her testimony was entitled, "Causes and Effects of Sex Discrimination in the Bell System."

The impetus behind chapter five is the realization that we have backed ourselves into a corner. That is, if the theory is true, that scientists and lawyers speak distinct and dissimilar languages, neither of which (like any other language) can be understood apart from the culture of the group that it comes from, then, by pure deduction, we may conclude or predict that, in our case study, all communication between the scientists and the lawyers (including the judge) should have been precluded. Again, by pure deduction, we may also conclude or predict that the dissimilarity should have become readily apparent to the two groups upon their interaction, in the same way that a speaker of Chinese and a speaker of English discover immediately and effortlessly the linguistic gap separating them. Unsurprisingly, observations from our case study summarily refute both predictions.

While the scientists and the lawyers did not enjoy crystal-clear communication, they nonetheless exchanged words and, presumably, meanings of some sort. There were questions, followed by answers. Somehow, scientist and lawyer did make some sense of each other. Surely, the 8000 pages of courtroom transcripts are an artifact of something. What is it?

This question motivates the theory's re-formulation or refinement, which the refutation of its hypotheses necessitates. In moving the theory beyond a literal assertion that science and law are languages, we will explain scientist-lawyer communication as proceeding in the manner, first put forward by Cervantes and later re-formulated by Schutz, in which Don Quixote communicated with Sancho Panza. The improved theory will emphasize one of

our themes, which is that the same words, for different individuals, may refer not only immediately to (1) different meanings and logics, but also ultimately to (2) the respective cultures that the different individuals come from.

Chapter five provides examples of how the scientists (Dr. Ashenfelter and Dr. Laws) and the lawyers (Mr. Levy and Mr. Powers of AT&T, Mr. Copus of the EEOC, and the Presiding Examiner [the judge]) engaged in the phenomenon of "talking past", and then posits two conditions that make the sustained interaction--the Quixotic communication--possible. The re-formulated theory is then summarized diagrammatically in the form of maps (like those we have already seen in this introduction). Chapter five closes with a discussion of how the two conditions are but a special case of Popper's "metaphysics" or self-sealing realities, first mentioned in chapter one.

In chapter six, we observe that the scientific witness occupies a can't-win/can-lose position, where "winning" simply means communicating his viewpoint for what he intends it to mean. At the same time, the cross-examining lawyer enjoys a can-win/can't-lose position, where "winning" for him means stonewalling the scientist's testimony. If the cross-examining lawyer so chooses, he may throw open to question not only the scientific testimony's premises, but also the premises to these premises and then the premises to these. At the other end of the scientific testimony, the cross-examining lawyer may rattle off ad infinitum counter-example after counter-example to the scientist's prediction or null hypothesis. The explanation that (1) a lawyer may do this to a scientist, but (2) two lawyers may not do this to each other, or (3) two scientists may not do this to each other, has

to do with the cultural and institutional aspects of science and law, thereby underscoring this study's theme that science and law are not systems of "objective", detached logic, but man-made artifacts whose meanings are bound to the culture--the scientific culture or the legal culture--that they come from.

The scientist occupies a can't-win/can-lose position because, in the courtroom, he is an alien, cut off from the collegial support and the institutional forces of his home turf that normally sanction his expertise and give it meaning. The cross-examining lawyer enjoys a can-win/can't-lose position because, being in the courtroom, he is operating on his home turf, where his own legal perception of the scientific testimony is reinforced by the similar, parallel perceptions of his fellow natives of the same legal culture. Moreover, they are all bound or required to reason in the same way by the same legacy--the body of precedents and statutes--of which they are the inheritors and in which they secure their identities. The cross-examining lawyer cannot "lose" in that, without lifting a finger, the cross cultural differences between science and law have already obfuscated the scientific meaning to the scientific testimony; the cross-examining lawyer can "win" in that, if he so chooses, he may actively proceed to obfuscate further the scientific testimony, especially where the initial obfuscation, due to the science-law cross-cultural differences, does not sufficiently serve his purpose.

In chapter seven, we provide a recapitulation of the study's method and findings, and then a set of recommendations about how to improve scientist-lawyer communication, both in the short-run (band-aids for remedying

the ill effects) and in the long-run (interventions in the underlying structure that is setting up the problem).

Chapter eight consists largely of a literature review, which we purposely delay to the end-part of this study. The sociological-anthropological detachment with which this study views "scientific natives" and "legal natives" is, I believe, unique to the research on the relationship between science and law, particularly the role of scientific expert testimony. The singularity of this study and its contribution to the law-review literature and the social-science literature can be more easily explained in retrospect, that is, with familiarity of the study itself as the reference point from which other studies may be viewed. From this reference point, the law-review literature will appear like a collection of stories in which legal natives write ethnocentrically about the scientific visitors they receive, and the social-science literature will appear like stories in which scientific natives write ethnocentrically about a foreign land (the courtroom) and their adventures there. While some of these other studies do make mention of differing logics in science and law, they do not acknowledge the existence of the cultural and institutional forces existing in science and law, or the role that these forces play in setting up the clash we observe in the courtroom. For example, the literature often views science as being "objective" in the sense of possessing an existence independent of its human creators; it places science on a pedestal, far removed from the status of an artifact manufactured by scientific natives, without whose presence the "objectivity" of science actually disappears.

Chapter nine places the entire study in the context of my own motiva-

ting research interests. As a case study, the role of scientific expertise in the courtroom is but an instance of the general relationship between knowledge and policy-making. The purpose in chapter 9 is not to generalize to the larger case, but to suggest additional applications of our theory, in which the further testing of hypotheses may lead to further refutations, and then further improvements in the theory.

Chapter 1 Science and Law

1.1 Science and Law as Languages

I present here an explanation of scientific reasoning (what the lawyers were looking at), legal reasoning (how the lawyers, who were the policy-makers, did their looking), and the differences between the two (which will help us diagnose what happened to the input of scientific knowledge to the process of judicial policy-making).

I take the stance that science and law are languages that scientists and lawyers "speak". While the literatures of the philosophy of science and the philosophy of law contain references to science and law as languages in a metaphoric way, I mean it in a literal way (at least in the opening part of this study).

No language, such as English or Chinese, exists apart from the culture of which it is an artifact. It has no meanings other than the ones that its speakers attach to it. Language is a human creation. Furthermore, the meanings that a language conveys are understandable, whether to its own speakers or to an observing linguist, only in the context of the culture that it comes from. One may not learn to speak Chinese without also learning Chinese culture.

The same ideas apply to science and law. Neither is a mere collection of logical rules and findings with an objective existence that is independent of the meanings that people--scientists or lawyers--attach to it. Otherwise, the pages of scientific journals and law reviews would be filled with

nonsensical ink marks. The meanings that render the ink marks into a language in turn exist in cultural contexts--the contexts of the community of scientists and the community of lawyers. Each community plays host to internal cultural and institutional forces that shape the attitudes and actions of its members. Logical and social standards for acceptable scientific reasoning, as well as the enforcement of these standards, are rooted in the respective cultures. One may not practice law or science correctly without also being sensitive to, and bound by, these standards. In this sense, science and law are more than mere systems of logic. The assertion that science and law are languages is a summary way of embracing this recognition.

It is essential to recognize the cultural contexts of science and law. They dispel the myths that science is the objective, straightforward application of scientific method and that law is mere syllogistic reasoning. Philosophers have long recognized that if science were left solely to the logic of scientific method, and law solely to the logic of the syllogism, science and law as we know them today would not exist.

1.2 The Fundamental Difference between Science and Law

The literatures of the philosophy of science and the philosophy of law provide endless accounts of each language, alone, as well as comparisons of the two. Fortunately, there is just one aspect out of the many in the literature that is fundamentally relevant to the lawyer-scientist interactions in our case study. And it is one that the literature has overlooked.

The lawyer-scientist interaction during the cross-examination is a social transaction over knowledge. The scientist offers his knowledge as an expert witness. His immediate purpose is to clarify, defend, and convey his knowledge. Because knowledge is a key concept, we need to define it before identifying the fundamental difference between science and law.

Knowledge is made up of the mental constructs that a knower creates and maintains in the world's image. "Constructs" is an apt term because it captures the characteristic of knowledge as a replica or scale model, residing in the knower's mind, of the external reality that the knowledge is a construct of. It is a re-creation and depiction of reality, but distinct from it, sharing the same relationship as a map to a territory, a photograph to a scene, a representation to a presentation. In the situation where the knower is a scientist engaged in scientific research, the mental constructs are called a "scientific theory". The theory is the scientist's map of the territory he is exploring. Scientists improve their maps with further exploration, which is the purpose to scientific research.¹

The question that frames our interest, as observers, in the cross examinations is: how may the constructs that reside in the scientific mind

come to reside in the legal mind, if at all?

Aside from being important to a theory of how scientists and lawyers communicate, this question is also important for a more general reason. Scientific knowledge is always being touted as the best, if not the only, form of knowledge for use in policy-making. In this case study, we will observe what actually happens to it. Also, policy-makers in our society are, more often than not, and for better or for worse, legally trained. The findings we come up with may throw light on other situations in which legally trained policy-makers attempt to apply scientific findings, such as environmental policy or policy for medical malpractice.

Now, when we actually attempt to answer the question, we come across a fundamental difference in the maps that guide the inquiry of scientists and lawyers. The difference prevents lawyers from seeing territories in the way that scientists do.

As we will argue, the difference lies in the divergent ways that they justify their mental constructs. The difference makes them unable to justify their viewpoints to each other. When they speak to each other with this purpose, they wind up talking past one another.

In law, justification is premise-oriented. In science, justification is conclusion-oriented. Consider:

Men are mortal.

Socrates is a man.

Socrates is mortal.

To a lawyer, this is an instance of applying the law ("Men are mortal")

to the facts ("Socrates is a man") and then reaching a decision on the law as applied to the facts ("Socrates is mortal").

To a scientist, this is an instance of a theory ("Men are mortal"), an initial condition that an actual situation specifies ("Socrates is a man"), and a hypothesis or prediction, derived from the theory, about the given situation ("Socrates is mortal"). The hypothesis or prediction is testable (by, for example, observing whether Socrates dies after ingesting poison).

At first this may appear to be a case of different people looking at the same thing in different ways. I agree that the lawyers and the scientists are doing their looking in different ways, but I believe they are looking at different things, not the same thing.

The words about mortal men and Socrates may be the same in pronunciation and spelling to lawyers and to scientists, but the respective meanings and logics (the "grammars") that tie the words together are different.

In law, what is of interest is the justification of the decision ("Socrates is mortal"). The decision is justifiable only if its premises--the law ("Men are mortal") and the facts ("Socrates is a man")--are themselves sound. Is the precedent a controlling one? Is the statute unambiguous? Are there any competing precedents or statutes that would yield a different decision on the same facts? Are the facts correct? Justification in law is premise-oriented.

In science, it is not the hypothesis ("Socrates is mortal") but the general theory ("Men are mortal") whose justification is of motivating interest. The hypothesis, called a prediction if it involves a future time element, is derived logically from the theory, with the help of the

initial conditions. As such, the hypothesis is a sort of indicator. If what the hypothesis imagines "in theory" is borne out in reality, then the theory from which the hypothesis originates is taken to be justified. Because, as Popper states², the hypothesis is a conclusion drawn from the theory, we say that justification in science is conclusion-oriented.

There is a concrete consequence to the divergent ways that scientists and lawyers justify their knowledge.

When I was reviewing the courtroom transcripts of the AT&T case, the back of my mind played host to a recurring hypothetical image in which the lawyer would see the footnoted studies or articles in a scientist's written testimony as if they were the cited precedents and statutes, similarly footnoted, in a legal brief. The lawyer would then proceed to look up the footnoted articles in the same way he would look up the cited precedents and statutes because, in his eyes, the footnoted studies and articles would be the premises upon which the justification of the scientist's testimony rested.

In fact, I observed, in cross examination after cross examination, that the lawyers had attacked footnoted articles, among other types of premises, and proceeded to dispose of them in an apparent effort to dispose of the scientific testimonies citing them, sometimes to the consternation of the scientists being cross examined. Some of the scientists realized the nature of the game and played along. Other scientists did not catch on, and were manipulated.

What took place in these lawyer-scientist interactions was a clash between the legal culture and the scientific culture. Lawyers and scien-

tists are trained in, and socialized into, different professional communities. In the administrative court hearings in the AT&T case, each group spoke in its own language to the other group, and listened in its own language to what the other had to say. The lawyers saw the scientific testimonies as premise-oriented. The scientists defended them as conclusion-oriented. There was a confusion of tongues. The shared language of English masked the confusion, giving to both groups the impression that they were dealing with the same thing. Yes, the words were the same to the two groups, but the substantive meanings underlying them were different.

A diagnosis of actual lawyer-scientist interactions will require a deeper understanding of the two languages than I have provided so far. Further discussion of each language follows.

1.3 The Logic of Science

Induction and deduction are two modes of reasoning which everyone confuses. The distinction between the two is crucial to justification in science.

Induction

Simply stated, induction is generalization. It is the name for the natural human tendency to make up a general rule after seeing something repeated "n" times. I come to know that "men are mortal" after observing "n" actual examples of men dying. Generalizing across the "n" examples, I am practicing inductive inference.

While induction is the process by which I formulate the mental construct "men are mortal", it is quite another matter to establish that the construct is an accurate one, that is, whether or not the formulation is correct. The question is, can induction be used to justify the accuracy of mental constructs? Is induction a useful mode of justification in science?

A critic of induction could challenge my construct that "men are mortal" by pointing out that there are centenarians, among whom one or two might turn out to be immortal. In this face of this possibility, how would I justify my mental construct?

I could point back to the "n" examples that provided the basis for my inductive inference. Surely, those "n" deaths are examples that support and illustrate the accuracy of the mental construct that "men are mortal".

I could take the position that the theory is accurate because it rests on sound examples. That is, I induced it correctly.

But there is a problem here, as my critic would be quick to point out. And the problem would have nothing to do with whether or not men are truly mortal. It would be that I am assuming that inductive inference is a valid way to derive accurate constructs. Yet how would I know this? That is, how would I know that my assumed construct, "inductive inference leads to accurate constructs", was itself in conformance with reality?

To establish this, I could point to a number of examples in which inductive inference had led to accurate constructs. But there would be another problem. It would be that I am applying the principle of induction to justify itself. The application would be based on the same construct whose validity I was just seeking to establish. To answer the question as to whether the application were justifiable, I could point to a number of such applications ...

The result of the exchange with my imagined critic is that there is no empirical basis for establishing that inductive inference is a sound way of justifying mental constructs. Karl Popper stated the problem of induction tersely:

To justify it, we should have to employ inductive inferences; and to justify these we should have to assume an inductive principle of a higher order; and so on. Thus the attempt to base the principle of induction on experience breaks down, since it would lead to an infinite regress.³

While inductive inference is useful in the formulation of theories,

it provides the scientist no leg on which to stand when justifying his theories. A paramount purpose that constructs in science must serve is justifiability. A scientist must be able to justify his theory to other scientists, whether in journals, books, lectures, or conferences. It is crucial that his mode of justification be defensible. If deployed as a defense, inductive inference will only lead to an infinite regress. If the scientist may not employ inductive justification, what alternative is open to him?

Before turning to a discussion of the alternative of deductive justification, I think it would be instructive to describe the concrete consequences that induction would have for the everyday work of scientists. For scientists, it is also these consequences, and not just the abstruse logical paradox leading to infinite regress, that rule out induction as a constructive mode of justification.

Suppose that there indeed exist some immortal men. They survive despite disease, starvation, wounds, poisons, and old age. My mental construct, "men are mortal", would therefore be a deficient map of reality. However, if I, as a scientist, were to proceed to justify my map inductively, I would never have to admit any inaccuracy in it. Inductive justification would shield the inaccuracy from detection. I could hang on to my theory forever.

The reason for this is that, inductively speaking, I may flatly ignore any counterexample. When presented with some men who do not die of gunshot wounds and poisons that other men would find fatal, I could simply respond that the original "n" examples are still evidence of something and surely,

they illustrate that there is some truth to my theory. Besides, for every counter-example, I could marshal an equal or greater number of new examples to support my theory. I could argue that the "weight" of the evidence is against the counter-examples. And I may legitimately do all of this, as long as I believe that induction is a permissible way of justifying the accuracy of mental constructs.

The concrete problem that becomes evident here is that inductive justification serves to seal off the mental constructs from the reality they are supposed to depict. Even where in error, a mental construct will always be "confirmed" by the original examples from which it was formulated, even if these examples only make up an incomplete or unrepresentative case of the general situation. Additional "confirming evidence" would serve to mask the error from detection even further, offering instead the illusion of "proof" that the theory "applies" to reality. If the scientist were to continue to develop and articulate the theory, the inductive mode of justification would only seal off the theory from reality more tightly. The theory would become a closed system, existing only in the scientist's mind and not being accountable to the examples of the real world. The theory would be metaphysical--a far cry from the "objectivity" that is the hallmark of science.

Another consequence of inductive justification would be the breakdown of communication within the scientific community. Rival scientists with rival viewpoints could all marshal "confirming evidence" for their rival theories. In this manner, all theories, even if mutually exclusive, would appear plausible and correct, according to the criterion of inductive justi-

fication. As a result of this, constructive discussions and criticisms among scientists would not be able to take place. Scientists would be talking past one another in the process of busily marshalling evidence for their respective theories.

Falsifiability

The alternative to inductive justification is deductive justification, also referred to as falsifiability. According to Karl Popper:

From a new idea, put up tentatively, and not yet justified in any way--an anticipation, a hypothesis, a theoretical system, or [a mental construct]--conclusions are drawn by means of logical deduction ...

We may if we like distinguish four different lines along which the testing of a theory could be carried out ... [The last of the four] is the testing of the theory by way of empirical applications of the conclusions which can be derived from it.

The purpose of this last kind of test is to find out how far the new consequences of the theory--whatever may be new in what it asserts--stand up to the demands of practice, whether raised by purely scientific experiments, or by practical technological applications. Here too the procedure of testing turns out to be deductive. ... certain singular statements--which we may call 'predictions'--are deduced from the theory; especially predictions that are easily testable or applicable ... Next we seek a decision as regards these ... derived statements by comparing them with the results of practical applications and experiments [that is, reality]. If this decision is positive, that is, if the singular conclusions [the derived statements, the 'predictions', which are themselves mental constructs] turn out to be acceptable, or verified, then the theory has, for the time being, passed its test: we have found no reason to discard it. But if the decision is negative, or in other words, if the conclusions have been falsified, then their falsification also falsifies the theory

from which they were logically deduced.

It should be noticed that a positive decision can only temporarily support the theory, for subsequent negative decisions may always overthrow it. So long as a theory withstands detailed and severe tests and is not superseded by another theory in the course of scientific progress, we may say that it has 'proved its mettle' or that it is 'corroborated' by past experience.⁴

At first, it may appear that deductive justification is a weak and tenuous way of establishing the accuracy of a scientist's mental constructs. He may not assert that his theory is "correct" because the logic of deductive justification limits him to knowing that his theory is either falsified or not yet falsified. At best, then, his theory is "not incorrect". His theory is open to being overthrown by contradictory evidence, which may appear at any moment. Always open to the possibility of being proven wrong, such a theory would seem to be a weakly constructed one. Certainly, from the point of view of a scientist who wishes to enhance his professional reputation, it would be preferable to have a theory strong enough to withstand all critical efforts aimed at disproving it.

Actually, there is nothing weak about deductive justification. A theory's being falsified is different from its being falsifiable. A falsifiable but unfalsified theory is as strong as any.

Furthermore, being open to falsification is hardly a liability when we consider the alternative. Unfalsifiable theories, which result from the application of the inductive mode of justification, are closed systems of mental constructs. Existing only in the knower's mind, they are not required to attach to reality, except perhaps in their formulation. Falsifi-

able theories, on the other hand, enjoy the benefit of having errors that are detectable and hence correctable; their portrayal of reality is therefore accountable to it, which allows the portrayal to improve with continued research.

Falsifiability is not an abstruse notion in science. It is widely accepted throughout the scientific community. Thomas Kuhn, whose school of thought rivals Popper's, states that "no field is potentially a science" unless its theories are cast according to "Sir Karl's demarcation criterion."⁵ Also, the null hypothesis in inferential statistics is an artifact whose usage, or at least recognition, in all scientific fields attests to the widespread integration of falsifiability as a logical norm in scientific thinking.

Recapping My Position

I take the position that falsifiability is a logical characteristic, or logical form, that mental constructs must possess in order for scientists to both recognize them and work with them as scientific theories. For the purposes of diagnosing the scientist-lawyer communication in my case study, I will be turning to falsifiability as the characteristic that distinguishes the scientific language.

Having established my position, I now quickly move to narrow its scope by pointing out explicitly what my position is not. My position is a humble one (and humble positions are easier to defend than bold ones).

First, my position is not that a major agenda item in scientific research is to turn up counter-examples to one's own or another's theory.

I believe there is not a consensus among philosophers of science as to whether scientists regularly seek out counterexamples, or whether they engage usually in something else, as part of their "normal" scientific activity. However, there is more of a consensus on the issue that the logical form of scientific theories is falsifiability (which is sometimes called testability--after all, inductively justified viewpoints are self-sealing and hence not testable against experience). My position is about the logical form, perhaps the grammar, of the scientific language; it is not a diagnosis of or a prescription for the everyday behaviors of people who are scientists, although it is of course related to these things.

Second, my position is not that all knowledge need be cast in falsifiable form. I have only been describing the knowledge that scientists possess. Scientists pursue the peculiar purpose of developing constructs for the sake of developing constructs; in plainer language, they seek the truth for its own sake. Thus, a truthful depiction is the ruling criterion in the situation where one scientist attempts to persuade others to accept his theory. This is definitely unlike the case for the engineer, whose mental constructs must not only depict things the way they are, but also envision them for what they should be. When one engineer attempts to justify his knowledge to other engineers, the ruling criterion is not so much a truthful and accurate depiction of reality as it is the capacity to manipulate and mold reality to achieve humanly defined ends.

An interesting case in which knowledge is not, and need not, be cast in falsifiable form is the knowledge of lawyers. The reason here, as in the case of the engineer, is that the lawyer has a different purpose from

the scientist's. The lawyer even lives in a different world--the community of lawyers. (I will give the knowledge of lawyers a thorough discussion in the following section.)

Finally, my position is not that falsifiability constitutes the philosophy of science. (I have singled it out because it happens to be fundamental to the diagnosis of scientist-lawyer communication.) With regard to the justification of a scientist's constructs, there are more aspects than falsifiability. And with regard to the general issue of the development of scientific knowledge, there are more aspects than justification.

1.4 The Language of Science

Science is not the strictly objective, straightforward application of scientific method. Scientific findings have meanings that are inseparable from the procedures that produce them; these are procedures not only of logic, but also of cultural and institutional forces that shape the attitudes and actions of members of the scientific community. The logic of science alone is insufficient to run the engine of the scientific enterprise. There is one major point where quintessentially human factors enter and play essential roles in scientific inquiry, not in a way that is (in the derogatory sense of this word) "subjective", but in a way that is reasoned, controlled, and in keeping with the spirit of science.

This point exists in the context of justification, dealing with "the problem of theory-choice"⁶ or the question of "How and why do we accept one theory in preference to others?"⁷ Popper and Kuhn have different perspectives on this issue.

Popper states:

Every test of a theory, whether resulting in its corroboration or falsification, must stop at some basic statement ["a statement about a factual occurrence"⁸, either a hypothesized/predicted occurrence or, here, an observed occurrence] or other which we decide to accept. If we do not come to any decision, and do not accept some [observation] or other, then the test will have led nowhere. But considered from a logical point of view, the situation is never such that it compels us to stop at this particular [observation] than at that, or else give up the test altogether. For any [statement about an observation, which is itself a set of mental constructs] can again in its turn be subjected to tests, using as a touchstone any of the [hypotheses or pre-

dictions] which can be deduced from it with the help of some theory, either the one under test, or another. This procedure has no natural end. Thus if the test is to lead us anywhere, nothing remains but to stop at some point or other and say that we are satisfied, for the time being. [Emphasis added.]⁹

...

The [observations] at which we stop, which we decide to accept as satisfactory, and as sufficiently tested, have admittedly the character of dogmas, but only in so far as we may desist from justifying them by further arguments (or by further tests). But this kind of dogmatism is innocuous since, should the need arise, these statements can easily be tested further.¹⁰

What finally prevents or obviates the need for further testing is that, eventually, scientists reach observations that "are accepted as the result of a decision or agreement; and to that extent they are conventions."¹¹

Popper concludes:

The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon rock-bottom. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and when we cease our attempts to drive our piles into a deeper layer [that is, when scientists decide or agree to accept an observation as definitely corroborating or falsifying a theory], it is not because we have reached firm ground. We simply stop when we are satisfied that they are firm enough to carry the structure, at least for the time being.¹²

This also fits in with Popper's definition of objectivity.

... the objectivity of scientific statements lies in the fact that they can be intersubjectively tested.¹³

...

... inter-subjective testing is merely a very important aspect of the more general idea of intersubjective criticism, or in other words, of the idea of mutual rational control by critical discussion.¹⁴

Where the discussants are (1) members of the same, scientific community, (2) practitioners of the same set of methods and standards, and (3) subject to the same cultural and institutional forces, it is easy for us to see that a "decision or agreement" is reachable.

Therefore, although human factors do enter scientific inquiry in the form of intersubjective discussions, the process is nonetheless reasoned, subject to rational control, and in keeping with the critical spirit of science.

(What happens when an outsider to the scientific community is in a position to rule on whether or not to accept a scientist's observation? [This happened in the EEOC-AT&T litigation, where members of the legal community were in the position to pass judgment on a scientist's observations. We will diagnose the situation in later chapters.] One possible consequence is that a Pandora's box of observations may be opened up; that is, every observation offered is in turn questioned and scrutinized, as if it were a theory itself. "This procedure has no natural end." Another possible consequence is that the bold structure of science, resting on piles driven into a swamp, collapses. In either case, divorced from the cultural and institutional forces of its own community that serve to shore it up, the bold structure of science falls short of its intended meaning, as in the eyes of the lawyer who cross examines the scientific expert witness.)

Kuhn is more explicit in his account of human factors in scientific

inquiry.

Some of the principles deployed in my explanation of science are irreducibly sociological, at least at this time. In particular, confronted with the problem of theory-choice, the structure of my response runs roughly as follows: take a group of the ablest available people with the most appropriate motivation; train them in some science and in the specialties relevant to the choice at hand; imbue them with the value system, the ideology, current in their discipline (and to a great extent in other scientific fields as well); and, finally, let them make the choice. If that technique does not account for scientific development as we know it, then no other will. There can be no set of rules of choice [that is, there can be no "logical criteria" or "methodological rules or conventions" ¹⁵] adequate to dictate desired individual behavior in the concrete cases that scientists will meet in the course of their careers. Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains.

That position is intrinsically sociological ...¹⁶

My purpose here is not to compare or contrast Popper and Kuhn. I merely wish to introduce the importance of the cultural and institutional forces that pervade the scientific community, that imbue scientific knowledge with meanings special to scientists, and that (in my depiction) render science into a language. (I give a precise definition to "cultural and institutional forces" in the following chapter.)

1.5 The Logic of Law

Deductive inference or falsification is the mode of reasoning that scientists use to justify scientific constructs. What is the mode of reasoning that lawyers use to justify legal constructs?

According to John Dewey, the logic "which has had the greatest historic currency and exercised greatest influence on legal decisions, is that of the syllogism."¹⁷ This is a position with which classic legal thinkers, like Roscoe Pound and Justice Oliver Wendell Holmes, appear to be in agreement. Indeed, the overbearing influence and consequent problems of syllogistic reasoning, as actually manifested in our nation's judicial decision-making, constitute a danger which at times seems to unite the writings of the three men.¹⁸

What is the logic of the syllogism? Dewey explains:

Those ignorant of formal logic, the logic of the abstract relations of ready-made conceptions to one another, have at least heard of the standard syllogism: All men are mortal; Socrates is a man; therefore, he is mortal. This is offered as the model of all proof or demonstration. It implies that what we need and must procure is first a fixed general principle, the so-called major premise, such as 'all men are mortal'; then in the second place, a fact which belongs intrinsically and obviously to a class of things to which the general principle applies: Socrates is a man. Then the conclusion automatically follows: Socrates is a mortal. According to this model every demonstrative or strictly logical conclusion 'subsumes' a particular under an appropriate universal. It implies the prior and given existence of particulars and universals.¹⁹

The major premise is a legal precept, which is pre-existing in the form of legislative law (a code or statute) or case law (judge-made law, consist-

ing of precedents that past cases have established). As such, "the law" is an abstraction, a man-made artifact, a set of constructs residing inside the minds of lawyers.

Of course, "the law" does not include just any mental constructs, but only those of a certain form and specification. In science, we saw that their form is to replicate and portray reality; their specification, to do so, accurately. In law, their form is twofold. First, as in science, they must include a depiction of reality, but more specifically, their depiction is of the fact situation to which the law is meant to be applied. That is, they provide a general map of the territories that "the law" may encounter. Thus it can be argued that a certain precedent is inapplicable because the facts in the case at hand are different from the facts in the given precedent. The territory must be the one portrayed on the map in order for "the law" to be applied.

Second, unlike in science, the mental constructs in law must also provide not only a map but a blueprint, or legal remedy, as to how to order the reality into what it should be. The blueprint indicates how the law governs social behavior. At least with regard to blueprints, we can therefore say that constructs and reality bear different relationships in science and law: In science, the reality governs the constructs; in law, the constructs govern the reality.

The specification required of legal maps is that they delineate clearly the territories, the actual fact situations, which the law is meant to apply to and to govern. The specification required of blueprints is that the design--the re-ordering--be a just solution to the controversy at hand.

A concrete example of a legal precept of "the law" is what lawyers call a "rule of law". In it, according to Pound, a "definite detailed legal result [the blueprint] is attached to a definite detailed state of facts [the map]."20 In other words, Pound is saying that, in a rule of law, both map and blueprint are articulated unambiguously ("definite").

For example, it would be a rule of law that asserted, where a horse is stolen and then sold to an unwitting buyer (the map), the horse's original owner may sue the unwitting buyer to recover the horse (the blueprint).21 In another example, it would be a rule of law that federal contractors who discriminate in employment on the basis of race or sex (the detailed state of facts) may have their federal contracts revoked (the detailed legal result).

In these examples, "the facts" to which the law is applied consists of the territory, as actually given. It is the situation in which Joe steals a horse from John and then sells it to unwitting Fred. It is the less-than-parity representation of women and minorities in middle management at AT&T.

In my own thinking, I liken the map to a pattern. The map's pattern is the general, abstract description of those specific concrete situations whose behaviors "the law" is meant to govern. If the pattern fits--if the territory appears to be the one on the map--then "the law" is applied to "the facts", and the legal remedy (the blueprint) that "the law" contains is implemented. Fred returns the horse to Joe. The federal government revokes its contracts (or, better still, is entitled to do so in the process of negotiating a settlement).

To summarize, as the logic of the syllogism would have it, "the law" is applied to "the facts", from which "the decision" automatically and mechanically follows.

The logic of the syllogism is more than a plausible reconstruction that we, as observers, offer to explain how lawyers develop and justify their constructs. Lawyers themselves have explicitly institutionalized the logic in the maxim, stare decisis et non quieta movere ("to abide by the precedents and not to disturb settled points"²²), also referred to more simply as the "doctrine of precedents":

A solemn decision upon a point of law, arising in any given case, becomes an authority in a like case, because it is the highest evidence which we can have of the law applicable to the subject, and the judges are bound to follow that decision so long as it stands unreversed, unless it can be shown that the law was misunderstood or misapplied in that particular case.²³

A judicial precedent is an adjudged case or decision of a court of justice, considered as furnishing an example or rule for the determination of an identical or similar case afterwards arising, between the same or other parties, in the same or another court, or a similar question of law.

... the primary idea of a precedent is that of a rule judicially established and presumptively binding ... It declares or enunciates the rule or principle of law which must (not may) be followed in the decision of similar cases in the future ...²⁴

Precedents, as do codes and statutes, play the role of the major premise, "the law", in syllogistic reasoning. The doctrine of precedents underscores the fact that justification in law is premise-oriented.

1.6 The Language of Law

The logic of the syllogism, operating in great measure through its institutionalized form as the doctrine of precedents, is esthetically pleasing, but hardly universally practicable.

Dewey explains:

[The logic of the syllogism] implies that for every possible case [territory] that may arise, there is a fixed antecedent rule [map and blueprint] already at hand; that the case in question is either simple and unambiguous, or is resolvable by direct inspection into a collection of simple and indubitable facts, such as 'Socrates is a man' [that is, the territory is unobscured and may be seen and surveyed without problem].²⁵

In other words, neither the major premise nor the minor premise may always be readily available. What then?

In actual day-to-day applications, "the law" does not always contain clearly articulated maps and blueprints. A rule of law, which is the happy situation in which map and blueprint have already been clearly drawn up, may not exist for the case at hand. Instead, there may be a legislated statute that the courts have yet to interpret or test; a precedent whose decision, while adequate to the controversy it had decided, is ambiguous with regard to the controversy at hand; or a number of equally plausible, competing precepts that offer contradictory legal remedies. Pound frames these practical problems in the following way:

... decision of a controversy according to law involves
(1) selection of the legal material on which to ground the decision, or as we commonly say, finding the law;

(2) development of the grounds of decision from the material selected, or interpretation in the stricter sense of that term; (3) application of the abstract grounds of decision to the facts of the case.²⁶

As for finding the law:

It may involve nothing more than a selection from among fixed precepts of determined content calling only for a mechanical ascertainment of whether the facts fit the rule ... Or it may involve selection from competing analogies, urged by the respective parties as the ground of decision. Here, as it were, there is to be an inductive selection. Or it may involve selection by logical development of conceptions or principles. Here, as it were, there is a deductive selection. If these fail, it calls for selection from outside of the legal system in whole or in part--from custom, from comparative law, or from economics.

How, in practice, do courts determine when to resort to one of these and when to another and in what order? ... As things go it is apparent that courts proceed in the order of (1) selection with reference to fixed precepts, (2) inductive or deductive selection, and (3) selection from outside the legal system. As between inductive selection and deductive selection the practice of the courts and even of individual judges varies. There is no standard method of determining between them ... Likewise there is no standard practice [for] determining when to invoke custom, when comparative law, when morals and when economics, in case selection must be made outside the legal system ... ²⁷

If the map and blueprint have been drawn up clearly and thereby amount to a rule of law, then "selection with reference to fixed precepts" proceeds.

If the law has been drawn up, but is substantially at variance with other laws, then "inductive or deductive selection" takes place. If no precept yet exists, then "selection from outside of the legal system" provides the guide to reasoning in lieu of a pre-made map-blueprint.

As for interpreting the law, or developing the grounds of decision from the law that is found in the foregoing manner:

... the usual process is one of traditional legal reasoning ... But in new and difficult cases this merges in, and in all cases is influenced by, current moral, political and social ideas, especially fixed pictures of the end of law and of an ideal legal and social order, by reference to which, consciously or subconsciously, the tribunal determines how far possible interpretations will yield a just result in the individual case and judges of the intrinsic merit of the different developments of the legal materials potentially applicable which are urged by the contending parties. Along with these we must put an intuition of what will achieve justice in action and what will not, expressing the experience of the magistrate both as lawyer and as judge. The traditional legal reasoning represents the experience of generations of judges in the past. It is rooted in some sort [of] traditionally transmitted judicial intuition founded in experience.²⁸

(We might observe, incidentally, that in the EEOC-AT&T litigation, the EEOC lawyers had interpreted Title VII of the Civil Rights Act of 1964 with an unrelenting eye to the "current moral, political and social ideas, especially fixed pictures of the end of law and of an ideal legal and social order." In the late 1960's and early 1970's, the picture was framed by the ideal of equality. At the time of the AT&T case, the picture had evolved to include sex in addition to race as a dimension by which to enforce equality. Though reluctant to do so, the AT&T lawyers also acknowledged sex as a dimension to equal employment opportunity, although they did not apply this notion retroactively, to 1964, in assessing AT&T's employment practices, as the EEOC lawyers had done.)

Related to the interpretation of the law is determining the ratio

decidendi of a case ²⁹, or distinguishing the holding from the dicta.

As for application of the abstract grounds of decision to the facts of the case, Pound explains that the application "may be purely mechanical". Yet he is quick to add: "Frequently application of the legal precept, as found and interpreted, is intuitive." He sees intuition as essential to judicial decision-making:

Nor need we be ashamed to confess that much that goes on in the administration of justice is intuitive ... Standards, applied intuitively by court or jury or administrative officer, are devised for situations in which we are compelled to take circumstances into account; for classes of cases in which each case [each actual territory] is to a large degree unique. For such cases we must rely on the common sense of the common man as to common things and the trained common sense of the expert as to uncommon things. Nor may this common sense be put in the form of a syllogism. To make use once more of Bergson's discussion of intelligence [in contrast to] instinct, the machine [whose precision in functioning is like that of intelligence] works by repetition; "its use is mechanical and because it works by repetition there is no individuality in its products" ... On the other hand, in the hand-wrought product the specialized skill of the workman, depending upon familiar acquaintance with particular objects, gives us something infinitely more subtle than can be expressed in rules. In the administration of justice some situations call for the product of hands, not of machines. Where the call is for individuality in the product of the legal mill--i.e., where we are applying law to human conduct and to the conduct of enterprises --we resort to standards and to intuitive application. [Emphasis added.]³⁰

In the last remark, Pound is not just explaining how to derive a decision logically when applying the law to the facts, but also arguing for a second criterion that the decision must fulfill in addition to that of logical consistency: it is the criterion of justice.

In the end, even if all the problems regarding logical consistency that Dewey and Pound allude to can be resolved, there may still arise the most vexing problem of all, which is that the decision ultimately rendered can be unjust. Consider hypothetically a law that states that all people who possess unregistered guns (the detailed state of facts) must be sentenced to a year in jail, where the length of the sentence is not subject to plea-bargaining and may not be shortened by parole (the detailed legal result). Suppose then, a little old lady, sweet and grandmotherly in every respect, is found to possess in her house a gun that her husband, now deceased, had purchased twenty years ago without her knowledge. The logic of the syllogism would sentence her to a year in jail. Although logically justifiable, such a decision would be patently unjust.

The doctrine of precedents poses the problem of sanctifying rules of law in the face of changing times, evolving notions of justice, and unforeseeable circumstances. The logic of syllogistic reasoning and the accompanying doctrine of precedents may be functional in theory, but in practice there is no guarantee that they will function unproblematically in producing justifiable constructs.

In summary, it is at the points of (1) selecting the precept from competing analogies; (2) selecting the precept from outside the legal system; (3) interpreting or developing the grounds of decision from the selected precept; and (4) intuitively applying the grounds of decision to the facts, especially when justice is a criterion equal to that of logical consistency; that we observe the introduction of subjectivity, common sense, custom, pictures of the ideal legal and social order, and the experience of the de-

cision-makers, to the logic of reasoning in law.

These factors illustrate the cultural and institutional forces that operate in the community of lawyers, imbuing their logic with values and meanings that (in my depiction) render law into a language.³¹

To recap:

The life of the law has not been logic: it has been experience. The felt necessities of the time, the prevalent moral and political theories, intuitions of public policy, avowed or unconscious, even the prejudices which judges share with their fellow-men, have had a good deal more to do than the syllogism in determining rules by which men should be governed. [Holmes (32)]

Like other tools [legal precepts] must be modified when they are applied to new conditions and new results have to be achieved. Here is where the great practical evil of the doctrine of immutable and unnecessary antecedent rules [the doctrine of precedents] comes in. It sanctifies the old; adherence to it in practise constantly widens the gap between current social conditions and the principles used by courts. The effect is to breed irritation, disrespect for law, together with a virtual alliance between the judiciary and entrenched interests that correspond most nearly to the conditions under which the rules of law were previously laid down. [Dewey 33]

To take an example that is no longer controversial, note how such pictures of the social and political order [selected from outside the established precepts of the legal system] and reference of legal questions thereto, dictated the divergent conclusions of the judges in the Dred Scott case. For we deceive ourselves grossly when we devise theories of law or theories of judicial decision that exclude such things from "the law". Indeed, they give the latter their living content and in all difficult cases are the ultimate basis of choosing, shaping and applying legal materials in the decision of controversies. [Pound (34)]

1.7 Law and Science, Compared

We have come a long way from my vow to take the sociological-anthropological approach to the study of the artifact, called "science", and the role it plays in the social activity, called "judicial policy-making". It may appear that I have done other than I had vowed. The discussion of science and law may have seemed first about logic, with cultural and institutional forces playing only the subsidiary role of filling in where the logic leaves gaps. If this has been the impression, I shall correct it here.

The same overt behavior (say a tribal pageant as it can be captured by the movie camera) may have an entirely different meaning to the performers: What interests the social scientist is merely whether it is a war dance, a barter trade, the reception of a friendly ambassador, or something else of this sort. [Schutz 35]

If we go back to Max Weber's conception of the subjective interpretation of meaning in social action, we will have a clearer notion of what is at issue here. Weber maintains that the primary task of the sociologist is to understand the meaning an act has for the actor himself, not for the observer. [Natan-son 36]

The task before us in this case study is to discern how the lawyers (the plaintiff lawyer, the defendant lawyer, as well as the judge) made use of the expert knowledge that the scientists presented to them. The verbal exchanges between the lawyers and the scientists provide us a window on, first, how the lawyers perceived, processed, and reacted to the scientific knowledge earmarked for their use in judicial policy-making and, second, how the scientists in turn made sense of the lawyers' legal reasoning.

We may liken the cross examination of the scientific witness to Schutz's tribal pageant. Is it a war dance? Is it a barter trade? Is it the reception of a friendly ambassador? Or is it something else that we see through the window?

To find out the answer, we must understand the meaning the cross examination had to the lawyers and scientists who participated in it. We must interpret the meanings to the artifact, called "scientific testimony", and the social activity, called "judicial policy-making"; that is, the meanings for the people who wielded the artifact and participated in the activity. This is the key to the specific task of diagnosing scientist-lawyer communication in the courtroom.

In the way that Schutz's camera captures the tribal pageant, the official court transcripts capture the lawyer-scientist exchanges. How do we, as observers, proceed to understand the exchanges?

Words with the same overt spellings and pronunciations may have entirely different meanings to different speakers and listeners, including the observer. This is emphatically the case where the speakers are lawyers and scientists, each group being highly trained in, and socialized into, their own way of thinking. Words, like "premise", "conclusion", "data", and "facts" have different, incomparable meanings to the two groups. This is because the different meanings are rooted in different internalized logics, different institutional frameworks, and the consequent, different perceptions of the "shared" situation.

The purpose to my discussion on science and law has been to provide the basis for an interpretive understanding of the meanings to the lawyer-

scientist exchanges, which we will be observing in the next section of the study. (We will explain what we mean by "interpretive understanding" in the next chapter, where we discuss the social-science methodology we are applying in our own inquiry.) Thus at no point in the earlier discussion did I retreat from my avowed sociological-anthropological approach. I was not imagining scientists and lawyers to be creatures of logic; I was merely demonstrating how their respective approaches to justification imbue their words with particular meanings.

Words leave a trail of powerful clues. Just as the verb "to accuse" is a clue to "(1) an accuser, (2) a person accused, (3) a person before whom the accusation is presented, (4) an act charged against the accused, and (5) a principle by which the act may be condemned,"³⁷ the words in the documents of the AT&T case are rich for what they reveal in the way of the institutional forces and the everyday realities that impinged on the lives of the participants in the AT&T case. They are artifacts invaluable to our diagnosis of the policy-making process.

I propose that the words, as captured by the court transcripts, had different meanings to the lawyers and the scientists as illustrated in Diagram 1, on the following page.

The diagram illustrates the symmetrical opposition between premise-orientation and conclusion-orientation. In science, the validity of the conclusion serves to indicate the validity of the premise. In law, the validity of the premise serves to indicate the validity of the conclusion. (The diagram refers to law specifically in the process of judicial policy-making that takes place in the courtroom setting; that is, this is law in

Diagram 1

SCIENCE

THE THEORY,
whose justification
is the primary
concern

major premise

Men
are
mortal.

THE INITIAL
CONDITIONS,
from a specific
situation

minor premise

Socrates
is a
man.

THE HYPOTHESIS,
a conclusion drawn
from the theory
and an indicator
of its soundness

conclusion

Socrates
is
mortal.

LAW

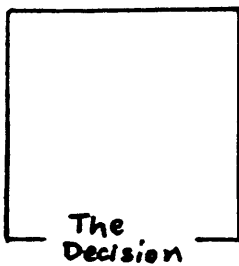
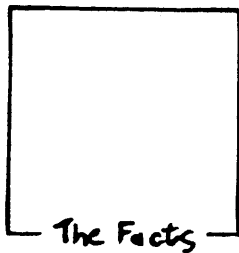
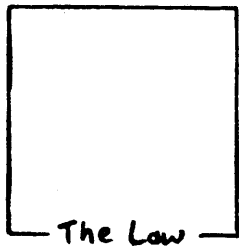
THE LAW,
a premise to the
decision and an
indicator of its
soundness

THE FACTS,
a premise to the
decision and an
indicator of its
soundness

THE DECISION,
whose justification
is the primary
concern

Diagram 2

THE
LAWYER'S
MAP



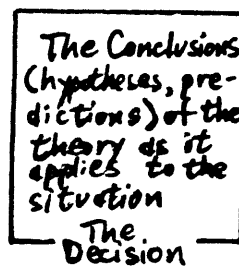
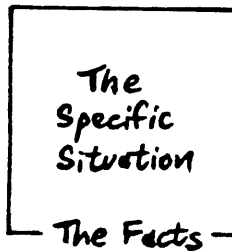
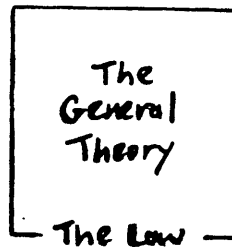
THE
TERRITORY

The
General
Theory

The
Specific
Situation

The Conclusions
(hypotheses, pre-
dictions) of the
theory as it
applies to the
situation

THE LAWYER'S PROJECTION
OF HIS MAP ONTO
THE TERRITORY



the form that the judge ultimately renders in his decision, and that the plaintiff lawyer and defendant lawyer present in their own arguments to the judge.) The diagram also makes it clear that the primary concerns of science and law are in justifying different things: in science, it is the premise; in law, it is the conclusion.

What happens when a lawyer encounters scientific testimony? What happens when legal methods are used to evaluate scientific theories?

If the lawyer's interest is in assessing the validity of certain calculations or estimates (hypothesized or predicted) of the theory, then the subsequent dynamic takes place, as illustrated in Diagram 2, on the page following Diagram 1. We have the curious result where, to the lawyer, justification in science is premise-oriented. The lawyer proceeds to question the validity of the hypothesized or predicted estimates by scrutinizing the theory. The Ashenfelter cross examination, in the next section of the study, will look more carefully at the phenomenon of the lawyer's projection of meanings and values, taken from his own legal culture, onto the artifacts presented by a member of the scientific culture.

Chapter 2 Social Science and Natural Science

To what extent does chapter one's description of the scientific language, in being taken from the philosophy of (natural) science, apply to social science? After all, Popper and Kuhn, whose writings we relied on, wrote about natural scientists. The judgment that the philosophy of science applies in whole or in part to social science either entails a leap of faith or, preferably, is a point that can be demonstrated. With regard to the purpose of this study, we need to demonstrate only that the philosophy of science applies "in part", namely, the part pertaining to justification. Does the scientific meaning to justification, which in chapter one we borrowed from the philosophy of (natural) science, also apply to social science?

The prior paragraph addresses the dual role of social science in this study: social science as this study's object (the testimonies which social scientists, as expert witnesses, offered in our case study) and social science as this study's method (the sociological-anthropological manner by which we formulate and justify our theory of scientist-lawyer communication). In a reflexive way that happens to improve the rigor of this study, how we characterize social science as this study's object must apply to our practice of social science as this study's method, and at the same time, how we characterize social science as this study's method must apply to our observation of social science as this study's object.

The importance of social science as this study's method cannot be overstated. In our observations and explanations of scientist-lawyer communication, we will in affect be claiming that we are able to enter the

minds of other men and to tell what goes on there. Yet, is this task possible? Isn't this sort of inquiry "subjective"? May we determine in an objective manner the subjective things that go on inside people's heads? How may I, as an observer, justify my assertions in this study, so that they supersede opinion and become science? In short, the following question is of critical importance: What is the method by which we are conducting this study?

In this chapter, we present a general description of inquiry in social science, including a clarification of its relationship to inquiry in natural science. The initial impression, based on the first two paragraphs, may be that our task is to extrapolate a description of social science from natural science. Actually, our task is not so much to extrapolate, as it is to liberate a description of social science from natural science. In the past, it has been social scientists themselves who have aped the approach of natural science (especially economists and those other social scientists who perform hypothesis testing, whether with laboratory controls or with statistical controls). It is not so much a free choice that we are making, as it is a circumstance in the historical development of the social sciences that we are coming to grips with, when we answer the question "What is social science?" by considering, first, "What is natural science?"

2.1 Formulation Versus Justification

With all the talk about justification in the first chapter, the reader may have wondered about the flip-side to justification, namely, formulation. Before the scientist may proceed to justify his theory, he must have his theory already in hand. How does he formulate the theory in the first place?

To this question, natural science and social science provide very different responses. Exploring the responses will help us put together a general description of social science.

2.2 Formulation in Natural Science

Where do theories come from in natural science?

Kuhn's notion of "paradigm" merits mention here. Paradigm refers not only to the state-of-the-art theory prevailing in the natural scientist's school of thought, but also to the social (perhaps "sociological") and political forces that transmit the theory and sanction its use within the given scientific community. Under such circumstances, there is no need for the natural scientist to engage in the formulation of a theory; instead, states Kuhn, "the scientist must premise current theory as the rules of his game" (emphasis in the original).¹ The natural scientist operates inside the framework of the prevailing theory, where his work largely consists of articulating the theory by eliminating its ambiguities.²

While there is some diversity in the respective views of the two men, Popper would agree with Kuhn that there is normally a theory, already in

place, for the natural scientist to use:

A scientist engaged in a piece of research, say in physics, can attack his problem straight away. He can go at once to the heart of the matter: to the heart, that is, of an organized structure. For a structure of scientific doctrines is already in existence; and with it, a generally accepted problem-situation.³

The description in the preceding paragraphs, especially the one about the notion of paradigm, applies to natural science only during "normal" times, that is, when the state-of-the-art theory lives up adequately to the scientific community's task of explaining the data it confronts. During less normal times--when the prevailing theory's hypotheses or predictions are refuted repeatedly by experience--there is a crisis in the community. Then, through the social and political forces of the scientific group, the old theory is deposed and a new, competing theory is installed, culminating in an episode that Kuhn calls a "scientific revolution".

The new theory, of course, must be formulated by somebody. What are the steps by which one or another individual natural scientist formulates a new theory? The following passage by Popper indicates the typical natural-science attitude towards formulation:

I said above that the work of the scientist consists in putting forward and testing theories.

The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man--whether it is a musical theme, a dramatic conflict, or a scientific theory--may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge.

This latter is concerned ... only with questions of justification or validity (Kant's quid juris?) [emphasis in the original]. Its questions are of the following kind. Can a statement be justified? And if so, how? Is it testable? Is it logically dependent on certain other statements? Or does it perhaps contradict them? In order that a statement may be logically examined in this way, it must already have been presented to us. Someone must [already] have formulated it, and submitted it to logical examination [emphasis added].

Accordingly, I shall distinguish sharply between the process of conceiving a new idea [the stage of formulation], and the methods and results of examining it logically [the stage of justification] ...

... my view of the matter, for what it is worth, is that there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process [emphasis added]. My view may be expressed by saying that every discovery contains 'an irrational element', or 'a creative intuition', in Bergson's sense. In a similar way Einstein speaks of the 'search for those highly universal laws ... from which a picture of the world can be obtained by pure deduction. There is no logical path', he says, 'leading to these ... laws. They can only be reached by intuition, based upon something like an intellectual love ('Einfühlung') of the objects of experience.'⁴

Another philosopher of science shares the same point of view:

The act of discovery escapes logical analysis; there are no logical rules in terms of which a "discovery machine" could be constructed that would take over the creative function of the genius. But it is not the logician's task to account for scientific discoveries; all he can do is to analyze the relation between given facts and a theory presented to him with the claim that it explains these facts. In other words, logic is concerned only with the context of justification. [Reichenbach ⁵]

There are two observations I would like to add to these insights.

First, I can think of no formal methods that natural scientist employ in

the stage of formulation. The formal methods that are responsible for the famed ability of natural science in quantify, predict, and control are situated in the stage of justification, not formulation. (Such methods, for example, include those associated with experimental design and statistical inference.) My second observation is that natural scientists refer to formulation as "discovery" (as in the famous distinction between "the context of discovery" and "the context of justification"). By labeling the process of formulation as one of "discovery", a person may then easily relegate the process to the status of "psychology", which contains "an irrational element" or "a creative intuition", and for which "there is no such thing as a logical method".

2.3 Formulation in Social Science

For social science, do there exist "paradigms" from which a social scientist may premise a theory as the rules of his game?

There are two reasons for us not to tackle this question.

First, the question is unnecessary with regard to our purpose of inquiring into the nature of formulation in social science. Whether or not "paradigms" exist in social science would neither obviate nor materially alter this inquiry. Suppose that "paradigms" do exist in social science; we would still have to ask how, during episodes of crisis and revolution in a given community of social scientists, the social-science theory is formulated. Suppose that paradigms do not exist in social science; we would still have to ask how it is that the individual social scientist goes about formulating the theory he uses. Therefore, whether or not paradigms exist, we would still need to inquire about the nature of formulation in social science.

Second, the term "paradigm" is so ambiguous that it might subvert this study's credibility and substantive discussion. According to Kuhn:

It has now been several years since a book of mine, The Structure of Scientific Revolutions, was published ... Monitoring conversations, particularly among the book's enthusiasts, I have sometimes found it hard to believe that all parties to the discussion had been engaged with the same volume. Part of the reason for its success is, I regretfully conclude, that it can be too nearly all things to all people.

For that excessive plasticity, no aspect of the book is so much responsible as its introduction of the word "paradigm" ... Critics, whether sympathetic or not, have been unanimous in underscoring the large

number of different senses in which the term is used. One commentator, who thought the matter worth systematic scrutiny, prepared a partial subject index and found at least twenty-two different usages ...⁶

While a tempting task, it would take us beyond both the scope and the purpose of this study if we were to follow up on Kuhn's own clarification to his concept of "paradigm", and then to see whether his result (concluded with regard to natural science) also holds for social science. Furthermore, for this study, like Kuhn's, to dwell on "paradigms" would be to invite, as does Kuhn's, the possibility for it to become "too nearly all things to all people"--a possibility which amounts to a serious digression from this study's purpose of establishing a theory of how scientists and lawyers communicate in the courtroom.

In either case--whether or not "paradigms" exist in social science--formulation in social science requires an approach that is systematic and calculated, unlike the approach that natural science takes to formulation, which Popper characterized (above) as a process "for which there is no such thing as a logical method" and which is driven by "an irrational element" or "a creative intuition".

The reason that social science needs a systematic and calculated approach is that its subject matter is richer than the subject matter of natural science. Social subject matter inevitably touches on human beings, whose own understandings the social scientist must first interpret and appreciate in order to formulate, much less justify, his theory. This is an additional challenge with which the natural scientist need not be concerned. The natural scientist, whose subject matter is molecules, atoms, electrons,

and other things of this sort, encounters no corresponding element in his subject matter that he must first appreciate. Schutz, whose ideas I am attempting to convey, states the issue clearly:

The world of nature, as explored by the natural scientist, does not "mean" anything to molecules, atoms, and electrons. But the observational field of the social scientist--social reality [society]--has a specific meaning and relevance structure for the human beings living, acting, and thinking within it. By a series of common-sense constructs they have pre-selected and pre-interpreted this world which they experience as the reality of their everyday lives. It is these thought objects of theirs which determine their behavior by motivating it. The thought objects constructed by the social scientist, in order to grasp this social reality, have to be founded upon the thought objects constructed by the common-sense thinking of men, living their daily life within their social world. Thus, the constructs of the social sciences are, so to speak, constructs of the second degree, that is, constructs of the constructs made by the actors on the social scene, whose behavior the social scientist has to observe and to explain in accordance with the procedural rules of his science. [Emphasis added.]⁷

Whether a construct exists on the "first level" or on the "second level" depends on whose viewpoint we are referring to. The constructs in the minds of the people in the situation that the social scientist is observing are the first-level constructs. The constructs in the minds of the social scientist himself, which comprise his scientific theory, are the second-level constructs. In order to formulate properly the second-level constructs, the social scientist must first have an appreciation for the first-level constructs. "Thus, the constructs of the social sciences are ... constructs of the constructs made by the actors on the social scene."

The general importance of first-level constructs to social-science inquiry cannot be overstated. To bring to life the importance of both the distinction and the relationship between the two levels of constructs, I present examples from the works of Lon Fuller and Herbert Gans.

Fuller tells a story as if designed for the purpose of illuminating the significance of what we call first-level constructs.

I see at a distance a boy who holds in his hand a small, gray, roundish object. He seems to be contemplating this object intently. After a period of hesitation, he places the object carefully between his palms and repeatedly presses on it. He then relaxes his grip, holds the object loosely in his left hand, and begins to look about him on the ground. He apparently finds what he wants, for he bends over and picks up a stick. This he uses for a while to prod or push against the object. He then throws the stick away and bends over to strike the object several times against a rock. Shortly he gives up this activity and walks about as if undecided what to do next. Suddenly he begins to gather sticks together, arranges them on the ground in a pile, lights a match to them, places the object in the fire, and then stands off in an attitude of expectancy.

Now it is obvious that something happens to this account, a sudden accretion of meaning occurs, when we learn that the boy was throughout trying to open a clam. Without this clue I could not interpret what I observed, retain accurately in memory the shape of events that occurred, or give a coherent account of what happened. [Emphasis added.]⁸

Before the point where Fuller informs us that the small round object is a clam in the boy's eyes, the depiction that we, as observers, had been formulating in our minds was really an inaccurate (more specifically, "empty") reconstruction of the boy's situation. Only with an appreciation for the first-level constructs--the constructs made by the actor on the social scene--may we become able to formulate a meaningful depiction.

Second-level constructs that are based only on publicly observable physical behavior, and not on first-level constructs, are not true-to-life. Knowing that the small round object is a clam, or a dynamite cap, is essential to the formulation of an accurate, meaningful, and not merely behavioral theory. To return to one of Schutz's own examples, the first-level constructs will help us distinguish whether the same overt behavior "is a war dance, a barter trade, the reception of a friendly ambassador, or something else of this sort."

In The Urban Villagers, Herbert Gans provides a glimpse of the American experience with urban renewal, as revealed in the instance of Boston's former West End.

The first-level constructs--the understanding that the working-class residents themselves had of the West End--depicted the district as a viable community or urban village. To them, the West End was a neighborhood, populated by families and friends whose presence gave the district the quality of home. The second-level constructs--the understanding that city planners, redevelopment officials, politicians, journalists, and other outside observers had of the West End--depicted the district not as an urban village with the positive social meaning of home, but as a slum whose physical delapidation warranted its clearance. With second-level constructs not founded on first-level constructs, the policy of bulldozing the West End and eradicating its residents' way of life was not only possible, but realized.

Negative policy consequences are not always present to magnify the error contained in second-level constructs, but I believe that the case of

the West End, as much as Fuller's story about the boy, does illuminate the significance of both the distinction and the relationship between first-level constructs and second-level constructs.

I call the social scientist's appreciation for the first-level constructs his interpretive understanding. This is because the social scientist must interpret the observed situation for the meaning it has for the natives: the small round object means food; the run-down building means home. I call the social scientist's second-level constructs, which comprise his explanation of the observed situation, his scientific understanding.

The interpretive understanding and the scientific understanding refer to the first-level constructs and the second-level constructs, respectively. The scientific understanding is based on the interpretive understanding; equivalently stated, the second-level constructs are based on the first-level constructs.

Because the interpretive understanding plays an indispensable role in the formulation of the social scientist's theory, the following general considerations are important: By what explicit, logical methods may the social scientist grasp the first-level constructs? By what approach may the social scientist come to see the world from the viewpoint of the observed people? How may the social scientist put into practice Weber's postulate of subjective interpretation? Because of these considerations, the formulation of social-science theories calls for a calculated, logical analysis, in contrast to the formulation of natural-science theories, for which one highly regarded and widely accepted philosopher asserts "there

is no such thing as a logical method."

It is beyond the scope of this study to track down fully the ramifications to these questions about formulation in social science. Because our immediate purpose is the modest one of describing the language of social science, a brief look at social science's responses to these questions will suffice.

There is a continuum. At one extreme, the social scientist may seek a rich and detailed understanding of the first level constructs made by the actor(s) on the social science. Here, the social scientist can be said to be embarking on "the exploration of the general principles according to which [the observed] man in daily life organizes his experiences." Such a journey would require "the procedures of a phenomenological analysis of the so-called natural attitude."⁹ I refer the reader to the complete works of Alfred Schutz for a rigorous explanation of the methods by which the social scientific observer may grasp the first-level, common-sense constructs of the people he observes.

The the other extreme, the social scientist may seek nothing more than a non-detailed, perhaps wooden, understanding of the first-level constructs. The freedom from detail, however, allows for a powerful approach, best represented in microeconomics. Rather than surveying the territory (taking a look at the people of interest and their institutions) and then drawing up a map (the interpretive understanding), the social scientist may simply make an assumption about the first-level constructs. For example, microeconomists typically assume that each individual they observe is no different from "rational economic man".

For the microeconomist, an adequate interpretive understanding is captured as soon as he has collected enough information to calibrate the parameters in rational economic man's utility curves. Surely, the microeconomist's interpretive understanding delivers a being who is more wooden and less life-like than what Schutz's phenomenological analysis would deliver, but the tradeoff may be worthwhile, depending on the observer's purpose in inquiry.

Bridging, perhaps, the two extremes is ethnography, which is an approach of anthropology and sociology. Not requiring an all-out phenomenological reduction,¹⁰ but also not simply assuming the interpretive understanding, many anthropologists and sociologists gain their interpretive understanding by doing an ethnography or performing a participant-observation study.

The ethnographic approach is that of anthropology, and, to a more limited extent, sociology, under the stiff but precise tag, participant observation. As practiced, this approach allows a fieldworker to use the culture of the setting (the socially acquired and shared knowledge available to the participants or members of the setting) to account for the observed patterns of human activity. In organizational studies, the patterns of interest are typically the various forms in which people manage to do things together in observable and repeated ways. Procedurally, the ethnographic method is described by Conklin¹¹ as involving "a long period of intimate study and residence in a well-defined community employing a wide range of observational techniques including prolonged face-to-face contact with members of local groups, direct participation in some of the group's activities, and a greater emphasis on intensive work with informants than on the use of documentary or survey data." [Van Maanen¹²]

The variety of methods established for developing the interpretive

understanding is indicated by the proliferation of schools of thought among practitioners of ethnography; the schools go by the names of thick description, ethnoscience, componential analysis, cognitive anthropology, and ethnomethodology.¹³

In summary, we observe that these methods, unique to social science, underscore the importance of the first-level constructs to the formulation of the social scientist's own second-level constructs.

2.4 Justification in Social Science

How may the social scientist determine whether or not his constructs are accurate?

As for the interpretive understanding, there is an elegant and perhaps poetic criterion that the ethnographer should fulfill, as set forth by Frederick Gearing in his study of the Fox, a Native American tribe in Iowa:

"One [such as the ethnographer] comes to recognize himself in another guise." That guise is of course the codes, the cultures, carried in the heads of [the observed] men to their encounters, there affecting their behaviors. When one elicits the codes one sees through them the man, the universally human, oneself. The face of the Fox is your own.¹⁴

A more formal expression of the same idea is:

[A frequently posed question] in connection with doing ethnographic studies concerns the criteria for judging the descriptive adequacy of the completed ethnography. These criteria are at once very simple and enormously complex. If, after having completed the ethnography, the observer can communicate the rules for proper and predictable conduct as judged by the people studied, he or she has produced a successful product. The ethnographer is like the linguist who has studied and recorded a foreign language so that others can learn the rules for producing intelligible speech in that language. As Fraake¹⁵ says, the adequacy of ethnography is to be evaluated "by the ability of a stranger to the culture (who may be the ethnographer) to use the ethnographer's statements as instructions for appropriately anticipating the scenes of the society. [Sanday¹⁶]

Sanday's position may be taken a step further. Because any inadequacies in the interpretive understanding will be passed on to the

scientific understanding, the subsequent testing of the latter in the stage of justification will help reveal inadequacies in the former. For example, a justified (falsifiable but not falsified) scientific understanding would be a sufficient condition for indicating an adequate interpretive understanding, while a falsified scientific understanding would open up the possibility that the interpretive understanding requires revision.

Schutz provides an explanation that ties together the issues pertaining to both formulation and justification. Recalling that the social scientist's constructs are distinct from the reality he observes, just as a map is distinct from the territory it portrays, we realize that the actual people whom the social scientist observes become, in the social scientist's mind, constructs or "puppets" of the original beings. The social scientist first makes observations, perhaps in the style of an ethnography, for the purpose of gathering clues for the first-level constructs that he imputes to the minds of his puppets, and then proceeds to formulate and scientifically test the subsequent second-level constructs, that is, his scientific theory. Schutz explains fully the social-science device of puppets or "homunculi":

How does the social scientist proceed? He observes certain facts and events within social reality which refer to human action and he constructs typical behavior or course-of-action patterns from what he has observed. Thereupon he co-ordinates to these typical course-of-action patterns models of an ideal actor or actors, whom he imagines as being gifted with consciousness. Yet it is a consciousness restricted so as to contain nothing but the elements relevant to the performing of the course-of-action patterns observed. He thus ascribes to this fictitious consciousness a set of typical notions, purposes, goals, which are assumed to be invariant in the specious consciousness of the imaginary actor-model. This homunculous or puppet is

supposed to be interrelated in interaction patterns to other homunculi or puppets constructed in a similar way ...¹⁷

Yet these models of actors are not human beings living within their biographical situation in the social world of everyday life. Strictly speaking, they do not have any biography or any history, and the situation into which they are placed is not a situation defined by them but defined by their creator, the social scientist. He has created these puppets or homunculi to manipulate them for his purpose. A merely specious consciousness is imputed to them by the scientist, which is constructed in such a way that its presupposed stock of knowledge at hand (including the ascribed set of invariant motives) would make actions originating from it subjectively understandable, provided that these actions were performed by real actors within the social world. But the puppet and his artificial consciousness is not subjected to the ontological conditions of human beings [that is, the institutional, biological, physical, and other real world constraints that an individual encounters as pre-given upon being born or upon entering a situation]. The homunculus was not born, he does not grow up, and he will not die. He has no hopes and no fears; he does not know anxiety as the chief motive of all his deeds. He is not free in the sense that his acting could transgress the limits his creator, the social scientist, has predetermined. He cannot, therefore, have other conflicts of interests and motives than those the social scientist has imputed to him. He cannot err, if to err is not his typical destiny. He cannot choose, except among the alternatives the social scientist has put before him as standing to his choice. Whereas man, as Simmel has clearly seen, enters any social relationship merely with a part of his self and is, at the same time, always within and outside of such a relationship, the homunculus, placed into a social relationship is involved therein in his totality. He is nothing else but the originator of his typical function because the artificial consciousness imputed to him contains merely those elements which are necessary to make such functions subjectively meaningful. [Emphasis added.]¹⁸

... Among these homunculi with which the social scientist populates his model of the social world of everyday life, [there are] sets of motives, goals, roles--in general, systems of relevances--[which the social scientist distributes] in such a way as the scientific problems under scrutiny require. Yet-- and this is the main point--these constructs are by no means arbitrary. [Emphasis added.] They are subject to the postulate of logical consistency and to the postulate of adequacy. The latter means that each term in such a scientific model of human action must be constructed in such a way that a human act performed within the real world by an individual actor as indicated by the typical construct would be understandable to the actor himself as well as to his fellow-men in terms of common-sense interpretation of everyday life. Compliance with the postulate of logical consistency warrants the objective validity of the thought objects constructed by the social scientist; compliance with the postulate of adequacy warrants their compatibility with the constructs of everyday life.

As the next step, the circumstances within which such a model operates may be varied, that is, the situation which the homunculi have to meet may be imagined as changed, but not the set of motives and relevances assumed to be the sole content of their consciousness. I may, for example, construct a model of a producer acting under conditions of unregulated competition, and another of a producer acting under cartel restrictions, and then compare the output of the same commodity of the same firm in the two models. In this way, it is possible to predict how such a puppet or system of puppets might behave under certain conditions and to discover certain "determinate relations between a set of variables, in terms of which ... empirically ascertainable regularities ... can be explained." This ... is Professor [Ernest] Nagel's definition of a theory.¹⁷, continued

(Incidentally, then, our response to the question, "Is it at all possible to determine in an objective manner the subjective things that go on inside people's heads?" is the avowal that we make no effort at all to enter people's heads (certainly an impossible task), but that we may deduce,

through empirically testable theories (like Schutz's theory of a producer), what in fact may or may not be going on in these people's minds. Hence our task is no different, and no more impossible, than the task of the physicist, who may not directly see atoms, molecules and electrons (certainly an impossible task, because there are no microscopes fine enough), but who may nonetheless deduce, through empirically testable theories, that atoms, molecules, and electrons surely do exist.)

With Schutz's insights, we are ready to provide a description of the language of social science.

With regard to the issue, "Do social scientists speak the same language as natural scientists?", we conclude that the issue is not so much that social science is different from natural science, as it is that social science must stand up to methodological challenges in addition to those that it shares with natural science. The additional challenges are situated in the stage of formulation, where the social scientist develops his interpretive understanding.

Natural scientists are not required, in their normal practice, to formulate theories: "the [natural] scientist must premise current theory as the rules of his game."¹ The natural scientist encounters the theory as pre-given by the "paradigm" or the "structure of scientific doctrines ... already in existence."³ Yet, even when a theory's predictions fail and a new theory needs to be formulated, then, at least according to Popper and Einstein, the enterprise of natural science enjoys the luxury of (1) being able to relegate formulation to the status of "psychology" (that is, containing "an irrational element" or a "creative intuition", for which "there

is not such things as a logical method"), and (2) still be able to formulate durable theories.

Social scientists, however, are normally required to approach formulation with one or another logical method, whether it is doing an ethnography, performing a phenomenological analysis, or calibrating the utility curves of rational economic man. The product of such formulation is the interpretive understanding, which is the basis for the scientific understanding that is tested in the subsequent stage of justification.

The commonalities between social-science inquiry and natural-science inquiry are situated in the stage of justification. Here, it is the task of all scientists to test the match between the given reality and their reconstruction of that reality. For such testing to proceed, the scientist's constructs must be falsifiable, so that any inaccuracy in them will be detectable and therefore correctable. It makes no difference to this criterion whether the constructs' subject matter is nature or society. The alternative to falsifiable constructs, whatever the subject matter, would be metaphysical, self-sealing constructs, which are anathema to social science just as they are to natural science.

Because of these commonalities in the stage of justification, the theories of social science are therefore subject to the same rigors of logic and empirical testing as the theories of natural science:

The system of typical [second-level] constructs designed by the scientist has to be established with the highest degree of clarity and distinctness ... and must be fully compatible with the principles of formal logic. [Emphasis added.] Fulfillment of this postulate [the postulate of logical consistency] war-

rants the objective validity of the thought objects constructed by the social scientist ... [Schutz 19]

The scientific constructs formed on the second level, in accordance with the procedural rules valid for all empirical sciences, are objective ideal typical constructs and, as such, of a different kind from those developed on the first level of common-sense thinking which they have to supersede. They are theoretical systems embodying general testable hypotheses ... This device has been used by social scientists concerned with theory long before this concept was formulated by Max Weber and developed by his school. [Emphasis added; Schutz 20]

2.5 Social Science and Natural Science, Compared

If we must at all distinguish between social science and natural science, one major distinction worth drawing would not be that social scientists speak a different or inferior language, but that they speak a language that explicitly addresses both formulation and justification. As a metaphoric way of summarizing the discussion to this point, we can say that the language of natural science is like a subset or "regional dialect" that addresses only justification. Because of this, we dare say that it is social science that provides the standard for natural science to live up to rather than it being the other way around, since inquiry in natural science is but a limiting case of inquiry in social science.

Another distinction would pertain to the meaning of the word "subjective". In natural science, it is a derogatory label attached to inquiry that is uncontrollable, uncorroborable, intuitive, biased, value-laden, and personal. Such is often the naturalist's description of the practice of interpretive understanding, where the charge is made that "understanding the motives of another man's action depends upon the private, uncontrollable, and unverifiable intuition of the observer or refers to his private value system."²¹

In social science, the interpretive understanding is also called "subjective", but with a very different meaning. It is "subjective" not because it is uncontrollable, but because it refers to the meanings held by the human subjects being observed (for example, Weber's postulate of his "subjective" interpretation of meaning, or Schutz's edict that his pup-

pets' actions be "subjectively" understandable). The interpretive understanding is controlled, first, by Schutz's postulates of adequacy²² and logical consistency and, ultimately, by the rigor of traditional scientific logic and testing in the stage of justification.

There is thus nothing unscientifically subjective about the interpretive understanding in particular or social science in general. Strictly speaking, because the interpretive understanding precedes and is essential to the scientific understanding, the former is more accurately characterized as prescientific, as opposed to unscientific or even non-scientific.

What makes social science social is the prescientific interpretation it performs to uncover the social meaning that it encounters as already present in its subject matter. What makes social science scientific is the explanation it offers of the subject matter, an explanation it puts up to the same rigors of logic and empirical testing as required of explanation in natural science.

The prescientific interpretive understanding, which refers to the first-level constructs, is situated in the stage of formulation. The scientific, explanatory understanding, which refers to the second-level constructs, is situated in the stage of justification. The two stages of inquiry are complementary, not mutually exclusive.

A third distinction would be that it is not only controllable and proper, but essential, for the social scientist to involve his values unashamedly when conducting his inquiry. He must be able to apply a sense of values in order to capture the intended meanings in what he is observing. (Otherwise, in no way could the observer capture the meaning signified by

Fuller's small round object or see that the "face of the Fox is your own.") To achieve this sort of understanding without the help of a value system would not be humanly possible. The product of a social inquiry, devoid of value, would be a strictly behavioral theory, empty of meaning. Instead, through the employment of logical methods, which may include but are not restricted to those of ethnography, the social scientist actually embraces and harnesses his values in a constructive, controllable way for the purpose of formulating a meaningful theory, that is, a social-science theory that embodies a true-to-life depiction. Not faced with the challenge of the interpretive understanding, the natural scientist need concern himself with his values only as a potentially troublesome source of bias in the stage of justification (a concern that equally burdens the social scientist).

The final distinction would be that inductive inference, which is anathema to inquiry in natural science, plays an indispensable role in inquiry in social science. The reason, again, has to do with the additional challenge of interpretive understanding that social science confronts in the stage of formulation. Consider the contrast in the following remarks by Sanday, who is discussing two anthropologists, and Kuhn.

Benedict was committed ... to Boas's Baconian dedication to the primacy of induction. Boas ... continually warned his students against making generalizations before all the facts were recorded, sifted, compared, and carefully analyzed. [Sanday 23]

... neither Sir Karl nor I is an inductivist. We do not believe that there are rules for inducing correct theories from facts, or that theories, correct or incorrect, are induced at all. [Kuhn 24]

We may explain the seeming contradiction by observing that Kuhn and Popper are referring to the stage of justification--ostensibly the only stage of explicit logical concern to inquiry in natural science--while Benedict and Boas are referring to the stage of formulation, particularly the development of the interpretive understanding.

Formulation is necessarily inductive. In the development of the interpretive understanding, a social scientist's construct is the mental image that is left behind after the social scientist has observed something "n" times. The image is also called the ideal type, typification, kernel, sedimentation, essence, or generalization.²⁵

Inductive inference is, of course, not permissible in the stage of justification. This applies to both social science and natural science. Justification in science is not inductive, but deductive (that is, not premise-oriented, but conclusion-oriented), as maintained by Popper and Kuhn, and as implied by Schutz ("scientific constructs formed [by the social scientist] on the second level [are to be] in accordance with the procedural rules valid for all empirical sciences"). In the stage of justification, the constructs of social science must be put up to the same rigors of logic and empirical testing that confront the constructs of natural science.

If social science, as actually practiced, ever appears less rigorous than natural science, it is because inquiry in social science is more challenging than inquiry in natural science. Often, social science even eludes capture in the convenient form of mathematics. Overall, the language of social science is more difficult to learn, much less speak.

Does the commonly observed fact, that social-science theories survive less often the rigors of logic and empirical testing than do natural-science theories, signify that social science is less scientific? Our response to this concern is a resounding no. Scientificity, as we have defined it, is a quality residing in the form, not the substance, of a theory. This distinction is important to our inquiry into scientist-lawyer communication, because the outcome of the communication (we will demonstrate) turns more-so on the difference in the logical forms between science and law (the former being conclusion-oriented, and the latter, premise-oriented) than on the difference in the substance between what the scientists and the lawyers say.

2.6 Social Science as This Study's Method

For the purpose of building up an interpretive understanding of the actors in the AT&T case, we may not readily apply the ethnographic approach of sociology and anthropology. For the purpose of testing the theory we have been formulating, we may not proceed to run an experiment, at least in the conventional way that laboratory scientists do.

The reason is that our subject matter is embedded in the past .

With regard to the formulation of our theory, the consequence is that, unless we may board a time machine, we may not engage in "a long period of intimate study and residence" among the scientists and the lawyers in their organizational setting, "employing a wide range of observational techniques including prolonged face-to-face contact with members of [the scientific and legal] groups, direct participation in some of the group's activities, and [an] emphasis on intensive work with informants ..." We may not build our interpretive understanding in the rich manner that Van Maanen and Sanday describe, because the world of the AT&T case is no longer in existence.

With regard to the formulation as well as justification of our theory, the past nature of the events poses two problems. First, the events of the AT&T case are unique and nonrecurrent. Such is the nature of case studies as well as most situations that social-science inquiry confronts. Because the events only happen once, there is the problem that they may not be re-run, as in an experiment, to provide observations for testing predictions drawn from the theory. The second problem has to do with

the completeness of our data. Unlike scientists who may gather first-hand data to study the phenomena of today, we confront a no-longer existing subject matter, therefore requiring that observations be reconstructed from a trail of clues, such as documents and memories, that the passing events had left behind. The problem is that the vantage point offered by the reconstruction from documents provides, at best, only a partial glimpse of the events. This problem applies to the observations we need for the task of justification, as well as the task of formulation.

The composite problem we face in our method is the problem of history. How do we retrieve the past for present observation? How do we carry out the scientific study of no-longer existing events?

I do not know the general answer to this puzzle, but I offer here a case-specific solution.

The Interpretive Understanding

The sort of ethnographic interpretation that Van Maanen and Sanday describe would of course be ideal, but it is indispensable only where the observer is encountering a community of which he has absolutely no prior knowledge, as in the case of a British anthropologist encountering an Eskimo village which has had no outside contacts. The anthropologist would have to build up an understanding from scratch.

Happily, our task is an easier one. The communities we are scrutinizing in the AT&T case--the community of scientists and the community of lawyers--happen to have been already scrutinized quite thoroughly by other observers. We may build on their work, rather than duplicating it.

In this light, the philosophy of science and the philosophy of law are like ready-made ethnographies. Philosophic observers, like Popper, Kuhn, Schutz, Nagel, Dewey, and Pound, would be the ethnographers in this metaphor.

For example, because Popper offers a detailed reconstruction or "thick description" of the thought patterns in the "savage mind" of the scientific native, we may liken Popper to a cognitive anthropologist. In the same way, Kuhn is like an ethnomethodologist whose "subjective interpretation" of symbols and myths indicates that periodic occurrence of what the scientific natives consider "revolution"; it recurs whenever they experience a breakdown in their rituals and beliefs. Also, we may regard Schutz as the social scientist's ethnographer par excellence--as if he were an ethnographer of ethnographers--who commands an unusually clear vantage point for discerning the constructs residing in the minds of social scientists.

What Popper, Kuhn, and Schutz do for scientific natives, Dewey and Pound do for legal natives.

In addition to building on the accumulated work of these "ethnographers", we also take advantage of the fact that the scientific community and the legal community both happen to engage in a high degree of self-interpretation. This is evident in the artifacts they create, which they call "textbooks". These artifacts serve the function of inculcating into each community's members what its symbols and practices mean. Naturally, as observers, we take advantage of these texts for our own purpose. (For example, in performing our later interpretation to uncover the meaning contained in the linguistic symbols, "sampling" and "statistical signifi-

cance," we will rely on texts from the scientific community.) Furthermore, as if for our convenience, the natives of the legal community have even compiled their own lexicon (they call it Black's Law Dictionary) which provides clues to the meanings of their words (which bear the likes of "prima facie" and "stare decisis et non quieta movere"), rendering them practically understandable to the English-speaking observer.

Of immense help to my research has been one of Harvard's ethnographic collections. Known as "The Law School Library", it houses a wide assortment of legal texts and other legal artifacts, many of which are on public display. Legal aspirants may also be observed in their natural setting.

To supplement the prior work of other ethnographers, I have been making my own observations, reconstructed from documents, of the scientists and the lawyers in the AT&T case. (This must be what is meant by an "archival ethnography.") Original observations are necessary to make sure that the previously developed interpretive understandings in fact apply, to fill in the details of how they apply, and to provide clues for their modification where they fail to apply. An important detail that this study has identified, for example, was a lawyer's premise-oriented map that had directed him away from a pertinent portion of the scientific territory, namely, the portion where the prediction is tested against observation.

Now that we have defined the role of the interpretive understanding in this study, we are finally in a position to point out the importance of chapter one, "Science and Law." It presents this study's prescientific, interpretive understanding of the first-level constructs residing in the minds of the scientists and the lawyers in the AT&T case. To be exact, I

am talking not about scientists and lawyers, but puppets of scientists and lawyers. I am imputing premise-oriented constructs to the minds of the lawyer-puppets and conclusion-oriented constructs to the minds of the scientist-puppets.

Supplemented by original observations that start in the chapter on the cross examination of Dr. Ashenfelter, the interpretive understanding will be our basis for formulating a scientific understanding of the AT&T case.

The Scientific Understanding

It is a problem familiar to social science: the subject matter at hand consists of events that are nonrecurrent and unique. We certainly may not re-run the AT&T case in an experiment to provide observations by which to test predictions drawn from our theory.

However, all is not lost. Experiments, especially those conducted under controlled, laboratory conditions, are only a limiting form of what Nagel calls "controlled investigation".

In a controlled experiment, the experimenter can manipulate at will, even if only within limits, certain features in a situation (often designated as "variables" or "factors") which are assumed to constitute the relevant conditions for the occurrence of the phenomenon under study, so that by repeatedly varying some of them (in the ideal case, by varying just one) but keeping the others constant, the observer can study the effects of such changes upon the phenomenon and discover the constant relations of dependence between the phenomenon and the variables. Controlled experiment thus involves not only directed changes in variables that can be reliably identified and distinguished from other variables, but also the reproduction of effects induced by such changes upon the

phenomenon under study.²⁶

Controlled investigation consists in a deliberate search for contrasting occasions in which the phenomenon is ... manifested ... and in the subsequent examination of certain factors discriminated in those occasions in order to ascertain whether variations in these factors are related to differences in the phenomena--where these factors as well as the different manifestations of the phenomenon are selected for careful observation because they are assumed to be relevantly related. From the perspective of the logical role which empirical data play in inquiry it is clearly immaterial whether the observed variations in the assumed determining factors for observed changes in the phenomenon are introduced by the scientist himself [in a controlled experiment], or whether such variations have been produced "naturally" and are simply found by him--provided that in each case the observations have been made with equal care and that the occurrences manifesting the variations in the factors and in the phenomenon are alike in all other relevant respects. It is for this reason that experimentation is often regarded as a limiting form of controlled investigation ...

In short, although it is possible to make scientific headway without experiment, either controlled experimentation ... or controlled investigation ... appears to be indispensable.²⁷ [Emphasis added in both quotations.]

To illustrate how "experimentation" and "controlled investigation" pertain to our case study, we provide examples.

One experiment I would like to perform, but which I cannot, would be to shift Mr. Levy and Dr. Ashenfelter (whom we observe in the next chapter) into the setting of a seminar of graduate students in economics. By varying the institutional setting, the experiment would be varying the institutional forces. No longer subject to the institutional forces of the courtroom, but instead subject to the institutional forces of the sci-

entific academy, the interaction between Mr. Levy (the cross-examining lawyer) and Dr. Ashenfelter (the scientific witness) should proceed differently and should culminate in a different outcome; that is, Dr. Ashenfelter, not Mr. Levy, should be the one in the driver's seat and, with the peer-group pressure against Mr. Levy, the burden should be on Mr. Levy, rather than Dr. Ashenfelter, to prove himself. This amounts to a hypothesis or prediction, providing a test of the theory that scientist-lawyer communication depends not only on the cultural forces (the premise-oriented constructs in the lawyer's mind and the conclusion-oriented constructs in the scientist's mind), which this experiment would be controlling for, but also on the institutional forces (as present in the settings of the courtroom and the scientific academy), which this experiment would be varying. However, because the AT&T case is unique and nonrecurrent, we may not conduct an experiment to provide any observations for testing this procedure.

Yet, in the manner Nagel describes, controlled investigation is a viable alternative. For example, to test the basic theory that Mr. Levy was operating with a premise-oriented map, we may observe variations in Mr. Levy's "territory": the territory of scientific assumptions, the territory of statistical inference, and the territory of hypothesis testing. This amounts to what Nagel calls the "contrasting occasions in which the phenomenon [the legal premise-oriented map] is ... manifested," where "such variations have been produced 'naturally' and are simply found by [the scientist]." Across the three occasions, we may corroborate the theory that Mr. Levy was reading from a premise-oriented map. We may also corroborate the theory that scientist-lawyer communication depends

on both the cultural and institutional forces. However, to avoid any charges or misunderstandings that we would be using the same data (the Ashenfelter cross examination) for both formulating and justifying our theory of scientist-lawyer communication, we will use the Ashenfelter cross-examination only for formulating our theory, saving the testing of the theory for the chapter on the cross examination of another scientific witness, Dr. Judith Long Laws.

The theory we are testing is made up in part of two "puppets": one, the representation of a scientist; the other, a lawyer. The procedure by which we carry out the empirical testing of the theory consists of varying the human beings--actual scientists, like Dr. Ashenfelter and Dr. Laws, and actual lawyers, like Mr. Levy and the Presiding Examiner--against whose real-life actions we compare the wooden movements of the scientist-puppet and the lawyer-puppet.

To those readers who are unperturbed by anthropological jargon, we say that the theory revolved around the "roles" (or "slots") which are a component part of a more-or-less steadfast social structure (here, organizational structure), into and out of which individual occupants (like Mr. Levy and Dr. Ashenfelter) move. The theory involves the role that is called "lawyer"; the role that is called "scientist"; the opportunities and constraints that are attached to each role and that occupants of the roles thereby encounter as pre-given; and the relationships allowed between the two roles, in the given structural setting.

Thus the unique and nonrecurrent nature of our subject matter in no way detracts from the scientificity of this study's social inquiry. Con-

trolled investigation permits hypothesis testing, just as controlled experimentation would permit under more tractable, less challenging conditions. It is as if (1) we had been present during the unfolding of the events, but saved up our observations for the current study, and (2) instead of our selecting which factors to control and which to vary, this selection was made by someone else on the research team. History and social science in general are not alone in relying on "natural experiments"; Nagel states that for astronomy, geology, and at one time embryology, "lack of opportunity for controlled experiment has not prevented scientists from arriving at well-grounded general laws."²⁸

There is one final point worth mentioning with regard to social science as this study's method. It deals with the issue "on whether science is viewed as a body of propositions or as the enterprise in which they are generated, as product or as process,"²⁹ and it also speaks to the problem, mentioned earlier, that our observations, being reconstructed from documents, offers at best a partial view of the events we seek to depict in our theory.

It is essential to acknowledge that the theory, at this study's completion, will be far from complete. If we were to obtain a more complete picture by looking through additional documents or interviewing more people, we could easily render a better theory with the more complete data.

However, it is not our purpose to leave no stone unturned, at least with regard to the 200 boxes of documents in storage at the National Archives in Washington, D.C. As a scientist, I construe my task more usefully as concerned with setting up a researchable enterprise, where

- (1) the theory I offer is as accurate as possible, in that it explains all the data I have so far come across;
- (2) the logical form of the theory allows it to be falsified;
- (3) the theory may be improved, or struck into conformance with the facts, whenever contradictory observations do appear; and
- (4) other scientists may be able to duplicate or carry on the research that I perform.

I see this study as but one step in a progression. The next step would be the theory in its succeeding, improved form, whether the improvement is effected by another scientist or by myself. The improvement is effected by another scientist or by myself. The improvement would occur after an encounter with contradictory evidence, which the improved theory must account for. New evidence, some of it contradictory to the theory, will inevitably turn up as additional documents are reviewed and more people are interviewed.

This type of progression, in which further research leads to further observation and then further improvements in the theory, is not unique to this study, to historical research, or to social science in general. Further evidence may always turn up, whether we are dealing with the subject matter of social science or the subject matter of natural science.³⁰ In this sense, at any moment in any social-science inquiry, all the data immediately available are always only partial. The "partial view" afforded by documentary data therefore does not detract from the scientificity of histori-

cal inquiry, any more than the "partial view" afforded by currently available data detracts from the scientificity of an accepted theory in physics, which remains open to the possibility that, in continued testing, "subsequent negative decisions may always overthrow it."³¹

The eventual falsification of a theory, in my opinion, is no cause for negative criticism. Theories are set up in the first place to be falsified. What is the point of falsifiability if not falsification?

The term "scientific theory" is not an honorific label that we reserve for the progression's final, perfected step. It is a term for what is merely the latest in a series of stopping points along the progression. Besides, it would be impossible to achieve a perfected theory: no map may become a territory; no puppet may become a man.

I think it is folly whenever a scientist stakes his reputation on the validity of one of these temporary stopping points. "It is recalled that the whole history of science is a graveyard of abandoned theories that were once thought to yield 'explanations'."³² For this reason, I think of science as the enterprise that produces scientific knowledge, and I take the atypical, un-macho stance of looking forward to the falsification, abandonment, and improvement of the temporary stopping point that I humbly offer in this study, because it will mean that the researchable enterprise that I have set up is underway.

2.7 Social Science as This Study's Object

To recap, our depiction of social-science inquiry consists of (1) developing a prescientific, interpretive understanding of the first-level constructs in the minds of the subjects, which serves as (2) the basis for formulating the scientist's own scientific, explanatory understanding, consisting of the second-level constructs that make up the theory, and then (3) justifying the theory by comparing its hypotheses or predictions against observation, whether through controlled experiment or controlled investigation.

Is this depiction of social science an accurate one? Does it apply to social scientists in general? In light of these concerns, some qualifications to the depiction are in order.

As in any other community, there is a division of labor in the community of scientists. In natural science as in social science, there is a division drawn along the lines between different subject matters. Physicists study atoms, molecules, electrons, and other things of this sort, while biologists scrutinize life forms. Sociologists focus on group settings while psychologists focus on individuals.

A more interesting division of labor in the scientific community takes place along the lines of the different stages of inquiry. There are some natural scientists who boldly formulate new theories, while others conservatively collect data to illustrate and justify old theories (see Ian Mitroff, The Subjective Side of Science). I believe there is a division of labor along similar lines among social scientists, separating those who seek

interpretation from those who seek explanation, from those who scientifically test such explanations.

A few remarks about the division of labor in the social-science community will serve to qualify this study's depiction of social science.

Some social scientists conduct inquiry exclusively within the stage of formulation, namely, developing the interpretive understanding. Sanday observes the positions taken by two anthropologists:

Geertz believes that "man is an animal suspended in webs of significance he himself has spun." Geertz takes culture to be those webs and the analysis of culture to be "not an experimental science in search of law but an interpretive one in search of meaning." For Geertz anthropological analysis requires ... wading through the clusters upon clusters of symbols by which man confers significance upon his own experience ... What defines the ethnographic enterprise, Geertz says, "is the kind of intellectual effort it is: an elaborate venture in, to borrow a notion from Gilbert Ryle, 'thick description'."

...

[Geertz] says that "the essential task of theory building is not to codify abstract regularities but to make thick description possible," "not to generalize across cases but to generalize within them." He likens this process to clinical inference which begins with a set of symptoms and attempts to place them within an intelligible frame. In culture the symptoms are symbolic acts or clusters of symbolic acts. Theory is used to ferret out the unapparent import of things. Cultural theory is diagnostic, not predictive.

Such a view of how theory functions in an interpretive science suggests to Geertz the distinction between "inscription" (thick description) and "specification" (diagnosis) as contrasted with the distinction between description and explanation found in the experimental or observational sciences. The distinction Geertz favors is "between setting

down the meaning particular social actions have for the actors whose actions they are [that is, the first-level constructs], and stating, as explicitly as we can manage, what the knowledge thus attained demonstrates about the society in which it is found and, beyond that, about social life as such [that is, the second-level constructs]."

Similarly, Mead declared no sympathy for the attempt to build a science of man which "would conform to the ideal science, physics" and her sympathy for "those who have insisted on the complexity and uniqueness of significant events in the life of a bird or the life of a human being." Even more to the point, Mead argued against the relevance of statistics for problems that required complex situational and emotional statements of context and compared her role to that of an insightful medical diagnostician. The student of the more intangible and psychological aspects of human behavior, she said, "is forced to illuminate rather than demonstrate a thesis" just as the physician and the psychiatrist "have found it necessary to describe each case separately and to use their cases as illuminations of a thesis rather than as irrefutable proof [sic] such as it is possible to adduce in the physical sciences." [Emphasis added. References deleted. Sanday 33]

The relevance of the passage to this study is that, in the division of labor in the community of social scientists, there are some members who do not address at all what we have been calling the stage of justification, in which a scientific explanation is tested by comparing its hypotheses or predictions against observation, whether through controlled experiment or thorough controlled investigation. Instead, these social scientists regard the prescientific, interpretive understanding of first-level constructs to be, in itself, a complete inquiry. Indeed, it may be ethnocentric on our part, as observers, to call their work "pre"-scientific; seeing their work as central, they set aside the scientific, explanatory understanding because,

they feel, it is inappropriate to the subject matter (the "complexity and uniqueness" of life) or inappropriate to their purpose in inquiry (meaning, not law; diagnosis, not prediction; illumination, not proof; interpretation, not explanation). Their work, in their eyes, is not merely preparatory to something else; their work is not "pre"-scientific, but scientific, as in Sanday's own phrase, "interpretive science" (although, of course, Sanday means something different by the word "science" from what Popper or this study means by that word).

None of this is to say that these social scientists perform no tests to justify the products of their inquiry. The point is that their tests exclude deductive (conclusion-oriented) justification, which is the sort that Popper and this study associate with falsifiable (hence, scientific) constructs. (One test that ethnographers perform is whether they "can communicate the rules for proper and predictable conduct as judged by the people studied." Another test would be the requirement that the interpretive understanding not contradict any of the facts from which it is induced.)

Personally, I feel we could easily add a scientific test at the end of what Geertz or Mead would consider a completed inquiry. We could (1) impute, based on our interpretive understanding, certain constructs to the specious consciousness of homunculi; (2) formulate a theory to explain the the homunculi's actions in their institutional setting; (3) derive falsifiable hypotheses or predictions from the theory; and then (4) test the hypotheses or predictions against observation, whether through controlled

experiment or controlled investigation. Because this procedure would be in addition to the inquiry performed by Geertz or Mead, it would be complementary or optional to their inquiry, and in keeping with their spirit. Scientific explanation complements "prescientific" interpretation.

Our immediate purpose, however, is not to debate Geertz's and Mead's school of thought. In the abbreviated analysis that we are here performing of social scientists who are like Geertz and Mead, our task is to uncover the meaning that social science has for them; our task is not to impose, ethnocentrically, our own beliefs of what social science is or should be. The pertinent issue here is that there exist social scientists, for whom we may regard Geertz and Mead to be representatives, who do not practice the deductive, conclusion-oriented justification that characterizes this study's depiction of science. Instead, their inquiry dwells in what this study identifies as the stage of formulation, specifically the development of the interpretive understanding. Social scientists whose inquiries dwell in the stage of formulation (where the purpose in inquiry is interpretation, meaning, diagnosis, or illumination) to the exclusion of deductive, conclusion-oriented justification (where the purpose in inquiry is explanation, general law, prediction, or proof) include some, but certainly not all, anthropologists, historians, sociologists, and psychologists. Because these social scientists explicitly disavow the latter stage, they do not speak science in the way this study depicts science. The social scientists that this study therefore does apply to are restricted to those who take the position that the accuracy of their social-science theory depends ultimately on the results of hypothesis testing, experimentation, or some

other aspect integral to the stage of deductive, conclusion-oriented justification. There must be clues in our documents that the social scientist, whom we are observing, indeed embraces this position. Dr. Ashenfelter, for example, employed conclusion-oriented reasoning in his explanation of the quintessentially scientific artifact of statistical inference; acknowledged he was deriving a prediction from economic theory; and explicitly embraced hypothesis testing in an expanded version of his written testimony that he prepared for publication after the conclusion of the AT&T case.

We should take notice that, in applying Popper's demarcation criterion (that the distinguishing trait of scientific theories is their falsifiable form) to social science, our theory of scientist-lawyer communication will therefore humbly apply only to those social scientists who work with falsifiable theories and who practice conclusion-oriented justification. Regretfully, our theory will not apply to, for example, followers of Marx, Freud, and Adler, whose approaches Popper believed were inductively self-sealing and whom Popper specifically had in mind when he developed his demarcation criterion.

2.8 Appendix to Chapter Two and Summary of Three Steps in "Social Science": Interpretation, Explanation, Justification

1. The Establishment of the Interpretive Understanding. In general, it is essential in social inquiry to interpret "the meaning an act has for the actor himself, not for the observer."³⁴ Indeed, it is only in the biased light projected by the scientifically-trained observer that the legal, premise-oriented meaning to science might appear "irrational"; from the viewpoint of the judge and the lawyers themselves, the meaning that they attached to science was perfectly rational and, given the rules and customs of the courtroom, required. These meanings, however subjective they may be, are pre-existing as an integral part of the subject matter that I, as an observer, encounter when conducting my inquiry; therefore, I must uncover these meanings like any other data, lest I risk formulating my theory on the basis of seriously incomplete information. In establishing an interpretive understanding, I come to understand the world in the way that the observed people (the scientists, the lawyers, the judge) understand it.

I look upon the philosophy of science, the philosophy of social science, and the philosophy of law as providing conveniently (if somewhat unconventionally) "ethnographies" or "thick descriptions" of how natural scientists, social scientists, and lawyers understand their respective worlds.

2. The Formulation of the Scientific Theory. The interpretive understanding that I, as an observer, possess in my mind is not sufficient in itself to constitute a scientific theory that explains scientist-lawyer

communication. While essential, it is not enough to set up a mirror that descriptively captures the salient artifacts and meanings in the world of the people whom I am observing.

For example, in performing a study of the sexist behavior of middle-level managers in corporations, it would not be enough for me to understand the corporate world in the sexist way that the managers understand it. In addition, I would need to formulate a theory that, while being based on the interpretive understanding, transcends it by providing a detached explanation of what makes the managers' sexist behavior both possible and realized.

In general, a theory consists of propositions or mental constructs, related to one another according to the rules of logic. Strictly speaking, these mental propositions are abstract or "empty" until applied to a concrete phenomenon, whether in nature or society. (Application of a theory involves attaching the mental propositions [ideally, one by one] to their respective correlates in the concrete world.) A theory is therefore a fiction, bearing the same relationship to the depicted reality as a map to a territory, a photograph to a scene, a representation to a presentation. In this way, the actual people being observed become, in social-science theories, "puppets" that are representations of the original beings.

The objective in my construction of a social-science theory that explains scientist-lawyer communication is for the wooden movements of my scientist-puppet and lawyer-puppet to predict or match the real-life actions of the many scientists and lawyers that I encounter in my observations.

I use my interpretive understanding of the scientific culture and the legal culture as the source that provides the thoughts, motives and logics

which I impute to the minds of the scientist-puppet and the lawyer-puppet. A social science theory, however, must account for more than just the individuals; it must also account for the immediate institutional setting that these individuals themselves encounter as "given" when they appear on the scene. I thus use my understanding of the institutional forces of the courtroom to provide the "rules of the game", or the opportunities and constraints, which specify the publicly-observable movements allowed to the scientist-puppet and the lawyer-puppet. With such a theory, I transcend the merely empathetic, interpretive understanding, and I provide an explanation that also accounts for the institutional forces, of which the observed people may not even be consciously aware.

(A less graphic, but more conventional term for "puppet" is "role". For example, in the same way that the role of mother in a kinship structure specifies a pattern of behavior common to all the particular individuals who come to occupy that role, my scientist-puppet specifies a pattern of behavior common to all the particular individuals who are trained and socialized as scientists, at least when they are testifying as expert witnesses in a courtroom setting. For me to say that the wooden movements of my scientist-puppet predict or match the real-life actions of observed scientists means, simply, that I have defined accurately the sociological-anthropological role of scientific expert witness.)

3. The Justification or the Testing of the Theory. What makes a theory scientific is independent of its substance referring to nature or society. Instead, I regard scientificity as a quality that stems from the logical

form of the mental propositions that constitute the theory.

First, the propositions must be logically consistent.

However, while this is certainly a necessary condition for a theory to be scientific, it is far from being a sufficient condition. Consider that non-Euclidean geometries, while purely imaginary, are also made up of logically consistent propositions. In addition to being logical the propositions must attach to a concrete reality. That is, the theory must be empirical; the logical form of the theory must allow any mismatches between the theory's depiction (the map) and the reality being depicted (the territory) to be detectable and, hence, correctable. The logical form of the theory must allow the theory to be refuted or falsified, just in case it contains any inaccuracy.

Thus, hypothesized or predicted events, based on the theory, are checked against observed events. If the observation refutes the hypothesis or prediction, an inaccuracy in the theory is indicated and its correction is called for. If the observation corroborates the hypothesis or prediction, the theory stands as accurate, at least until a more stringent test comes along and succeeds in producing an observation that refutes it, whereupon the correction is made, and the next stringent test is awaited.

The alternative to a falsifiable theory is a theory whose propositions, even if logically consistent, may be as metaphysical or nonempirical as those of a non-Euclidean geometry. Any inaccuracies in the theory's depiction would not be detectable, rendering the theory into a fiction with no correlates in the concrete world.

In my dissertation, I test my theory of scientist-lawyer communication

by specifying patterns of behavior that, I predict, should persist across different scientists, different lawyers, different scientific fields, and different fact situations. I make corrections wherever they are called for.

I see my dissertation as but one step in a progression. The next step would be the theory in its succeeding, improved form. This type of progression, in which further observations lead to further improvements in the theory, characterizes both social-science inquiry and natural-science inquiry. In the context of this progression, a theory's falsification is not a poor reflection on the theory. Theories are deliberately set up as falsifiable. What is the point of falsifiability if not falsification? A theory's falsification is, therefore, actually a good sign; it indicates that the theory can be improved, that it may be struck into conformance with reality.

The term "scientific theory" is not an honorific label that I reserve for the progression's final, perfected step. It is a term that denotes what is merely the latest in a series of stopping points along the progression. Besides, it would be impossible to achieve a perfected theory: no map may become a territory; no puppet may become a man.

I think it is folly whenever a scientist stakes his reputation on the validity of one of these temporary stopping points. "It is recalled that the whole history of science is a graveyard of abandoned theories that were once thought to yield 'explanations'."³⁵ For this reason, I think of science not as a body of knowledge, but as the enterprise that produces the knowledge, and I take the atypical, un-macho stance of looking forward to the falsification, abandonment, and improvement of the temporary stopping

point that I humbly offer in my dissertation, because it will signify that I have succeeded in setting up a researchable, scientific enterprise.

Chapter 3 Formulating the Theory of Scientist-Lawyer Communication: The Cross-Examination of Dr. Orley Ashenfelter

The cross-examination of Dr. Orley Ashenfelter will illustrate clearly what we mean by premise-orientation in law and conclusion-orientation in science. As an economist who testified on behalf of the EEOC, Dr. Ashenfelter represented the most self-consciously "scientific" of the social sciences. As we will observe, he "spoke" justification in a conclusion-oriented way.

Dr. Ashenfelter's cross examination is useful to us for two reasons. It illustrates concretely the chasm separating the scientific and legal cultures and it illustrates the theme that scientist-lawyer communication also turns on cultural and institutional forces, rather than just the forces of logic alone.

The nuts and bolts of Dr. Ashenfelter's cross examination covered two areas: EEOC Exhibit 2A (consisting of charts and tables which Dr. Ashenfelter had helped the EEOC to prepare and which the EEOC was submitting as "statistical evidence", proving discriminatory employment practices at AT&T) and his written testimony, "Telephone Rates in the Absence of Discrimination" (an economic analysis providing estimates of the percentage by which AT&T's costs, and hence telephone rates, would have fallen if AT&T had employed women to the full extent that their productivity justified).

3.1 Exchange No. 1: The Differing Scientific and Legal Meanings to Scientific Assumptions

All scientific theories rest on assumptions. Assumptions are essential to science for their simplifying function. A scientific theory, as a depiction, is only a depiction. It can never fully capture or replicate the original. No map is a territory.

Scientists and lawyers attach different meanings to scientific assumptions.

To the scientist, the justification of a scientific theory lies in its conclusions, which are its hypotheses or predictions. If what the theory hypothesizes or predicts is corroborated by experience, the theory stands and the inaccuracy that its assumptions build into it is of no consequence.¹

To the lawyer, the justification of any testimony lies in its premises. Scientific assumptions, being inherently inaccurate premises, present irresistible targets to the lawyer.

The following exchange allows us to observe the different scientist/lawyer meanings for three assumptions in Dr. Ashenfelter's testimony. They are the assumptions that men and women are equally productive, that AT&T's production process is linearly homogenous, and that men and women share the same occupational categories.

Assumption about Men and Women Being Equally Productive

PRESIDING EXAMINER: Mr. Levy, may I interrupt here. I am intrigued by a sentence in the second sentence [sic] on page three [in Dr. Ashenfelter's written testimony, "Telephone Rates in the Absence of

Discrimination"] about men and women being equally productive.

MR. LEVY [lawyer for AT&T]: You have stolen two hours of cross examination, Mr. Examiner.

PRESIDING EXAMINER: This sounds like a revelation. Kidding aside, did you jump over that?

MR. LEVY: I am still hung up on the first sentence, Mr. Examiner.

PRESIDING EXAMINER: I am sorry. Go ahead. I thought you had passed over that.

MR. LEVY: Not a chance.

[The passage being discussed is:

An examination of the Bell Telephone Company's employment data reveals that there are a number of occupations in which women comprise a very small fraction of the work force. One explanation for this phenomenon is that, although men and women are equally productive, the latter suffer from a desire on the part of management not to employ women on the same terms as men, i.e., discrimination. Since male and female workers are not being employed entirely in relation to their true productivities, a loss of overall efficiency results ... 2]

[Later on in the cross examination:]

MR. LEVY: Respecting the parenthetical clause, "although men and women are equally productive," is that stated as a second or supplemental hypothesis, or is that an assertion of fact?

DR. ASHENFELTER: No, it is meant to be a part of the hypothesis.

...

MR. LEVY: Have you made any in-depth analysis of the relative productivity of men as a group and women as a group in Bell occupations?

DR. ASHENFELTER: No, I have not.

MR. LEVY: Are you familiar with data relating to weightlifting or pole climbing abilities of women as a group versus men as a group?

DR. ASHENFELTER: I am not really familiar with that, no.

MR. LEVY: Are you familiar with work by motivational psychologists relating to whether women, when confronted with male-role opportunities, seek to avoid success?

[Dr. Ashenfelter responds negatively.]

MR. LEVY: ... Have you considered, Dr. Ashenfelter, the possibility of significant differences between men as a group and women as a group on such things as verbal ability, perception of detail, or numerical or mechanical computation in positing this corollary to your hypothetical of equal productivity.

DR. ASHENFELTER: If you mean have I tried to determine whether or not it is true that men and women are equally productive in the jobs involved, the answer is, I have not.

[pages 1560-1562
of the transcripts]

MR. LEVY: ... I previously asked you about possible physiological or psychological differences, including job preferences between men as a group and women as a group. Looking at the numerical constructs and equations that begin in the middle of page 4 of your testimony, is it correct that those considerations are not taken into account in your numerical constructs and that those constructs are directed solely to the theoretical assumptions that all differences in male-female jobs is due to employer job discrimination?

[The last of the three equations is crucial to Dr. Ashenfelter's testimony. It is his means of providing estimates of the percentage by which telephone rates would decline, if discrimination were to disappear. Dr. Ashenfelter's response has to do with traditional assumptions made about economic behavior.]

PRESIDING EXAMINER: This goes again back to your hypothesis that the productivity is equal?

DR. ASHENFELTER: Yes, that is right. It assumes the productivity is the same.

MR. LEVY: I take it while you did not directly answer, it is implicit in your answer that that was the assumption or hypothesis³ you proceeded under, and you did not reflect in your numbers anything for societally instilled job preferences or biological differences between men as a group and women as a group, or physiological differences between men

as a group and women as a group?

DR. ASHENFELTER: It is correct ...

[1568-1570]

Assumption about the Production Function

A production function is a formula. Plugging in numerical values for both the prices and quantities of inputs to AT&T's actual production process, Dr. Ashenfelter was able to obtain numbers for the amount of goods and services that AT&T produced. Different formulas which economists use for the production function reflect different types of actual production processes. The formula for a production function is therefore a scientist's mental construct, put in mathematical form, of a firm's actual production process. Depending on the actual production process at hand, one formula for the production function may be a more accurate depiction than another.

MR. LEVY: Further on page 4, Doctor, you asked us to suppose that the process whereby Bell produces its services can be approximated by using a Cobb-Douglas production function ...

[1571]

...

MR. LEVY: Is a Cobb-Douglas production function a linear, homogeneous function?

DR. ASHENFELTER: Yes, that is correct.

DR. LEVY: If it were not first-order homogeneous, would Equation No. 2 be derivable in your formulations?

[Equation No. 2 is a formula for calculating the price of AT&T's goods and services. Equation No. 1 is the Cobb-Douglas production function.]

DR. ASHENFELTER: I forgot to add one important point ... I would like to add parenthetically, for people who don't know what [linear homogeneity] means--

PRESIDING EXAMINER: Most of us.

MR. COPUS [a lawyer for the EEOC]: Perhaps your testimony could be in parenthesis, and this will be the main part.

[Dr. Ashenfelter explains linear homogeneity.]

MR. LEVY: Getting back to my question--and I am not sure it has been answered--I asked, if the production function you used was not first-order homogeneous, Equation No. 2 could be quantified, or would a numerical equation be derivable?

DR. ASHENFELTER: No, it could not be.

PRESIDING EXAMINER: Is this a convenient stopping point, Mr. Levy?

MR. LEVY: All right, Mr. Examiner.

[1572-1574]

MR. LEVY: Did you make any investigation to determine that [the Cobb-Douglas] production function was realistic as applied to Bell?

DR. ASHENFELTER: No ...

MR. LEVY: If the Bell System ... did not fall within the model of a linear homogeneous production function, would that in fact be an appropriate formulation to use?

[Dr. Ashenfelter responds affirmatively, but provides qualifications.]

MR. LEVY: Doctor, why didn't you choose to use other professionally recognized functions such as VES, variable elasticity of substitution?

[Dr. Ashenfelter gives his reasons.]

MR. LEVY: Had you used VES, could you have derived equations comparable to [your] equations at the steps where you have developed your equations 2 and 3?

DR. ASHENFELTER: Equation 3, which is the one we used to directly calculate the effect of a reduction or elimination of discrimination [on] telephone rates would be the same with the VES production function if it were homogeneous of degree 1.

[Dr. Ashenfelter elaborates his answer.]

MR. LEVY: Then your answer to my question is you did not investigate this?

DR. ASHENFELTER: That is correct.

...

MR. LEVY: Is that equation [No. 2] based on equation 1?

DR. ASHENFELTER: Yes.

...

MR. LEVY: Is your third equation on page 6 [Equation No. 3] derived from equations 1 and 2?

DR. ASHENFELTER: ... yes, it can be derived from equations 1 and 2, but it can also be derived from less restrictive assumptions.

MR. LEVY: In this instance, from what was it derived?

DR. ASHENFELTER: That is not a very meaningful question because I mean it exists and [the written testimony] states it comes from equations 1 and 2, but as I have said, it can also come from a less specific formulation of the problem.

[Mr. Levy changes the topic.]

[1576-1580]

Assumption about Occupational Categories:

MR. LEVY: Getting back to the first full sentence on page 3, and at later points in your testimony, you use the word "occupations". By what do you mean as occupations as you use that in this piece of testimony?

DR. ASHENFELTER: By "occupations" ... you could also take that to mean occupation in the aggregate sense of like the craft occupations.

MR. LEVY: Do you mean like the EEO-1 category?

DR. ASHENFELTER: Yes ... it would be correct to say "occupation" where occupation meant EEO-1 report-form occupation.

MR. LEVY: In "Minority Employment Patterns, 1966" [one of Dr. Ashenfelter's publications], you expressed the conclusion, " ... the economic meaning of 'occupations' such as clerical worker and craftsman are very different for the two sexes at the level of aggregation available from the EEO-1 data."

MR. COPUS [a lawyer for the EEOC]: What page is that, Mr. Levy?

MR. LEVY: Page 1.

[Mr. Copus is interested because Dr. Ashenfelter's theory assumes occupational categories to be the same for the two sexes.]

[1557]

Analysis of the Exchange:

Mr. Levy's line of questioning raises a serious issue for our consideration. May a theory be deemed accurate if it is derived from inaccurate assumptions?

All theories involve simplifying assumptions that, strictly speaking, are inaccurate for what they simplify away. In a sense, therefore, all theories can be called inaccurate because they rest on assumptions that are themselves inaccurate.

Such an extreme position, fundamental to Mr. Levy's line of questioning, would be absurd to a scientist. Even the tried-and-true theories of physics would have to be written off, if such a position were adhered to. To illustrate this point, I suggest we hypothetically assess Boyle's law, a well known theory in physics, from the vantage point of this extreme position.

(Boyle's law concerns the behavior of gases. It assumes that the molecules that make up a gas are just like miniature rubber balls, possessing perfect elasticity. This means that a ball that falls to the ground is so elastic that [assuming no friction] it may bounce back to the height from which it is dropped, and that it may do so, again and again. The assumption of perfect elasticity applies to all the collisions of the molecules--against one another and against the walls of the gas's container. From this and other assumptions, a scientist may readily derive $PV=nRT$, which is a form of Boyle's law.⁴ It states that the pressure, P , of a gas times the volume, V , of its container is equal to the quantity, n , of molecules in the gas, times a constant, R , times the temperature, T . The relationship, $PV=nRT$, is a famous one, known to all first year physics and chemistry students.)

How well would Boyle's law stand up to legal scrutiny?

MR. LEVY: Doctor, you use the word, "rubber balls" in page three of your testimony. By what do you mean as rubber balls as you use that in this piece of testimony? ... Have you made any in-depth analysis about how the actual shape of molecules is different from that of rubber balls? ... Did you investigate the difference that the actual shape of the molecules would have made to your computations? ... On page two, you state that the gas's container is made out of steel. Doctor, why didn't you choose to use other scientifically recognized substances, such as copper or zinc? ... Did you investigate what difference the use of copper or zinc or any other substance would have made to your computations? ... Then your answer to my question is you did not investigate this? ... In one of your publications, "Elasticities Manual, 1966", you state the finding that the elasticity of rubber is less than perfect--

MR. COPUS: What page is that, Mr. Levy?

MR. LEVY: Page one.

PRESIDING EXAMINER: Mr. Levy, may I interrupt here. I am intrigued by an assumption in the Doctor's testimony on page three, about the volume of the rubber balls being zero.

MR. LEVY: You have stolen two hours of cross examination, Mr. Examiner.

PRESIDING EXAMINER: This sounds like a revelation. Kidding aside, did you jump over that?

MR. LEVY: I am still hung up on the first sentence.

PRESIDING EXAMINER: I am sorry. Go ahead. I thought you had passed over that.

MR. LEVY: Not a chance.

[There is some cross-examination in which the scientist explains his assumptions.]

MR. LEVY: Getting back to my question--and I am not sure it has been answered --I asked, did you derive " $PV=nRT$ " from the intermediate equations, which you had in turn derived from the assumptions?

DR. ASHENFELTER: That is not a very meaningful question because--

MR. LEVY: Let me ask you a very simple question, Doctor. Did you make any in-depth investigation to determine that your assumptions were realistic as applied to the actual molecules in your experiment?

DR. ASHENFELTER: No, I did not.

PRESIDING EXAMINER: Is this a convenient stopping point, Mr. Levy?

MR. LEVY: All right, Mr. Examiner.

The absurdity that the hypothetical exchange magnifies has its basis in the different meanings that the scientist and the lawyer attach to the assumptions.

To the scientist, assumptions are essential for their simplifying

function. No map is a territory; simplifications must be made. Assumptions screen out details that are marginal to the depiction of the original. What is marginal and what is not are matters that are up to the scientist's purpose, and skill, in inquiry.

To the scientist, Boyle's law works, even if molecules really are not volumeless rubber balls possessing perfect elasticity. Boyle's law is predictive. Its hypotheses and predictions are corroborated by experience. The scientist finds this situation to be satisfactory because, according to his value system, justification is conclusion-oriented.

The inaccuracy inherent in a simplifying assumption is of consequence to the scientist only when one of the theory's predictions is falsified by experience. The scientist may then proceed to improve the theory by revising one or another of the simplifying assumptions. A more realistic assumption is more likely to formulate a more accurate theory. However, the justification of the improved theory, which is separate from its formulation, would still lie in the empirical testing of the hypotheses or predictions drawn from it. Thus, in the scientist's way of thinking, the accuracy of an assumption, like the accuracy of the theory of which it is a part, is known through its conclusions.

(We can be sure that Dr. Ashenfelter would agree with this assessment. In re-writing his testimony for publication after the conclusion of the AT&T case, Dr. Ashenfelter discussed, in terms of hypothesis testing, the accuracy of the assumption of a linearly homogeneous production function.⁵⁾

To the lawyer, scientific assumptions appear different. The actual Levy-Ashenfelter exchange makes clear that the meaning Mr. Levy attached to

the assumptions were different from Dr. Ashenfelter's. How did this come to pass? That is, how did Mr. Levy come to see the assumptions as targets worth aiming at?

I believe that the following dynamic took place in the Levy-Ashenfelter exchange, as indicated on the diagram on the following page.

To Mr. Levy, Dr. Ashenfelter's computation for the cost of discrimination at AT&T (in the diagram, "The Conclusion") was not justifiable because it was derived from inaccurate premises. In applying the law to the facts to reach a conclusion, the conclusion is justifiable only if its premises--the law and the facts--are in order. In Mr. Levy's eyes, they were not in order.

The law is appropriate for applying to the facts only if "the facts fit the rule".⁶ The facts that Mr. Levy presented about (1) male-female productivity, (2) production functions, and (3) occupations did not fit the theory's assumptions about (1) men and women being equally productive, (2) the production function being linearly homogeneous, and (3) occupational categories being the same for the two sexes. If the facts do not fit the rule, the rule does not apply. To Mr. Levy, Dr. Ashenfelter's theory did not apply.

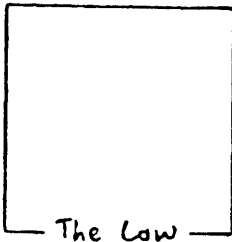
Mr. Levy's map of the world was one in which the law is applied to the facts to reach the conclusion. In projecting his map onto the territory of the scientific testimony, Mr. Levy saw the justification of the scientific testimony as resting on its premises.

Mr. Levy's meanings for the artifacts of major premise, minor premise, and conclusion were rooted in his own legal culture; respectively, they were

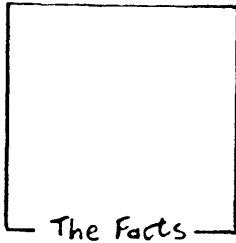
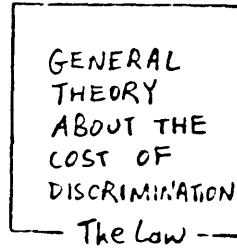
MR.
LEVY'S
MAP

THE
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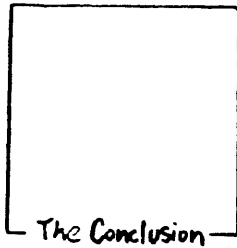
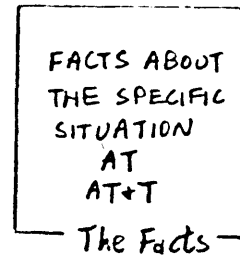
PROJECTING THE MAP
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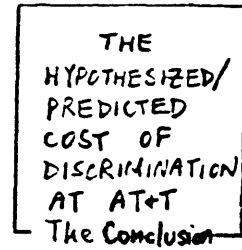
GENERAL
THEORY
ABOUT THE
COST OF
DISCRIMINATION



FACTS ABOUT
THE SPECIFIC
SITUATION
AT
AT&T



THE
HYPOTHESIZED/
PREDICTED
COST OF
DISCRIMINATION
AT AT&T



the law, the facts, and the conclusion. Dr. Ashenfelter's meanings for the same artifacts were the theory, the initial conditions, and the hypothesis. Upon projecting the meanings from his own culture onto Dr. Ashenfelter's presentation of the artifacts, Mr. Levy proceeded to see Dr. Ashenfelter's scientific artifacts as if they were legal artifacts.

For example, because Dr. Ashenfelter computed the cost of AT&T's discrimination from equation no. 3, it was important for Mr. Levy to attack the pyramid of premises on which he believed the equation to be resting. Was equation no. 3 derived from equation no. 2? Was equation no. 2 derived from equation no. 1? What difference would it have made in the computed cost of AT&T's discrimination if the inaccuracies of the assumptions contained in equations 1 and 2--about a linearly homogeneous production process and equal male-female productivity--had been corrected? It was a premise-oriented map that directed Mr. Levy's attention to the premises.

Ethnocentrism

While at first it may appear curious, Mr. Levy's projection of his map should be unsurprising to us. It is more commonly called "ethnocentrism". People project the meanings and values that they normally attach to the objects of their own culture onto the objects, similar in appearance, of a different culture.

Though the popular connotation attached to "ethnocentrism" frames it as a deliberate and condescending act, the sense in which I describe "ethnocentrism" depicts it as arising subconsciously and innocently, where its

consequences are randomly innocuous or harmful. It is in this manner that Mr. Levy saw Dr. Ashenfelter's scientific testimony not for what it was (i.e., conclusion-oriented), but for what it was not (i.e., premise-oriented). Rather than drawing up a map after surveying the territory, Mr. Levy projected a pre-existing premise-oriented map onto the conclusion-oriented territory.

We, as observers, may not judge Mr. Levy as acting irrationally lest we be guilty of ethnocentrism ourselves. For a lawyer, indeed for any person, it is a natural act to see the world in familiar, pre-established terms or "typifications", which are the mental sedimentations or kernels that remain after the person has experienced something "n" times over the course of his biography.⁷ In plainer language, it is natural to use a map as a guide to an unfamiliar territory.

Dr. Ashenfelter's testimony, replete with economic jargon, economic logic, and mathematics, was unfamiliar territory to Mr. Levy.

Before proceeding to the next exchange, we take note that Mr. Levy did not stand alone in projecting a legal, premise-oriented map. We observe the same projection by the other lawyers who were present: the Presiding Examiner ("I am intrigued by a sentence ... about men and women being equally productive") and even the lawyer who was sponsoring Dr. Ashenfelter, namely, Mr. Copus ("What page is that, Mr. Levy?").

3.2 Cultural and Institutional Forces

Now is an opportune time to define the "cultural and institutional forces" that we have been mentioning since the introduction to this study. "Cultural and institutional forces" have to do with the notion of social structure.

A social structure is made up of the rules, formal and informal, that an individual finds given upon entering a situation. Although man-made, a social structure has a life of its own, outlasting the lives of the individuals who momentarily pass through it.

A social structure is also made up of the "slots" or the "roles" in addition to the given or ready-made rules. The individuals who are momentarily passing through the social structure are occupants of the slots. Slots in the family structure include father, mother, sibling, and cousin. Slots in a corporate structure include chief executive officer, vice president, and middle manager. Slots in a courtroom structure include judge (presiding examiner), plaintiff, and expert witness.

The individual who is momentarily occupying a slot is subject to the rules that come pre-given with the slot. A person who is a father, middle manager, or judge behaves according to the pre-specified rules, or opportunities and constraints, that he encounters in his role as father, middle manager, or judge.

"Social structure", in the manner defined, exists from the viewpoint of the sociological-anthropological observer. To the people who actually live in the slots and encounter the pre-given rules--the people who are so

busy being fathers, middle managers, and judges that they have no time for sociological-anthropological reflection--the social structure presents a very different meaning.

The meaning of a social structure to its inhabitants--the impression it makes on the hearts and minds of the people who live in it--is what I call "culture". Culture is just the flip side notion to social structure.⁸

A long-time inhabitant of a social structure may internalize the rules he encounters. I define "cultural forces" as the influence of rules that have been internalized. They operate from within the individual. Mr. Levy's subconscious and automatic projection of his legal map onto Dr. Ashenfelter's scientific testimony is an illustration of cultural forces in action. No doubt, Mr. Levy internalized the premise-oriented legal map over his many years of being a lawyer, starting in law school. As observers, we have taken notice of Mr. Levy's projection as an artifact of his culture, or social structure.

Of course, for whatever reason, not all rules are internalized. Individuals are also subject to the influence of imposed rules. Because they are imposed from the outside--from the institutional environment--I refer to these as "institutional forces". They operate from without the individual.

Together, the cultural forces and the institutional forces make up the structural forces.

The first exchange provided examples of cultural forces. It provided us the opportunity to see that the lawyer and the scientist carried different cultural notions of justification into the cross examination. The

communication, or lack thereof, was due to the operation of the respective cultural forces.

The next exchange will provide examples of institutional forces.

3.3 Exchange No. 2: The Differing Scientific and Legal Meanings to Sampling, Standard Error, and Statistical Significance

In this exchange, Mr. Levy was cross examining Dr. Ashenfelter on EEOC Exhibit 2A. Their discussion revolved around the concepts of sampling, standard error, and statistical significance.

These concepts are quintessentially scientific, as will be later explained. Of special interest to us is that Mr. Levy did not understand these concepts for their scientific meaning.

(My stance is not that a lawyer may never be able to master inferential statistics in a way that would be meaningful to a scientist. I am just noticing that, in this instance, there were some artifacts whose meanings for Dr. Ashenfelter and for Mr. Levy were different. My stance is that we should take advantage of the apparent difference in meanings. It provides us the opportunity to identify the institutional forces that were brought to bear on Mr. Levy in the meaning he did attach to sampling, standard error, and statistical significance.)

The Meaning of Sampling, Standard Error and Statistical Significance to the Scientist

Sampling is an aid to the scientist in the testing of theories. More often than not, there are limitations in time and money that prevent the scientist from looking at the actual situation of interest in its entirety. The scientist looks at just a representative portion of it instead, which he calls a "random sample". The result is therefore an educated guess about the actual situation. For example, if a random sample, taken from

an actual population, is 20 percent minority, it would be a good guess that the actual population itself is about 20 percent minority. The larger the sample, the more accurate the guess.

"Statistical significance" describes the accuracy of the guess. For example, the scientist may put up a theory that "company A and company B both have 20 percent minority employees". A logical conclusion, or hypothesis, following from the theory would be that "there is no difference in the percentage of minorities at companies A and B". The scientist could then test the theory by comparing the hypothesis of "no difference" against observed fact.

Suppose the scientist makes his observation by looking only at a representative portion of each of the two employee populations. Suppose the samples produce the guesses that company A is 19.5 percent minority and company B is 20.5 percent minority.

The scientist must, of course, account for the rough nature of the guesses. The difference in the guesses may very well be the result of random error, which is inevitable in sampling and which small samples magnify. The guesses of 19.5 percent and 20.5 percent, while different, might be so approximate that the scientist might not feel himself to be on solid-enough ground to assert that there truly exists a difference between the true percentages (whatever they may be).

On the other hand, if the sample sizes are sufficiently large, the scientist might feel that the guesses of 19.5 percent and 20.5 percent are sufficiently accurate to indicate that there truly exists a difference (a "statistically significant" difference) between the true percentages (what-

ever they may be). Here, the statistical significance to the difference would allow the scientist to take the conclusive action of rejecting the hypothesis of "no difference". The theory would be rejected, or falsified, by association.

In the case where the sample sizes are too small, the lack of statistical significance would render the test inconclusive.

A good portion of the field of inferential statistics is about how to manage the approximate nature of the guesses that sampling introduces to hypothesis testing. "Sampling error" and "standard error" are quantitative measures of the error inevitable in sampling. Because sampling, standard error, and statistical significance are tools that the scientist uses in hypothesis testing, their meanings are rooted in the context of conclusion-oriented justification. In this sense, they are quintessentially scientific artifacts.

The following excerpts will reveal Dr. Ashenfelter's familiarity with these artifacts and Mr. Levy's non-scientific understanding of them.

MR. LEVY: Is it correct in the case of Chesepeake & Potomac of Virginia for the year 1966 the table indicates that it had a 2.05 percent minority clerical employment versus a 3.00 area average?

[The area average is the percentage of clerical workers who were minority for all employers in the C&P service area.]

[Dr. Ashenfelter responds affirmatively to Mr. Levy's question.]

MR. LEVY: Do you know the sampling error or the standard error of that three

percent census figure for minority percentage?

[Dr. Ashenfelter explains that the figure is taken from "EEOC establishment reports".]

MR. LEVY: Do you know the sampling error or the standard error of that three percent figure derived from those sample statistics?

DR. ASHENFELTER: I guess I should say, strictly speaking, that is not a sample. Strictly speaking, if all employers filed their EEO-1 reports [the EEOC establishment reports] on time as they are supposed to do, then if we carefully define ... the universe [or population] in terms of the employers who are required to report, then strictly speaking this should be the complete universe [or population]. There should be no sampling problem at all ...

[Perceiving at this point that Mr. Levy is misusing the notion of sampling error, Dr. Ashenfelter proceeds to explain it.]

... Your initial question was about the census of population where what happens is typically they will take a five percent sample. If, for example, you ask me in West Virginia what percentage of the population--what percentage of employed people are clerical workers ... Since the occupational information is not a complete enumeration, the fraction of all employees in West Virginia, say, who are clerical workers would exist. There is such a number, but since the census only asks about five percent of the people what their occupation is,

the percentage that came from the census would be only an estimate of what the true proportion of employed people who are clerical workers was, let us say, in West Virginia. So, we have to ask the question, how good an estimate is this estimate based on only a fraction of the total people who are in the area. That is for the census of population based on a conventional random sample. The EEO-1 are not random samples. Strictly speaking, they are not a sample ... if we define [the] universe [or population] to include only those employers who are subject to the reporting requirements, and we assume everybody [including AT&T] fulfills their legal obligation, then strictly speaking, we have all employment [not just a sample] in the area. Is that good enough or should I go into it more? [Emphasis added.]

MR. LEVY: Let me ask you a very simple question, Doctor. Is it your testimony that all employers in that area or all employees of a clerical nature in that area are included in EEO-1 reports that form a basis for this?

DR. ASHENFELTER: No ... First, I said if the legal requirements are satisfied by all the employers who are subject to them, and secondly I said if we carefully state which employers ... are legally required to respond ... then this would not be a sample in the way in which the census data are sampled.

MR. LEVY: Given those two situations, isn't this in fact based upon what can be viewed as a sample of all employers of clerical persons in the area?

DR. ASHENFELTER: Since there is some non-response [by employers], you mean?

MR. LEVY: And since it does not include employers with less than 25 employees, or employers of a governmental nature, federal, state, or local.

DR. ASHENFELTER: ... It would depend on whether or not I would want to consider [the EEO-1 reports to be] a sample of total employment. I would want to know, really, first how you want to define the universe of employees. Is that clear?

MR. LEVY: That is clear, Doctor. If you were to be viewing this from the perspective of it being a sample of all employers of all clerical persons in the area, then under that conceptualization there would be some measure of standard error associated with it as an estimate of the total population, would it not?

DR. ASHENFELTER: Strictly speaking, in the conventional way in which we talk about sampling and sampling errors, no. The reason for that is because ...

[Dr. Ashenfelter then explains that there may be some error, not due to the built-in incompleteness of sampling, but due to some non-response by employers, all of whose answers are solicited for a complete picture in the EEO-1 reports. An unconventional "standard error", defined to refer not to sampling but to such non-response, could conceivably be calculated.]

... In other words, defining what a standard error means in that context would require some significant additional kinds of comparison,

which incidentally I have done.

MR. LEVY: Do I understand what you are saying is there is some measure of sampling error, but you cannot really estimate it; is that it?

DR. ASHENFELTER: No, that is not quite right ... There are ways of [getting an idea of the precision of EE0-1 data], but they are less formal than would be implied by saying that we could calculate a standard error or measure of sampling variation for a given employment statistic.

...

MR. LEVY: Getting back to the numbers themselves, Doctor, the difference between 2.05 percent as shown on the table [for Bell's percentage of minority clerical employment] and 3 percent as shown on the table [for all employers' percentage of minority clerical employment in Bell's service area] is, if my mathematics is correct, .95 of one percent, is that right?

DR. ASHENFELTER: Yes.

MR. LEVY: Is that a statistically significant difference in the circumstance of these numbers as they have been discussed?

DR. ASHENFELTER [perceiving that Mr. Levy is misusing the notion of statistical significance]: ... Let me say first of all, however, before we say statistically significant, I would like to make clear precisely what that means in this context ...

[Dr. Ashenfelter explains, again, that the figures of 2.05 and 3.00 were not the result of sampling, but of complete reporting; statistical significance and sampling error would be applicable concepts only in the context of sampling. Any error in the figures would be due not to sampling, but to errors in the complete reporting itself, such as in AT&T's reporting of its 2.05 figure to the EEOC. Dr. Ashenfelter explains he could compute this type of error, although he has not done so for this particular case.]

MR. LEVY: The graphic display in Chart 202 purports to convey to the eye of the beholder that C&P of Virginia in 1966 lagged behind the area by a magnitude of 31 on a scale of 100 [that is, $(3.00-2.05)/3.00=31/100$]. Is such a magnification of a small difference, one you say without precise calculations you could not estimate was statistically significant, normally [made] in descriptive statistics?

DR. ASHENFELTER: First of all, I think I would like to say that one of the presumptions in your question was not quite what I said ...

[1506-1516]

Although Mr. Levy did not display a scientific understanding of sampling, standard error, and statistical significance, we must realize that these words signified meaning of some sort to him. As long as he spoke these words and formulated questions with them, and as long as he had a purpose in carrying out the cross examination, there was meaning in these words to him. What was the meaning?

To answer this, we must first discern Mr. Levy's--indeed AT&T's--purpose in the courtroom. The purpose will help us tell whether Mr. Levy was carrying out a war dance, a barter trade, a reception of a friendly ambassador, an exercise in premise-oriented justification, or something else of this sort.

The point is that it was only from Dr. Ashenfelter's viewpoint that Mr. Levy "misunderstood" certain scientific terms. While we must account for Dr. Ashenfelter's viewpoint, it would be quite another matter for us to embrace his viewpoint as our own; we could do so only at the risk of "going native"--that is, over-identifying with one or more of the people we are studying. Instead, we should strive to understand or interpret Mr. Levy in his own context. In this regard, the importance of what I call the "institutional forces" becomes apparent. Imagine how the Levy-Ashenfelter interaction and the outcome to it would have been different if the two men had been interacting not in a courtroom, but before a seminar of graduate students in economics. The tables would have been turned on Mr. Levy and the burden would have been on him, not Dr. Ashenfelter, to legitimize himself.

In the courtroom, Mr. Levy's approach was socially sanctioned and socially approved, just as Dr. Ashenfelter's approach in the seminar would have been. To interpret properly Mr. Levy's meaning for statistics and his purpose in the courtroom, we must therefore account for the institutional forces that he was subject to.

The Meaning of Sampling, Standard Error and Statistical Significance to the Lawyer:

Individuals who occupy the role of lawyer in a courtroom setting are not free to construe whatever arguments they believe to be convincing. They must devise their arguments within the opportunities and constraints that "the law" hands down to them: stare decisis et non quieta movere. To make sense to the other natives in the courtroom, not to mention persuading them, a lawyer must employ logic and findings that make sense to them. For the lawyers in the AT&T case, there existed legislative law and case law-- a legacy of mental constructs created and sanctioned in the resolutions to past controversies--that spelled out the manner for justifying or discrediting statistical evidence. Mr. Levy was laboring under the weight of past decisions in his attempt to make sense out of the statistical evidence, EEOC Exhibit 2A. In this way, institutional forces were brought to bear on the meaning Mr. Levy attached to sampling, standard error, and statistical significance.

An equivalent way of saying this is that Mr. Levy's knowledge about statistics was socially derived; it was drawn from specific findings in precedents and from general legal principles, such as a prima facie case or a per se violation of the law. However, AT&T and the EEOC had interpreted the law differently in this regard.

The position espoused by the EEOC was the "principle of a statistical prima facie case of discrimination".⁹ To members of the legal culture, a prima facie case is one "that will prevail until contradicted by other evidence".¹⁰ The EEOC's interpretation of the law was that "statistical proof of substantial under-representation of minorities or females in certain jobs establishes a prima facie case of employment discrimination. Once such

facie case has been made, the burden then shifts to the defendant to rebut the prima facie case ..."¹¹

What is the logic behind shifting the burden to the defendant to disprove the statistics? The EEOC explained, "even though one inevitable consequence of systematic discrimination is statistical underrepresentation of females and minorities", the converse of this is not generally true: "exclusion is not the inevitable cause of underrepresentation"; for example, there might be legally accepted job-related qualifications, rather than systematic discrimination, that give rise to the exclusion or statistical underrepresentation. "That is why," the EEOC stated, "statistics constitute only a prima facie case."¹² The evidence is tentatively probative, becoming decisive only if the defendant fails to come forward with its own "exculpatory evidence" or "controverting evidence".¹³

Furthermore, according to the EEOC, not just any exculpatory, controverting evidence would do:

... the Courts have accepted only two defenses as satisfactory explanations for statistical data showing a substantial underrepresentation of females and minorities ...

The first defense requires the Bell System to show that job-related qualifications explain the statistical disparities shown by Petitioners [the EEOC] ...

The second acceptable defense requires the Bell System to show that it has made every reasonable effort to recruit qualified females and minorities for the jobs in which those groups are substantially underrepresented.¹⁴

In referring to itself as Petitioners, the EEOC justified its interpretation by appealing to precedent:

In its Memorandum in Support of Motion to Strike, Petitioners cited Fogg v. New England Tel., ___ F. Supp. ____, 5 FEP Cases 7 (D.N.H. 1972) in support of the use of statistics to establish a prima facie case of sex discrimination. In Fogg, the plaintiff submitted exactly the same type of statistical data presented by Petitioners in this case, and the Court found unlawful discrimination based on the statistical showing.¹⁵

To summarize, the EEOC's position was that "statistics showing substantial underrepresentation of minorities and females constitute a prima facie (but not conclusive) case of unlawful discrimination and shift the burden to the defendant to explain the statistical disparities,"¹⁶ where the additional exculpatory, controverting evidence must take either of the two pre-specified forms.

The position espoused by AT&T was in opposition to the EEOC's. AT&T interpreted the law to mean "that a statistical showing of underrepresentation of females or minorities constitutes a per se violation of the law."¹⁷

Whereas evidence in a prima facie case is put up as tentative and rebuttable, and is considered decisive only upon the defendant's failure to rebut it with additional exculpatory, controverting evidence, evidence in a per se violation is put up as "conclusive", which means it is not rebuttable by other evidence,¹⁸ but must be justified or discredited in and of itself.¹⁹

In light of this, it is easy for us to interpret, from Mr. Levy's viewpoint, the meaning that EEOC Exhibit 2A signified. With the purpose of discrediting the EEOC's "conclusive" statistical evidence, Mr. Levy was attempting to have one of the EEOC's own witnesses testify about the "error" (sampling error, standard error, whatever error) in EEOC Exhibit 2A. In Mr. Levy's eyes, Dr. Ashenfelter's technical discourses about the inappli-

cability of "standard error" constituted a smokescreen, blocking exposure of the error that, to Mr. Levy, surely did exist. Indeed, after persistent questioning, Dr. Ashenfelter did admit that there was error and imprecision, whether conventionally or unconventionally defined. (Perhaps, with continued questioning ...)

In the same spirit, Mr. Levy attempted to establish that EEOC Exhibit 2A's alleged difference, between AT&T's 2.05 percent minority employment of C&P of Virginia vs. the 3.00 percent minority employment at other firms in the same area, was a picky one. To Mr. Levy, the ".95 of one percent difference" between the two statistics lacked "significance". No matter that "statistical significance" held a special meaning to Dr. Ashenfelter, or that Dr. Ashenfelter could spew discourses on technical issues. The pertinent issue to Mr. Levy was to ferret out the "error" and lack of "significance" in the EEOC's statistical evidence.

The source of my observations and quotations about AT&T's position and the EEOC's position consists of memoranda that AT&T, the EEOC and the FCC's Common Carrier Bureau had written. These memoranda are artifacts that conveniently reveal to us the meanings that the lawyers attached to the statistics in EEOC Exhibit 2A. The artifacts contain such subheadings as "The Principle That a Statistical Proof of Underrepresentation Constitutes a Prima Facie Case of Discrimination is Unchallenged"²⁰ and "Respondents Have Mischaracterized the Bureau's Statement of the Law on the Use of Statistical Evidence in Cases of Employment Discrimination."²¹

What is revealed to us is that, regardless of what scientists mean by statistics, the legal meaning to statistics must come from "the law",

that is, the body of pre-existing legal precepts, which in this situation were decisions to prior court cases that had established rules regarding statistics in its role as evidence. What would happen if a lawyer propounded a meaning not derived from a pre-existing legal precept? The EEOC and the FCC's Common Carrier Bureau believed that AT&T's per se positions to be based on no legal precept whatsoever; in summarizing its position, the EEOC stated:

The EEOC further demonstrated that the testimony of [five] Bell System witnesses ... does not relate to the two accepted defenses to a prima facie case [that is, the testimony does not relate to rules established, sanctioned, and accepted by precedents] ... Thus, to avoid burdening the Record with testimony which is not legally cognizable [emphasis added], the EEOC moved to strike this testimony as immaterial.²²

Now, if the EEOC's lawyers saw no "legally cognizable" meaning in AT&T's position, because of a supposed lack of basis in legal precepts, then what "legally cognizable" meaning could we in general expect a lawyer to see in a scientist's explanation of statistics, which is based not at all on pre-established legal precepts, but on ad hoc logical conclusions?

In trying to make himself understood to Mr. Levy, Dr. Ashenfelter was therefore up against not only the premise-oriented legal map in Mr. Levy's head, but also the institutional forces that sanctioned and approved Mr. Levy's line of questioning in the courtroom setting. Dr. Ashenfelter's ad hoc conclusion-oriented explanations ("So, we have to ask the question, how good an estimate is this estimate based on only a fraction of the total people who are in the area?") simply did not command the same legitimacy as did established precepts, principles, and other legal premises.

The force wielded by the courtroom setting may be seen in the structure of the administrative hearing, which is itself a monument to the role of legal precepts, principles and precedents as the authoritative source of meaning in the legal language. According to Dr. Phyllis Wallace in Equal Employment Opportunity and the AT&T Case:

Government hearings are often rather complex and protracted legal encounters, but in most cases they follow the same general procedural format. After a Hearing Examiner is assigned to a case, the parties file pretrial briefs giving their preliminary interpretations of the law. Then, each part moves to discover facts from the other party to support its contentions. Third, each side presents witnesses who are cross-examined. Fourth, the parties move that exhibits, or documents of various kinds, be submitted into the record; there are challenges and some are admitted, others excluded. Fifth, each party submits a proposed finding of facts based upon the record and conclusions of law. Sixth, the hearing examiner writes a decision passing judgment on the facts, the law, and the law as applied to the facts. The recommended decision of the hearing examiner may be appealed to an appeal board. Seventh, a final decision in the case is made by the presiding authority of the agency (which at the FCC consists of a seven member board of presidentially appointed commissioners). These are the basic steps in a hearing procedure. The current case [the AT&T case] did not progress beyond step four, at which point a settlement was reached, over two years after the case had been opened.²³

Given the structure of the hearing and the exalted role it ascribed to "the law" (certainly a role different from what we would expect if Mr. Levy and Dr. Ashenfelter were interacting instead in an economics seminar or in the street), (1) Dr. Ashenfelter was at a severe disadvantage in trying to convey his scientific meanings, and (2) Mr. Levy did not even have the choice of entertaining any meaning, except a "legally cognizable" one, for

the statistical evidence.

With the weight of the courtroom hearing and the body of legal precepts bearing on him, Mr. Levy's non-scientific understanding of Dr. Ashenfelter's testimony is plausible to us. He was acting with a deliberate purpose in mind, and not blindly stonewalling Dr. Ashenfelter's patient explanations, in asking repeatedly and relentlessly for the "standard error" and "statistical significance". When Dr. Ashenfelter asked if his explanation of sampling was clear, and Mr. Levy replied, "That is clear, Doctor," I believe that Mr. Levy was being sincere, in light of the cultural rules and institutional forces that he was subject to. Only if we "go native" in adopting Dr. Ashenfelter's perspective as our own might Mr. Levy's line of questioning appear obnoxious, obstinate, ignorant, and purposeless (save for the naively imputed purpose of grilling the witness in order to confuse him), as it may have appeared on our initial reading of it.

On the other side of the interaction, Dr. Ashenfelter was operating under his own cultural rules. He was an outsider to the courtroom and new to its institutional forces. He was a scientist from the scientific world, the scientific culture, the scientific institutional framework. He spoke a language different from Mr. Levy's. The two men, being products of different cultures, attached different meanings to the same artifacts, namely, the words "sampling", "standard error", and "statistical significance".

In speaking to each other, they spoke past one another. What was a war dance to Mr. Levy was a barter trade to Dr. Ashenfelter.

3.4 Exchange No. 3: Differing Scientific and Legal Meanings to Testing the Prediction

There is the lurking possibility that Mr. Levy knew how to "speak" science all along and was well familiar with the scientific meanings to sampling, standard error, statistical significance, and simplifying assumptions. Accordingly, Mr. Levy may have feigned ignorance in a deliberate attempt to obfuscate the testimony. Given the adversarial nature of the interaction, the possibility is a probable one.

I rule out the possibility for three reasons. I take the stand that Mr. Levy would not have (1) wittingly made himself vulnerable, or (2) wittingly failed to strike where Dr. Ashenfelter was easily vulnerable.

In the first instance, Mr. Levy made himself vulnerable when he asked Dr. Ashenfelter for the sampling error. Strictly speaking, when there is no sampling but complete reporting, the sampling error is zero. I doubt that Mr. Levy was wittingly providing Dr. Ashenfelter the opportunity to enter into the record that EEOC Exhibit 2A was error-free.

Mr. Levy treaded dangerous waters a second time when he asked for the statistical significance between AT&T's 2.05 percent minority employment and the corresponding 3.00 percent for other employers in the same area. Because there was no sampling but complete reporting, the "sample" percentage was the actual population percentage, so that the "guess" of the "sample" was absolutely accurate, as far as sampling was concerned. Scientifically speaking, then, the degree of statistical significance that Mr. Levy was asking for was astronomically high.

If Mr. Levy had truly been able to speak science, he would not have made himself vulnerable. It was only by the grace of Dr. Ashenfelter, in realizing that Mr. Levy misunderstood these concepts and in subsequently accomodating Mr. Levy with unconventional definitions of them, that no such damaging responses were entered into the record.

In another sense, Mr. Levy did not luck out. The persistence in his questioning served to provide a platform on which Dr. Ashenfelter established his credibility and legitimized, by association, the EEOC's statistical evidence.

The third reason introduces us to the next exchange. It is that Mr. Levy could have demolished Dr. Ashenfelter in the latter's own scientific language where this golden opportunity presented itself. Mr. Levy, unaccountably, let it slip by. His action does become accountable, however, if we rule out the possibility that he really knew how to speak science.

The Achilles heel in Dr. Ashenfelter's estimates of the cost of discrimination at AT&T was that he did not test his hypothesized or predicted values. Dr. Ashenfelter was aware of this. In the version of his work that he published after the conclusion of the AT&T case, he explained in greater detail the calculations behind the estimates he had presented in his testimony. He had performed a multiple regression analysis to obtain the wage differentials between men and women. "Our purpose in including so many righthand variables," Dr. Ashenfelter explained about the regression analysis, "is not, of course, to conduct tests on particular determinants of the hourly earnings of workers, but simply to derive a prediction ..."²⁴ Not having tested his predictions, Dr. Ashenfelter had not, strictly

speaking, justified his theory in a scientific way. In the words of Karl Popper: "So long as a theory withstands detailed and severe tests ... we may say that it has 'proved its mettle' or that it is 'corroborated' by past experience." Dr. Ashenfelter's theory had neither proved its mettle nor been corroborated by experience.

(Of course, the impossibility of setting up a control group would have hindered the observation of the "experience"--the actual cost of discrimination. However, the lack of a control group has never been an impediment to scientists intent on testing predictions. Scientists, especially economists, employ a wide assortment of what they call "quasi-experimental" designs and methods that provide the next best thing to a control group.²⁵ The statistical controls are functional equivalents of what Nagel calls "natural experiments". While multiple regression analyses permit quasi-experimental designs that test predictions in natural experiments, Dr. Ashenfelter deliberately chose not to proceed in this direction.)

How was it possible that Mr. Levy neglected to cross examine Dr. Ashenfelter on this crucial point? In the following exchange, Mr. Levy actually stumbles over the point, and then, not recognizing its significance, ends the cross examination.

MR. LEVY: A distinguishing difference between inference statistics and descriptive statistics is that in the former one can estimate the expected error in the derived results that is caused by random error in the analysis; is that correct?

DR. ASHENFELTER: Yes.

MR. LEVY: Typically in such analyses this error is measured by a statistic called the standard error.

DR. ASHENFELTER: Yes.

MR. LEVY: Have you computed such a measure for the \triangle log P estimate on page 8 [the estimate of the cost due to discrimination at AT&T?]

DR. ASHENFELTER: It could be computed and I have not.

MR. LEVY: Without such a measure, how would you determine whether the results are not significantly different from zero?

DR. ASHENFELTER: I would argue by the logic of the process, the number has to be greater than zero ...

MR. LEVY: No further questions, Mr. Examiner.

[1620-1621]

For Mr. Levy, a golden opportunity had slipped by. He could have had the EEOC's own expert witness enter into the record that the estimated cost of discrimination at AT&T was untested, and hence without empirical justification. Satisfied with Dr. Ashenfelter's logical deduction ("I would argue by the logic of the process ..."), Mr. Levy ended the cross examination, never having asked for an empirical justification of the predicted estimates through testing.

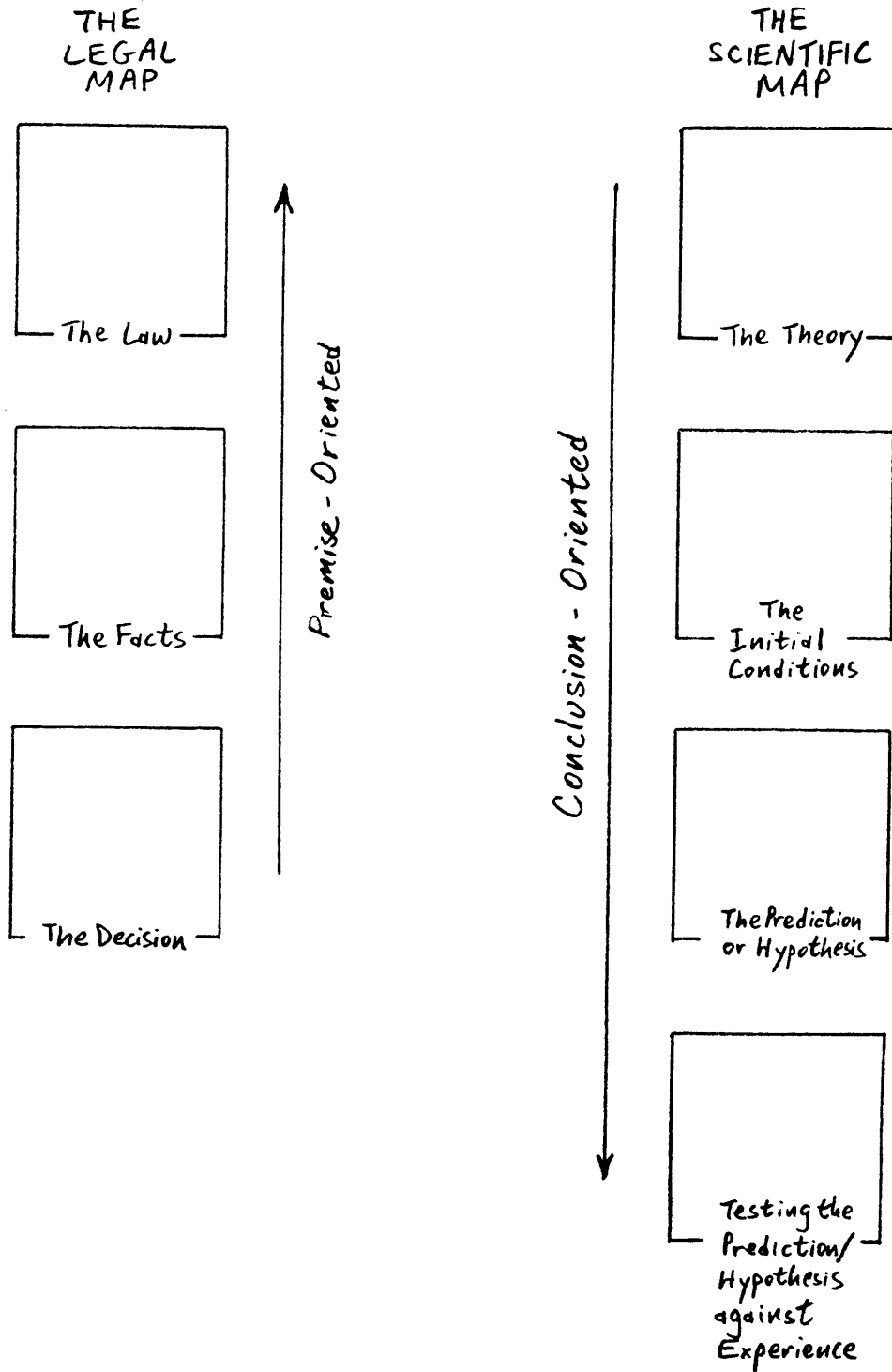
On earlier occasions in the cross examination, Mr. Levy did not fail to articulate points that he hoped to enter into the record (for example,

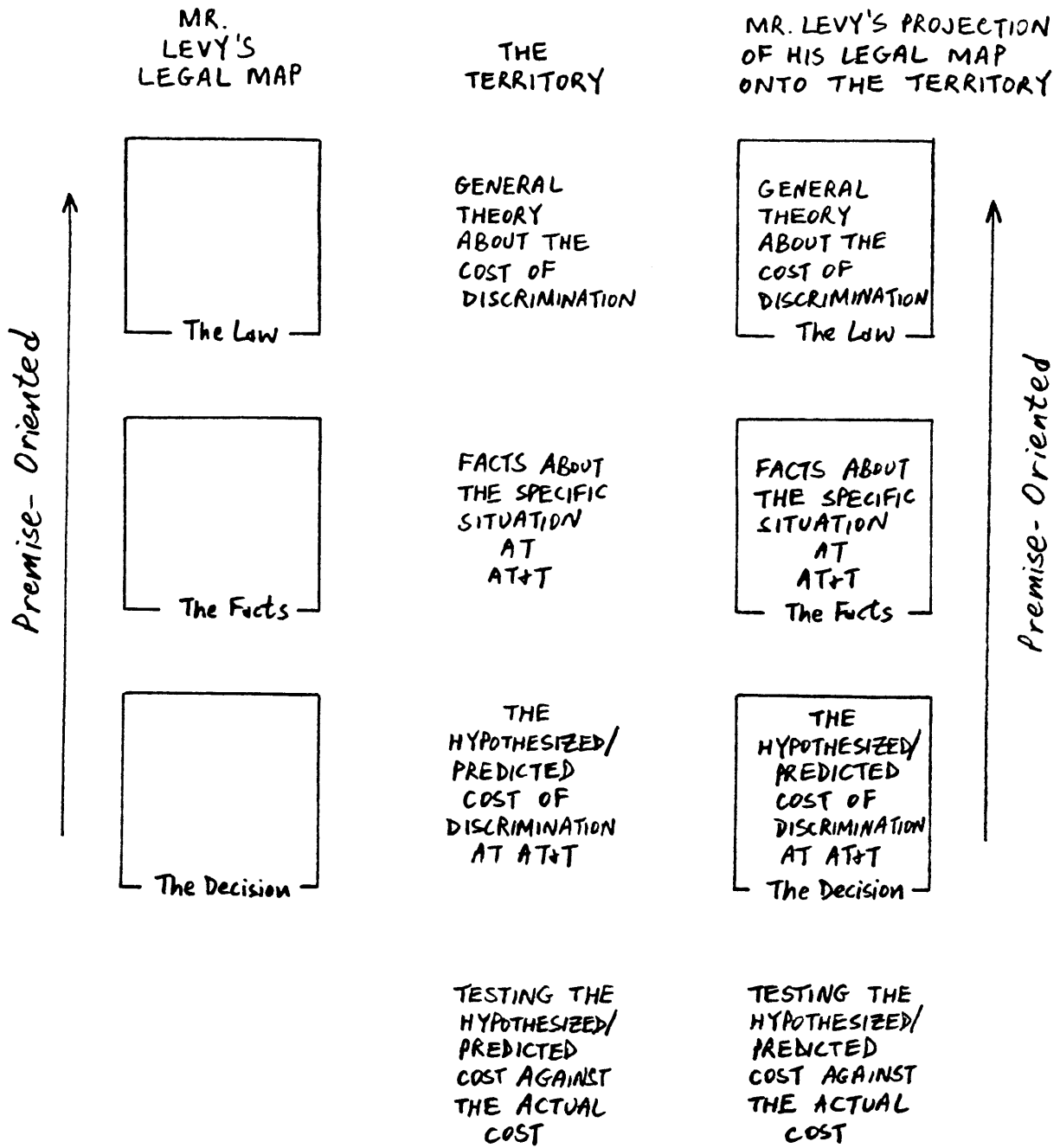
"Then your answer to my question is you did not investigate this"). How is it that he was not persistent on the issue of empirical justification or testing, whereas he was persistent on less crucial issues?

I posit the explanation that Mr. Levy did not understand science. (Conversely, if he did understand science, he would not have let the golden opportunity slip by.) Operating not with a scientific map but with a legal map, Mr. Levy's attention was pointed in the wrong direction, as diagrammed on the following page. A lawyer who is seeking justification is pointed in the direction of the premises. A scientist who is seeking justification is pointed in the direction of the conclusions. Mr. Levy, in scrutinizing what he believed to be the basis for the justification of Dr. Ashenfelter's testimony, thus looked to the premises (for example, the theory's simplifying assumptions). Mr. Levy was premise-oriented in his outlook.

Returning to the earlier instance in which Mr. Levy ethnocentrically projected his legal map onto Dr. Ashenfelter's testimony, we observe something else that took place in the final exchange, as illustrated on the page following the first diagram.

We see that the scientific territory runs off the legal map. Not only is the lawyer's attention pointed in the wrong direction, but the map which serves as his guide provides him with not so much as an outline to a crucial part of the scientific territory. Even if he were to stumble across the uncharted territory ("Have you computed such a measure for the $\Delta \log P$ estimate"), he would not be able to appreciate its meaning ("No further questions, Mr. Examiner"). Insofar as the legal map (finding the law, finding the facts, applying the law to the facts to reach the decision) is internalized





as a cultural notion, it is hardly surprising at all that Mr. Levy did not recognize or even look for Dr. Ashenfelter's Achilles heel.

There is one final point we must consider. Dr. Ashenfelter admittedly did not make testing his purpose. He meant "simply to derive a prediction." Could Dr. Ashenfelter's avowal that he was simply deriving--and not testing, a prediction, have successfully defended him from an attack by Mr. Levy?

I think not. Dr. Judith Long Laws, we will see, employed a similar defense in her cross examination, but to no avail. In the situation of the cross examination, the lawyer does not wittingly fail to strike where the scientific testimony is easily vulnerable; in the cross examination of Dr. Laws, the lawyer was well aware of the testimony's vulnerable points. In the cross examination of Dr. Ashenfelter, the lawyer was not so aware.

3.5 Perspective on the Ashenfelter Cross Examination

The Levy-Ashenfelter exchange has provided us an introduction to the scientific language, the legal language, and the dynamics that accompany their interaction.

We observed Dr. Ashenfelter "speaking" science. In his eyes, scientific assumptions were premises that played a formulative role, not a justificative one, in theory building.²⁶ Dr. Ashenfelter saw justification as being known not through premises, but conclusions, which we observed in his familiarity with statistical inference and in his mentions of hypothesis testing.²⁷

Dr. Ashenfelter's conclusion-oriented justification clashed head on with Mr. Levy's premise-oriented justification. We observed the justificative function Mr. Levy attached to Dr. Ashenfelter's scientific premises. Also, AT&T's position concerning an alleged per se violation of the law provided us an example of the premise-oriented justification that lawyers typically practice.

Our observations of Mr. Levy introduced the notions of cultural forces and institutional forces. As the sediment or kernel of rules that Mr. Levy internalized, his legal premise-oriented map constituted the cultural force that we observed in action when Mr. Levy projected the map onto Dr. Ashenfelter's testimony. Dr. Ashenfelter's scientific conclusion-oriented map represented the cultural force that operated from within him when he provided answers to Mr. Levy's questions.

Mr. Levy and Dr. Ashenfelter were not free men, able to convince one another solely on the basis of logic. Mr. Levy had the weight of his insti-

tutions on his side; Dr. Ashenfelter, momentarily estranged from his home turf, had to contend with an alien one. The impact of the institutional forces rested in (1) the body of established precepts, principles and other legal premises, and (2) the setting and structure of the courtroom/administrative hearing, which sanctioned and approved the legitimacy of the body of established premises.

3.6 The Artifact, Called "Knowledge"

At a deeper level, the Ashenfelter cross-examination as a whole is a counterexample to the rationalist belief's depiction of policy-making. Here, we simply introduce some ideas that we explore more fully in the final chapter.

An integral notion to the rationalist belief is that knowledge can be imparted or passed on, from one individual to another. Thus, professors of business management attempt to pass on a "knowledge base" of economics, sociology (usually in the form of organizational behavior), statistics, and other scientific knowledge to their students, who graduate and then presumably impart their knowledge to co-workers, who all then presumably apply the knowledge to a problem, solving it. In such a depiction, it is as if knowledge were like a rock, which could be the same rock to all individuals to whom it is passed.

I do not believe that knowledge has a durable, objective form, being the same to all individuals. I believe that it varies like a gestalt diagram, its image being dependent on the person possessing it. Knowledge is in the mind of the beholder. It cannot be passed from one person to another, like a rock or other physical object, but consists of mental constructs that must be re-created anew in every knower, like the image seen in a gestalt diagram. As such, re-creation provides the entry point for cultural and institutional forces to come to bear on the knowledge a person eventually comes to espouse and advocate.²⁸

Dr. Ashenfelter's lack of success in "imparting" his scientific know-

ledge of statistics to Mr. Levy illustrates my belief. Dr. Ashenfelter was unable to impart or pass on his mental constructs in the form he intended, that is, as if they were stable and objective in form like a rock; at best, he could only present it, like a gestalt diagram, for Mr. Levy to see. The image that Mr. Levy constructed in his mind turned out to be different from the intended one, owing to cultural and institutional forces that flipped the gestalt switch the other way.²⁹

Insofar as the rationalist belief posits the existence of a "knowledge base" that can be shared, it oversimplifies reality to the point of fantasy. For example, knowledge of the homogeneous and stable form that is implied by the phrase, "knowledge base", does not even begin to address how Dr. Ashenfelter's barter trade appeared to be a war dance to Mr. Levy.

Further, the rationalist belief posits just one form of rationality and knowledge--the scientific--as legitimate for use in policy-making. All other forms are "unscientific". Yet, as the Levy-Ashenfelter exchange reveals, scientific knowledge is scientific only to scientists. Perhaps among scientists a scientific finding may be passed around like a rock. Outside the scientific community, however, the rock may no longer be hard, objective, or durable; thus, it may no longer be a rock. It becomes a sponge.

Policy-makers outside the scientific community employ their own community's knowledge; in doing so, as did the EEOC's lawyers, they may very well achieve success in policy-making, even if their "knowledge base" is a non-scientific one. Legal knowledge is as rigorous as scientific knowledge, and is likely not alone in being a legitimate non-scientific form of know-

ledge for use in policy-making.

Chapter 4 Justifying or Testing the Theory of Scientist-Lawyer Communication: The Cross Examination of Dr. Judith Long Laws

The cross examination of Dr. Judith Long Laws is illuminating in three ways. It serves as a corroboration, for the most part, of the theory that we have so far developed to explain scientist-lawyer communication. Where it does not corroborate, but refutes, the theory, it serves as an impetus for refining those aspects of the theory that are lacking. Finally, in conjunction with the cross examination of a physician, Dr. Andre Hellegers, it serves to reveal how a legal line of questioning, when applied to scientific testimony, can lead to either an infinite regression or an infinite progression of questions, undercutting the testimony either way and setting up a can-lose/can't-win situation for the scientist, where "winning" simply means conveying her viewpoint.

The aspect of the Laws cross examination which we examine in this chapter is how, as part of the "natural experiment" of our case study, it introduces variations in the circumstances surrounding our subject matter, thereby giving us the opportunity to corroborate our theory. At the very least, it varies the particular individuals who occupy the "slots" of scientist and lawyer in scientist-lawyer communication. It therefore presents a source of additional real-life actions against which to compare the wooden movements of our theory's scientist-puppet and lawyer-puppet. To the extent that the wooden movements predict and match the real-life actions, we will have evidence that corroborates the existence of the theorized premise-oriented legal language, the theorized conclusion-oriented scientific language, and the theorized clash between the two in the setting of the

courtroom.

Along with the variations in the particular individuals come variations in their actions. The new AT&T lawyer, Mr. Thompson Powers, focused the courtroom's attention on the scientific testimony's minor premise ("the facts" to the lawyer, "the initial conditions" to the scientist), whereas Mr. Levy had concentrated on the major premise ("the precept" to the lawyer, "the theory" to the scientist). Another variation is that Dr. Laws covered less of the scientific territory with her written testimony than Dr. Ashenfelter had covered with his; that is, Dr. Laws applied the scientific theory to the initial conditions, whereas Dr. Ashenfelter had, in addition, derived a prediction.

Aside from the variations we may observe across cross examinations, there are also the variations within a cross examination. For example, within the Laws cross examination, there are variations in the particular individuals who occupied the role of lawyer in scientist-lawyer communication. They were Mr. Powers, Mr. Copus, and the Presiding Examiner. The Laws cross examination will also allow us to observe that the scientist experienced miscommunication not only with the cross-examining lawyer, but also with the neutral Presiding Examiner, and even with her supportive lawyer, Mr. Copus. (This observation, incidentally, will demonstrate the existence of scientist-lawyer miscommunication, independent of the possibility that the miscommunication is due to the cross-examining lawyer's motivation for intentionally obfuscating the testimony.)

In short, the variations in the circumstances that the Laws cross examination introduces provide a test of our theory. Given that it introduces (1) a

different scientist who was applying (2) a different theory to (3) a different set of facts, all of which came under the scrutiny of (4) different lawyers (Mr. Powers, Mr. Copus, and the Presiding Examiner), whose collective attention was focused on (5) a different portion of the scientific territory, a corroboration of our theory would follow from the observation of the same scientist-lawyer miscommunication, as resulting from the same clash between the same premise-oriented legal language and the same conclusion-oriented scientific language, that we observed in the Ashenfelter cross examination. We would be observing either a remarkable coincidence, or corroboration, of the existence of our theorized cultural and institutional forces.

The purpose of this chapter is merely to provide such a corroboration of our theory.

4.1 Exchanges Revealing the Different Meanings Existing in the Written Testimony for the Scientist and the Lawyer

A few introductory remarks are necessary to make the exchanges intelligible.

An assistant professor of social psychology, with a joint appointment in the departments of sociology and psychology at Cornell University, Dr. Laws testified on behalf of the EEOC. Her written testimony was entitled, "Causes and Effects of Sex Discrimination in the Bell System".

However, if there is any lesson we may learn from the Laws cross examination, it would have, ironically, little to do with sex discrimination in the Bell System. Instead, the lesson would be that the courtroom's assessment of scientific testimony is largely independent of the testimony's substantive content. Whether favorable or unfavorable, the ultimate assessment would hinge on the testimony's form: in what way, if any at all, does the testimony happen to be "legally cognizable"?

A scientific testimony is legally cognizable if it has premises to which the courtroom may apply its procedures of premise-oriented justification. In Dr. Ashenfelter's case, the premises happened to survive the legal scrutiny (even though Dr. Ashenfelter had not designed them for this purpose), and so the testimony was assessed favorably. However, in Dr. Laws' case, we will observe that the testimony disintegrated upon the legal scrutiny of its premises; Dr. Laws' testimony was thus assessed unfavorably, as indicated by the extent of revision and dilution required before being allowed to enter the record as evidence.

To a small extent, Dr. Laws reconstructed observations from documents in a way similar to ours in this study. To a greater extent, Dr. Laws relied on second-hand observations recorded by other social scientists in certain documents, which were studies that AT&T had performed on itself. (The EEOC obtained these in-house AT&T studies through the administrative hearing's discovery process.) The documents which Dr. Laws cited were the premises to her testimony. As such, they became the focus of the courtroom's premise-oriented scrutiny.

Dr. Laws' purpose was not to test a hypothesis, but to illustrate her social-psychological theories with data from AT&T. (All scientific theories consist of mere propositions related to one another according to the rules of logic. Strictly speaking, propositions are abstract or "empty" until applied to a concrete phenomenon existing in nature or society. Divorced of such data, which serve to bring them to life by illustrating them, scientific theories remain purely fictional. To illustrate a theory with data, therefore, is to attach the mental propositions [ideally, one by one] to their respective correlates in the concrete world. In her written testimony, Dr. Laws used data from AT&T to illustrate expectancy-value theory, equity theory, and theory of relative deprivation.)

The purpose of the lawyers (I use "lawyers" to include the Presiding Examiner) was, of course, to establish the validity of the scientific testimony. When reading the transcripts, I conjured an image in which Mr. Powers, with the approval and active cooperation of both the Presiding Examiner and Mr. Copus, made sense of the documents and studies that Dr. Laws footnoted, annotated, and cited in her written testimony as if they had the same

function as the precedents and statutes that are similarly footnoted, annotated, and cited in legal memoranda. For example, Mr. Powers would do his homework by looking up ("Shepardizing") these footnoted studies and documents, in addition to others that he believed were more pertinent (and, to his own argument, more favorable). He would then execute his purpose of establishing the validity of the testimony by questioning these premises or "precepts". In his eyes, footnoted studies and documents appeared as the premises which served to justify the scientific testimony, much as precedents and statutes are the premises that justify a legal opinion. This depiction of legal behavior turned out to be accurate, insofar as it stood up to the test of comparing favorably with the observed actions of Mr. Powers, Mr. Copus, and the Presiding Examiner.

The Laws cross examination is interesting because it demonstrates that the clash between science and law occurs even if justification is not the scientist's purpose. For while justification may not be the scientist's purpose, it is assuredly the purpose that the lawyers have in being in the courtroom. Subsequently, the scientist's own notions about justification are drawn into the discussion; the scientist must make sense of, and articulate responses to, the legal actions aimed at establishing the testimony's validity. The lawyers' actions thus signify one meaning to the scientist and another meaning to the lawyers themselves, reflecting the respective cultural forces that they bring to scientist-lawyer interactions.

In the Laws cross examination, a readily observable focal point for the different meanings was the footnoted documents and studies in the scientific testimony. For Dr. Laws, they had the simple and innocuous meaning of empi-

rical data, intended only to illustrate her theories. For Mr. Powers, they had the crucial meaning of being the premises on which the testimony's validity rested.

I have selected twenty-two exchanges which reveal the different meanings that were attached to the documents. Sometimes, Dr. Laws, Mr. Powers, Mr. Copus, and the Presiding Examiner articulated these meanings, as well as their purposes in being present, in their very own words, thereby facilitating our social-science task of developing an interpretive understanding.

The reader may choose to skim the exchanges and then proceed to the next section, where reprises of illuminating exchanges will be presented. The reader may then return to the original presentation of the exchanges when questions arise about the context.

Exchanges Revealing the Different Meanings

Exchange No. 1

MR. POWERS: Dr. Laws, you indicate that your testimony provides an analysis of Bell System practice with respect to women. What caused you to undertake this analysis?

DR. LAWS: I am interested in the real world implications of the work that I do even though I happen to be specialized as a very theoretical kind of person. This particular inquiry in the organization of the Bell System is a perfectly natural experiment to demonstrate the workings of some of the theoretical models that I deal with. In fact, there are great bodies of data contributed by the Bell System, itself, which

are better than anything I could make up ... So it was an irresistible opportunity.

[Page 722 of the transcript]

Exchange No. 2

MR. POWERS: Is your study the product of your analysis of the documents which they [the EEOC] provided plus the theoretical knowledge you had acquired?

DR. LAWS: Yes, that is right.

MR. POWERS: Did you refer to any documents other than those which you cite in your testimony?

DR. LAWS: I haven't been terribly specific about citing all of the documents which are already part of the exhibits for this hearing. There are many of those. What I did was use standard bibliographic and reference form for materials which might not be familiar to members of the [Federal Communications] Commission or to the parties to the hearing so that I am assuming, and maybe I shouldn't do this, that everyone knows the findings of fact. When I make assertions about the Bell System they are all based on those documents.

MR. POWERS: That, again, for clarification, refers to Exhibit 2, I believe, to Mr. Speck's testimony which is now called "Summaries". Isn't that the title of it?

MR. COPUS: That is correct ... [But] I think what the witness said was that she based her findings on not only the exhibit which is now labeled "Summaries of Documents", attached to Exhibit 1, but she also based her analysis on the considerable examination of an original [in-house] Bell document ["The Houston Study"] which we supplied [to her]. If you request we will provide you with a list of the documents on which she relies.

DR. LAWS: Would it be helpful to you in the cross examination on the testimony to have these items footnoted specifically so that you can have reference to the document containing the data? I have written something for the ease of reading. If it would help you to have specific citations, I will be glad to give them.

MR. POWERS: ... what I am referring to and what I do request is a list of documents which the EEOC furnished to Dr. Laws which was the basis of her study.

MR. COPUS: We will certainly provide you with that list.

[726-727]

Exchange No. 3

MR. POWERS: Did you conclude that the EEOC had provided you all the documents from the Bell System that would be relevant to the study you had undertaken?

DR. LAWS: Yes. I didn't use them all. I saw an absolutely exhaustive compilation of data.

MR. POWERS: In reaching that conclusion, did you examine the discovery request of EEOC to AT&T in the Bell System?

DR. LAWS: What do you mean by discovery request?

MR. POWERS: Most of the data was provided by the Bell System in response to specific request that the EEOC made. I was inquiring whether part of the basis of your judgment that the documents were complete in terms of relevant documents was based on an analysis of those discovery requests.

DR. LAWS: Yes. I made some selections from those lists ...

[728-729]

Exchange No. 4

MR. POWERS: How would you characterize [the written testimony]?

DR. LAWS: I would say it is an analysis of a problem illustrated with data as opposed to a study which is [a] specific research project undertaken to test [a] specific hypothesis. In fact, you really can't use data of this sort [that is, the numerical data in AT&T's own in-house studies, obtained by the EEOC from AT&T through the discovery process] to test [a] hypothesis really, because the data were collected for a different purpose. They were simply used in an illustrative way to give a sense

of numbers and a concreteness to the argument ...

[737]

Exchange No. 5

[The discussion is over the phrase in Dr. Law's written testimony, "Bell practices a sex-segregation more extensive than that observed in society at large".]

MR. POWERS: ... Have you made an analysis of employment practices of other major companies or institutions other than the Bell System that gives you a basis for that statement?

DR. LAWS: No.

[Dr. Laws and Mr. Powers discuss this point. In the discussion, Mr. Powers asks the following questions:]

MR. POWERS: Are you aware of information about specific companies or institutions which display less sex segregation than the Bell System?

...

The purpose of [the last] question is to lead to one about the model you use in terms of your evaluation of the data. Is it a model based on a specific company or institution? On a body of principles accepted by social psychologists, or is it based on your own concept?

...

What analysis did you make of all formal recruiting done in the Bell System?

DR. LAWS: You are asking me questions about [the written] testimony. I think that in order to help you out I should probably annotate a copy of the testimony with reference to specific documents which are part of your exhibit and that I think should be part of the cross examination on testimony.

PRESIDING EXAMINER: Well, how does this matter stand? Mr. Copus, do you want to supply an annotated version of that?

MR. COPUS: Yes.

PRESIDING EXAMINER: We are speaking of the phrase on page three [Dr. Laws writes: "All formal recruiting is sex-specific ..."]. Is that it, Mr. Powers?

MR. POWERS: Yes. I think we are entitled to determine at this time what [the] basis is for that conclusion in terms of the review of documents. I gather that it is clear that it is simply a matter of review of documents for that conclusion.

MR. COPUS: Mr. Examiner, as we indicated earlier, we will provide the phone company with a complete annotation of all the documents which were provided to Dr. Laws with specific annotations to the parts of her testimony as they relate to those documents.

[738-741]

Exchange No. 6

MR. POWERS: I am not at the moment doing anything more than trying to determine the basis of your conclusions. I take it, it is the data.

DR. LAWS: That is correct.

[753-754]

Exchange No. 7

DR. LAWS: I would like to add one thing to your observation which I think is very apropos and that is to say that I have--I think, I hope, and if this is not the case, I hope you will correct me--tried to avoid making statistical generalizations about the whole Bell System.

The Chesapeake & Potomac Telephone study [an in-house AT&T study that Dr. Laws cited in her written testimony] is interesting and illustrative but I don't construe it as my job to reconcile in a statistical way this finding and I don't remember--if you say I cited it I must have cited it, I don't remember doing so with the C&P study ... I am interested in the C&P study because it focuses in on a problem for us.

I won't say that that problem necessarily in a descriptive way represents the total Bell System. However, the findings are so striking in this study that I am moved to ask a variety of questions about what is going on there.

That is legitimate in terms of the way I am using data in my report.

If you are going to argue, for example, in rebuttal that this is an exceptional situation, then you have to justify ... why there should be [significant] differences.

I don't have to do that because that is not the argument I am making.

MR. POWERS: I am not engaged at the moment in a direct argument with you. I am just trying to clarify what you have presented to this proceeding [which is the purpose to the voire dire, currently underway]. In line with your point about not generalizing from a specific study, I would like to inquire about some of your statements on page six. The study to which you refer is of the operator position in Houston. Isn't that correct? That is referred to in line four.

[Dr. Laws' statement reads: "These data suggest that precisely the women who are most promising are being lost to the Bell System by short-sighted personnel policies. This is the conclusion of the Houston study mentioned above."]

DR. LAWS: I think not because the Houston operator citation is EEOC R-798. This sentence begins "In another study" with a citation, so in a subsequent statement, it presumably is based on both of those.

Again, I can't say a great deal about documents which I have not read in their entirety, which I wish to do before submitting to cross-examination on them [which follows the voire dire]. I can't say a great deal until I have reviewed those.

[759-761]

Exchange No. 8

MR. POWERS: Dr. Laws, at the bottom of page nine of your testimony, you state that college graduates are usually recruited in the Level 2 and above. What is the basis for that statement?

[The statement reads: "A second route into management is via recruitment of college graduates, usually into Level 2 and above."]

DR. LAWS: Again, I am talking about documents whose exhibit numbers I cannot specify at this time ...

[765]

Exchange No. 9

DR. LAWS: ... In general, in an experiment, you control a great many of the sources of variance that you are not prepared to measure. You manipulate only certain variables. There is no reason why it has to be done in the laboratory, in fact.

Exchange No. 10

MR. POWERS: I would like to move to page 30. I am referring to a statement midway on the page, "Part of the answer may be found in structural obstacles which a sexist society puts in their way; for example, the tendency of high school counselors (encouraged by companies like Bell) to counsel women away from 'men's fields'.

I am asking you if you have any personal knowledge that Bell has encouraged high school counselors to counsel women away from men's fields?

DR. LAWS: I have looked at, as I mentioned before, a great deal of material used in the actual recruiting efforts of Bell and I will be glad to discuss those in detail when I have had the opportunity to review the documentary basis of the testimony.

[786-787]

Exchange No. 11

MR. POWERS: I would like to direct your attention to the footnote on page 43, referring to the act of perpetuation of the notion of BFOQ [Bona Fide Occupational Qualification] in the Bell System. Do you recall the date of the document that you cite in that footnote?

[Dr. Laws' footnote reads: "It should be noted that individuals in high management positions within the Bell System actively perpetuate the notion of the BFOQ (see EEOC R-1025).]

DR. LAWS: No, I don't.

MR. POWERS: I show you this document and ask you if that is not the document to which you refer [that is, EEOC R-1025].

DR. LAWS: Yes, it is.

MR. POWERS: The date of that document is 1966, is it not?

DR. LAWS: Yes.

MR. POWERS: Are you aware of the different positions that individual Bell companies have taken on the question of the BFOQ for the operator since 1966?

DR. LAWS: No, I don't think so ...

...

MR. POWERS: ... I ask you if you are aware of the differing positions they have taken on the question since that time. As I understand your answer, you were not aware of--

DR. LAWS: That is correct. The reason for footnoting such a memorandum as the one you have just cited, and I doubt very much if you would disagree with the logic of this, is that positions taken by top management are extraordinarily influential, rather more so than the same position expressed by someone at the lower level in the authority structure.

It is very significant this was done. The significance would not be vitiated in any way if I put this footnote in the past tense.

[793-795]

Exchange No. 12

MR. POWERS: ... Would you indicated, Dr. Laws, specifically where those citations should be referenced in the body of page three [in the written testimony]?

[Upon returning for cross examination, Dr. Laws provided a new list of annotations of the documents ("citations") from which she formulated her written testimony.]

DR. LAWS: Line 18--Well, the last sentence on page three carries over to the first line of page four, and these two sets of citations which are located ... Are you with me?

MR. POWERS: ... I am not sure.

DR. LAWS: Do you want me to go back?

MR. COPUS: Dr. Laws, would you specify which word on line 18 should there be a footnote symbol after and the annotations that go with that footnote.

DR. LAWS: Which word--I am afraid I was not that punctillious.

MR. COPUS: There may be some question about line 18 because of the headnote on that page.

DR. LAWS: The sentence I am talking about, and I don't think I want to break it down any further, is ...

[1333]

Exchange No. 13

MR. POWERS: Do I understand that you would propose to combine the citations shown under arabic 5 and 6 [in the new list of annotations] and have

them refer to the full sentence?

DR. LAWS: ... It is immaterial to me whether you want to lump them. They are part of the same argument. It is possible, for example, [if] I might anticipate your strategy in cross examination, you might take me through this testimony word by word or you might do it paragraph by paragraph. If I am lucky, you will do it page by page. I am not really sure.

PRESIDING EXAMINER: Don't encourage such an appalling adventure.

MR. POWERS: I don't plan to go through it in that detail, but it is important to me in terms of reviewing the annotations to have shown very specifically what these documents are cited for.

[1334-1335]

Exchange No. 14

MR. POWERS: ... let's move to page seven. Here, Dr. Laws, you state ["The prospects for women's movement into and through management levels are bleak. The sex segregation in recruiting for the entry level positions (for Craft and Traffic jobs) is perpetuated in management"] and I believe the reference that is shown for that statement is document R-1033, a statement from an AT&T Vice President, dated May 12, 1965 ...

...

I ask you if you are aware that that document was officially canceled

by AT&T on November 13, 1968, 3 1/2 years ago, and that the canceling document was furnished to the EEOC in discovery under its request D-9?

DR. LAWS: No, I am not aware of any cancellation.

[There is discussion among Mr. Gartner, who is an EEOC lawyer, Mr. Powers, and the Presiding Examiner over the citation to document R-1033.]

DR. LAWS: If I may, as long as it is my conclusions that are being talked about, I wouldn't like to leave the impression that there are not other data in support of those conclusions that are part of this record.

I would direct the attention of people in this hearing to pages 66 through 127 of Exhibit 2, the Charts and Tables, Appendix A ...

PRESIDING EXAMINER: You made a good recovery, Doctor.

[1341-1344]

Exchange No. 15

[Dr. Laws had originally written: "Some Bell companies report 200% turnover among short-term employees in this occupation [operator]. (EEOC C-1540)."]

DR. LAWS: ... I was checking the force loss studies again [document EEOC C-1540] and since I could not find a specific citation of 200 percent, although in fact I had found it before, I altered [the written testimony] to read "more than 100 percent", which is a more correct citation ...

...

MR. POWERS: To the extent that you are citing document C-1540 for your statement that some Bell companies report 100 percent turnover among short-term employees, I am interested in determining where you find any support in this document for the 100 percent figure ...

DR. LAWS: I would have to take time to go through the whole report to make sure not only that I found one appropriate citation as you have graciously helped me do but that I specifically specify all of them.

MR. POWERS: I hate to intrude on your lunch, but I wonder if that could be done during the lunch hour.

PRESIDING EXAMINER: I think the difficulty is, Dr. Laws, you have given us one specific citation for the statement.

I think if you have others you should locate them and advise us.

DR. LAWS: I think I have stated before I was willing, since in my testimony I make absolutely no pretense that I am presenting an up-to-date and complete statistical presentation of the Bell System.

This is not the intention of my testimony. That is not the kind of expertise that I am here for, that I used data only illustratively ... that is the case and I think I have made it explicitly for that.

It does not matter to me whether I have 10 studies to cite when they fit into the specific argument or only one. So, in fact, I have limited myself to this document. I don't think I need to go back over the whole set of force loss studies to reinforce it but as long as we are talking about this document I would be better pleased, and I guess

Mr. Powers would too, if I would review it exhaustively and make sure you have as many page citations as are relevant within this document.

PRESIDING EXAMINER: Would you undertake to do that during the lunch period?

MR. POWERS: I don't need the cumulative citations. I would be interested in one or more that support that statement.

...

MR. JUNTILLA [lawyer for the FCC's Common Carrier Bureau]: On the top of page 10 [of document C-1540], I find the statement "resignations and dismissals in many districts are running well over 200 percent".

You might take a look that.

DR. LAWS: I knew I didn't make that up.

[1347-1350]

Exchange No. 16

PRESIDING EXAMINER: ... The only evidence in the record at this point is what [Dr. Laws] said and the fact that Counsel [Mr. Powers], who is not a witness, takes a differing view and that is not evidence. The only evidence we have is the interpretation of Dr. Laws ... bear in mind any statement of Counsel, unless there is some kind of stipulation, is not evidence.

[1361]

Exchange No. 17

MR. POWERS: Dr. Laws, I would like to ask you a series of questions about the Houston study [Psychological Factors Affecting Operator Turnover, an in-house study by Southwestern Bell, which is one of the documents on which Dr. Laws formulates her testimony], that is, EEOC Document R-798 ...

...

Back on page 5 of your testimony, there is a statement that ["Dissatisfaction with opportunities for advancement was the second most frequent cause of termination among Houston Operators (EEOC R-798), salary being the first"] ...

...

Would you indicate where in that survey instrument [mentioned on pages 26 through 28 of the Houston study] you find the question asked regarding the reason for termination among the operators?

DR. LAWS: I can't answer that ... It is not uncommon [for such a study] to reproduce a questionnaire ... Since that was not done here [in the Houston study], I did not pay much attention to the questionnaire so I doubt very much if I can specify a question from it ...

...

MR. POWERS [quoting a statement from the Houston study : "In order of great-

est response, terminating operators least like salary level, opportunity for advancement ..."]]: Is that the statement and is it the data based on that statement that you use as a basis for your statement that dissatisfaction with the opportunities for advancement was the second most frequent cause of terminations, salary being the first?

DR. LAWS: It is likely. You are asking me to attest to an historical protest that I didn't keep documentary records ...

As I said, I have read this report carefully and more than once and really base my use of it on a reading of the total document.

So, I would be very hard put to say on this, unless I reproduced the data in which case I should footnote it, but I would be very hard put to say where in a given document a particular statement comes from.

[1367-1370]

Exchange No. 18

MR. POWERS: In light of this discussion, Mr. Examiner, we believe the witness has failed to show any statements in this document [the Houston Study] that support her assertions as to what is the most frequent cause of termination, either the first or second most frequent cause of termination, and therefore we move to strike the sentence that begins on line 14 and runs through line 17 on page 5 [that is, "Dissatisfaction with opportunities ... salary being the first"].

PRESIDING EXAMINER: In accordance with previous rulings of the same type, I will deny the motion.

[In previous rulings, the Presiding Examiner stated that "striking" any testimony would be physically difficult to carry out, and that such would be unnecessary as long as the record made clear what deficiencies existed in the testimony.]

DR. LAWS: I would like to enter an objection to that conclusion. It is very deeply offensive to me. I did not agree to any such thing.

In fact, I said I cannot at this time specify particular footnoting of location. I did not say I could not ever. I did not refuse to take some time in a recess, if it is absolutely essential, to look that up, and I very much dislike the language of your assertion that I am unable to do so.

I don't believe I said anything of the kind.

[1377]

Exchange No. 19

PRESIDING EXAMINER [to Dr. Laws]: You are giving dissertations on all his questions as a whole and it is not very helpful. Try to clarify your question, Mr. Powers.

[1381]

Exchange No. 20

DR. LAWS: ... in order to answer the kind of question that you have continually put to me is where in a document of some 50 pages do you find

some wording that supports that assertion, I would have to have time to study the document, and I would be glad to answer the question in the detail that you seem to require.

If we are going to have more questions on this document, then I should take whatever formal steps are necessary to ask for time to study this study so that I can give you more specific answers to your questions.

PRESIDING EXAMINER: The problem is, Dr. Laws, that you have in several instances cited specific documents and in some cases a single document by your parenthetical insertion of the reference supposedly as the basis for that statement or at least that is what I presume to mean.

Mr. Powers is entitled to test the validity of your conclusion in relation to the documents which you cite as the supporting documents.

It is unfortunate if it is going to require you to delve into the document again, but at the same time, they are certainly appropriate questions to be asked.

DR. LAWS: I don't disagree with that.

[1382]

Exchange No. 21

MR. POWERS: We would simply move at this point to strike that reference [in the written testimony] as based on [its] not being probative ... and as not being based on adequate data in the Southwestern Research

Study [that is, the Houston study].

[1417]

Exchange No. 22

MR. POWERS: Dr. Laws, yesterday afternoon, I asked you with reference to these two studies that are referred to [and used] in the Houston study, these two tests [the Armatas test and the Edwards Personal Preference Schedule], whether at the time you prepared--whether you had any knowledge as to whether or not either of those had ever been validated in the sense of a study to show that they were able to measure what they purported to measure?

[In the ensuing discussion Dr. Laws makes clear that she has no knowledge of the tests' validities.]

MR. POWERS: Isn't it true that until you establish that a study or a test instrument can in fact predict or measure the trait or criteria that it is undertaking to measure, you can't even be sure that it is accurately portraying the results within the test group?

DR. LAWS: There is a problem with that but, again, it depends what criterion measure you are trying to predict to ...

I don't know if I can say any more without going into a whole philosophy of test construction, but again, let me just say in summary that when you are talking about validity, it is very important to specify the criterion to which you are trying to predict and many

studies are not trying to predict at all.

They are descriptive studies as in fact this [the Houston study] is and the Edward is used descriptively as is the other measure in this study.

MR. POWERS: But it was a basis in the study and in your conclusions for certain statements about the characteristics of terminating operators, was it not?

DR. LAWS: Yes. That is a correlational statement. That is to say, that is what I mean by descriptive ...

...

MR. POWERS: I believe we have only one more question about the Houston study and I think we have probably laid the basis for disposing of it ...

4.2 Analysis of the Exchanges: The Meaning of the Documents for Mr. Powers, Mr. Copus, and the Presiding Examiner

Like excerpts from a movie, presented as a sequence of film clips, the reprise of exchanges from the transcript captures and portrays of the cross examination of Dr. Judith Long Laws. How do we, as members of the audience viewing the film clips, proceed to understand the exchanges?

Apparently, words with the same spellings and pronunciations had entirely different meanings for Dr. Laws, on the one hand, and for Mr. Powers, Mr. Copus, and the Presiding Examiner, on the other hand. What were the meanings?

A promising entry point for our inquiry into the meanings is the fascination that the documents held for Mr. Powers.

The documents prompted Mr. Powers to release the following barrage of questions at the start of the cross examination:

Is your study the product of your analysis of the documents which [the EEOC] provided plus the theoretical knowledge you had acquired?

[Exchange No. 2 (E2)]

Did you use or refer to any documents other than those which you cite in your testimony?

[E2]

I think our request would be for the document, a list of the documents provided to her.

[E2]

Did you conclude that the EEOC had provided you with all the documents from the Bell System that would be relevant to the study you had undertaken?

[E3]

... did you examine the discovery request of EEOC to AT&T in the Bell System? ... Most of that data was provided by the Bell System in response to specific requests that the EEOC made. I was inquiring whether part of the basis of your judgment that the documents were complete in terms of relevant documents was based on an analysis of those discovery requests.

[E3]

... what request or what other information in the documents caused you to believe the Bell System had provided all relevant materials concerning motivation and job enrichment?

[729-730]

You didn't have any impression, did you, that the data was provided by EEOC and specifically in connection with your particular interest.

[730-731]

Did you ever inquire of the EEOC whether there were any additional documents on motivation or job enrichment from the Bell System or make any other requests to find out if there were additional documents from the System that would be relevant to you?

[731]

I believe you also indicated that you made some express request to the EEOC for supplementary data, for other information. I would request that we have a list of that data and information.

[737-738]

Clearly, Mr. Powers attached a particular meaning to the documents.

Whatever the meaning was, however, it was a meaning to which Dr. Laws was either oblivious or unconcerned:

I haven't been terribly specific about citing the documents ...

[E2]

What do you mean by discovery request?

[E3]

Again, I can't say a great deal about documents which I have not read in their entirety ...

[E7]

Again, I am talking about documents whose exhibit numbers I cannot specify at this time ...

[E8]

I used data only illustratively ... It does not matter to me whether I have 10 studies to cite when they fit into the specific argument or only one.

[E15]

What was the legal meaning to the documents? We will observe below that the Presiding Examiner provides us the answer in his own words. We will consider, first, some other observations by way of introduction.

While the Presiding Examiner directed his remarks at the courtroom, we may cast him in the role of "narrator" in our movie, because his explanations of the issue at hand provided clarifications not only for the actors caught up in the plot, but also for us viewers in the audience. First, consider this example:

The only evidence in the record at this point is what [Dr. Laws] said and the fact that Counsel [Mr. Powers], who is not a witness, takes a differing view ... is not evidence. The only evidence we have is the interpretation of Dr. Laws ... bear in mind, any statement of Counsel, unless there is some kind of stipulation, is

not evidence.

[E16]

That is, in the eyes of the Presiding Examiner, Dr. Laws' testimony had the meaning of "the facts", to which "the law" (here, Title VII of the Civil Rights Act of 1964 and other statutes) was to be applied. In the legal culture, the function of the expert witness in providing "the facts", is a normal practice. Black's Law Dictionary points out:

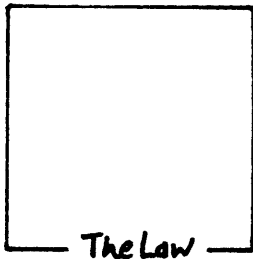
If scientific, technological, or other specialized knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, training, or education, may testify thereto in the form of an opinion or otherwise.¹

What we observe in the Presiding Examiner's pronouncement is but a single instance of a common practice in the legal community, that is, reading from the legal map ("the law", "the facts", "the conclusion") in order to make sense of the territory being encountered. For example, when a lawyer encounters a region made up of statutes, regulations, and precedents that other lawyers present to him, he reads from the legal map in order to place these as "the law". When encountering a region containing descriptions of the actual situation (controversy) in question, the legal native, in reading from the legal map, identifies the region as "the facts". The Presiding Examiner was reading from the legal map in the latter way when he classified Dr. Laws' expert testimony as falling under "the facts". This is summarized in the diagram on the following page.

The Presiding Examiner's pronouncement, that Dr. Laws' testimony was

READING DR. LAWS' EXPERT TESTIMONY
AS "THE FACTS"

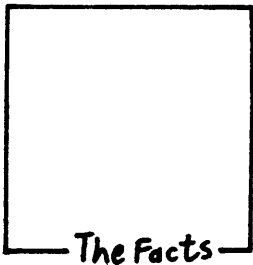
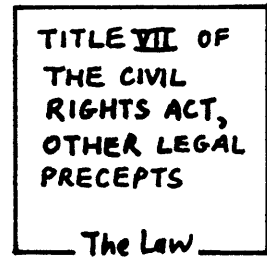
THE LEGAL
MAP



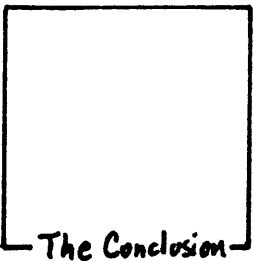
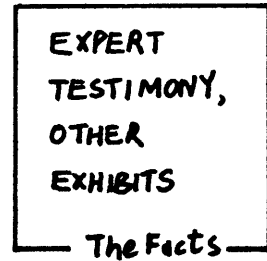
DIFFERENT
REGIONS ACROSS
THE LEGAL
TERRITORY

TITLE VII OF
THE CIVIL
RIGHTS ACT,
OTHER LEGAL
PRECEPTS

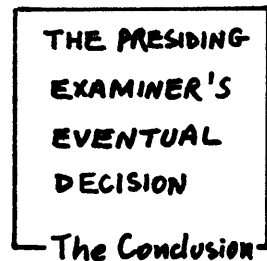
PROJECTING THE
LEGAL MAP ONTO THE
REGIONS ACROSS THE
LEGAL TERRITORY



EXPERT
TESTIMONY,
OTHER
EXHIBITS

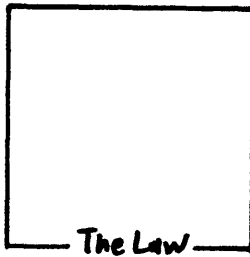


THE PRESIDING
EXAMINER'S
EVENTUAL
DECISION



READING DR. LAWS' CITED DOCUMENTS
AS "THE FACTS"

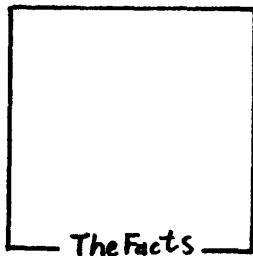
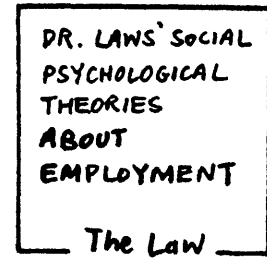
THE LEGAL
MAP



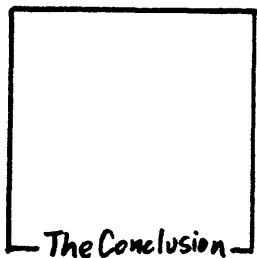
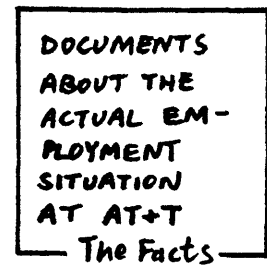
DIFFERENT ELEMENTS
WITHIN THE REGION
OF DR. LAWS' EXPERT
TESTIMONY

DR. LAWS' SOCIAL
PSYCHOLOGICAL
THEORIES
ABOUT
EMPLOYMENT

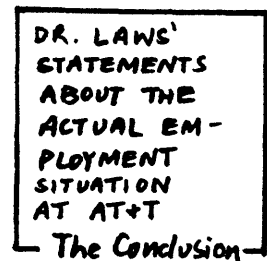
PROJECTING THE LEGAL
MAP WITHIN THE REGION
OF DR. LAWS' EXPERT
TESTIMONY



DOCUMENTS
ABOUT THE
ACTUAL EM-
PLOYMENT
SITUATION
AT AT+T



DR. LAWS'
STATEMENTS
ABOUT THE
ACTUAL EM-
PLOYMENT
SITUATION
AT AT+T



evidence, also explains the intensity and the meticulousness with which he, Mr. Powers, and Mr. Copus approached the sentences, phrases, words and annotations in the testimony, for any "fact" that was contained therein, and that alleged sexism, had the potential of proving, upon application of Title VII, that AT&T discriminated in employment on the basis of sex. As we will see in the next section, the gravity of Dr. Laws' testimony for the lawyers far outweighed its gravity to Dr. Laws herself. For Dr. Laws, she had only her purpose of "illustration" to worry about.

Given the identification of Dr. Laws' testimony as "the facts", the validity of "the facts" still had to be determined. Indeed, this was the very purpose of the cross examination. For this purpose, the Presiding Examiner (and consequently, Mr. Powers and Mr. Copus, who took their cues from the Presiding Examiner) again read from the legal map. In this reading, however, the Presiding Examiner was not attempting to identify the different regions across the legal territory, but to identify specific elements within one of the regions, namely, the region of the expert testimony. In this particular reading, the legal meaning to the documents is manifested for us to observe. This is summarized in the diagram on the page following the first diagram.

The expert testimony was made up of (1) the social-psychological theories that Dr. Laws introduced about work motivation and employment (expectancy-value theory, equity theory, theory of relative deprivation); (2) descriptions of the actual employment situation at AT&T, as revealed in the documents that Dr. Laws cited; and (3) Dr. Laws' own statements about the employment situation at AT&T, in the light of her theories and her documents. In the

reprise of exchanges to be presented shortly, we may observe numerous instances in which Mr. Powers, Mr. Copus, and the Presiding Examiner projected the legal map onto these elements within the region of the expert testimony, thereby attaching (1) the meaning of "the law", as the major premise, to the social-psychological theories; (2) the meaning of "the facts", as the minor premise, to the documents that Dr. Laws cited; and (3) the meaning of "the conclusion" to Dr. Laws' own statements about AT&T. This is the same as Mr. Levy's projection of the legal map onto Dr. Ashenfelter's testimony. The fact that the legal map defines justification as being premise-oriented is spelled out in another of the Presiding Examiner's narrations, where he clarified, for Dr. Laws, the legal meaning to the documents:

Mr. Powers is entitled to test the validity of your conclusion in relation to the documents which you cite as the supporting documents.

[E20]

The Presiding Examiner and Mr. Powers, with the active cooperation of Mr. Copus, applied this reasoning throughout the cross examination.

The Presiding Examiner did not choose his words in [E20] randomly. In fact, the Presiding Examiner was employing two of four "buzz words" that recurred throughout the Laws cross examination. The lawyers referred to the documents as the basis that supported Dr. Laws' own statements or conclusions about AT&T's employment practices. The "buzz words" are explained here to help the reader make sense of the following reprise of exchanges.

Insofar as "the law" is applied to "the facts", they combine as the basis from which the conclusion follows. Furthermore, insofar as the conclu-

sion is justifiable only if "the law" and "the facts" are in order, the validity of the conclusion is supported by the validity of its basis. The lawyers identified Dr. Laws' statements about the employment situation at AT&T (in the light of her scientific theories and her documents) as being her conclusions.

Because the lawyers projected the meaning of "the law" onto the social-psychological theories, and the meaning of "the facts" into the documents, the theories and the documents together constituted the basis to Dr. Laws' testimony. However, because the lawyers hardly questioned the theories at all, we can say that the supporting documents alone made up the basis to Dr. Laws' statements or conclusions about AT&T's employment practices, at least from the viewpoint of Mr. Copus, Mr. Powers, and the Presiding Examiner.

The following reprise of exchanges is presented in order to (1) illustrate the lawyers' purpose, which was to establish the validity of Dr. Laws' testimony, and (2) further substantiate the meaning that the lawyers attached to the documents. The presence of the buzz words generally indicates an instance in which the lawyers were reading from the legal map and, therefore, invoking premise-oriented justification.

MR. POWERS: ... what I am referring to and what I do request is a list of documents which the EEOC furnished to Dr. Laws which was the basis of her study.

MR. COPUS: We will certainly provide you with that list.

[E2]

PRESIDING EXAMINER: We are speaking of the phrase on page three [Dr. Laws' statement, "All formal recruiting is sex-specific ..."]. Is that it, Mr. Powers?

MR. POWERS: Yes. I think we are entitled to determine at this time what [the] basis is for that conclusion in terms of the review of documents. I gather that it is clear that it is simply a matter of review of documents for that conclusion.

MR. COPUS: Mr. Examiner, as we indicated earlier, we will provide the phone company with a complete annotation of all the documents ...

[E5]

MR. POWERS: I am not at the moment doing anything more than trying to determine the basis of your conclusions.

[E6]

MR. POWERS: Dr. Laws, at the bottom of page nine of your testimony, you state that college graduates are usually recruited in level 2, and above.

What is the basis for that statement?

[E8]

MR. POWERS: ... it is important to me in terms of reviewing the annotations
to have shown very specifically what these documents are cited for.

[E13]

MR. POWERS: ... I believe the reference that is show for that statement
[made by Dr. Laws in her written testimony] is document R-1033 ...

[E14]

MR. POWERS: To the extent that you are citing document C-1540 for your
statement that some Bell companies report 100 percent turnover among
short term employees, I am interested in determining where you find
any support in this document for the 100 percent figure ...

[E15]

PRESIDING EXAMINER: I think the difficulty is, Dr. Laws, you have given us
one specific citation [to a document] for the statement [in the written
testimony]. I think if you have others you should locate them and
advise us.

[E15]

MR. POWERS: I don't need the cumulative citations. I would be interested

in one or more that support that statement.

[E15]

MR. POWERS [quoting a statement from the Houston study: "In order of greatest response, terminating operators least like salary level, opportunity for advancement ..."]: Is that the statement and is it the data based on that statement that you use as basis for your statement that dissatisfaction with the opportunities for advancement was the second most frequent cause of termination, salary being first?

[E17]

MR. POWERS: In light of this discussion, Mr. Examiner, we believe the witness has failed to show any statements in this document [the Houston study] that support her assertions [in the written testimony] ... and therefore we move to strike the sentence that begins on line 14 and runs through line 17 on page 5 [of the written testimony].

[E18]

PRESIDING EXAMINER: The problem is, Dr. Laws, that you have in several instances cited specific documents and in some cases a single document by your parenthetical insertion of the reference supposedly as the basis for that statement or at least that is what I presume to mean.

Mr. Powers is entitled to test the validity of your conclusion in relation to the documents which you cite as the supporting documents.

[E20]

MR. POWERS: We would simply move at this point to strike that reference [in the written testimony] ... as not being based on adequate data in the Southwestern Research study [that is, the Houston Study].

[E21]

In summary, the legal map made the following behavior possible by the legal actors: (1) to identify, as containing the meaning of "the facts", the documents that Dr. Laws cited; (2) to notice the absence of documents when Dr. Laws cited none, and to ask her to provide them; and (3) for Mr. Powers to do his homework of looking up additional documents that would subvert "the basis" consisting of those which Dr. Laws cited.²

4.3 Analysis of the Exchanges: The Meaning of the Documents for Dr. Laws

In the eyes of the lawyers, Dr. Laws' testimony appeared shoddy and poorly prepared. To them, her conclusions lacked a solid basis. The supporting documents often did not say exactly what her written statements or conclusions presumed. Sometimes, no supporting documents existed. The consensus in the courtroom was that Dr. Laws presented a weak testimony.³

If I, an an observer, view the Laws cross examination in the manner that a movie-goer views a movie, it would be natural for me to side with the lawyers' assessment of Dr. Laws' testimony. At least for me, there is a natural and unwitting tendency to "get involved" when watching a movie. In making sense of the plot, I follow the events in the same way that the actors do. For example, upon my first reading of the Laws cross examination, I subconsciously adopted the prevailing logic, that is, the logic used by Mr. Powers, Mr. Copus, and the Presiding Examiner, who all appeared familiar with the ground rules of the situation, and I shared the pain experienced by Dr. Laws, who apparently had no inkling of what was going on.

As scientific observers, however, we need to observe in a manner more systematic than the passive empathy of the movie-goer. (That is, there is a limitation to the analogy in which we, as scientific observers of the AT&T case, are like members of an audience watching a movie.) In our case, we must also actively develop an understanding of Dr. Laws' testimony for the meaning that it had for Dr. Laws herself. Without this essential datum, our interpretive understanding of the subject matter would be seriously incomplete.

In addition to the transcripts of the cross examination, there is one other artifact that we may take advantage of in order to discern the meaning of the documents for Dr. Laws. It is her written testimony itself. She wrote in its opening:

First we will summarize the conditions of employment the Bell System provides for women employees (at both the Operator and management levels). Then we will examine these in the light of scientific theories and research on work motivation.⁴

After the summary of employment conditions, Dr. Laws stated:

So far we have discussed only the objective facts of conditions facing women within the Bell System. Such conditions affect work motivation and job performance in ways which are predictable from theories and research findings on organizations. [Dr. Laws, however, neither derives nor tests such predictions.] We will now review some of these in order to get a better understanding of the effects of Bell's policies on women employees.⁵

Dr. Laws' purpose was simply to use "the objective facts" about the concrete situation at AT&T to provide an illustration (rather than justification) of her social-psychological theories. She stated during her cross examination:

I am making application to the Bell System of general principles concerning what might be called theories of organization and the theories of work motivation and so forth.

It is part of my contribution to these proceedings that some applications of theories which have really been around for a period of time, the reserve labor force, for example, had not been made to women and the problems women experience ... so I think throughout my testimony, you will find back and forth, the kind of elucidation of theory with re-

ferences to where pieces of information from the Bell System fit in or where the Bell System fits into that kind of analysis.

[1362]

In order to illustrate general "scientific theories and research on work motivation", she applied them to the initial conditions or "the objective facts of conditions facing women within the Bell System", and wound up with an "elucidation of theory with references to where pieces of information from the Bell System fit in". In this way she could convey to the courtroom "a better understanding of the effects of Bell's policies on women employees". As additional excerpts of Dr. Laws' remarks will soon clarify, her purpose was the illustration, rather than the justification, of her theories.

Dr. Laws' game plan, of course, clashed head-on with the purpose of the lawyers. They did not envision themselves as being present merely to receive Dr. Laws' presentation of her understanding; they were present to take apart her understanding.

The lawyers released a barrage of questions, all aimed at establishing her testimony's validity. In response to the barrage of questions pummeling her, Dr. Laws repeatedly affirmed that the questions were inappropriate to her purpose. Her affirmations fell upon deaf ears in the courtroom (for reasons we will eventually turn to). However, as unpleasant the situation may have been for Dr. Laws, the situation is one that we as observers may take advantage of; it clarifies--sometimes by way of provoking Dr. Laws into expressing in her very own words--the purpose that she was proceeding with.

In her own words:

I would say [the written testimony] is an analysis of a problem illustrated with data as opposed to a study which is [a] specific research project undertaken to test [a] specific hypothesis.

[E4]

As such, illustration for Dr. Laws was a perfectly legitimate purpose:

... many studies are not trying to predict at all. They are descriptive studies ...

[E22]

Where illustration, rather than justification, is the purpose, even out-of-date (perhaps, "historical") data may be legitimately used:

... I make absolutely no pretense that I am presenting an up-to-date and complete statistical presentation of the Bell System ... I used data only illustratively.

[E15]

It is very significant this was done. [Dr. Laws is referring to the past practice of a sexist policy, no longer in force, at AT&T.] The significance would not be vitiated in any way if I put [the written testimony's description of the policy] in the past tense.

[E11]

What those citations are intended to do is to provide examples of the kinds of [employment] practices which are in violation of those statutes, or are questionable, and about which there is some controversy that is relevant to the argument here. It is not intended to demonstrate that each of these documents applied to 1970, rather, to give some substance to what is stated in general [in the written testimony].

[749-750]

In this light, what meaning did Dr. Laws attach to the documents? Given that her purpose was simply illustration, not justification, we may interpret her inattention to the documents as being an indication of their having only a secondary importance for her. Dr. Laws thus did not approach the documents in a careful or meticulous way, and it would not have made any difference to her if she had done so:

It does not matter to me whether I have 10 studies to cite when they fit into the specific argument or only one.

[E15]

I haven't been terribly specific about citing all of the documents ...

[E2]

As I said, I have read this report carefully and more than once and really base my use of it on a reading of the total document. So ... I would be hard put to say where in a given document a particular statement comes from.

[E17]

For Dr. Laws, the documents from which her research had drawn could be looked up and referenced, if the need were to arise:

... I said I cannot at this time specify particular footnoting of location. I did not say I could not ever. I did not refuse to take time in a recess, if it is absolutely essential, to look that up ...

[E18]

This last remark stands in sharp contrast to the legal meaning to the documents. For the lawyers, the documents were the very basis on which the validity of Dr. Laws' testimony rested; "footnoting of location" was therefore to be automatically provided in all instances, not just to be looked up, upon request, during a recess.

Behind the difference in purpose (illustration for Dr. Laws, justification for the lawyers) were the different meanings to justification. We may safely presume that Dr. Laws understood justification in the conclusion-oriented way typical of scientists; she displayed a comfortable grasp of hypothesis-testing and experimentation. As for her grasp of hypothesis-testing, she was familiar to the extent of knowing its limitations:

I would say [the written testimony] is an analysis of a problem illustrated with data as opposed to a study which is [a] specific research project undertaken to test [a] specific hypothesis. In fact, you really can't use data of this sort [that is, the numerical data in AT&T's in-house studies] to test a hypothesis really, because the data were collected for a different purpose.

[E4]

As for her grasp of experimentation, Dr. Laws even sounds a bit reminiscent of Nagel:

In general, in an experiment, you control a great many of the sources of variance that you are not prepared to measure. You manipulate only certain variables. There is no reason why it has to be done in the laboratory, in fact.

[E9]

In general, social psychology is an experimental science, one which, unlike

most of the social sciences, is even able to conduct controlled laboratory experiments. Given Dr. Laws' familiarity with hypothesis-testing, experimentation, and even the philosophy of test construction [E22], we observe that her understanding of justification was in conclusion-oriented, not premise-oriented, terms.

While Dr. Laws did not make justification her purpose, her understanding in conclusion-oriented terms nonetheless played a key role in setting up the miscommunication that unfolded between her and the lawyers. In her eyes, validity was something that was assessed through the procedures of hypothesis-testing and experimentation, not the prior scientific studies she happened to footnote in the body of her paper. By proceeding to evaluate her testimony through these footnoted studies, the lawyers succeeded only in talking past Dr. Laws; they were ascribing a function to the footnoted studies that did not appear on Dr. Laws' scientific map and that, therefore, were not cognizable by her (to coin a phrase parallel to one we have already heard, the legal meaning to the footnotes were not "scientifically cognizable"). In the following chapter, which analyzes the Quixotic dynamics of lawyer-scientist interaction, we will present diagrams to clarify the dynamics of the miscommunication. Here, we need only draw the observation that the different purposes--illustration for Dr. Laws, justification for the lawyers--did not neutralize the clash between science and law, but served only to obscure it with another layer of confusion.

Respecting both Dr. Laws' purpose and the purpose of the lawyers, we may practice "double vision" by painting two completely different pictures from many of the same words in the reprise of excerpts below. We may paint a picture in which Dr. Laws was admitting her mistakes and making up excuses

for a shoddy testimony, or we may paint a picture in which Dr. Laws was avowing, with no apologies and no excuses, the position she was taking. The former picture existed in the eyes of the lawyers; the latter, in the eyes of Dr. Laws. With regard to our purpose, which is to develop an understanding that Dr. Laws' actions had for Dr. Laws herself, and not just for the other actors on the scene, we need to maintain a sense of double vision in order to appreciate the realities of both pictures.

Also, the reprise of excerpts thoroughly demonstrates Dr. Laws' purpose as being illustration, as opposed to justification, and the meaning that she attached to the documents.

I haven't been terribly specific about citing all of the documents ... What I did was use standard bibliographic and reference form for materials which might not be familiar to members of the [Federal Communications] Commission or to the parties to the hearing ...

[E2]

I have written something for the ease of reading. That was the intention in the draft.

[E2]

What do you mean by discovery request?

[E3]

I would say [the written testimony] is an analysis of a problem illustrated with data as opposed to a study which is [a] specific research project undertaken to test [a] specific hypothesis.

[E4]

I think that in order to help you out I should probably annotate a copy of the testimony with reference to specific documents ...

[E5]

I have ... tried to avoid making statistical generalizations about the whole Bell System. The Chesapeake & Potomac Telephone study [an in-house AT&T study that Dr. Laws cited in her written testimony] is interesting and illustrative but I don't construe it as my job to reconcile in a statistical way this finding and I don't remember--if you say I cited it I must have cited it, I don't remember doing so with the C&P study ... I am interested in the C&P study because it focuses in on a problem for us ... [The] findings are so striking in this study that I am moved to ask a variety of questions about what is going on there. That is legitimate in the way I am using data in my report.

[E7]

Again, I can't say a great deal about documents which I have not read in their entirety [but which are documents that Dr. Laws cites in her written testimony] ... I can't say a great deal until I have reviewed those.

[E7]

Again, I am talking about documents whose exhibit numbers I cannot specify at this time.

[E8]

I have looked at, as I mentioned before, a great deal of material ... I will be glad to discuss those in detail when I have had the opportunity to review the documentary basis of the testimony.

[E10]

It is immaterial to me whether you want to lump [the citations]. They are part of the same argument.

[E13]

I would have to take time to go through the whole report to make sure not only that I found one appropriate citation as you have graciously helped me do but that I specifically specify all of them.

[E15]

... I make absolutely no pretense that I am presenting an up-to-date and complete statistical presentation of the Bell System. This is not the intention of my testimony. That is not the kind of expertise that I am here for, that I used data illustratively ... that is the case and I think I have made it explicitly for that.

[E15]

It does matter to me whether I have 10 studies to cite when they fit into the specific argument or only one. So, in fact, I have limited myself to this document.

[E15]

You are asking me to attest to an historical protest that I didn't keep documentary records ... As I said, I have read this report carefully and more than once and really base my use of it on a reading of the total document. So ... I would be very hard put to say where in a given document a particular statement comes from.

[E17]

I would like to enter an objection to that conclusion. It is very deeply offensive to me. I did not agree to any such thing. In fact, I said I cannot at this time specify particular footnoting of location. I did not say I could not ever. I did not refuse to take some time in a recess, if it is absolutely essential, to look that up, and I very much dislike the language of your assertion that I am unable to do so.

[E18]

... I would have to have time to study the document [cited in the written testimony], and I would be glad to answer the question in the detail that you seem to require ...

[E20]

In the light of her purpose of illustration (that is, in the light of

the meaning that Dr. Laws' actions had for Dr. Laws herself), Dr. Laws' responses, above, were all eminently reasonable, and not indicative at all of a poorly prepared written testimony. The quality of her research was good, as measured by the criteria she had chosen.

"I have written something for the ease of reading," Dr. Laws proclaimed early in the cross examination. "That was the intention in the draft" [E2]. With regard to the ease of reading, Dr. Laws was under no a priori obligation to cite any documents. The documents, and the data that they contained, were important to Dr. Laws insofar as they fulfilled her purpose, which was "to demonstrate the workings of some of the theoretical models that I deal with" [E1] and "to give a sense of numbers and concreteness to the argument ..." [E4]. For Dr. Laws, the documents were optional and interchangeable. They were not the indispensable basis that supported the conclusions or statements in her written testimony, which was their meaning for Mr. Powers, Mr. Copus, and the Presiding Examiner.

4.4 The Clash between Science and Law: The Disparate Meanings to the Footnotes

How did premise-oriented justification clash with conclusion-oriented justification in the Laws cross examination?

One grouping of artifacts that served as a focal point for the clash was the alpha-numeric symbols, contained between parentheses, which appeared in the body of Dr. Laws' testimony. The legal and scientific natives called these artifacts, "footnotes".

For a lawyer, a footnote in a legal memorandum generally signals a precedent or a statute, providing the basis that supports the conclusions to the argument. While scientists do not footnote prior scientific studies in their research reports for the same reason that judges footnote precedents in their opinions, we observe in the Laws cross examination that Mr. Powers, Mr. Copus, and the Presiding Examiner nonetheless mapped this legal function of footnotes onto Dr. Laws' usage of them. In the words of the Presiding Examiner himself, this is what he presumed the footnotes to mean:

... Dr. Laws ... you have in several instances cited specific documents and in some cases a single document by your parenthetical insertion of the reference supposedly as the basis for that statement or at least that is what I presume it to mean. Mr. Powers is entitled to test the validity of your conclusion in relation to the documents which you cite as the supporting documents.

[E20]

For the lawyers, the footnotes addressed the very purpose of the cross examination, which was to establish the validity of the expert testimony,

and hence "the facts" to which "the law" (particularly Title VII) would be applied.

An indication of the gravity of the footnotes' role in the eyes of the lawyers was the painstaking attention that they commanded from the lawyers. Consider especially Mr. Copus' insistence on detail; it is a meticulousness characteristic of the way in which lawyers, but not scientists, would approach footnotes:

MR. POWERS: ... Would you indicate, Dr. Laws, specifically where those citations should be referenced in the body of page three [in the written testimony]?

[Upon returning for the current session of the cross examination, Dr. Laws provided a new list of annotations ("citations") of the documents that she had read when preparing her written testimony.]

DR. LAWS: Line 18--well, the last sentence on page 3 (in the written testimony] carries over to the first line of page 4, and these 2 sets of citations which are located ... Are you with me?

MR. POWERS: ... I am not sure.

DR. LAWS: Do you want to go back?

MR. COPUS: Dr. Laws, would you specify which word on line 18 should there be a footnote symbol after, and the annotations that go with that footnote.

DR. LAWS: Which word--I am afraid I was not that punctilious.

[E12]

And it is for good reason that Dr. Laws was not that punctilious.

We need to place Dr. Laws' actions in the context that she was coming from. She had earned a Ph.D. in a social-science discipline. She held a joint appointment in two social-science departments in a university. She was a member of professional scientific organizations. (Dr. Laws spelled these things out at the beginning of the cross examination [actually, the *voire dire*], when Mr. Powers questioned her qualifications.) Dr. Laws carried with her, into the alien territory of the courtroom, her own culture-bound notions of logic and rationality; that is, notions from the scientific culture of which she was a product and to which she was a contributing member. She carried in her mind the scientific map (in which the theory is applied to the initial conditions, from which a hypothesis may be derived, which is then tested against experience, whether in a natural or laboratory experiment). It was natural, if not imperative, for Dr. Laws to associate validity with hypothesis testing [E4] and experimentation [E9], rather than with any prior scientific studies or other documents she may have happened to footnote in the body of her paper. The footnotes were thus pre-empted from conveying the crucial issue of validity to Dr. Laws.

What meaning, then, did the footnotes have for Dr. Laws?

Sometimes, Dr. Laws used footnotes to refer to AT&T's in-house studies or memoranda, simply for their illustrative value. In speaking to Mr. Powers, Dr. Laws stated:

The reason for footnoting [the in-house] memorandum as the one you have just cited, and I doubt very much if you would disagree with the logic of this, is that positions taken by top management are extraordinarily influential, rather more so than the same position expressed by someone at the lower level in the authority structure. It is very significant this was done.

[E11]

In an earlier instance, Dr. Laws explained to the Presiding Examiner and Mr. Powers, who were discussing her footnote to the Chesapeake and Potomac study:

The C&P Telephone study is interesting and illustrative ... if you say I cited it I must have cited it, I don't remember doing so with the C&P study because it focuses in on a problem for us ... the findings are so striking in this study that I am moved to ask a variety of questions about what is going on there. That is legitimate in terms of the way I am using data in my report.

[E7]

At some other times, Dr. Laws provided footnotes merely for the convenience they allowed the reader in locating the appropriate documents:

Would it be helpful to you in the [later] cross-examination on the testimony to have these items footnoted so that you can have reference to the document containing the data? ... If it would help you to have specific citations, I will be glad to give them.

[E2]

You are asking me questions about [the written testimony]. I think that in order to help you out I should probably annotate a copy of the testimony with reference to specific documents ...

[E5]

... I cannot at this time specify particular footnoting of location ...

[E18]

However, even late into the cross examination, Dr. Laws never caught on to the different meaning with which Mr. Powers, Mr. Copus, and the Presiding Examiner spoke of the footnotes. As already explained, mediating her understanding of the footnotes in particular, and the documents in general, was her scientific, conclusion-oriented approach to justification, which reserved the ultimate determination of validity for hypothesis testing and experimentation. Throughout the cross examination, the footnotes never had the same meaning for both Dr. Laws and the lawyers.

Because of the clash between the scientific culture and the legal culture, the scientific witness and the lawyers were never able to succeed in establishing a mutually meaningful dialogue.

There was one dynamic in the Laws cross examination that deserves special consideration. It was the eruption of the clash, from the dimension of the opposing logics of science and law, into the additional dimension of emotional contention. Exacerbating the clash, the emotional contention served to seal off any remaining possibility for establishing a dialogue. The best examples of contention focus on Dr. Laws:

It is possible, for example, [if] I might anticipate your strategy in cross examination, you might take me through this testimony word by word or you might do it paragraph by paragraph. If I am lucky, you will do it page by page. I am not really sure.

[E13]

I think I have stated before I was willing, since in my testimony I make absolutely no pretense that I am presenting an up-to-date and complete statistical presentation of the Bell System. This is not the intention on my testimony. That is not the kind of expertise that I am here for, that I used data only illustratively ... that is the case and I think I have made it explicitly for that. It does not matter to me whether I have 10 studies to cite when they fit into the specific argument or only one ... I don't think I need to go back over the whole set of force loss studies ...

[E15]

I knew I didn't make it up.

[E15]

You are asking me to attest to an historical protest ...

[E17]

I would like to enter an objection to that conclusion. It is very deeply offensive to me. I did not agree to any such thing ... I very much dislike the language of your assertion ... I don't believe I said anything of the kind.

[E18]

The contention is mirrored in both Mr. Powers' reaction:

I am not engaged at the moment in a direct argument with you. I am just trying to clarify what you have presented ...

[E7]

and the Presiding Examiner's attempts at mediation:

Don't encourage such an appalling adventure.

[E13]

I think the difficulty is, Dr. Laws ...

[E15]

You are giving dissertations on all his questions as a whole and it is not very helpful. Try to clarify your question, Mr. Powers.

[E19]

The problem is, Dr. Laws ...

[E20]

In addition to the contention, which was a product of the clash between the cultural forces (consisting of Dr. Laws' scientific map and the lawyers' legal map), the institutional forces of the courtroom reinforced the miscommunication.

As a member of the legal culture, the Presiding Examiner was not qualified to act as a neutral party that could clarify the scientific witness' role or testimony; in fact, the Presiding Examiner actually enforced the cross-examining lawyer's imposition of the premise-oriented mode of justification. In addition, we also find that the EEOC lawyer was guilty of complicity; in going along with the cross-examining lawyer and the Presiding Examiner, he actually increased the peer group pressure that mounted against Dr. Laws and that, as a result, obfuscated the meaning that she intended to convey with her testimony.

To summarize, the miscommunication that we observe in the Laws cross examination is consistent with, and hence corroborates, the existence of our theory's hypothesized cultural and institutional forces. The predicted pattern of behavior--the clash between opposed modes of justification--persists

even though we observe (1) a different scientist who was applying (2) a different theory to (3) a different set of facts, all of which came under the scrutiny of (4) different lawyers, whose collective attention was focused on (5) a different portion of the scientific territory.

Chapter 5 Refining the Theory of Scientist-Lawyer Communication: Don Quixote and Sancho Panza

5.1 Refuting and Improving the Theory

Alfred Schutz is the author of a provocative article, "Don Quixote and the Problem of Reality". The article opens in a wistful way. Seemingly for the sheer amusement of doing so, Schutz entertains, as being real, a fictional character whose curious adventures pose a challenging puzzle for sociological explanation. As Schutz's analysis proceeds, however, we come to feel more so humbled than entertained. It dawns on us that, for Schutz (no less Cervantes), Don Quixote is but a model for each of us in our everyday lives, illuminating how we actually talk past one another when we are under the impression of communicating, and also, how we actually live in separate, individual worlds when we are under the impression of all living in the same world.

Schutz's Don Quixote speaks to two issues in our study of the EEOC-AT&T litigation.

First, we might wonder how, in general, scientists and lawyers are able to talk to one another at all. If the theory is true, that the two groups speak distinct and dissimilar languages, neither of which (like any other language) can be understood apart from the culture of the group that it comes from, then, by pure deduction, we conclude or predict that, in the EEOC-AT&T litigation, all communication between the two groups should be precluded.

Of course, observations from the case study summarily refute the prediction (thereby indicating that a portion of the theory is inaccurate and in need of reformulation). While the scientific expert witness was typically un-

able to make his testimony understood (at least for its scientific meaning) to the lawyer (whether the cross-examining lawyer, the scientist's sponsoring lawyer, or the Presiding Examiner), the scientist and the lawyer nonetheless exchanged words and, presumably, meanings of some sort. There were questions, followed by answers. Somehow, the scientist and the lawyer made sense of each other. Surely, the 8,000 pages of transcripts are an artifact of something. What is it?

"How is it possible," writes Schutz, "that the private world of Don Quixote," who is rooted in his own private and imaginary world, "is not a solipsistic one, that there are other minds" like the mind of Sancho Panza, who is rooted in the world of common sense, "within this reality, not merely as objects of Don Quixote's experience, but sharing with him, at least to a certain extent, the belief in its actual or potential reality?" (Emphasis added.)¹ I leave it to the reader to decide which one--the scientist or the lawyer--is Don Quixote, and which is Sancho Panza.

Second, we might wonder how it was possible that the scientists and the lawyers in our case study, who were likely as well educated and intellectually gifted as are we, the observers, did not come to realize what we are realizing in this study. Indeed, their vantage point even offered them a direct view of the full events, whereas we may only reconstruct the events through the intervening medium of the transcripts, and even if then, only partially. If the theory is true, that scientists and lawyers speak distinct and dissimilar languages, then, by pure deduction, we may predict that the dissimilarity should become readily apparent to them upon their interaction, in the same way that a speaker of Chinese and a speaker of English discover

immediately and effortlessly their own dissimilarity. Again, observations from the AT&T case refute our prediction. The scientists and the lawyers spoke to each other as if under the impression that they all shared the same frame of reference--that is, that they were all speaking the same language.

"How does it come that Don Quixote can continue to bestow the accent of reality on his sub-universe of fantasy if it clashes with the paramount reality in which there are no castles and armies and giants but merely inns and flocks of sheep and windmills? ... How does Don Quixote succeed in maintaining the belief in the reality of the closed sub-universe once chosen as the home base in spite of the various eruptions of experiences which transcend it?"²

We start off this chapter, therefore, with an admission of error. In the original formulation of our theory, our assertion was literal, not figurative, that science and law are languages; this assertion is incorrect. Whereas "speakers" of science and law do recognize some difficulty in communicating with each other, as do speakers of Chinese and English, scientists and lawyers nonetheless succeed in communicating with each other to a greater extent than do monolingual speakers of Chinese and English. Scientists and lawyers enjoy at least the possibility of talking past each other, while under the impression of communicating. How is this sort of communication--Quixotic communication--possible?

Our purpose here is not merely to observe the fact that Quixotic communication occurs between the scientific expert witness, on the one hand, and the plaintiff lawyer, defendant lawyer, and judge, on the other hand. The observa-

tion would hardly be an original one. Instead, our purpose is to uncover the elements in the observed situation that make Quixotic communication possible. By diagnosing what in fact happens, we will by laying the foundation to a prescription for how it may happen better (in particular, how scientists and lawyers may become "bilingual" and "bicultural", the comparison to language being figurative). The prescription will be presented toward the end of this study.

We will first present a demonstration, consisting of exchanges from the cross examination, of the Quixotic communication that took place between Dr. Laws and the lawyers. Second, we will posit two conditions that serve to sustain Quixotic communication, using exchanges from both the Laws and Ashenfelter cross examinations to provide illustrations. Third, going beyond an application of Schutz's analysis, we will reformulate the dynamics of Quixotic communication in terms of maps and territories. Finally, we will summarize the chapter by tying in the idea of self-sealing, metaphysical mental constructs, which we first mentioned when explaining Karl Popper's demarcation criterion.

5.2 The Phenomenon of "Talking Past"

Dr. Laws, on the one hand, and Mr. Powers, Mr. Copus, and the Presiding Examiner, on the other hand, expressed in their very own words their respective meanings to the documents and their respective purposes in being present. As surely as we are able to read from the transcript what each side had to say, we can be sure that each side heard the other express its opposing meaning and opposing purpose. Mysteriously and Quixotically, however, each side then proceeded as if it had not heard the other side speak, that is, as if under the impression that the other side shared the same frame of reference. In this section, we provide examples of how Dr. Laws and the lawyers talked past each other. The reader is forewarned that we will be drawing our observations from many of the same exchanges that appeared in prior chapters.

The Lawyers: Talking Past Dr. Laws

The lawyers articulated repeatedly the importance with which they viewed the documents and the purpose they had in being present. They made their position obvious.

Rather than beat a dead horse by re-printing all the quotations already given in the previous chapter, we give three short, but representative, examples of the documents' importance to the lawyers.

MR. COPUS: Mr. Examiner, as we indicated earlier, we will provide the phone company with a complete annotation of all the documents which provided to Dr. Laws, with specific annotations to the parts of her testimony as they relate to those documents.

[E5]

MR. POWERS: ... it is important to me in terms of reviewing the annotations to have shown very specifically what these documents are cited for.

[E13]

PRESIDING EXAMINER: ... Dr. Laws, you have given us one specific citation for the statement. I think if you have others you should locate them and advise us.

[E13]

Yet, if the lawyers spoke plainly enough so that we, as observers, are able to understand what they meant, then why was Dr. Laws unable to understand? Dr. Laws never came to accept the importance in either the Presiding Examiner's direction to her, "I think if you have others you should locate them and advise us" or Mr. Powers' avowal of his purpose, "it is important to me ... to have shown very specifically what these documents are cited for." These and countless similar remarks never did "sink in" for Dr. Laws. Instead, she persisted in denying the importance of the documents; for example:

It does not matter to me whether I have 10 studies to cite when they fit into the specific argument or only one.

[E15]

With this statement, Dr. Laws succeeded in contradicting even the direction that her own lawyer--Mr. Copus--had provided her (see directly above, [E5]). Denying signals from all three lawyers about what was important in the

courtroom, Dr. Laws in effect sealed herself off from the people around her, enclosing herself in her own private reality.

What was Dr. Laws' reality? What was Dr. Laws' conception of what the lawyers were asking for? In her own words:

... you might take me through this testimony word by word or you might do it paragraph by paragraph. If I am lucky, you will do it page by page. I am not really sure.

[E13]

I would have to take time to go through the whole report to make sure not only that I found one appropriate citation as you have graciously helped me do but that I specifically specify all of them.

[E15]

I would be better pleased, and I guess Mr. Powers would too, if I would review it [the document] exhaustively and make sure you have as many page citations as are relevant within this document.

[E15]

You are asking me to attest to an historical protest that I didn't keep documentary records ...

[E17]

... the kind of question that you have continually put to me is where in a document of some 50 pages do you find wording that supports that assertion ...

[E20]

In Dr. Laws' private world, the lawyers were asking her to provide more and more citations to more and more documents, for no purpose that was apparent to her. To Dr. Laws, the lawyers' fixation on the documents were nonsensical; after all, it did not matter to her whether she had "ten studies to cite when they fit into the specific argument or only one."

Our observation is that Dr. Laws succeeded in maintaining her private belief in what was important and what was nonsensical, despite the explicit contradictions offered to her by not only the cross-examining lawyer, but also the Presiding Examiner as well as her own lawyer.

Now, given the observation that Dr. Laws behaved in a certain way, what is the explanation for how such behavior was possible? That is, how did her private reality survive despite the various irruptions of experiences transcending it? We will return to this concern after observing this same phenomenon in the lawyers' behavior, and asking the same question of their situation.

Dr. Laws: Talking Past Mr. Powers, Mr. Copus, and the Presiding Examiner

Dr. Laws articulated repeatedly the purpose she had in being present and the lack of importance with which she viewed the documents. Examples:

I have written something for the ease of reading. That was the intention in the draft.

[E2]

I haven't been terribly specific about citing all of the documents.

[E2]

I would say [the written testimony] is an analysis of a problem illustrated with data ...

[E4]

I am interested in the C&P study because it focuses in on a problem for us ... That is legitimate in terms of the way I am using data in my report.

[E7]

... I used data only illustratively ... that is the case and I think I have made it explicitly for that.

[E15]

[I] really base my use of it on a reading of the total document.

[E17]

Yet, if Dr. Laws spoke plainly enough so that we, as observers, are able to understand the casual and illustrative way in which she meant to use the documents, then why were Mr. Powers, Mr. Copus, and the Presiding Examiner unable to understand? Why did Dr. Laws' avowals never "sink in" for them?

What was the lawyers' reality? What did they understand Dr. Laws to be saying and doing?

MR. COPUS: ... I think what the witness said was that she based her findings on not only the exhibit ... but she also based her analysis on the considerable examination of an original Bell document ["The Houston Study"] ... If you request we will provide you with a list of the documents on which she relies.

[E2]

MR. POWERS: ... we believe the witness has failed to show any statements in this document that support her assertions.

[E18]

PRESIDING EXAMINER: You are giving dissertations on all his questions as a whole and it is not very helpful.

[E19]

PRESIDING EXAMINER: The problem is, Dr. Laws, that you have in several instances cited specific documents and in some cases a single document by your parenthetical insertion of the reference supposedly as the basis for that statement [in the written testimony] or at least that is what I presume it to mean.

[E20]

The lawyers never did accept, much less address, the simple purpose of illustration, for which Dr. Laws repeatedly avowed she was using the documents and the data that they contained. The lawyers instead persisted in believing that Dr. Laws was approaching the documents in the way that they considered important. Dr. Laws' assertions of her own purpose just did not make sense to the lawyers; to them, it sounded as if she had been giving dissertations on all their questions.

While the lawyers described Dr. Laws as doing one thing, she was actually doing another, as she spelled out clearly and repeatedly.

How was it possible for the lawyers to maintain their belief that Dr. Laws was relying on statements from the documents as the basis to support the validity of her testimony's conclusions, despite her repeated denials that the documents mattered to her in this way? How was it possible that the lawyers' version of reality survived despite the various irruptions of experiences transcending it?

5.3 The Condition of the Existence of Supporting Evidence and/or No Contradictory Evidence

Schutz writes:

"Under what circumstances do we think things real?" William James asks this question in one of the most remarkable chapters of his Principles of Psychology and starts from there to develop his theory of various orders of reality. Any object, so he finds, which remains uncontradicted is ipso facto believed and posited as absolute reality. [Emphasis added.³]

On the first short expedition Don Quixote is alone [that is, without Sancho Panza] ... Don Quixote remains undisturbed master in his subuniverse; he is not refuted by the behavior of his fellow-men who, as Cervantes states, "fall in with his humor" ... Nothing and nobody ... starts a quarrel by saying something inadmissible which would contradict the experience held by Don Quixote to be true. [Emphasis added. ⁴]

As we will scrutinize in the next section, Dr. Laws and the lawyers certainly did say things which were contradictory to the other's notion of reality--that is, the notion of what was happening in the cross examination. However, this should not cloud our observations of other things that each side said, which did not contradict the other side's reality.

One condition conducive to Quixotic communication, which Schutz puts forward and which I embrace, is that there are instances in which the respective realities go uncontradicted. Furthermore, in addition to the absence of contradictions, there may be the presence of actions that, however unintentionally, have the effect of supporting the other reality.

A subtle and simple way by which this may unfold is that the scientist, by the mere act of obliging the lawyer's question, perpetuates and rein-

forces the lawyer's reality. It is not pertinent that the scientist's response is favorable or unfavorable, as judged by the lawyer; what is pertinent is merely that the scientist provides a response, for in doing so, it appears to the lawyer that the scientist is operating in the same frame of reference. The phenomenon of "talking past one another" thereby proceeds, with no contradictions at the moment to derail it.

An insightful illustration that crystallizes the salient aspects on how something "which remains uncontradicted is ipso facto believed and posited as absolute reality" is an amusing exchange from the musical, Fiddler on the Roof. I present this exchange for the purpose of illustrating the explanation in the prior paragraphs. I then follow this with two exchanges from the Ashenfelter cross examination and six from the Laws cross examination to illustrate how, under the condition of "no contradictions", the Quixotic phenomenon of "talking past" was sustained between the scientists and the lawyers in the EEOC-AT&T litigation.

From Scene Three (page 26)⁵

GOLDE: Tevye, I have to tell you--

TEVYE: Shh. I'm praying. (Prays.)

GOLDE-- (Having waited a moment.): Lazar Wolf wants to see you.

(TEVYE begins praying again, stopping only to respond to GOLDE, then returning to prayer.)

TEVYE: The butcher? About what? (Prays.)

GOLDE: I don't know. Only that he says it is important.

TEVYE: What can be important? I have nothing for him to slaughter. (Prays.)

GOLDE: After the Sabbath, see him and talk to him.

TEVYE: Talk to him about what? If he is thinking about buying my new milk cow (prays) he can forget it. (Prays.)

GOLDE: Tevye, don't be an ox. A man sends an important message, at least you can talk to him.

TEVYE: Talk about what? He wants my new milk cow! (Prays.)

From Scene Four (pages 32-33)

(TEVYE enters. LAZAR, who has been watching the door, turns away, pretending not to be concerned.)

...

TEVYE: ... Good morning, Reb Lazar.

LAZAR: Ah, Tevye. Sit down. Have a drink. (Pours a drink.)

...

[Lazar's purpose is to ask Tevye for the hand of one of his daughters in marriage.]

LAZAR: Tevye, I suppose you know why I wanted to see you.

TEVYE (Drinks): Yes, I do, Reb Lazar, but there is no use talking about it.

LAZAR (Upset): Why not?

TEVYE: Why yes? Why should I get rid of her?

LAZAR: Well, you have a few more without her.

TEVYE: I see! Today you want one. Tomorrow you may want two.

LAZAR (Startled): Two? What would I do with two?

TEVYE: The same as you do with one!

LAZAR (Shocked): Tevye! This is very important to me.

TEVYE: Why is it important to you?

LAZAR: Frankly, because I am lonesome.

TEVYE (Startled): Lonesome? What are you talking about?

LAZAR: You don't know?

TEVYE: We're talking about my new milk cow. The one you want to buy from me.

LAZAR (Stares at TEVYE, then bursts into laughter. TEVYE stares at him.)

TEVYE: What's so funny?

LAZAR: I was talking about your daughter. Your daughter, Tzeitel! ...

While Tevye and Lazar enjoy the good fortune of discovering the contradiction between their viewpoints, the scientist and the lawyer in the courtroom are not always so lucky. Instead, their Quixotic communication proceeds without being derailed, so that the same object appears as a cow to one discussant but as a daughter to the other discussant.

Two Exchanges with Dr. Ashenfelter

We now turn to two exchanges, presented earlier, from the Ashenfelter cross examination. Like in the exchange between Tevye and Lazar, the same words carried different meanings to the different speakers, thereby allowing their opposing notions of reality not only to co-exist, but also, insofar as no contradictions became evident, to remain mutually invisible.

1. Assumption about Men and Women Being Equally Productive

PRESIDING EXAMINER: Mr. Levy, may I interrupt here, I am intrigued by a sentence in the second sentence [sic] on page three [in Dr. Ashenfelter's written testimony, "Telephone Rates in the Absence of Discrimination"]

about men and women being equally productive.

MR. LEVY: You have stolen two hours of cross examination, Mr. Examiner.

PRESIDING EXAMINER: This sounds like a revelation. Kidding aside, did you jump over that?

MR. LEVY: Not a chance.

[The passage being discussed is:

An examination of the Bell Telephone Company's employment data reveals that there are a number of occupations in which women comprise a very small fraction of the work force. One explanation for this phenomenon is that, although men and women are equally productive, the latter suffer from a desire on the part of management not to employ women on the same terms as men, i.e., discrimination. Since male and female workers are not being employed entirely in relation to their true productivities, a loss of overall efficiency results ...]

[Later in the cross examination:]

MR. LEVY: Respecting the parenthetical clause, "although men and women are equally productive," is that stated as a second or supplemental hypothesis, or is that an assertion of fact?

DR. ASHENFELTER: No, it is meant to be a part of the hypothesis.

...

MR. LEVY: Have you made any in-depth analysis of the relative productivity of men as a group and women as a group in Bell occupations?

DR. ASHENFELTER: No, I have not.

MR. LEVY: Are you familiar with data relating to weightlifting or poleclimbing abilities and productivity of women as a group versus men as a group?

DR. ASHENFELTER: I am not really familiar with that, no.

MR. LEVY: Are you familiar with work by motivational psychologists relating to whether women, when confronted with male-role opportunities, seek to avoid success?

[Dr. Ashenfelter responds negatively.]

MR. LEVY: ... Have you considered, Dr. Ashenfelter, the possibility of significant differences between men and as a group and women as a group on such things as verbal ability, perception of detail, or numerical or mechanical computation in positing this corollary to your hypothetical of equal productivity?

DR. ASHENFELTER: If you mean have I tried to determine whether or not it is true that men and women are equally productive in the jobs involved, the answer is, I have not.

[1560-1562]

MR. LEVY: ... I have previously asked you about possible physiological or psychological differences, including job preferences between men as a group and women as a group. Looking at the numerical constructs and equations that begin in the middle of page 4 of your testimony, is it

correct that those considerations are not taken into account in your numerical constructs and that those constructs are directed solely to the theoretical assumptions that all differences in male-female jobs is due to employer job discrimination?

[The last of the three equations is crucial to Dr. Ashenfelter's testimony. It is his means of providing estimates of the percentage by which telephone rates would decline, if discrimination were to disappear. Dr. Ashenfelter's response has to do with traditional assumptions made about economic behavior.]

PRESIDING EXAMINER: This goes again back to your hypothesis that the productivity is equal?

DR. ASHENFELTER: Yes, that is right. It assumes the productivity is the same.

MR. LEVY: I take it while you did not directly answer, it is implicit in your answer that that was the assumption or hypothesis you proceeded under, and you did not reflect in your numbers anything for societally instilled job preferences or biological differences between men as a group and women as a group, or physiological differences between men as a group and women as a group?

DR. ASHENFELTER: It is correct ...

[1568-1570]

Each of the six responses that Dr. Ashenfelter provided to Mr. Levy was quintessentially Quixotic. Dr. Ashenfelter's assertions that he had

not actually determined whether men and women are equally productive commanded a certain significance--most likely, a damaging one--from the viewpoint shared by Mr. Levy and the Presiding Examiner ("You have stolen two hours of cross examination, Mr. Examiner"), but had only an innocuous meaning for Dr. Ashenfelter (who, being a scientist, defined the veracity of his theory as being known through its predictions, not its assumptions). As the exchange unfolded, Dr. Ashenfelter's responses, which happened to be favorable to Mr. Levy's line of questioning, actually served to encourage and reinforce, rather than merely fail to contradict, Mr. Levy's reality.

2. Assumption about the Production Function

MR. LEVY: Further on page 4, Doctor, you asked us to suppose that the process whereby Bell produces its services can be approximated by using a Cobb-Douglas production function ...

[1571]

...

MR. LEVY: Is a Cobb-Douglas production function a linear, homogeneous function?

DR. ASHENFELTER: Yes, that is correct.

MR. LEVY: If it were not first-order homogeneous, would Equation No. 2 be derivable in your formulations?

[Equation No. 2 is a formula for calculating the price of AT&T's goods and

services. Equation No. 1 is the Cobb-Douglas production function.]

DR. ASHENFELTER: I forgot to add one important point ... I would like to add parenthetically, for people who don't know what [linear homogeneity] means--

PRESIDING EXAMINER: Most of us.

MR. COPUS [a lawyer for the EEOC]: Perhaps your testimony could be in parenthesis, and this will be the main part.

[Dr. Ashenfelter explains linear homogeneity.]

MR. LEVY: Getting back to my question--and I am not sure it has been answered--I asked, if the production function you used was not first-order homogeneous, Equation No. 2 could be quantified, or would a numerical equation be derivable? ...

DR. ASHENFELTER: No, it could not be.

PRESIDING EXAMINER: Is this a convenient stopping point, Mr. Levy?

MR. LEVY: All right, Mr. Examiner.

[1572-1574]

MR. LEVY: Did you make any investigation to determine that [the Cobb-Douglas] production function was realistic as applied to Bell?

DR. ASHENFELTER: No ...

MR. LEVY: If the Bell Sytem ... did not fall within the model of a linear

homogeneous production function, would that in fact be an appropriate model to use?

[Dr. Ashenfelter responds affirmatively, but provides qualifications.]

MR. LEVY: Doctor, why didn't you choose to use other professionally recognized functions such as VES, variable elasticity of substitution?

[Dr. Ashenfelter gives his reasons.]

MR. LEVY: Had you used VES, could you have derived equations comparable to [your] equations at the steps where you have developed your equations 2 and 3?

MR. ASHENFELTER: Equation 3, which is the one we used to directly calculate the effect of a reduction or elimination of discrimination [on] telephone rates would be the same with the VES production function if it were homogeneous of degree 1.

[Dr. Ashenfelter elaborates his answer.]

MR. LEVY: Then your answer to my question is you did not investigate this?

DR. ASHENFELTER: That is correct.

...

MR. LEVY: Is that equation [No. 2] based on equation 1?

MR. ASHENFELTER: Yes.

...

MR. LEVY: Is your third equation on page 6 [Equation No. 3] derived from equations 1 and 2?

DR. ASHENFELTER: ... yes, it can be derived from equations 1 and 2, but it can also be derived from less restrictive assumptions.

MR. LEVY: In this instance, from what was it derived?

DR. ASHENFELTER: That is not a very meaningful question because I mean it exists and [the written testimony] states it comes from equations 1 and 2, but as I have said, it can also come for a less specific formulation of the problem.

[1560-1580]

When Dr. Ashenfelter responded (1) that he had not determined whether the Cobb-Douglas production function was realistic as applied to Bell and (2) that he had not investigated whether the use of the VES production function would have made a difference, he was providing responses which had a particular meaning for Mr. Levy. (We know that the responses had a particular significance for Mr. Levy because he solicited them: "Did you make any investigation to determine that [the Cobb-Douglas] production function was realistic as applied to Bell?" and "Then your answer to my question is you did not investigate this?") We may safely presume that Mr. Levy looked upon Dr. Ashenfelter's negative responses as being admissions of shoddy or incomplete work.

From Dr. Ashenfelter's viewpoint, however, the use of the Cobb-Douglas production function, rather than the VES production function, had no bearing, favorable or unfavorable, on his depiction of employment discrimination at AT&T. To him, his prediction or conclusion as to the "effect of a reduction or elimination of discrimination [on] telephone rates would be the same with the VES production function." Thus, his subsequent response ("That is correct," that he had not investigated whether the use of the VES would have made a difference) was innocuous, insignificant, and inconsequential to himself, even though it was important (even favorable) to Mr. Levy.

The salient point in these two exchanges is that Dr. Ashenfelter's responses did not pose any contradictions to Mr. Levy's reality. Indeed, Dr. Ashenfelter's responses were at times favorable to it, thereby encouraging it and further sanctifying it. Through social interactions in which contradictions did not arise, the simultaneous construction of separate realities was possible and realized.

Six Exchanges with Dr. Laws

While relatively more contradictions and argumentation characterized the Laws cross examination, there were still numerous instances in which Dr. Laws performed actions wholly consonant with the lawyers' frame of reference. We observe in the following two exchanges that Dr. Laws actually provided additional annotations/citations/footnotes, thereby confirming and reinforcing the importance with which the lawyers viewed the documents.

Exchange i

MR. POWERS: ... Dr. Laws, you indicated [at an earlier session] you would provide us with a list of the documents furnished to you by the EEOC and certain additional citations to those documents supporting your testimony. I understand from Mr. Copus and Mr. Gartner that you were furnished the following documents:

First, a set of preliminary EEOC staff findings which are an earlier version of the summaries that were proffered in Exhibit 2, Appendix C, and I would invite the confirmation of EEOC counsel as well as the witness, because, as I understand it, this was a joint determination that they made.

Second, that certain statistical charts and tables were provided.

Third, that all of the turnover studies furnished by AT&T in response to the discovery requests of the Commission were provided and, fourth, all other documents cited in your testimony and in the list of additional citations were also made available.

I ask you and EEOC counsel if that is the complete list of the documents you were given in connection with the preparation of your testimony?

DR. LAWS: There is an additional group of documents that you have not referred to specifically, having to do with current and recent recruiting materials used within the Bell System. If you are interested in what document numbers those are, I can give you a list at this time.

Would that be helpful?

MR. POWERS: I think it would be.

DR. LAWS: Z-649, W-546, R-689, C-487, M-447, W-578, R-643, Y-92 and Y-93;
Z-644, T-110, C-491, M-443, M-440, R-628, R-612, Z-679, Z-691, R-1295,
Z-686, C-515, C-516. I think that is all.

[1318-1319]

Exchange ii

MR. POWERS: Dr. Laws, a list of annotations to your testimony was supplied to us by the EEOC. Do you adopt that list of annotations as your own?

DR. LAWS: I would like to see it, please. ... Yes, this is it.

[There is discussion between the Presiding Examiner and the EEOC lawyer over whether the list is complete.]

PRESIDING EXAMINER [to the EEOC lawyer]: The difficulty is, if the request was for a list of the appropriate annotations to her testimony, it ought to be complete. I don't think we are only dealing with the question of it being complete or do you want to do that now?

DR. LAWS: It is complete. There are no additions ...

[1324-1325]

Dr. Laws' responses are excellent illustrations of Quixotic communica-

cation. Her very action of naming the documents ("Z-649, W-546, R-689, ..., C-516. I think that is all") made perfect sense within the lawyers' schemes of interpretation, whether the lawyer was Mr. Powers, the Presiding Examiner, or Mr. Copus. We may review, in their own words, each lawyer's scheme of interpretation:

I think we are entitled to determine at this time what [the] basis is for that conclusion [in the written testimony] in terms of the review of documents. I gather that it is clear that it is simply a matter of review of documents for that conclusion.

[Mr. Powers (E5)]

Mr. Powers is entitled to test the validity of your conclusion in relation to the documents which you cite as the supporting documents.

[The Presiding
Examiner (E20)]

We will certainly provide you with that list [of documents which was the basis of Dr. Laws' study].

[Mr. Copus (E2)]

In citing the twenty two documents, Dr. Laws waltzed right into the lawyers' reality. This again happened in exchange ii, where Dr. Laws affirmed Mr. Powers' reality ("Yes, this is it," she said, in obliging Mr. Powers' question, "Do you adopt that list of annotations as your own?") as well as the Presiding Examiner's reality ("It is complete," she said in obliging his concern about the list). Her responses posed no contradictions

to the lawyer's notion that the documents were the basis supporting the validity of her testimony's conclusions.

None of this is to say that Dr. Laws ever "gave in" to the lawyers' viewpoint. For her, her actions of providing additional citations of documents was a meaningless exercise, having no major or minor importance. (We know this because it was during a subsequent exchange when she re-asserted: "It does not matter to me whether I have 10 studies to cite ... or only one.") One reason that she consented to performing the exercise was the peer group pressure. Another reason, as already explained in the previous chapter, was Dr. Laws' self-image of providing citations merely for the convenience that they allowed the reader in locating documents that, she believed, were important for their illustrative value.

The tension between her meaning to the documents and the lawyers' meaning did sometimes break through, allowing us to observe what she meant, as in the next exchange:

Exchange iii

MR. POWERS: Would you indicate, Dr. Laws, specifically where those citations should be referenced in the body of page three [in the written testimony]?

[Upon returning for cross examination, Dr. Laws provided a new list of annotations of the documents ("citations") from which she formulated her written testimony.]

DR. LAWS: Line 18--well, the last sentence on page three [in the written

testimony] carries over to the first line of page four, and these two sets of citations which are located ... Are you with me?

MR. POWERS: ... I am not sure.

DR. LAWS: Do you want me to go back?

MR. COPUS: Dr. Laws, would you specify which word on line 18 should there be a footnote symbol after and the annotations that go with that footnote.

DR. LAWS: Which word--I am afraid I was not that punctilious.

MR. COPUS: There may be some question about line 18 ...

DR. LAWS: The sentence I am talking about, and I don't think I want to break it down any further, is ...

[E12]

In asserting, "Which word--I am afraid I was not that punctilious," Dr. Laws was letting us know that she (still) did not attach the same significance to the documents as the lawyers did (despite her going through the motions of providing additional citations to documents). For Dr. Laws, her actions of indicating "specifically where those citations should be referenced in the body of page three [of her testimony]" were empty of the meaning that they signified to the lawyers. But, as in the other exchanges just cited, Dr. Laws went through the motions nonetheless: "The sentence I am talking about, and I don't think I want to break it down any further, is ..." Again, Dr. Laws

posed no contradictions to the lawyers' notion that the documents were the basis supporting the validity of her testimony's conclusions.

The following two exchanges are illuminating for what they reveal about Dr. Laws' acclimation to the courtroom environment. By this point, she caught on to the fact that the lawyers were requiring citations from her (although, in subsequent instances where she was provoked, she revealed that her own meaning to the documents was still unchanged), and so she went through the motions of providing the citations.

Exchange iv

MR. POWERS: Dr. Laws, in connection with the revision that you have made of the top paragraph on page four [in the written testimony], as I understand it, you are now citing in support of your new first paragraph not only the documents R-986, W-223 and Z-712, but also those documents which I think you listed on the record of current and recent recruiting materials and which are set forth in ... the document which the EEOC supplied us; is that correct?

DR. LAWS: ... If I may, I think then what I want to do is to amend this testimony to include reference only to the following documents: Z-679, Z-619, R-1295, Z-686, C-515, C-516 ...

MR. POWERS: Therefore, all of the citations initially appearing at the end of the first full paragraph on page four are to be stricken from your testimony?

DR. LAWS: Yes, the whole first paragraph has then been replaced and I think the most efficient thing to do would be to include these citations at the conclusion of the two paragraphs that replace those.

PRESIDING EXAMINER: Those you read are a replacement for the remaining three citations?

DR. LAWS: Yes.

PRESIDING EXAMINER: Shall we strike those?

DR. LAWS: Yes.

MR. POWERS: Mr. Examiner, we will want to take a look at these six brochures during the break. We may want to come back to that.

PRESIDING EXAMINER: I am sure that will be much more enjoyable than lunch.

[1338-1340]

Exchange v

MR. POWERS: ... let's move to page seven. Here, Dr. Laws, you state ["The prospects for women's movement into and through management levels are bleak. The sex segregation in recruiting for the entry level positions (for Craft and Traffic jobs) is perpetuated in management"] and I believe the reference that is shown for that statement is document R-1033, a statement from an AT&T Vice President, dated May 12, 1965 ...

...

I ask you if you are aware that that document was officially canceled by AT&T on November 13, 1968, 3 1/2 years ago, and that the canceling document was furnished to the EEOC in discovery under its request D-9?

DR. LAWS: No, I am not aware of any cancellation.

[There is discussion among Mr. Gartner, who is an EEOC lawyer, Mr. Powers, and the Presiding Examiner over the citation to document R-1033.]

DR. LAWS: If I may, as long as it is my conclusions that are being talked about, I wouldn't like to leave the impression that there are not other data in support of those conclusions that are part of this record.

I would direct the attention of people in this hearing to pages 66 through 127 of Exhibit 2, the Charts and Tables, Appendix A ...

PRESIDING EXAMINER: You made a good recovery, Doctor.

[E14, 1341-1344]

By voluntarily amending her citations and by complacently accepting the Presiding Examiner's request to "strike" three of them, Dr. Laws gave no indication at all that she attached a contradictory meaning to the documents. Her publicly observable actions conformed to what the lawyers expected of her; she "behaved" according to their framework. And in substituting one document for another that the lawyers deemed to be inadequate, Dr. Laws' performance made so much sense within the lawyers' frame of reference that the Presiding Examiner even praised her: "You made a good recovery, Doctor." Again, Dr. Laws waltzed right into the lawyers' reality.

In these two exchanges, not only did Dr. Laws fail to refute the lawyers, but also, she acted as if she had fallen in with their humor. "Don Quixote ...," Schutz writes, "is not refuted by the behavior of his fellow-men who, as Cervantes states, 'fall in with his humor'."

Finally, Quixotic communication, involving the failure of contradictions to arise, may also be realized through a mistaken or confused response by the scientist. Consider Dr. Laws' response to the Presiding Examiner:

PRESIDING EXAMINER: ... Mr. Powers is entitled to test the validity of your conclusion in relation to the documents which you cite as the supporting documents.

It is unfortunate if it is going to require you to delve into the document again, but at the same time, they are certainly appropriate questions to be asked.

DR. LAWS: I don't disagree with that.

[E20]

However, judging Dr. Laws by her many self-avowals of purpose, we may safely presume that Dr. Laws was more so acquiescing than agreeing when she responded, "I don't disagree with that." This response nonetheless validated the lawyers' belief in what they were doing.

5.4 The Condition of the Existence of "Good Arguments for Explaining Away Discrepancies"

What about those instances in which contradictions were not just plainly voiced, but also plainly heard? I am referring to those instances, already quoted in section 5.2, where Dr. Laws, on the one hand, and the lawyers, on the other hand, articulated repeatedly their respective meanings and purposes. How was it possible for each side to hear, but then not to register, what the other had to say?

Schutz writes:

[On his] second expedition Don Quixote is no longer alone. He has to establish a "sub-universe of discourse" with the fellow-men with whom he shares a face-to-face relationship within the world of common sense. This refers first of all to Sancho Panza, his squire, the representative of everyday thinking ... With great skill Cervantes shows ... the devices by which a common sub-universe of discourse is established between knight and squire. Both have good arguments for explaining away discrepancies. [Emphasis added, denoting another condition that makes Quixotic communication possible.]⁶

A good example of this is Don Quixote's conception of Mambrino's helmet. Consider, first, Don Quixote's and Sancho Panza's respective schemes of interpretation:

Both refer to the same matter of fact which is, in terms of Don Quixote's private sub-universe, Mambrino's miraculous helmet, and in terms of Sancho Panza's paramount reality of everyday life, an ordinary barber's basin.⁷

How does Don Quixote reconcile Sancho Panza's assertions that Mambrino's

helmet is really just a barber's basin? Don Quixote believes:

... Mambrino's helmet, that object of immense value, appear[s] to everyone a barber's basin, thus protecting its owner from persecution by all those who would understand its true meaning.⁸

Strictly speaking, Sancho Panza's assertions therefore do not contradict Don Quixote's reality. Indeed, for Don Quixote, every such assertion provides additional evidence for the valuable helmet's altered appearance to other people. Schutz observes in general about Don Quixote's interpretation of the world: "Nothing remains unexplained, paradoxical, or contradictory."⁹

There are other "good arguments" that Don Quixote and Sancho Panza have "for explaining away discrepancies" in the course of establishing a dialogue --"a common sub-universe of discourse":

Don Quixote admits that Sancho Panza is not a knight and, therefore, subject to other laws; perhaps his fear prevents him from seeing and hearing right; if Sancho stealthily followed the two flocks of sheep for a short while he would discover that they were re-transformed into two armies as described by Don Quixote. On the other hand, Sancho is inclined to believe that the Knight's misfortunes are due to the fact that he has broken a solemn oath; or perhaps that he has power over real giants, but no power at all over phantoms. [Footnotes suppressed.]¹⁰

With these arguments for explaining away discrepancies, each person sharing in the interaction (1) reconciles any discrepancies or contradictions that the other person may happen to introduce and (2) accumulates additional evidence that, at the same time, fits his own argument and affirms his own reality.

What were Dr. Laws' arguments for explaining away, for never recognizing the different meanings and the different purpose that the lawyers brought with them to the interaction?

Just as Don Quixote argued to himself that Sancho Panza's behavior could be explained away by the latter's being subject to other than knightly laws, and just as Sancho Panza argued to himself that Don Quixote's behavior could be explained away by the latter's having broken a solemn oath, Dr. Laws explained away the lawyers' persistent questioning of the documents as being the execution of exercises that she found at best harmless and at worst annoying, namely, the exercises of (1) going through the testimony word by word, paragraph by paragraph, or, if she was lucky, page by page [E13], (2) redundantly demanding as many citations or all possible citations to the documents when she believed just one was sufficient [E15], (3) soliciting repeated protests from her that she did not keep records of the documents she had read [E17], and (4) continually searching for wording, usually in a document of some 50 pages, that supported an assertion in the testimony [E20]. These exercises were purposeless to Dr. Laws insofar as it did not matter to her whether she had "ten studies to cite when they fit into the specific argument or only one."

To review, in the background to the lawyers' reality, there were "the facts", which were contained in the sentences, phrases, words, and annotations of Dr. Laws' testimony, and which threatened to prove, upon application of Title VII, that AT&T discriminated in employment on the basis of sex. We just observed, however, that Dr. Laws did not see the lawyers' reality for what it was, but for what it was not, namely, the execution of purpose-

less exercises. Each time one of the lawyers persisted, in a punctilious way, in questioning the documentary basis of her testimony, Dr. Laws had additional evidence of the existence of these purposeless exercises.

Now, what about the lawyers? (By "lawyers", I include the Presiding Examiner.) What "good arguments" did the lawyers have for explaining away, for never recognizing the different meanings and the different purpose that Dr. Laws brought with her to the interaction?

It appears that the lawyers, when compared to either Don Quixote, Sancho Panza, or Dr. Laws, had a tremendous advantage in the way that they could construct an argument which was good for explaining away discrepancies. (These are the discrepancies between what they believed Dr. Laws to be doing and what Dr. Laws, herself, believed she was doing.) Whereas Don Quixote and Sancho Panza individually reconstructed individual arguments, the lawyers socially constructed a group argument. Each lawyer's espousal of legalistic reasoning reinforced and was reinforced by the remaining lawyers' espousal of the same legalistic reasoning.

Also, more than just the plural number of lawyers--that is, more than just the peer-group pressure--was operating on the lawyer-scientist dynamics. It is significant that the interaction was taking place in a courtroom (as opposed to, say, in a seminar of social-psychology graduate students or in a consulting firm run by social psychologists). The significance is that there are rules or expected patterns of behavior which come with each institutional setting, and which are encountered as "given" by the actors appearing on the scene. (This is simply the idea that there usually exists one or another social structure of opportunities and constraints [though not deter-

ministic rules] which mediate the motivations and free wills of human beings in any given institutional setting.) In the institutional setting of the courtroom, it is important to our analysis that the Presiding Examiner's argument for explaining away discrepancies carried a special weight in the eyes of both the plaintiff attorneys (including Mr. Copus) and the defendant attorneys (including Mr. Powers). The Presiding Examiner essentially had the final say as to the argument (that is, the Quixotic argument for explaining away discrepancies) that the courtroom was to embrace, because he was the person whose power it was to decide ultimately whether AT&T was in violation of the law; we would therefore expect that Mr. Powers and Mr. Copus made sure that their interpretation of Dr. Laws was consistent with the Presiding Examiner's.

What, then, were the arguments that the lawyers had for explaining away Dr. Laws' purpose (which she avowed was illustration) and her meanings to the documents (which she avowed were optional and not critical to her testimony)?

In a sense, the lawyers made the most of their institutionally sanctioned and, relative to Dr. Laws, exalted position: their argument for explaining away Dr. Laws' potentially disruptive avowals was to ignore them, to summarily dismiss them. The lawyers did not listen to Dr. Laws because they did not have to. Dr. Laws' reality did not so much escape unseen by the lawyers as it was snubbed, usually not even acknowledged, by them.

The lawyers cleanly executed this by simply resuming their line of questioning whenever Dr. Laws finished speaking, thereby treating Dr. Laws'

avowals as nothing more than interruptions, the substance of which therefore made no difference. (I found it insightful, when reading the transcript, that the lawyers' remarks were able to stand independently of Dr. Laws' responses, insofar as the lawyers' train of thought would remain understandable to me even when I skipped reading Dr. Laws' parts of the dialogue.) I present four examples of this. The underlining is intended to remind the reader that the lawyer is resuming his line of questioning, while at the same time ignoring the substance of Dr. Laws' avowal.

DR. LAWS: I would say that [the written testimony] is an analysis of a problem illustrated with data as opposed to a study which is [a] specific research project undertaken to test [a] specific hypothesis. In fact, you really can't use data of this sort to test [a] hypothesis really because the data were collected for a different purpose. They were used simply in an illustrative way to give a sense of numbers and a concreteness to the argument ...

MR. POWERS: I believe you also indicated that you made some express request to the EEOC for supplementary data, for other information. I would request that we have a list of that data and information.

[E4]

DR. LAWS: I would like to add one thing to your observation which I think is very apropos and this is to say that I have ... tried to avoid making

statistical generalizations about the whole Bell System.

The C&P study is interesting and illustrative but I don't construe it as my job to reconcile in a statistical way this finding ... I am interested in the C&P study because it focuses in on a problem for us.

I won't say that that problem necessarily in a descriptive way represents the total Bell System. However, the findings are so striking in this study that I am moved to ask a variety of questions about what is going on there.

This is legitimate in terms of the way I am using data in my report.

If you are going to argue, for example, in rebuttal that this is an exceptional situation, then you have to justify ... why there should be [significant] differences.

I don't have to do that because that is not the argument I am making.

MR. POWERS: I am not engaged at the moment in a direct argument with you. I am just trying to clarify what you have presented to this proceeding. In line with your point about not generalizing from a specific study, I would like to inquire about some of your statements on page six. The study to which you refer is ...

[E7]

DR. LAWS: I think I have stated before I was willing, since in my testimony I make absolutely no pretense that I am presenting an up-to-date and complete statistical presentation of the Bell System.

This is not the intention of my testimony. That is not the kind of expertise that I am here for, that I used data only illustratively ... that is the case and I think I have made it explicitly for that.

It does not matter to me whether I have 10 studies to cite when they fit into the argument or only one. So, in fact, I have limited myself to this document. I don't think I need to go back over the whole set of force less studies to reinforce it but as long as we are talking about this document I would be better pleased, and I guess Mr. Powers would too, if I would review it exhaustively and make sure you have as many citations as are relevant within this document.

PRESIDING EXAMINER: Would you undertake to do that during the lunch period?

MR. POWERS: I don't need the cumulative citations. I would be interested in one or more that support that statement. In connection with that and your review of the document, I would call attention to the table that appears on page 10 which is designed to show ...

[E15]

MR. POWERS [after quoting a statement from one of the documents that Dr. Laws cited]: Is that the statement and is it the data based on that

statement that you use as a basis for your statement that dissatisfaction with the opportunities for advancement was the second most frequent cause of termination, salary being the first?

DR. LAWS: ... You are asking me to attest to an historical protest that I didn't keep documentary records ... As I said, I have read this report carefully and more than once and really base my use of it on a reading of the total document. So, I would be very hard put to say on this, unless I reproduced the data in which case I should footnote it, but I would be very hard put to say where in a given document a particular statement comes from.

MR. POWERS: Let me ask a couple more questions along this line. Isn't it possible for employees both who terminate and those who remained employed to have similar likes and dislikes ...

[E17]

It was rare for the lawyers to acknowledge that there existed substance in Dr. Laws' avowals, and even rarer for the lawyers to respond to the substance. In the only instance of this that I was able to identify, the lawyer summarily dismissed Dr. Laws' feelings on the matter:

DR. LAWS [to Mr. Powers]: ... in order to answer the kind of question that you have continually put to me is where in a document of some 50 pages do you find some wording that supports that assertion, and I would have to have time to study the document, and I would be glad to answer the

question in the detail that you seem to require.

If we are going to have more questions on this document, then I should take whatever formal steps are necessary to study this study so that I can give you more specific answers to your questions.

PRESIDING EXAMINER: The problem is, Dr. Laws, that you have in several instances cited specific documents and in some cases a single document by your parenthetical insertion of the reference supposedly as the basis for that statement or at least that is what I presume it to mean.

Mr. Powers is entitled to test the validity of your conclusion in relation to the documents which you cite as the supporting documents ...

[E20]

There were two ways in which additional evidence, fitting the lawyers' argument and affirming their reality, accumulated.

First, returning to the diagram on page 187 in the previous chapter, we recall that each and every time that Dr. Laws referred to a document, whether in her written or oral testimony, the lawyers had additional "evidence" of their reality (consisting of the legal map in which the documents provided "the facts" of the basis that supported Dr. Laws' own statements or conclusions about AT&T's employment practices.) For example, we earlier observed that, in providing citations to twenty two documents, Dr. Laws waltzed right into the lawyers' reality (see section 5.3). Every bit of additional evidence served to reinforce and sanctify, in the eyes of the lawyers, their scheme of interpretation.

Second, from the viewpoint of each individual lawyer, his scheme of interpretation (which explained away, by ignoring, Dr. Laws' potentially disruptive avowals) was mutually or socially reinforced. Each and every time one lawyer observed any of the others applying the same scheme of interpretation, there was additional evidence validating their reality, especially if the other lawyer was the Presiding Examiner. In this way, the lawyers succeeded in reconciling the discrepancies between what they believed Dr. Laws to be doing and what Dr. Laws, herself, avowed she was doing.

5.5 Cross Cultural Miscommunication: The Two-Way Projection of Culture-Bound Maps

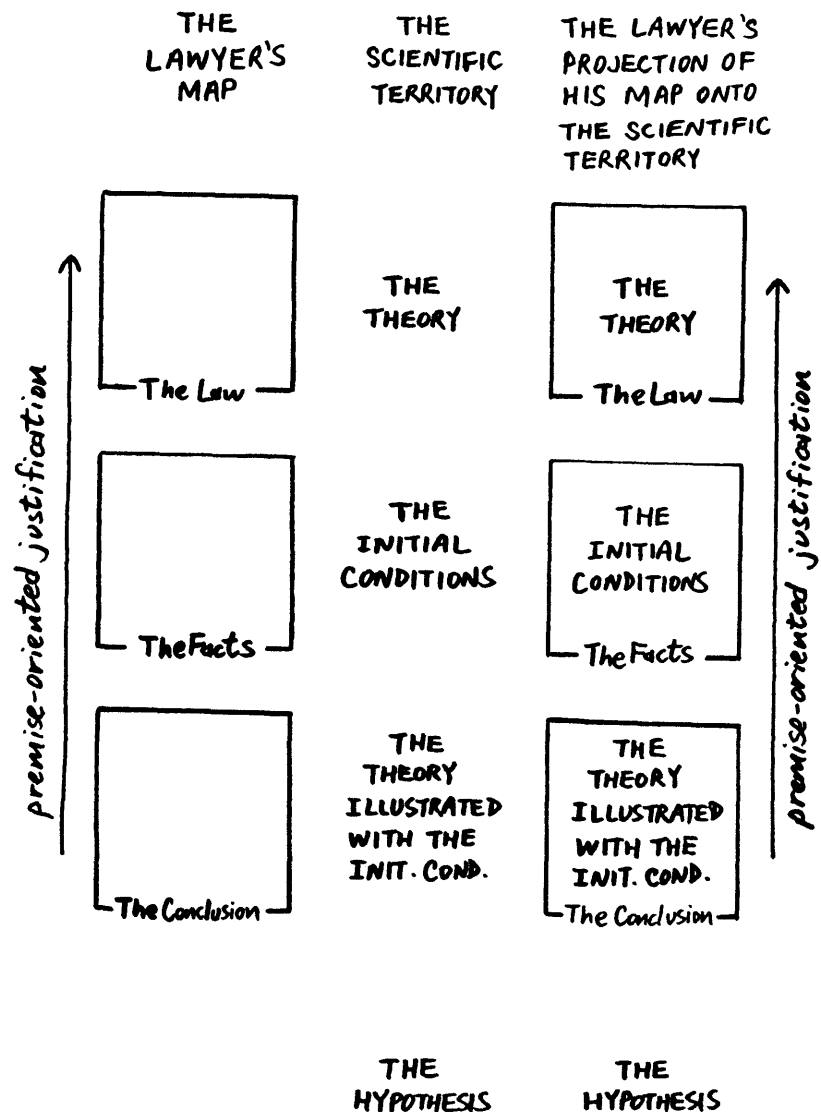
The lawyer's projection of his legal map onto what the scientist presents in the testimony has already been amply illustrated in the Ashenfelter and Laws cross examinations. But what about the other half of the story, namely, the scientist's projection of her scientific map onto what the lawyer says? Does this happen at all?

Surely this would happen if the scientist were allowed to cross examine the lawyer. However, there are other situations in which we may observe this event taking place, as the Laws cross examination will illustrate for us.

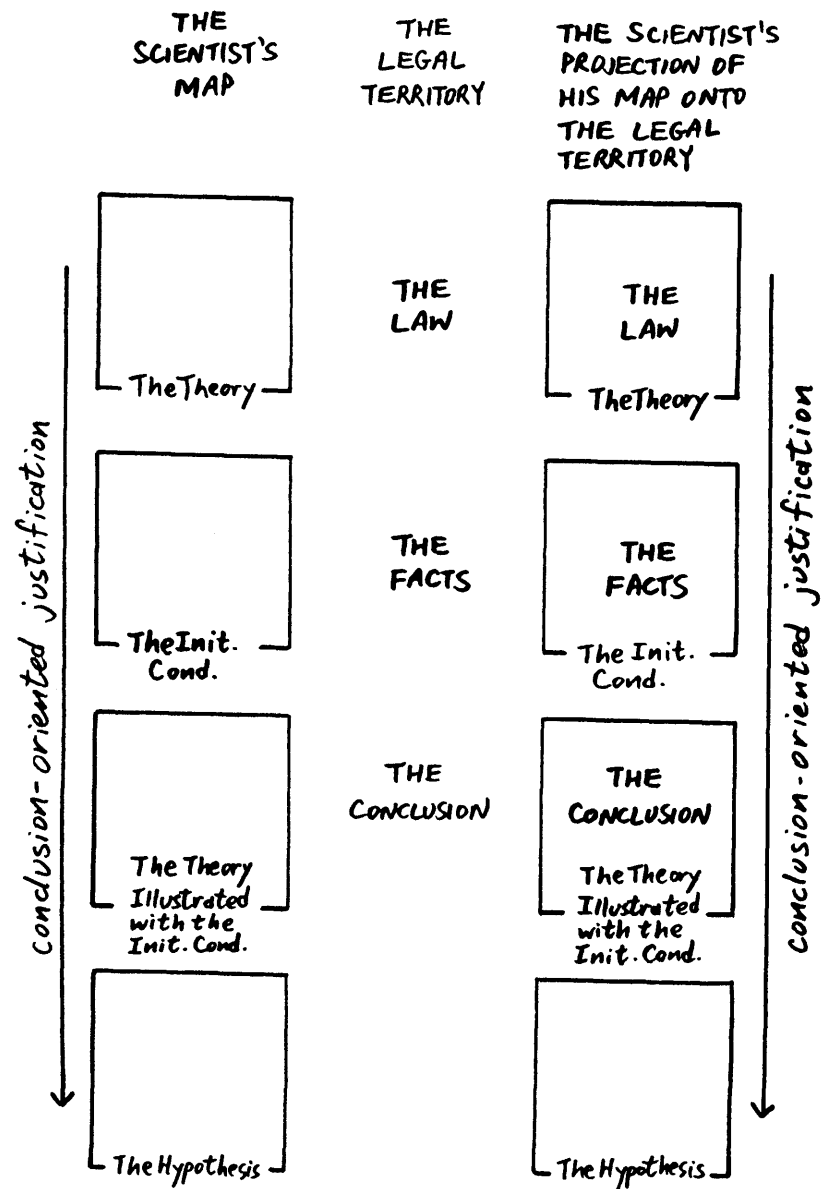
In general, the situation is a two-step process: (1) the lawyer comes to an understanding, in his legal premise-oriented way, of the scientific testimony, and (2) the scientist must then make sense of, and respond to, the lawyer's questions about the scientific testimony. It is in the second step that the scientist projects her scientific conclusion-oriented map onto the lawyer's understanding of the scientific testimony. See the diagram on the following page.

In this manner, the following dynamic unfolds: the lawyer attaches the function of "the law" to the theory, and "the facts" to the initial conditions, so that it is the validity of these premises on which rests the scientific testimony's "conclusions"; then, the scientist appears unable to respond to the lawyer's questions about the testimony, at least for what the lawyer means by them, because the scientist is herself projecting a map

MAP 1



MAP 2



of her own meanings onto the lawyer's words.

With regard to the diagram, Map 1 (the lawyer's ethnocentric projection of his legal map onto the scientist) has already been amply illustrated in prior chapters about the cross examinations of Dr. Ashenfelter and Dr. Laws. As for Map 2, Dr. Laws provides a convenient illustration.

In her cross examination, Dr. Laws had to make sense of what the lawyers referred to as being the basis and the conclusions in her testimony. In chapter four, we explained that the basis (consisting of the initial conditions) had the legal meaning of "the facts", while the conclusions (Dr. Laws' theories as applied to the initial conditions) had the legal meaning of being the syllogism's last step. In the manner that Map 2 illustrates, Dr. Laws projected her scientific meaning of "the initial conditions" onto what the lawyers meant to be "the facts"; at the same time, Dr. Laws also projected her scientific meaning of "the theory illustrated with the initial conditions" onto what the lawyers meant to be "the conclusions" (the syllogism's last step).

Thus, because of the respective projections of the two maps, scientists and lawyers wind up attaching different meanings and different functions (the function of conclusion-oriented justification and the function of premise-oriented justification) to the same words. We might say that "they cannot succeed in establishing a universe of discourse ... and, consequently, they cannot enter into a true social relationship ..."¹¹ In accumulating examples of confirming evidence for their respective understandings, they effectively seal themselves off from each other.

Quixotic communication therefore involves an encounter between two individuals who are from two different societies, but whose respective cultural forces (that is, maps) continue nonetheless to operate as the scheme of interpretation by which each individual makes sense of the other.

Because of the operation of these maps inside of each of the individuals, we observe that the lawyer's legal reasoning is not scientifically cognizable and the scientist's scientific reasoning is not legally cognizable.

Deserving mention is one more example of the lawyers' projection of the legal map; the resulting way in which it makes sense of the scientific territory is absolutely dumbfounding.

In view of this study's position that lawyers do not "speak" the conclusion-oriented language of science, the following statement by Mr. Powers appears to contradict our theory.

Isn't it true that until you establish that a study or a test instrument can in fact predict or measure the trait or criteria that it is undertaking to measure, you can't even be sure that it is accurately portraying the results within the test group?

What is fascinating is the way in which Mr. Powers would test the prediction here. Whereas a scientist would test the prediction against experience, whether in a natural or laboratory experiment, Mr. Powers would perform the test in the following way:

In terms of authorities to which one might go in trying to check on the validation of a test instrument, is The Mental Measurements Yearbook one of the authorities that would be referred to?

[E22]

That is, in Mr. Powers' way of thinking, in order to see whether or not "a test instrument can in fact predict or measure the trait or criteria that it is undertaking to measure," Mr. Powers would cite an authority (this is akin to citing a legal precept) to validate the prediction; it did not occur to him that he might test the prediction against experience.

5.6 Tying in "Confirming Evidence" with Self-Sealing Realities

In chapter 1, "Science and Law", we embraced Popper's position that induction--the process of arguing from "n" observed examples of a phenomenon to a general theory of that phenomenon--leads to a self-sealing, metaphysical theory, that is, a theory whose empirical inaccuracies are not detectable and, as a result, not correctable. This occurs where induction is applied as the means of justifying or testing the theory's accuracy.

In concrete terms, this means that it is not enough to seek out examples as "confirming evidence" of a theory's accuracy. Instead of marshalling supporting examples, justification consists of efforts at turning up counter-examples to the theory. The failure of counter-examples to appear would then be said to corroborate the theory, which would nonetheless remain open to being overturned by a counter-example that might appear at any later time. It is thus possible only to rule out or disprove a theory; a theory may never be proven correct. (A full explanation of the logic underlying this position was presented in section 1.3, "The Logic of Science.")

In Conjectures and Refutations, Popper presents some fascinating instances of self-sealing, metaphysical theories that result from the marshalling of supporting examples as "confirming evidence":

I found that those of my friends who were admirers of Marx, Freud and Adler, were impressed by a number of points common to these theories, and especially by their apparent explanatory power. These theories appeared to be able to explain practically everything that happened within the fields to which they referred. The study of any of them seemed to have the effect of an intellectual conversion or revelation, opening your eyes to a new

truth hidden from those not yet initiated. Once your eyes were thus opened you saw confirming instances everywhere: the world was full of verifications of the theory. Whatever happened always confirmed it. Thus its truth appeared manifest; and unbelievers were clearly people who did not want to see the manifest truth; who refused to see it, either because it was against their class interest, or because of their repressions which were still 'un-analysed' and crying aloud for treatment.

The most characteristic element in this situation seemed to me the incessant stream of confirmations, of observations which 'verified' the theories in question; and this point was constantly emphasized by their adherents. A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history; not only in the news, but also in its presentation--which revealed the class bias of the paper--and especially of course in what the paper did not say. The Freudian analysts emphasized that their theories were constantly verified by their 'clinical observations'. As for Adler, I was much impressed by a personal experience. Once, in 1919, I reported to him a case which to me did not seem particularly Adlerian, but which he found no difficulty in analysing in terms of his theory of inferiority feelings, although he had yet even seen the child. Slightly shocked, I asked him how he could be so sure. 'Because of my thousandfold experience', he replied; whereupon I could not help saying: 'And with this new case, I suppose, your experience has become thousand-and-one-fold.'¹¹

The practice of marshalling "confirming evidence" is not restricted to Popper's Marxists, Freudians, and Adlerians. In fact, we have already observed this practice in the interactions between Tevye and Lazar; Don Quixote and Sancho Panza; and the expert witness and the lawyers.

In the interaction between Tevye and Lazar, Lazar's continued responses to Tevye served to entrench Tevye more deeply in his (mis)understanding that Lazar wished to buy a cow from him. (Lazar's purpose was to ask Tevye for

his daughter's hand in marriage.) Not only the substance of Lazar's responses, but also the mere act of Lazar's obliging Tevye's remarks provided "confirming evidence" to Tevye for his own understanding of the situation. To recap:

LAZAR: Tevye, I suppose you know why I wanted to see you.

TEVYE (Drinks): Yes, I do, Reb Lazar, but there is no use talking about it.

LAZAR (Upset): Why not?

TEVYE: Why yes? Why should I get rid of her?

LAZAR [Knowing that Tevye has several daughters]: Well, you have a few more without her.

TEVYE: I see! Today you want one. Tomorrow you may want two.

LAZAR (Startled): Two? What would I do with two?

TEVYE: The same as you do with one!

LAZAR (Shocked): Tevye! This is very imporant to me.

TEVYE: Why is it important to you?

LAZAR: Frankly because I am lonesome.

[Tevye is startled.]

In the interaction between Don Quixote and Sancho Panza, Don Quixote sustained his belief in his private world of fantasy by collecting examples of "confirming evidence". In this manner, Sancho Panza's protests that Mambrino's helmet was just a barber's basin provided "confirming evidence" to Don Quixote for his belief that the miraculous helmet in fact altered its appearance to other people, "thus protecting its owner from persecution by all those who would understand its true meaning."¹²

I find most interesting of all the way in which Popper's Marxist "could

not open a newspaper without finding on every page confirming evidence for his interpretation of history; not only in the news, but also ... in what the paper did not say." Earlier in this study, we found that the lawyers similarly found confirming evidence for their Quixotic arguments in what the expert witnesses did not include in their testimony (for example, the fact that Dr. Ashenfelter did not investigate the use of a VES, as opposed to a Cobb-Douglas, production function, or the fact that Dr. Laws did not supply the appropriate citations).

To summarize, we notice that the two conditions that this chapter puts forth as making Quixotic communication possible both operate by providing "confirming evidence". The first condition--the existence of supporting evidence and/or no contradictory evidence--clearly operates in this manner; for example, when Dr. Laws provided additional citations to twenty two documents, she gave the lawyers twenty-two instances of "confirming evidence" that their reality--their legal scheme of interpretation--"applied" to her. (The remaining actions of the scientists that are cited in section 5.3 may be re-read in the same way.) The second condition--the existence of good arguments for explaining away discrepancies--operates with the same effect, by turning potential contradictions or refutations into confirmations. For example, each time the lawyers questioned another document, Dr. Laws did not encounter contradictory evidence that the documents signified something important to the lawyers, but instead found additional "confirming evidence" that the lawyers were persisting in a purposeless and annoying exercise. The lawyers had an even easier device for transforming potential contradictions into confirmations; given their plural number and their institutionally

sanctioned positions, they simply ignored Dr. Laws' potentially disruptive pleas ("potentially" in the sense that they would indeed have been disruptive if the lawyers had considered their substance) while looking to one another for "confirming evidence" of their way of understanding the situation.

The theme which emerges is that induction, when applied as the means of justifying or checking a viewpoint, serves to seal off one's mental constructs from the given situation they are intended to depict, even (and especially) including the situation of the face-to-face interaction. Whereas Popper developed his position on induction with regard to scientists, the self-sealing consequence of inductive justification occurs not only for the constructs in the minds of scientists, but for the constructs in the minds of all people--even us Sancho Panzas and Don Quixotes in our everyday worlds of common sense and fantasy. As we busily go about collecting additional examples of "confirming evidence" in the course of our everyday routines, we strengthen the convictions we have in our own realities while remaining blind, if only momentarily (like Tevye and Lazar), to the simultaneous existence of different realities.

Quixotic communication is therefore a special case of the general phenomenon of self-sealing constructs. It is the case involving individuals engaged in a conversation where (1) they start out with their respective, incompatible notions of what is being communicated; (2) they do not perceive any contradictions, but only "confirming evidence" for their respective notions; (3) all the while, they are under the impression of communicating; and (4) the conversation proceeds, and concludes, with each individual's notions sealed-off from those of the others.

Chapter 6 The Scientist as Alien in the Legal Culture

While we state only figuratively that scientists and lawyers speak different languages, we assert literally that scientists and lawyers come from different cultures. We observed in the previous chapter that, coming from different cultures, scientists and lawyers communicate only Quixotically, if this may be considered communication at all.

There is one other manifestation of the disparity in cultures. It is the endless questioning to which a lawyer may subject a scientific witness. The endless questioning, which takes the form of either an infinite regression or infinite progression, is a consequence of the existence of two cultures. As we will elucidate, two legal natives (whether plaintiff lawyer or defendant lawyer, or one of these and the judge) or two scientific natives typically reach closure in a discussion because they operate with the same notions from the same culture (and are so sanctioned by the shared institutional forces), but a legal native and a scientific native enjoy no shared, cultural notions, which are essential for closure. The consequence is that, in scientist-lawyer dynamics which exist in the courtroom setting, there is no natural end to the string of questions to which the cross-examining lawyer may subject the scientific witness, except perhaps for the end required by the patience of the judge and by the limitations of time. How is this possible? How may a lawyer's line of questioning, when put to a scientific witness, threaten to continue without end? We tackle this question in this chapter.

6.1 Legal Scrutiny of the Premise-End of Scientific Testimony: Infinite Regression

In the Laws cross examination, we observe the legal scrutiny not only of the scientific testimony's premises, but also of the premises' premises and then the premises to these. There appeared no natural end to this line of questioning. (One astute observer, with whom I discussed this phenomenon, dubbed it "Shepardizing the witness".)

Presented in the appendix to this chapter are examples of how Mr. Powers questioned (1) the premises to Dr. Laws' testimony and (2) the premises to these premises. Immediately below we present what is the most interesting and, for Dr. Laws, probably the most exacerbating of the examples, which involve Mr. Powers' questioning of (3) the premises to the premises to the premises to the testimony.

The Written Testimony

Dissatisfaction with opportunities for advancement was the second most frequent cause of termination among Houston Operators (EEOC R-798 [the Houston Study]), salary being the first.

[page 5 of the
testimony]

These data suggest that precisely the women who are most promising are being lost to the Bell System by short-sighted personnel policies. This is the conclusion of the Houston study mentioned above. A comparison of terminating Operators with a control group of employees with at least one year's tenure showed that the former were more able and more independent than the latter. The authors of the study lay a great deal of the blame for this loss on the authoritarian style of supervision which prevails in

the System in particular, an overemphasis on punctuality produces many incidents of friction which reduce the attractiveness of the job [sic]. The authors point out that many of the "resignations of persons whose task performance has been adequate but whose attendance has not (EEOC R-798, pp 6ff). The inflexible policies of the System appear to contribute to the very problem of which the System complains. Moreover, it seems likely that this rigidity negatively affects the more able and independent women to a greater degree, and contributes to the loss of those employees who are the best candidates for promotion into management.

Another feature of this situation is that able and autonomous women who might be good candidates for management may have difficulty relating to the available role models for women in management. These are older, less educated, rigid, authoritarian persons who do not invite identification.

[pages 6 and 7]

The recommendations based on the Houston study were to hire only women who could be counted on not to complain. This is not an isolated finding; in another study of terminating Service Representatives at C&P (EEOC Z-727), the authors recommend that the System stop recruiting ambitious, educated young women and take older employees who are "more realistic about (their) goals within the Company." (EEOC Z-727). The recommend looking for "someone who isn't looking for a glamorous career." (Emphasis supplied [by Dr. Laws])

[page 28]

Questioning the Premise to the Testimony

The prior scientific study that Dr. Laws mentioned in reference to these portions of the testimony (or, in Mr. Powers' eyes, the premise that Dr. Laws cited to support her testimony's conclusions) was the Houston study.

Mr. Powers attempted to discredit the use of this premise by pointing out that Dr. Laws' statement about the Bell System was a generalization that

went beyond what the Houston study, alone, allowed:

MR. POWERS: Now, when you talk in two sentences further down [on page six in the written testimony] about "The authors of the study lay a great deal of the blame for this loss [of promising women employees] on the authoritarian style of supervision which prevails in the System, "you are referring to Houston here ... you are referring to those conclusions in connection with Houston?"

DR. LAWS: No. It is in connection with the Bell System.

MR. POWERS: With the Bell System?

DR. LAWS: That is correct.

MR. POWERS: ... even though that study relates only to Houston?

...

This reference to "System" in the third line from the bottom and the second line from the bottom [of page six], is intended in fact to be Systemwide?

DR. LAWS: That is correct.

MR. POWERS: Not just Houston or Southwestern Bell?

DR. LAWS: Yes.

...

MR. POWERS: Would you want to suggest a revision at this point or when you reappear?

DR. LAWS: Revision of what sort?

MR. POWERS: I thought you were saying you wanted to limit this [that is, the testimony on page 6] to those female managers at the Houston office [as opposed to the Bell System in general].

DR. LAWS: Okay.

[761-764]

Another way that Mr. Powers questioned the Houston study as a premise was to point out its inconsistency with another premise, in effect weakening the significance to the Houston study:

MR. POWERS: On page 28 of your testimony [there is the statement: "The recommendations based on the Houston study were to hire only women who could be counted on not to complain. This is not an isolated finding; in another study of terminating Service Representatives at C&P (EEOC Z-727), the authors recommend that the System stop recruiting ambitious, young women and take older employees who are 'more realistic about (their) goals within the Company'."] I refer you to the recommendations which appear on the last page of [a different premise, the C&P study] headed "Recommendations". I ask you if such a recommendation appears on that page?

DR. LAWS: ... there is a section advocating ... don't restrict recruiting to young applicants ...

MR. POWERS: Wouldn't you agree there is a difference between a recommendation that says, don't restrict recruiting to a particular program and one that says, stop recruiting that group?

...

Would you agree that the judgment that the study or that the authors are recommending that the System stop recruiting ambitious, young educated women is a judgment that you have made based on reviewing the Summary and the Recommendations and is not explicitly stated by the authors?

DR. LAWS: Yes ...

MR. POWERS: Wouldn't that suggest some revision in your statement on page 28?

[1446-1450]

Questioning the Premise to the Premise to the Testimony

Suppose the premises to the testimony were all in order. (Here, this would mean that the testimony were amended to refer to Southwestern Bell only, not the Bell System in general. Or perhaps similar, social-science studies, performed on all the operating companies of the Bell System, could be presented to support the testimony's generalized statement.) The lawyer

could then attack the premises to these premises. In our example, the Houston study itself rested on the following premises: its survey instrument, used to procure data from employees; training manuals used throughout the Bell System which, Dr. Laws insisted, the study's authors observed as a form of data; and the Armatas test and the Edwards Personal Preference Schedule, both of which are test instruments that the Houston study employed. Mr. Powers' questioning touched on each of these premises to the premise:

MR. POWERS: Back on page 5 of your [written] testimony, there is a statement ["Dissatisfaction with opportunities for advancement was the second most frequent cause of termination among Houston Operators (EEOC R-798), salary being the first"].

...

Dr. Laws, is the instrument used to conduct [the interviews with the employees in] the Houston study set forth on pages 26 through 28 [of that study]? Is that the study instrument or the survey instrument?

DR. LAWS: It should be. Let me take a quick look at it. I have no reason to assume otherwise.

MR. POWERS: Would you indicate where in that survey instrument you find the question asked regarding the reason for termination among the operators?

DR. LAWS: I can't answer that because ...

MR. POWERS: So, you are saying that this statement about the dissatisfaction's being causes of termination may not be based on the answers to a specific question in the survey instrument?

DR. LAWS: Yes, that is a possibility

...

MR. POWERS: It is my understanding, based on your responses and my own reading of the document, that there is nothing in the survey instrument that asks why did you leave the Bell System.

[1367-1373]

MR. POWERS: Dr. Laws, you stated on voire dire that you felt you could make certain generalizations from the Houston study [to] the Bell System in general because of the access that the authors [of the Houston study] had to certain documents that you said you had not studied extensively but these included [Systemwide] training manuals to which they referred.

I wonder if you could indicate where in this [the Houston] study there is reference to training manuals which might have given the authors of the study a basis for reaching conclusions beyond the Houston location or the Southwestern Bell Company?

[1380]

MR. POWERS: ... I ask you now if you can indicate where, in the Houston study itself, you found reference to such training manuals or what other basis there is for considering that those [authors of the Houston study] had access to [system-wide] manuals that gave them a basis to make system-wide judgments [as opposed to restricting their judgments to Southwestern Bell].

...

Then directing your attention to ... page 20 [of the Houston study, where it states: "It must be stressed that the Clinical statements in this section represent interpretive judgments by the research psychologists. These judgments may or may not coincide with formal policy explicitly stated by the Company; rather they reflect interpretations of the system in general form. Such judgments attempt to provoke further exploration and discussion; they are not intended to be unequivocal or conclusive"]. In view of those statements, do you feel that you can rely on the brief interviews with supervisors to make judgments about the Bell System as a whole beyond Houston and Southwestern Bell?

DR. LAWS: I am willing to restrict those conclusions to Houston and Southwestern Bell ... I am perfectly willing to limit it to Southwestern Bell.

MR. POWERS: If those limitations would be made on page six [of the written testimony] where the reference is to the Bell System [rather than just

Southwestern Bell], we would be prepared to move on.

[1408-1411]

MR. POWERS: Dr. Laws: in further regard to the Houston study, that study is based in significant part at least on empirical data generated by two tests, one which I believe is referred to as the product of a Dr. Jim Armatas and I believe is referred to as a test for beliefs, and the other is [the] Edwards Personal Preference Schedule ...

...

... I would want to ask you about whether you had any knowledge at the time that you prepared your testimony as to the validation of those test instruments, and by validation, I mean any [other prior scientific] studies which would show whether or not those tests in fact measure what they purport to measure ...

[1383-1384]

Questioning the Premise to the Premise to the Premise to the Testimony

Suppose the premises to the premises to the testimony were all in order. The lawyer could then simply proceed to attack the premises to these premises to the premises to the testimony, as Mr. Powers here demonstrates for us. Here, the premise to the premise to the premise to the testimony is The Mental Measurements Yearbook, which Mr. Powers put forward in his question-

ing. (To review, The Mental Measurements Yearbook is the premise signifying the validity of the Armatas test and the Edwards Personal Preference Schedule, which are the premises to the Houston study, which is a premise to Dr. Laws' testimony.)

MR. POWERS: Referring now to these tests that the authors of the study used, the Armatas test for belief and the Edwards Personal Preference [Schedule], I asked you at the conclusion of our discussion yesterday afternoon if you know to what extent those studies [test instruments] had been validated or whether you had made any investigation concerning that at the time you prepared your testimony utilizing that study.

...

If the studies which have been done would indicate that it is not a valid measure or has not been validated, wouldn't that affect your reliance on it?

...

In terms of authorities to which one might go in trying to check on the validation of a test instrument, is The Mental Measurements Yearbook one of the authorities that would be referred to?

DR. LAWS: Yes.

MR. POWERS: Dr. Laws, in this connection, we have examined that publication and there are some studies of the Edwards Personal Preference Schedule

which appear in that book [these studies are the premises to the premises to the premises to the testimony], and recognizing that it would be difficult to present these to you right here without your having an opportunity to read it over and reflect, I would like to suggest that we would make this available to you during the break.

Again, I hate to intrude on your lunch, but I do think that we would like to have your opinion of whether or not the statements in that publication affect your judgment about the conclusions of the Houston study.

[1421-1425]

The cross-examining lawyer need not jump to the strategy of regressing immediately from the testimony to the testimony's premises, to these premises' premises, and so on. More prudently and more cost-effectively, he may first dwell on subjecting the testimony to an exhaustive questioning of all its premises from a variety of angles (for examples, see the appendix to this chapter), and then proceed to questioning the premises to the premises only when the latter withstand his first round of questioning. Should any of the premises to the premises survive his second round of questioning, then the premises to these may be thrown open to question. And so forth. In this manner, the lawyer may cost-effectively discredit the scientific testimony.

Insofar as the scientific witness secures a premise in his argument in the way that the courtroom requests (that is, by introducing another premise),

and insofar as the cross-examining lawyer may demand that every such premise, in turn, be secured, the scientific witness occupies a can't-win/can-lose position. That is, the scientific witness cannot win as long as the cross-examining lawyer chooses to throw open to question every premise to every premise, and the scientific witness can only lose as long as the cross-examining lawyer indeed follows through with the strategy of regressing from premise to premise to premise.

At best, the scientific witness may hope for a stalemate, which is what happened between Dr. Ashenfelter and Mr. Levy. The technical, mathematical nature of Dr. Ashenfelter's testimony obscured Mr. Levy's understanding of it, including the underlying levels of premises that could have been thrown open to question. In effect, Dr. Ashenfelter stalemated Mr. Levy: the latter did not win, but then, the former did not lose.

In summary, the cross-examining lawyer's option to pursuing an infinite regression into deeper and deeper levels of premises puts the scientific witness in a can't-win/can-lose position.

6.2 Legal Scrutiny of the Conclusion-End of Scientific Testimony: Infinite Progression

Dr. Andre Hellegers, who was a physician, a Professor of Obstetrics-Gynecology, a Professor of Physiology-Biophysics, and the Director of Population Research at Georgetown University, offered testimony on behalf of the EEOC

His theory was simply that pregnant women are capably of employment. The fourth sentence on the first page of his written testimony boldly put forth a null hypothesis by which he justified his theory:

There are to my knowledge no physiological data which warrant a rule that women in pregnancies should cease working. [Emphasis added.]

That is, if the theory (that pregnant women are capable of employment) is true, then the null hypothesis (that we may observe no physiological circumstances associated with pregnancy which automatically call for pregnant women to cease working) should also be true.

In the following sentence in his testimony, Dr. Hellegers again applied the reasoning of conclusion-oriented justification. A competing theory (that the development of complicating conditions [diabetes, hypertension] during pregnancy automatically makes the stoppage of work necessary) is refuted because the consequent hypothesis (that men and non-pregnant women with the same conditions [diabetes, hypertension] must also necessarily stop working) clearly runs counter to experience:

It should be recognized that if a woman were to develop

diabetes, hypertension, or certain other conditions in pregnancy, then it would be possible [that is, a possibility, not a certainty] that a stoppage of work would become necessary, but this in no way differentiates pregnancy from non-pregnancy, since this statement would be equally true for non-pregnant women, or indeed for men. No medical evidence can be adduced for the need to cease working in pregnancy.

In both instances, Dr. Hellegers reasoned the validity of the theory by looking to the conclusion (the hypothesis) that followed from the theory. As such, the validity or invalidity of the hypothesis served as an indicator of the validity or invalidity of the theory.

Further corroborating our understanding that Dr. Hellegers' mode of justification was the conclusion-oriented mode of scientific justification is the following exchange from the cross examinations:

MR. LEVY: On page 3 of your testimony, Doctor, you refer to a large scale analysis of work in pregnancy which compares results of pregnancy outcome between women with higher incomes and those with lower social classes and incomes.

...

Doctor, in that study which I have just had a moment to glance at quickly, is lower income identified as the sole and crucial factor in the correlations involved [that is, the correlation between income and class, on the one hand, and pregnancy outcome, on the other hand]?

DR. HELLEGERS: That is one of the core problems in all of the studies. Of course, you can get studies in every single way about work, who

works, and who does not work.

One of the problems involved in unraveling who works and who does not work so you can show a direct cause and effect relationship, it is precisely because I don't know of any study that has ever been able to keep all the variables constant and manipulate only one, which would be the proper scientific design.

I don't know of any study that has done that. So that what we are talking about here is that there is a constant association between [low] income and bad pregnancy outcome.

[1103-1105]

Compare the striking resemblance between the emphasized portion of Dr. Hellegers' remarks with the words of Ernest Nagel, the naturalist and the philosopher of science, whom we quoted earlier:

In a controlled experiment [which is a practice that takes place for the purpose of testing a theory's conclusions], the experimenter can manipulate at will, even if only within limits, certain features in a situation (often designated as "variables" or "factors") which are assumed to constitute the relevant conditions for the occurrence to the phenomenon under study, so that by repeatedly varying some of them (in the ideal case, by varying just one) but keeping the others constant, the observer can study the effects of such changes upon the phenomenon and discover the constant relations of dependence between the phenomenon and the variables.
[Emphasis added.]

At the risk of beating a dead horse, we take notice of Dr. Hellegers' familiarity with the concept of the requisite "proper scientific design" that can "keep all the variables constant and manipulate only one" in the

testing of a hypothesis, and we accept this familiarity as another indication that Dr. Hellegers' way of thinking was the conclusion-oriented mode of scientific justification.

The null hypothesis (that is, the hypothesis that something will not occur and hence will not be observed) is a standard tool, used widely throughout the scientific community. The null hypothesis fits in well with the scientific requirement that a theory be falsifiable, insofar as the observation of the hypothetically prohibited event would serve to falsify the theory. Also, because it is possible only to make an observation of something that does happen, as opposed to something that does not happen, a hypothesis that is to be falsifiable by observation must be a null hypothesis.

The scientific norm, that calls for knowing the validity of a viewpoint through the validity of its hypotheses, is so well integrated into the life of the scientific community that the following remarks by Popper (most of which were already presented in section 1.4) may seem "obvious beyond all question,"¹ perhaps obvious beyond the point worth mentioning, to practicing members of that community:

Every test of a theory, whether resulting in its corroboration or falsification, must stop at some basic statement ["a statement about a factual occurrence," either a hypothesized/predicted occurrence or, here, an observed occurrence against which the hypothesis is tested] or other which we decide to accept. If we do not come to any decision, and do not accept some basic statement [which we will now call "observation statement", or "observation" for short] or other, then the test will have led nowhere. But considered from a logical point of view, the situation is never such that it

compels us to stop at this particular [observation] rather than at that, or else give up the test altogether. For any [observation, whose accuracy, like any theory's, may be questioned] can again in its turn be subjected to tests, using as a touchstone any of them [hypotheses] which can be deduced from it with the help of some theory, either the one under test, or another. This procedure has no natural end. Thus if the test [of the theory, through comparing its hypothesis against observation] is to lead anywhere, nothing remains but to stop at some point or other and say that we are satisfied, for the time being.

It is fairly easy to see that we arrive in this way at a procedure according to which we stop only at a kind of statement that is especially easy to test. For it means that we are stopping at statements [hypotheses and observation] about whose acceptance or rejection the various investigators are likely to reach agreement. And if they do not agree, they will simply continue with the tests, or else start them all over again. If this too leads to no result, then we might say that the statements in question were not inter-subjectively testable, or that we were not, after all, dealing with observable events. If some day it should no longer be possible for scientific observers to reach agreement about [observations] this would amount to a failure of language as a means of universal communication. It would amount to a new 'Babel of Tongues': scientific discovery would be reduced to absurdity. In this new Babel, the soaring edifice of science would soon lie in ruins.²

...

The [observations] at which we stop, which we decide to accept as satisfactory, and as sufficiently tested, have admittedly the character of dogmas, but only insofar as we may desist from justifying them by further arguments (or by further tests). But this kind of dogmatism is innocuous since, should the need arise, these [observations] can easily be tested further.³

What finally obviates the need for further testing is that, eventually, scientists reach observations that "are accepted as the result of a decision or

agreement; and to that extent they are conventions."⁴ Popper concludes:

The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any 'given' or natural base; and if we stop driving the piles deeper [that is, when scientists decide or agree to accept an observation as definitely corroborating or falsifying a theory], it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being.⁵

At other places in The Logic of Scientific Discovery, Popper makes explicit reference to the social element in the scientific enterprise:

I shall therefore say that the objectivity of scientific statements lies in the fact that they can be intersubjectively tested.⁶

... inter-subjective testing is merely a very important aspect of the more general idea of inter-subjective criticism, or in other words, of the idea of mutual rational control by critical discussion.⁷

Where the discussants are (1) members of the same, scientific community, who are subject to the same cultural and institutional forces, and (2) practitioners of the same set of methods, which must be wielded according to the same set of standards, it is easy for us to see how it is feasible for (1) "mutual rational control" to work and (2) the convention of deciding or agreeing to accept a test as definitive, "at least for the time being," to be applied. In this way, the community of scientists avoids the "Babel of Tongues", and the edifice of science may soar.

What, then, happens when an outsider to the scientific community, such as a cross-examining lawyer, whose legalistic reasoning is abetted by the other lawyer and the judge, is in a position to question whether or not to accept the observation and whether or not to reject the null hypothesis? To answer this question, we might formulate a response that Popper has already suggested: if it is not possible for different observers (namely, the scientific witness, on the one hand, and the judge, the cross-examining lawyer, and the scientist's sponsoring lawyer, on the other hand) to reach agreement about observations or about rejecting or not rejecting hypotheses, this would amount to a "Babel of Tongues", and scientific knowledge would be reduced to absurdity; in this new Babel, science, whether as a body of knowledge or the reasoning process that produces this body of knowledge, would lie in ruins.

In this light, the quotations from Popper no longer appear obvious beyond the point worth mentioning, for they are uncannily prescient with regard to how scientists and lawyers (in particular, Dr. Hellegers and Mr. Levy) miscommunicate with each other in the courtroom.

Concretely speaking, how were Popper's words, written in 1934, prescient with regard to the AT&T case in 1971?

To answer this, we return to the null hypothesis that Dr. Hellegers spelled out conveniently (that is, as if for our benefit in conducting this study). His theory was simply that pregnant women are capable of employment. His null hypothesis was: "There are to my knowledge no physiological data which warrant a rule that women in pregnancies should cease working ...

No medical evidence can be adduced for the need to cease working in pregnancy."

Now, what did Mr. Levy, a man not a member of the same community as Dr. Hellegers, make of the hypothesized non-existence of physiological conditions that warrant that women in pregnancies should cease working?

Mr. Levy responded by promptly rattling off a laundry list of physiological conditions which, he argued, warrant that women in pregnancies cease working:

Does pregnancy commonly put stress on such organs as the kidney and liver?

[1095]

Couldn't continuation at a job involving exposure to toxic substances which would produce no harm to a normal non-pregnant woman prove to be harmful to a pregnant woman?

[1096]

But you would say that continuation of jobs involving potential exposure to various kinds of radiation or toxic matters or ultra-sound could be potentially harmful to the fetus?

[1096]

Can't continuation of certain jobs involving physical agility, stamina or dexterity, and by that I mean, for example, jobs which would involve truck driving, climbing poles or ladders, bending, lifting, squatting, working in confined manholes--can't such jobs and the continuation in such jobs tend to increase the risk of accident as pregnancy increases?

[1097]

Particularly in pregnancy, [added] weight can tend to lessen agility, speed of reaction, speed of movement, can it not, Doctor?

...

Can't factors associated with normal pregnancy such as increased fluid retention, nausea, swollen ankles, bladder pressure, generally lessen agility because of this 25 pound weight? Can't some or all of these result in serious diminution in the speed and efficiency required for the performance of certain jobs?

[1098-1099]

... Is morning sickness or nausea fairly common or at least not uncommon in the first trimester or pregnancy?

...

Could that result in diminution of the efficiency required for the satisfactory performance of certain jobs?

[1100]

... What about bladder pressure? Is it not uncommon for the developing fetus to impose greater pressure on the bladder than in the normal non-pregnant state?

...

If there were a job that required continued attendance at a station for let us say hour-long intervals and a normally pregnant woman could not sustain the bladder pressure that long. This could interfere, could it not, with the efficiency of that operation?

[1100, 1100-A]

Again, Doctor, are swelling of the ankles fairly common in pregnancy?

...

Can that not possibly have an effect on the satisfactory performance of certain jobs which would call for speed of movement, locomotion?

[1101]

Dr. Hellegers also put forth in his written testimony another null hypothesis in order to test, and falsify, a competing theory.

It is of some significance that women doctors and nurses, who are working on the obstetrical and other services at the hospital often continue working right up to the day of delivery. This of course would not be so if the medical profession thought that working in pregnancy was contra-indicated.

[page 2 of the
written testi-
mony]

That is, if the competing theory (that pregnant women are not capable of employment) is true, then the null hypothesis (that we would observe no women doctors or nurses working right up to the day of delivery) should also be true. Because the null hypothesis clearly runs counter to the observation that women doctors and nurses do work right up to the day of delivery, the competing theory is ruled out by association. Again, Mr. Levy induced a Bable of Tongues by stonewalling any agreement over the observation:

Are not the job requirements and potential hazardous exposures quite different from those of telephone workers than doctors and nurses?

[1101]

As a general matter, would not professional doctors and nurses have more knowledge about their pregnancies and about pregnancy generally than would the average or any industrial worker?

[1102]

Would it not also be a significant consideration, Doctor, that medical help is immediately available at a hospital if and when needed while that is not the case in the typical industrial environment?

[1102]

Should a hemorrhage or some other situation develop when a pregnant doctor or nurse is in the hospital, do they not have at hand medical help which the girl in the industrial environment would not necessarily have?

[1103]

The issue that concerns this study is not whether Mr. Levy's remarks contain any substance, or whether scientists might raise the same questions. Instead, the issue is one of closure. Why is that scientists (at least within a school of thought) normally reach closure in discussing scientific observations, but that a scientist and a lawyer do not? In general, at what point (if any at all) may the discussants hold off, "at least for the time being", further questioning about a given observation and a given hypothesis? At what point may the discussants agree to accept an observation, and to desist from testing it further? And how would the discussants know that they have reached this point?

The answer that Popper provides applies only where the discussants are all scientists. Here, it is because the discussants all reason from, and are all subject to, the same framework of scientific values--namely, the cultural and institutional forces which define science--that it is feasible for "mutual rational control" to result in a consensus, accepted or sanctioned throughout the scientific community, with regard to recognizing one or another observation as definitively testing the theory in question. It is not logic, but convention, that makes for this stopping point; logic alone, to reiterate Popper, is inadequate to this task:

... considered from a logical point of view, the

situation is never such that it compels us to stop at this particular [observation] rather than at that ... For any [observation, whose accuracy, like any theory's, may be questioned, which is precisely what Mr. Levy was doing to Dr. Hellegers' observations] can again in its turn be subjected to tests, using as a touchstone any of the [hypotheses] which can be deduced from it with the help of some theory, either the one under test, or another. This procedure has no natural end. [Emphasis added.]

Closure is thus reached not solely through logic, but also through convention: "Basic statements [statements about observations] are accepted as a result of a decision or agreement; and to that extent they are conventions."⁸

Closure is not reached in the courtroom because the discussants (the scientific witness and the lawyers) are not all members of the same community. It is because scientists and lawyers operate from, and are subject to, their respective, disparate sets of cultural and institutional forces, that the "mutual rational control" which works within the scientific community breaks down across the boundaries of that community. With no recourse to any socially sanctioned convention of deciding to accept an observation as definitive and to desist from testing it further, the inhabitants of the courtroom have only logic to rely on. But, "considered from a logical point of view, the situation is never such that it compels us to stop at this particular [observation] rather than at that"; Mr. Levy's laundry list of objections corroborates Popper's null hypothesis all too well: "This procedure has no natural end."

In summary, the cross-examining lawyer's option to pursue an infinite progression into further and further tests of given observations puts the scientific witness in a position where he cannot win (that is, he cannot

present what the court may consider a definitive test of his theory), but can lose (if the lawyer exercises this option).

6.3 Summary Remarks about Infinite Regression and Infinite Progression

We observed in the EEOC-AT&T litigation that infinite regression and infinite progression take place. We then relied on Popper's writings to help explain how infinite regression and infinite progression take place. However, there are a few additional points that will help round out the analysis.

How is it possible that a Lawyer may lead a scientific witness down the road of infinite regression, but may not accomplish the same thing to the argument of an opposing lawyer? That is, how is it that a plaintiff lawyer or a defendant lawyer may succeed in wielding an infinite regression against a scientific witness, but not against each other?

The reason has to do with the fact that the two lawyers are members of the same culture and speak the same language. They both appeal to the same body of knowledge, namely, the same body of legal precepts. One lawyer may certainly question the other lawyers' cited precedents (and the precedent to these precedents and then the precedents to these), but in such "Shepardizing" that takes place between lawyers, the regression into deeper and deeper levels of premises will eventually hit upon one or another statute or precedent (say a Supreme Court decision) whose definitiveness both lawyers will choose to accept, or are sanctioned to accept by the judge or (if need be) by the subsequent decision rendered in the appeals process. No prior scientific study that a scientific witness cites as a premise to his testimony commands a comparable degree of authority in the eyes of a lawyer or a judge because, in the legal community, the scientific auspices approving

the cited study, in not being a legislative body or the Supreme Court, are quite simply unrecognized and irrelevant. The scientist in the courtroom is an alien, cut off from the collegial support and the institutional forces of his home turf that normally sanction his expertise and give it meaning.

How is it possible that two scientists, who propound competing theories, may not wield an infinite regression or an infinite progression against each other?

The infinite regression into deeper and deeper levels of premises would not work because the ultimate test of an argument, at least for the natives residing in the scientific culture, focuses on the argument's conclusions, not its premises. Thus, to review, physicists may readily admit, without incurring any damage to the validity of Boyle's Law, the inaccuracy in the premise that gaseous molecules are volumeless rubber balls, possessing perfect elasticity. Likewise, economists may readily admit, without incurring any damage to the validity of micro-economic theory, the inaccuracy in the premise that people are rational economic men. Scientists do not dwell on the premises because, as they themselves state, an assumption is only an assumption.

The reason that infinite regression does not occur among scientists has already been explained by Popper. In the practice of intersubjective criticism or mutual rational control among these people of shared values, there emerges the convention of agreeing "to stop at some point or other and say that we are satisfied, for the time being."⁹

What makes infinite regression and infinite progression possible in a lawyer's cross examination of a scientific witness is the fact that the two

people come from different cultures, which means that the lawyer is not bound by the scientist's rules or conventions.

To summarize this chapter, we recall that, earlier in this study, we posited the fiction in which lawyers and judges approach the citations in a scientific testimony like the citations in a legal memorandum. A lawyer, with the encouragement and reinforcement of the other legal minds in the courtroom, looks up and checks the cogency of each such document or prior scientific study, cited in the scientific testimony, in the same legalistic way he would look up and check the cogency of each precedent or statute, likewise cited in a legal memorandum. In this fiction, a scientific argument, like a legal argument, would stand or fall according to the validity of its citations, which would be the premises on which the argument rests. Hypothesis testing would have no place in this procedure. Insofar as all theories are fictions, our observations in this chapter serve to corroborate this fiction's status as an accurate depiction of part of the dynamics of scientist-lawyer communication in the courtroom.

6.4 Appendix to Chapter 10: Additional Examples of the Legal Scrutiny of the Premises to Scientific Testimony

[Dr. Laws has just presented, during the cross examination, a new document, "M-5", to shore up the statement in her written testimony: "The informal recruiting by referral on which Bell relies heavily intensifies segregation not only by sex but by age and education, since employees recruit others like themselves for jobs like their own." Mr. Powers scrutinizes the original reference to this document M-5.]

MR. POWERS: Dr. Laws, first in connection with document M-5, would you please identify the portion of this document which you rely upon for the conclusion that "employees recruit others like themselves for jobs like their own".

DR. LAWS: Yes, I am on page six of that document ... In Particular, there is a discussion that group chief operators recruit among their own group and it goes on from there.

MR. POWERS: When the phrase is used "recruit among their own group" do you understand that to mean the group of which they are the chief operator?

DR. LAWS: Yes ...

MR. POWERS: The reference you have given us on page six [of document M-5] deals with recruitment of other employees, rather than recruitment of persons outside the company, does it not?

DR. LAWS: ... You are asking me if it refers only to those who are already employees of the company?

MR. POWERS: Yes.

DR. LAWS: I can't say.

MR. POWERS: I thought your answer to my first question was that the group that is referred to at the end of the sentence you cited is the group of which the operator is the group chief.

DR. LAWS: That is correct ...

MR. POWERS: So those people in that group are employees.

DR. LAWS: That is correct ...

MR. POWERS: Would you agree that the statement that you have cited does not directly support the statement in your testimony that "employees recruit others like themselves for jobs like their own?"

[Mr. Powers proceeds with his attempt at pinning down the source of Dr. Laws' statement. Along the way, Mr. Copus aids and abets Mr. Powers (see E12).]

MR. POWERS: ... Continuing with the document that we referred to, M-5, do you find any statement in that document upon which you could rely for the position that informal recruitment by referral "intensifies segregation not only by sex but by age and education"?

...

Doctor Laws, to try to expedite our questioning, I have asked you a

series of questions about document M-5 as to whether there is a specific statement in there that would support the conclusion about the intensification of segregation by sex, by age and education ... and whether there is any specific language in that document that states that employees recruit others like themselves for jobs like their own and I think you have answered that fully.

...

I believe we have covered in the morning session, Mr. Examiner, the witness' testimony on the references in those documents that would support the assertion that employees recruit others like themselves for jobs like their own. It is our position that based on her testimony those two documents, R-376 and M-5, do not support that statement, and we would move that the reference to them be stricken or that that statement "Since employees recruit others like themselves for jobs like their own" be struck from her testimony.

[1328-1359]

MR. POWERS: On page 5 [of the written testimony] you state that 48 percent of the female service representatives at C&P stated that their major reason for leaving was dissatisfaction with salary levels.

...

Now I believe that statement is based on document C-727. I would

like to draw your attention to a document C-1540 which you cited previously.

...

I am referring to the table on page 11 [in C-1540]. I believe that shows that 59 percent of females who resign from the traffic department of affiliated Bell companies in '68 stated that their reasons for doing so were not related to either the type of work they performed or the pay they received.

[756]

MR. POWERS: To the extent that you are citing document C-1540 for your statement that some Bell companies report 100 percent turnover among short-term employees, I am interested in determining where you find any support in this document for the 100 percent figure other than in connection with operators under six months.

DR. LAWS: I would have to take time to go through the whole report ...

DR. POWERS: ... In connection with that and your review of the document, I would call your attention to the table that appears on page 10 which is designed to show turnover rate among all operators as I understand it, and I would ask you if that graph does not indicate that the average systemwide turnover rate for all operators, not just short term opera-

tors, averages no more than 33 percent or between 30 and 40 percent annually.

[1348-1350]

MR. POWERS: I would like to direct your attention to the footnote on page 43 [in the written testimony], referring to the act or perpetuation of the notion of BFOQ [Bona Fide Occupational Qualification] in the Bell System. Do you recall the date of the document that you cite in that footnote?

[Dr. Laws' footnote reads: "It should be noted that individuals in high management positions within the Bell System actively perpetuate the notion of BFOQ (see EEOC-R-1025)."]

DR. LAWS: No, I don't.

MR. POWERS: I show you this document and ask you if that is not the document to which you refer [that is, EEOC R-1025].

DR. LAWS: Yes, it is.

MR. POWERS: The date of that document is 1966, is it not?

DR. LAWS: Yes.

MR. POWERS: Are you aware of the different positions that individual Bell companies have taken on the question of the BFOQ for the operator

since 1966?

DR. LAWS: No, I don't think so ...

[793]

PRESIDING EXAMINER: Do the citations remain in or are the out?

DR. LAWS: The first three remain in. We discussed them the last time I was here. On the second two, there were questions raised.

PRESIDING EXAMINER: So we strike reference to those?

DR. LAWS: Yes.

MR. POWERS: For clarification, the first three, R-986, was the 1965 document, W223 was the 1961 document, Z712 was a 1961 document. Those were the ones we indicated we felt were not appropriate in terms of time.

[1313]

MR. POWERS: ... Let's move to page seven [in the written testimony]. Here, Dr. Laws, you state ["The sex segregation in recruiting for the entry level positions (for Craft and Traffic jobs) is perpetuated in management"], and I believe the reference that is shown for that statement is

document R-1033, a statement from an AT&T Vice President, dated May 12, 1965.

...

I ask you if you are aware that that document was officially canceled by AT&T on November 13, 1968, 3 1/2 years ago, and that the canceling document was furnished to the EEOC in discovery under its request D-9?

DR. LAWS: No, I am not aware of any cancellation.

[1341-1342]

...

MR. POWERS: On page 10 [in the written testimony], in reference to the first full sentence on the page ... you have cited R-1033, the document to which I just referred, as having been cancelled. I would ask you if in view of that cancellation you would be prepared to eliminate that citation?

DR. LAWS: The citation, yes.

[1344-1345]

MR. POWERS: Dr. Laws, again, on page five of your testimony [there is the statement: "In the C&P study, 69% of terminating Operators gave the lack of opportunity for advancement as a major reason for leaving"] ...

...

I believe that 69 percent figure refers to a table on page 17 [in the C&P study]. This is again a reference to dissatisfactions [as opposed to terminations], is it not?

DR. LAWS: That is right.

MR. POWERS: Would you care to revise the sentence [in the written testimony] that we are discussing at the moment?

Chapter 7 Recapitulation and Recommendations: Interpretation, Explanation Justification, Intervention

7.1 Interpretation, Explanation, Justification

Here, we repeat the descriptions, from chapter two, of the three steps into which we broke down social-science inquiry, but we fill in the details of how, in the subsequent chapters, we actually applied the steps in shaping our understanding of the communication between Dr. Ashenfelter, Dr. Laws, and Dr. Hellegers, on the one hand, and Mr. Levy, Mr. Powers, Mr. Copus, and the Presiding Examiner, on the other hand.

Interpretation

In general, it is essential in social inquiry to interpret "the meaning an act has for the actor himself, not the observer." Indeed, it is only in the biased light projected by the scientifically-trained observer that the legal, premise-oriented meaning to the scientific testimony in our case study might have appeared "irrational"; from the viewpoint of the judge and the lawyers themselves, the meaning that they attached to science was perfectly rational and, given the rules and customs of courtroom, required. These meanings, however "subjective" they may have been, were ready-made and pre-existing as integral parts of the subject matter that we, the observers, encountered in the course of our inquiry. Therefore it was necessary for us to uncover these meanings like any other data, lest we were to risk formulating our theory on the basis of seriously incomplete information. In establishing our interpretive understanding, we came to understand the world in

the way that the observed people (the scientists, the lawyers, the judge) understood it.

Examples taken from the work of Gans, Fuller, and Schutz were used to highlight the significance of the interpretive understanding. It was possible, in the example we derived from the work of Gans, for the planners, the politicians, and the other outside observers of the West End to possess the belief that it was a blighted slum in need of eradication, in large part because they did not bother to establish an interpretive understanding--namely, that the West End had the meaning of home or urban village for the West End residents themselves. (Indeed, no interpretive understanding existed until a team of N.I.H.-funded social scientists began studying the West End, which took place at the time that its residents were being re-located--a time that proved to be too late.) Similarly, an observer of Lon Fuller's boy on the beach, whose actions were directed at a "small, gray, roundish object", can formulate no better than an empty understanding of the boy's publicly-observable movements, at least until the observer finds out that the roundish object is a clam in the boy's eyes. Schutz also provided an illuminating example:

The same overt behavior (say a tribal pageant as it can be captured by the movie camera) may have an entirely different meaning to the performers: What interests the social scientist is merely whether it is a war dance, a barter trade, the reception of a friendly ambassador, or something else of this sort.¹

It is by interpreting the meaning that the "overt behavior" has for the performers themselves that the social scientist comes to understand whether it

is a war dance, a barter trade, or the reception of a friendly ambassador. More importantly, for the policy-maker who seeks to design and then implement an intervention, presumably for the purpose of improving the lives of the observed people, the interpretive understanding will inform him as to whether the intervention should be aimed at improving a war dance, a barter trade, or the reception of a friendly ambassador. In the case of the West End, the policy-makers implemented an intervention for what they believed to be a war dance, but was actually a barter trade.

In this study, therefore, the establishment of an interpretive understanding was crucial to both an accurate theory and a sound set of recommendations regarding scientist-lawyer communication in the courtroom.

We did not take on the tremendous task of establishing, from scratch, an interpretive understanding of the courtroom interactions. Instead, we turned to the work already completed by other researchers who have successfully sought out the ways in which scientists and lawyers see the world. These researchers are called philosophers, and while they do not cast their work in the sociological-anthropological terminology of "establishing an interpretive understanding" or, equivalently, "doing an ethnography", their findings nonetheless deliver the meanings that scientists and lawyers attach to their respective ways of viewing the world. Insofar as the philosophy of science and the philosophy of law spell out the logics with which scientists and lawyers think, the two philosophies conveniently provide us the "first-level constructs" upon which we may build our theory or "second-level constructs" that depict the interactions in scientist-lawyer communication.

In particular, when comparing the two philosophies, we concluded that

the act of justification holds not only different, but opposing meanings for scientists and for lawyers. For scientists, the act of justification involves looking to the conclusions of an argument, while for lawyers the act involves looking to the premises. (The conclusion-oriented act of justification in science is also known by its more familiar name of "hypothesis testing", while the premise-oriented act of justification in law is more commonly referred to as "syllogistic reasoning" or "applying the law to the facts.") The philosophers or "ethnographers" whose findings we premised, rather than duplicated, were Popper, Kuhn, Schutz, Nagel, Dewey and Pound.

We also relied on the work of these philosophers for introducing to our study the social (perhaps, sociological) aspects existing in science and law, in addition to the logical aspects. Popper talked about "intersubjective criticism" and "mutual rational control" as playing key roles in scientific inquiry. Kuhn held that "[w]hatever scientific progress may be, we must account for it by examining the nature of the scientific group [not just the scientific individual], discovering what it values, what it tolerates, what it disdains"; Kuhn described his position as "intrinsically sociological".² Pound wrote that "we deceive ourselves grossly when we devise theories of law or theories of judicial decision that exclude [subjective pictures of desired social and political orders, existing outside the body of established legal precepts] from 'the law'. Indeed, they give the latter their living content and in all difficult cases are the ultimate basis of choosing, shaping, and applying legal material in the decision of controversies";³ for this reason, legal inquiry is more than the purely logical ("mechanical") application of the law to the facts, a detrimental process that Dewey described as "the doc-

trine of immutable and unnecessary antedecedant rules". Its evil, according to Dewey, was that "[i]t sanctifies the old; adherence to it in practice constantly widens the gap between current social conditions and the principles used by courts. The effect is to breed irritation, disrespect for law ..."⁴ Accepting the demonstrations by these "ethnographers" that extra-logical social considerations play constructive roles of central importance (not just troublesome roles of introducing subjective biases) in legal inquiry and scientific inquiry, we opened the door for the consideration of social factors (which we called "cultural and institutional forces") in the subsequent step of explanation in our social-science inquiry.

Explanation

The interpretive understanding that I, as an observer, possess in my mind is not sufficient in itself to constitute a scientific theory that explains scientist-lawyer communication. While essential, it is not good enough to set up a mirror that descriptively captures the salient meanings and constructs in the world of the people whom I am observing.

For example, in performing a study of the sexist behavior of middle-level managers in corporations, it would not be enough for me to understand the corporate world in the sexist way that the managers understand it. In addition, I would need to formulate a theory that, while being based on the interpretive understanding, transcends it by providing a detached explanation of what makes the managers' sexist behavior both possible and realized.

In general, a theory consists of propositions or mental constructs, existing in the mind of the scientist, which are related to one another ac-

ording to the rules of logic. Strictly speaking, these mental propositions are abstract or "empty" until applied to a concrete phenomenon, whether in nature or society. (Application of a theory involves attaching the mental propositions [ideally, one by one] to their respective correlates in the concrete world.) A theory is therefore a fiction, bearing the same relationship to the depicted reality as a map to a territory, a photograph to a scene, a representation to a presentation. In this way, the actual people being observed become, in social-science theories, "puppets" that are but representations of the original beings.

The objective that we pursued in this study was to create a theory of scientist-lawyer communication in which the wooden movements of our scientist-puppet and lawyer-puppet could predict or match the real-life actions of the actual scientists and lawyers that we encountered in our courtroom-based observations.

We used our interpretive understanding of the scientific culture and the legal culture as the source from which to impute the thoughts, motives, and logics into the minds of the scientist-puppet and the lawyer-puppet. In general, however, a social-science theory must account for more than just the individuals and what they feel and think; it must also account for the immediate institutional setting that these individuals themselves encounter as "given" or ready-made when they appear on the scene. We thus used our understanding of the institutional forces of the courtroom to provide the "rules of the game", or the opportunities and constraints, to specify further both the thoughts and the publicly-observable movements allowed to the scientist-puppet and the lawyer-puppet. With such a theory, we transcended the merely em-

pathetic, interpretive understanding, and we provided an explanation that also accounted for the institutional forces, of which the observed scientists and lawyers may not even have been consciously aware.

Mr. Levy's cross examination of Dr. Ashenfelter provided the empirical material from which we formulated our theory to explain scientist-lawyer communication. First, Dr. Ashenfelter and Mr. Levy illustrated in great detail what we would expect (based on our interpretive understanding of what justification means for scientists and for lawyers) from the behaviors of our theory's scientist-puppet and lawyer-puppet; Dr. Ashenfelter argued justification by looking to the conclusions of his testimony, while Mr. Levy looked instead to the premises. (The specific issues that we observed were the differing scientific and legal meanings to scientific assumptions, statistics, and the testing of predictions.)

Second, while a mere application of the interpretive understanding sufficed to explain how each of the individuals (the scientists and the lawyer) was behaving from his own perspective, the interpretive understanding alone was insufficient to explain the interactions between the two. At this point, we introduced the notion of the projection of maps onto territories; in particular, Mr. Levy ethnocentrically projected his legal scheme of interpretation (his premise-oriented "map") onto Dr. Ashenfelter's conclusion-oriented testimony (the "territory").

Essential factors that made the ethnocentric projection possible, we explained, were the cultural and institutional forces. "Cultural forces" refer to the notions that are internalized over the course of one's biography as a member of one's culture; for example, Mr. Levy's premise-oriented map was a

cultural force in that he internalized it over the course of his career, first as a law student and then as a practicing lawyer. "Institutional forces" are external to the individual, exerted on him by the environment of the immediate institutional setting. Cultural forces motivate behavior, usually automatically and subconsciously, from within the individual, as exemplified by Mr. Levy's projection of his internalized map; institutional forces motivate behavior from without the individual, as exemplified by the legal peer-group pressure in the courtroom that approved Mr. Levy's ethnocentric projection, but that, at the same time, alienated or isolated Dr. Ashenfelter while rendering his testimony not understood for what he intended it to mean.

By incorporating the operation of cultural and institutional forces into our theory, we did more than simply establish an interpretive understanding; in addition, we put forth a scientist-puppet and a lawyer-puppet of our own manufacture, into whose minds we imputed logics or cultural forces, and whose publicly-observable behaviors we made responsive to the institutional forces which we defined as being in their environment. Important cultural and institutional forces that we identified in the cross examinations of Dr. Ashenfelter, Dr. Laws, and Dr. Hellegers were (1) the legal peer-group pressure that mounted against the scientific witness; (2) the collegial support of the scientific community that the scientist found missing in the courtroom; and (3) the common sense processes (the "phenomenology") of everyday thinking and everyday interaction that made possible the ethnocentric projection of ready-made maps.

Hopefully, these constructs, which comprise our theory, fulfill Schutz's specifications:

The scientific constructs formed on the second level, in accordance with the procedural rules valid for all empirical sciences, are objective ideal typical constructs and, as such, of a different kind from those developed on the first level of common-sense thinking [whether the common-sense thinking of the observer or the observed] which they have to supersede. They are theoretical systems embodying general testable hypotheses ... This device has been used by social scientists concerned with theory long before this concept was formulated by Max Weber and developed by his school. [Emphasis added.⁵]

Justification

What makes a theory scientific is independent of its subject matter being in nature or society. Instead, I regard scientificity as a quality that stems from the logical form of the mental propositions that constitute the theory.

First, the propositions must be logically consistent.

However, while this is certainly a necessary condition for a theory to be scientific, it is far from being a sufficient condition. Consider that non-Euclidean geometries, while purely imaginary [that is, having no counterparts in the concrete world] are also made up of logically consistent propositions. In addition to being logical the propositions must attach to a concrete reality--namely, the reality it seeks to depict. In being empirical, the theory must be cast in a logical form that allows any mismatches between the theory (the map) and the reality being depicted (the territory) to be detectable and, hence, correctable. The logical form of the theory must allow the theory to be refuted or falsified, just in case it contains any inaccuracy; otherwise, the theory (however logical it may be) may turn out

to be no more empirical than a non-Euclidean geometry.

Thus, hypothesized or predicted events, based on the theory, are checked against observed events. If the observation refutes the hypothesis or prediction, an inaccuracy in the theory is indicated and its correction is called for. If the observation corroborates the hypothesis or prediction, the theory stands as accurate, at least until a more stringent test comes along and succeeds in producing an observation that refutes it, whereupon the correction is made, and the next stringent test is awaited.

In this study, we practiced this general procedure, indeed to the point where we refuted certain hypotheses, thereby necessitating corrections in our theory.

Mr. Powers' cross examination of Dr. Laws provided the empirical material that allowed us to test the theory against experience. Embracing Nagel's position that a controlled laboratory experiment is but a limiting case of "controlled investigation", for which naturally-occurring variations are as adequate (if somewhat less convenient) than laboratory-induced variations for testing hypotheses, we looked upon the Laws cross examination as providing naturally-occurring variations in the circumstances surrounding the subject matter. It introduced (1) a different person in the role of scientific witness, who applied (2) the theory of a different discipline to (3) a different set of facts, all of which came under the scrutiny of not just one, but (4) three different lawyers (the plaintiff lawyer, the defendant lawyer, and the judge), whose collective attention was focused on (5) a different portion of the scientific "territory" (that is, the initial conditions, rather than the theory as in Dr. Ashenfelter's case). All these variations provided a

source of additional real-life actions against which we were able to compare the wooden movements of our scientist-puppet and lawyer-puppet.

Our reliance on a "natural experiment" was neither novel nor unique; natural experiments are a standard tool in the inquiry conducted by astronomers, geologists, certain medical researchers, and other scientists who may not (and need not) set up control groups under laboratory conditions.

One hypothesis following from our theory was that the scientist should have argued justification in conclusion-oriented terms, and the lawyers, in premise-oriented terms. Corroborating the hypothesis was our observation of the unrelenting extent to which the plaintiff lawyer (Mr. Copus), the defendant lawyer (Mr. Powers), and the judge proceeded to pin down the premises of Dr. Laws' testimony, especially the documents that she relied on for providing the facts, or initial conditions, which she used simply to illustrate her theory. The lawyers' obsession with these premises did not make sense to Dr. Laws, because she understood justification as being the act of looking towards an argument's conclusions (its hypotheses or predictions). We observed that, as a consequence, Dr. Laws and the lawyers talked past each other.

Another hypothesis was that the legal peer-group pressure should have played a key role by supporting each lawyer, while alienating Dr. Laws. In fact, we observed numerous instances in which Mr. Copus (who was the lawyer sponsoring Dr. Laws) and even the judge actively supported the premise-oriented thrust of Mr. Powers' questioning.

However, there were two hypotheses which our observations of the Laws cross examination refuted. We had originally asserted that science and law

are languages, neither of which, like any other language, can be understood apart from the culture of the group that it comes from. Following from the theory, stated as such, were the hypotheses that (1) a scientist and a lawyer (like a speaker of Chinese and a speaker of English) should not understand each other at all, at least not through each other's words, and that (2) the scientist and the lawyer (like the speaker of Chinese and the speaker of English) should notice their differences immediately and effortlessly. We observed, however, that the scientist and the lawyer did understand each other, if only to a limited extent, and that each assumed the other to be speaking in the same frame of reference.

In reformulating and articulating the theory, which the refutation of its hypotheses necessitated, we moved the theory beyond the literal assertion that science and law are languages. The refined explanation was that scientist-lawyer communication proceeds in the manner, first put forward by Cervantes and later recast by Schutz, in which Don Quixote communicated with Sancho Panza. The empirical material we relied on for improving the theory consisted of the cross examinations of Dr. Ashenfelter, Dr. Laws, and Dr. Andre Hellegers. Still imputing premise-oriented logic to the mind of our lawyer-puppet and conclusion-oriented logic to the mind of our scientist-puppet, we explained that each of them either employs an argument of explaining away, or does not even notice in the first place, their mutual differences. Then, through the process of accumulating confirming evidence for his own (mis)understanding of what the other means, each of them winds up engaging in Quixotic communication. While we explained, as before, that the lawyer-puppet projects his legal premise-oriented map onto the scientist, we then

made the explanation symmetrical by pointing out that the scientist-puppet simultaneously projects his scientific conclusion-oriented map onto the lawyer. The cross cultural projection of incompatible, culture-bound maps is at the heart of Quixotic communication.

Furthermore, the mutual support that the lawyer-puppets (depicting the plaintiff lawyer, the defendant lawyer, and the judge) lend to one another function as a mechanism for consolidating, in the manner of a self-sealing prophecy, the "reality" of their legal understanding of the scientific testimony. The mutual support which the lawyer-puppets enjoy on their home turf (the courtroom) underscores the critical role of social, rather than only logical, factors in scientist-lawyer communication. Because of the social factors, we came to view the scientist-puppet as an alien in the legal culture, but nonetheless subject to this other culture's rules. In being cut off from the collegial support and the institutional forces of his own community that normally sanction his expertise and give it meaning, the scientist-puppet finds himself in a can't-win/can-lose position when testifying as an expert witness. The lawyer-puppet may make a mockery of the scientist-puppet by inflicting a line of questioning that, in the courtroom, has no natural end.

In summary, by refuting our initial explanation, we wound up with a theory whose puppets are less wooden, more life-like, and more fully reflective of actual scientists and lawyers whom we constructed the puppets to depict. But just as no map may become a territory and no puppet may become a man, no theory of scientist-lawyer communication can ever be made perfectly accurate. It was in this light that we pointed out that the purpose of scientific in-

quiry is not so much to achieve the end of a perfected theory as it is to engage in the process of continually improving the theory. Thus, our articulation of the theory is simply one step in a progression, the subsequent steps of which may be initiated by other researchers or myself who turn to additional empirical material, derive additional hypotheses, and then refine the theory when necessitated by the refutations of its hypotheses.

It should be pointed out that the theory applies not only to scientist-lawyer communication in litigation concerning employment discrimination, but to all instances of scientist-lawyer communication where the logical form of the scientist's testimony invokes the conclusion-oriented mode of justification. Where this condition holds, we may predict that the conflict between science and law will flare up, whereupon the dynamics of Quixotic communication will rapidly unfold. Conclusion-oriented justification characterizes the laboratory sciences as well as the sciences which rely on statistical controls in hypothesis testing. Thus, social scientists and natural scientists who represent these disciplines and who offer testimony on, for example, environmental protection, occupational health and safety, and medical malpractice are all potential sources of additional empirical material with which the theory may be further tested against experience, and further improved.

7.2 Intervention

Diagnosis is to prescription as the sciences are to the professions. The sciences seek to explain and portray reality, not to intervene in it. The professions seek to intervene in reality, for the sake of shaping it according to humanly defined notions of what is good and what is bad.

Just as there is a relationship between diagnosis and prescription, there is a relationship between the sciences and the professions. While a diagnosis is not a prescription, the diagnosis nonetheless informs the prescription. Similarly, scientific findings inform the design of policy interventions.

In general, a policy intervention exists in the context of the policy-making process, involving (1) a definition of the problem or setting of the goal, (2) a theory which explains the dynamics of the observed problem and which thereby informs the design of the intervention, and (3) the administration or implementation of the intervention, in order to solve the problem or realize the goal.⁶ The scientific approach may enter the process in the second step, where it may offer one or another of its theories for identifying the "policy variables" that the intervention might consider as candidates for manipulation.

The limited role of science in this process is highlighted by the evaluations of implemented, but unsuccessful, interventions. What are the reasons that an intervention may fail? They may be that (1) the problem was not framed practically or the goal was not set realistically; (2) the theory informing the intervention's design was grossly inaccurate; (3) the inter-

vention's implementation was bungled; or (4) any two or even all three of these possibilities had been operating simultaneously. Therefore, even a perfectly accurate theory, if one could ever be achieved, would by itself be inadequate to drive a successful policy intervention. The scientific theory plays an important, but limited, role in policy-making and problem-solving.

In this study, our purpose in inquiry has been simply to offer a theory of scientist-lawyer communication in the courtroom. The fruits of our inquiry are therefore inadequate to the task of addressing all three steps regarding intervention in scientist-lawyer communication. We will therefore present our recommendations only as speculations, in identifying the "variables" in scientist-lawyer communication that are or are not good candidates for manipulation.

In this regard, perhaps the major contribution of our theory is simply the recognition of the presence of cultural and institutional forces in scientist-lawyer communication. The nature of these forces is that they are solidly in place and will remain so for the long-term. They are not easily manipulable, especially in the short-term.

We cannot change, overnight, the cultural forces that motivate scientists and lawyers to think in their respective ways. For example, it would not be enough for lawyers to take a few classes in science. One or two science classes would hardly make a dent in the long-term cultural and institutional forces that function to make a lawyer's premise-oriented understanding not only a rational response, but a required response, to the issues that he confronts in scientific testimony.

Scientists and lawyers have been socialized into their respective ways of thinking and they secure their identities in the respective types of work that they do. It would be wholly unrealistic to attempt to undo their past socialization, to re-socialize them into different ways of thinking, and to provide them new identities, all in the short term.

Nor can we change, overnight, the institutional forces, namely, the rules, both formal and informal, which operate in a courtroom. Statutes and precedents--the body of legislative law and case law--provide not only the premises upon which lawyers and judges must build their arguments, but also the rules according to which evidence--including the testimony of scientific witnesses--may be used in such arguments. While the body of case law changes, it changes only one step at a time (literally, on a case-by-case basis) and it changes according to its own rules (which themselves exist as part of the case law). External interventions to this process do take place; these fall under the rubric of legislative law, but even these are instantly subsumed into the case law, and its slow process of self-regulated change, upon the first instance that the legislated law is applied in litigation.

Aside from the body of legal precepts, there are other forces that come along with the institutional setting of the courtroom. They are the informal rules--the social dynamics--which allow the lawyers to reinforce each other's (mis)understanding of the scientific testimony and which are responsible for relegating the scientist to an alien status. These informal institutional forces make possible the endlessness of the lawyer's line of questioning, whether through throwing open to scrutiny the premise to every premise or the conclusion to every conclusion, thereby invoking a "Babel of Tongues"

which undercuts the scientific testimony.

The policy-maker can ignore the long-term influence of the cultural and institutional forces no more than a scientist or a judge can. However, aside from the cultural and institutional "variables" that our theory of scientist-lawyer communication suggests to be resistant to manipulation, what other "variables" remain as promising points of intervention?

Our theory states that science and law are like languages, to the extent that neither can be understood, as intended, apart from the culture of the group that it comes from. To facilitate cross cultural communication, scientists and lawyers would therefore need to become not only "bilingual" (that is, familiar with both the premise-oriented and conclusion-oriented modes of justification), but also "bicultural" (that is, sensitive to the culture of the other profession). What are some plausible interventions involving bilingual/bicultural scientists or lawyers?

A Band-Aid Remedy

In situations where there are long-term, structural forces solidly in place, the long-term intervention would be to subvert the underlying structure which is setting up the problem in the first place. In the short-term, however, the underlying structure must be accepted as a "given" in the environment, and interventions are restricted to remedying the problematic effects caused by the structure. Surely, any intervention that falls short of addressing the root cause may be properly labeled a "band-aid" remedy, but this is the most we can hope for in the short term. It is beyond the scope of this study to provide an exhaustive laundry list of band-aids. In-

stead, we provide just one as an example.

We might as well take advantage of the situation in which the scientific witness (like the rape victim) becomes the person who is on trial ("I would like to enter an objection", Dr. Laws said [E18]). Specifically, the lawyer who is sponsoring the scientific witness may act as the scientist's advocate. For example, after the cross-examining lawyer is done with inflicting an infinite regression or progression of questions on the scientist, the sponsoring lawyer may proceed to pick up the pieces, calling the scientist back to the witness stand and asking the scientist about the scientific standards (to serve as a contrast to the cross-examining lawyer's legal standards) by which the validity of his testimony's facts are known. Generally, the scientist may not testify on conclusions of law, but he may testify on conclusions of fact, as he would be doing here.

As part of this strategy, the sponsoring lawyer may call in other scientists (from the same school of scientific thought, of course) for their testimony on the cogency of the first scientist's facts. Indeed, there is no reason or requirement that two or more scientists may not even testify together. Drs. Darryl Bem and Sandra Bem, for example, were two psychologists who testified together in the AT&T-EEOC litigation, and who stood up remarkably well to the cross-examining lawyer's questions.

One reason this band-aid is plausible is that, while it seeks to counter the forces that initiate and sustain Quixotic communication, its prescribed intervention still works within the rules and customs of the courtroom. The sponsoring lawyer who calls in additional scientific witnesses would be violating these rules and customs no more than the cross-examining lawyer who

focuses the judge's attention on the premises to the premises to the premises to the testimony. Another reason this band-aid is plausible is that it seeks to place the scientific witness in the company of his colleagues, thereby altering the scientific witness's status as an alien in the courtroom. Consider a group of scientists whose testimonies were to convey, in effect, that the community of scientists believe that (for example) Mr. Levy's nineteen counter-examples to Dr. Hellegers' null hypothesis are absurd. The medical scientist's peer-group pressure would be more effective than just the isolated medical scientist's denials alone in rendering the medical testimony credible. In effect, the scientific peer-group pressure would be neutralizing the legal peer-group pressure. If Dr. Laws had enjoyed the support of her scientific peers, the lawyers and the judge would not have simply ignored the contradictions she was voicing, but would have had to come to grips with other social psychologists testifying that Dr. Laws' illustration of social-psychological theories with empirical data was indeed a practice common to and acceptable by the community of social psychologists. The issues then would not have been whether or not Dr. Laws provided sufficient footnotes and annotations, but whether or not Dr. Laws' explanation of AT&T's employment practices amounted to "scientific fact" that could survive scientific testing. It would have been on this criteria that Dr. Laws' testimony would have been examined.

Don Quixote may easily explain away, to himself, the discrepancies between his own understanding of Sancho Panza's behavior and the actual behavior he observes Sancho Panza manifesting, but where Don Quixote must put up with two, three, or more Sancho Panzas, the ease with which he may explain

away the discrepancies (much less ignore them, as the lawyers and the judge did to Dr. Laws) will begin to disappear.

The final reason we mention for this band-aid being plausible is that the sponsoring lawyer would no longer be providing support to the cross-examining lawyer's premise-oriented questions. We recall that Mr. Copus had done this (doubtless, unwittingly) to the disbenefit of the scientists he himself was sponsoring. The band-aid proposes that the sponsoring lawyer cease his unwitting support for the cross-examining lawyer. This would also weaken the legal peer-group pressure that mounts against the scientist, thereby helping to break the Quixotic dynamics. Of course, for the sponsoring lawyer to engineer such a strategy, he would have to be not only "bilingual" (aware of the logic of science, aware that this logic is different from his own), but also "bicultural" (sensitive to the scientific peer-group pressure needed to give the witness's testimony both credibility and meaning).

As with all band-aids, there are problems that the underlying structure, still in place, would be bound to create. For example, we recall in Dr. Ashenfelter's case that there happened to exist (as part of the structure) a body of case law that spelled out the limited ways in which statistics could be used as evidence in employment-discrimination cases. Thus, even a team of colleagues could not have helped Dr. Ashenfelter's explanation of statistical inference to be better understood in the courtroom, not even if the lawyers and the judge had been sympathetic. The hands of the legal natives were tied by the case law's rules explaining the function of statistics as evidence. Explanations that existed outside the case law were not "legally cognizable". In this situation, the band-aid of having the scientist state

the scientific standards would have no impact, even if the lawyers and the judge had taken classes in statistical reference.

A Structural Intervention

With regard to the general goal of producing bilingual/bicultural scientists and lawyers, basic long-term structural changes in order. After all, if undesirable behavior in and between individuals (for example, Quixotic communication) can be traced to structural forces (cultural forces being their internalized form and institutional forces being their environmental form), then the prescription would be to change the structure. The alternative, which would be to focus the intervention on the individuals who come to occupy the structure, would be to blame the victim.

A courtroom operates by its own rules, but its rules do allow for change. If we are to design an intervention that does not violate the revered legal custom of stare decisis et non quieta movere ("to abide by the precedents and not to disturb settled points"⁷), then the intervention would be to introduce scientific reasoning into the courtroom in the form of a precedent.

To pursue the design of such an intervention, or prescription, would take us far afield from this study's purpose of simply offering a theory, or diagnosis, of scientist-lawyer communication. However, because we have already specified that we are only speculating, and because the task of intervention design is such a tempting one, we will nonetheless take at least the first few steps.

An observation from the recent "creation science" litigation is especially interesting. The observation is that, at first blush, it appears that

Judge William Overton was speaking science as a scientist would; he said in his decision:

The essential characteristics of science are: (1) It is guided by natural law; (2) It has to be explanatory by reference to natural law; (3) It is testable against the empirical world; (4) Its conclusions are tentative, i.e., are not necessarily the final word; and (5) it is falsifiable.

...

The methodology employed by creationists is another factor which is indicative that their work is not science. A scientific theory must be tentative and always subject to revision or abandonment in light of facts that are inconsistent with, or falsify, a theory. A theory that is by its own terms dogmatic, absolutist and never subject to revision is not a scientific theory.⁷

Significantly, Judge Overton was not so much supplanting his legal logic with scientific logic as he was reasoning deductively, syllogistically and legally in (1) positing a rule as to what may be considered science (the major premise); (2) looking at what constitutes creation science (the minor premise); and then (3) deciding that creation science is not a science (the conclusion). While premise-oriented justification and conclusion-oriented justification may readily clash with each other, like in the AT&T-EEOC litigation, Judge Overton demonstrates for us how the two may be rendered compatible: the legal natives accept the notion of conclusion-oriented justification (falsifiability) in a role concerning a very narrow issue of fact (the minor premise), namely, the factual issue of, "What is science?" If McClellan v. Arkansas may serve as a precedent, or as the first in a chain of precedents, which establish a rule of evidence regarding what may be consi-

dered science, then the intervention of changing the courtroom's structure, without violating or disrupting its normal operations, would be effected. The implications for scientific testimony, not only in creation-science cases but in other areas as well, are considerable. If the rule of evidence should refer to falsifiability as explicitly as does Judge Overton's decision, then cross-examining lawyers will seek to discredit scientific testimony in conclusion-oriented terms rather than premise-oriented terms; the lawyer sponsoring the scientific witness will be sure to point out to the judge that any premise-oriented questions which the cross-examining lawyer poses are irrelevant to the scientific evidence at hand. This will eliminate the problem that Dr. Laws faced: the problem of infinite regression.

There remains the problem of infinite progression, in that the cross-examining lawyer (with our hypothetical rule of evidence now directing his attention to the scientific testimony's conclusions or hypotheses) will present an endless stream of falsifying counter-instances to the scientist's null hypothesis. This was the problem that Dr. Hellegers faced. The solution here would be the same as in our band-aid, that is, to call in the scientific witness's colleagues, whose support may indicate the scientific absurdity of the cross-examining lawyer's endless counter-instances.

We may draw a similar observation regarding the opinion written by Judge Higgenbotham of the U.S. District Court in Vuyanich v. Republic National Bank (). Judge Higgenbotham, who received his undergraduate training as an engineer, performed a highly sophisticated, methodological analysis regarding the scientific evidence in an employment-discrimination case. Again, as with Judge Overton, Judge Higgenbotham was

not supplanting his legal logic with scientific logic; he was employing scientific logic strictly within the confines of the narrow questions "What is science?" (in this case, "What is econometrics and statistical inference?") and "Given that this evidence calls itself scientific (econometric), does it indeed say what it claims to say, based on its own criteria?" In Judge Higgenbotham's decision, we observe assimilation. As an individual at the boundary between science and law, Judge Higgenbotham manifested some scientific aspects that are being assimilated into the legal culture. Considering that employment-discrimination litigation, and its use of expert witnesses, have been going on for ten to fifteen years, it is only to be expected that assimilation is taking place.

For us to define assimilation and to document it in Vuyanich v. Republic National Bank would make for a fascinating inquiry, but would also be beyond the scope and purpose of this study. We introduce it merely as a part of our speculations regarding intervention. To follow through would require a second dissertation.

In any case, should the notion of the conclusion-oriented mode of scientific justification ever evolve to the point of occupying a niche among the rules of evidence, then it will become one among the rest of the precepts that lawyers internalize and reason from. In this way, lawyers as well as judges may someday become both "bilingual" and "bicultural".

Chapter 8 The Literature: A Commentary

The sociological-anthropological detachment with which this study views "scientific natives" and "legal natives" is, I believe, unique to the literature on the relationship between science and law, particularly the role of scientific expert testimony. Because the singularity of this study's approach, and hence its contribution, can be better grasped in retrospect (that is, with familiarity of this study itself as the reference point from which the literature may be viewed), my commentary on the literature is purposely being presented here, in the end-part of this study.

From such a reference point, I personally find the law-review literature to appear like a collection of stories in which legal natives write ethnocentrically about the exotic scientific visitors they receive, and the social-science literature to appear like stories in which scientific natives write ethnocentrically about a foreign land (the courtroom) and their adventures there. To be fair, I must state that this image is somewhat of an exaggeration, but I will present quotations from a scientist (Marvin Wolfgang) and a lawyer (David Sive) to demonstrate the existence of ethnocentrism in the literature.

A second characteristic that permeates the literature is the presumption that knowledge is or can be "objective"; for example, a scientific finding would be the same thing to all people: not just scientists, but also lawyers. Yet, as we concluded at the end of the chapter on the cross examination of Dr. Ashenfelter, it is only among scientists that a scientific finding might be passed around, giving off the same appearance to the different people, as

if it were a solid and durable object, like a rock. Outside the scientific community, however, the scientific finding appears different (because of the different, non-scientific cultural and institutional forces operating on the new people who are looking at it). No longer a rock, it is no longer solid in form or "objective", its form now changing with the human subject beholding it. An equivalent way of saying this is that the literature presumes that knowledge is homogeneous across different communities, whereas this study has demonstrated that, for example, a "finding" in the scientific community comes to mean something quite different in the legal community. (Perhaps, someday, when there is a rule of evidence requiring that scientific testimony be evaluated according to its own conclusion-oriented, scientific criteria, scientific knowledge may retain its scientific form or objectivity in the courtroom. However, until such a rule comes into existence [and by no means is it certain that this may happen], scientific knowledge will remain different things to scientists and to lawyers.) We will look at examples of this presumption in the literature.

A third characteristic of the literature has to do, again, with the lack of detachment of the writers, particularly when they reflect on "What is science?", "What is law?", and "How do science and law compare?". The metaphor of language which this study has relied on is useful for illuminating the problem here.

My ability to speak English will, by itself, hardly qualify me as a linguist, or as a grammarian of the English language. Yet, many scientists believe that their ability to "speak" science automatically qualifies them to draw observations about science in the way that a philosopher of science

would; lawyers behave similarly. Yet, unless they possess or capture the philosopher's imagination, these scientists and lawyers may analyze science and law no more ably than an everyday speaker of English may analyze the English language in the way that a linguist would. One's ability to speak a language does not automatically qualify oneself as a linguist; the linguist's ability is made possible by the greater degree of detachment he invokes in his observation and analysis.

Thus, scientists (and lawyers) who contribute to the literature on the role of scientific testimony draw conclusions about science that (in my humble opinion) are questionable. The conclusion that I object most strenuously to is that (1) the social sciences are not yet as "scientific" as the natural sciences, and (2) this lack of scientificity is the source of problems between social scientists and lawyers in the courtroom.

As already stated in chapter two, my position is that scientificity is a quality residing in the form, not the substance, of a theory. Cross-cultural miscommunication between scientists and lawyers is the consequence of the difference in the logical forms between science and law (justification being conclusion-oriented in the former, and premise-oriented in the latter); it is not the consequence of the fact that, due to the apparently greater accuracy of their substantive portrayals, natural-science theories survive more often the rigors of justification than do social-science theories. According to this study, even if the theories of social science were someday to become as successfully predictive as the theories of natural science, the clash in logic between social science and law would remain the bottleneck in communication between the two cultures. Indeed, even today we observe that the "pre-

dictive" theories of natural science are nonetheless stonewalled in the courtroom (in environmental litigation, for example), just as the theories of social science are. In this literature commentary, I will give examples of articles that exalt the substance of social science over its logical form as the reason for its limited reception in the courtroom.

The exaltation of substance over form is just one problem that follows from that lack of detachment in the literature. Another problem is that social-science writers and legal writers often compare science and law by making statements that, in my humbel opinion, are (again) questionable. (Either I am wrong, or my contribution to the literature is increased in value.) I will provide examples from the literature.

Finally, it would be presumptuous on my part if I claimed to be the only "linguist" of cross cultural communication between scientists and lawyers. I am not. However, while there are other observers who invoke the same or greater degree of detachment that I do, none has observed or articulated the clash between the premise-oriented justification of law and the conclusion-oriented justification of science. (The reason for this, of course, may be that my theory is mistaken.) Citations of articles from the literature will be given.

8.1 Ethnocentrism in the Literature

Marvin Wolfgang, Professor of Sociology at the University of Pennsylvania, provides a reflective, autobiographical account of his experience on the witness stand. While an eminent and qualified sociologist, Wolfgang nonetheless manifests ethnocentrism with regard to the legal community in his article, "The Social Scientist in Court" (The Journal of Law and Criminology, 65:239-242 [1974]):

It was further brought out under cross examination that Garland County, in which Maxwell [the black defendant, accused of rape] had been tried, was not included in the survey sampling of Arkansas counties. [Wolfgang had used a stratified random sample to refute hypothesis that "Among all defendants convicted of rape, there is no significant association between the race of the defendant and the type of sentence."] The state argued that failure to include Garland County was a fatal error, that the generalized conclusions drawn from [Wolfgang's] Arkansas rape-death penalty study could not apply to the Maxwell case.

Based upon my own research, this conclusion was absurd. We had taken a carefully drawn random sample ...

The social scientist who becomes involved in testifying and displaying research evidence must also be prepared for opinions that contravene the traditional scientific canons of response ... [Judge Henley] announced in his decision that the 'variables which Dr. Wolfgang considered are objective ... broad in instances ... imprecise ... Discrimination moreover is a highly subjective matter [and might not] be detected by a statistical analysis ... Statistics are elusive things at best, and it is a truism that almost anything can be proven by them.' These are common assertions made by persons who are not social scientists trained in statistics. Yet, the social scientist who becomes involved in testifying in this area must be prepared for arguments and decisions that are political or that reside in legal vicissitudes outside the framework of

social science inquiry and evidence. [Emphasis added, page 244.]

From a legal viewpoint, was Judge Henley's conclusion absurd (namely, the conclusion that the failure of Wolfgang's sample to include the pertinent county rendered Wolfgang's study inapplicable)? Surely from a scientist's point of view, it was absurd; however, for Wolfgang to write off Judge Henley's decision as being merely "political" or being a "vicissitude" is for Wolfgang to be projecting ethnocentrically his scientific values, standards, and expectations onto a man not from the scientific culture. Judge Henley, in my view, is not so much one of those "persons who are not social scientists trained in statistics" as he is a person trained in the traditions of his own legal culture. Wolfgang came to see Henley not for what he was, but for what he was not.

David Sive, a lawyer with the Park Avenue firm, Winer, Neuburger & Sive, wrote an article, "Scientists in the Courtroom", which appeared in the volume, Scientists in the Legal System (edited by William A. Thomas, Ann Arbor: Ann Arbor Science Publishers, 1974, pp. 103-108). Sive's attitudes is nothing less than presumptuous; he is ethnocentric to the extent that he may be called a legal supremacist:

Most scientists should recognize, and many of them will understandably have difficulty doing so, that, wrong or right, the basic theory is that by this testing of witnesses the best result ultimately will flow from the legal proceeding. True, many attorneys may appear to be discourteous or even abusive while doing this, but the better expert witnesses merely consider these instances as characteristic of attorneys' zeal and do not let it interfere with their reasoned answers. [Emphasis added, p. 107]

Any scientist who is qualified to act as an expert

can further the judicial proceedings by merely paying attention to the rules of procedure while he is testifying. As the scientist gains experience in the courtroom and appreciation for the methods by which the proceedings are handled, he will contribute even more to the process and enjoy being a part of the proceedings rather than on the sidelines. [Emphasis added, p. 108]

To be fair, I acknowledge that Sive recognizes difficulties in communication between lawyers and scientists. However, to blame the scientist by placing on him the burden of "paying attention" and to presume that "the best result ultimately will flow from the legal proceeding" is nothing short of high-handed ethnocentrism. Clearly, Sive is projecting his own legal notions of how the world does and should operate. Accepting his rules, the obedient scientist would even "enjoy being a part of the proceedings."

I have cited Wolfgang and Sive because their attitudes exemplify dramatically the ethnocentrism which I find in the literature and which, at the same time, distinguishes the literature from the approach taken in my study.

8.2 The Literature's Presumption of "Objective" Knowledge

The literature presumes that knowledge is homogeneous across the scientific and legal communities, whereas this study (particularly the analysis of the Ashenfelter cross examination) refutes any such presumption. There are numerous examples of this presumption in the literature.

Theories, assumptions, hypotheses, reliability and validity are concepts that can be readily transferred, without distortion of meaning, from the scientific treatise to the courtroom. [Wolfgang, p. 247]

Judges should profit from other disciplines. In a sense, a judicial decision represents social science in action. Judges should require more knowledge of the social sciences to enable them to fulfill their policy-making function of using law as a means to the ends of serving society wisely and to its good. [John Minor Wisdom, Judge, U.S. Court of Appeals for the Fifth Circuit, in his article, "Random Remarks on the Role of Social Science in the Judicial Decision-Making Process in School Desegregation Cases," Law and Contemporary Problems, 39:148 (Winter 1975).]

To give a simplistic answer to a difficult question: the role that the social sciences ought to play in the judicial decision-making process is, of course, the same as the role of any other science, whether medical, electronic, or atomic. In short, all sources of human information and knowledge properly contribute to the determination of the facts. [J. Braxton Craven, Jr., Judge, U.S. Court of Appeals for the Fourth Circuit, in his article, "The Impact of Social Science Evidence on the Judge: A Personal Comment", Law and Contemporary Problems, 39:151 (Winter 1975).]

... I would say that a comprehensively and properly trained social scientist would have all he needed except newdata to be a good lawyer and vice versa. [Michael Shriften, Professor of Philosophy, University of California, Berkeley, in his article, "Methods of Reasoning and Justification in Social Science and Law," Journal of Legal Education, 23:189 (1970).]

Questions of social principle and social fact are also in the domain of moral philosophy, logic, and behavioral science. Legal processes, therefore, entail inquiries that are in form and content no different from the inquiries that are the subject of these disciplines. Legal inquiry differs in the practical significance that attaches to the answers to the questions. [Geoffrey C. Hazard, Jr., Professor of Law at the University of Chicago School of Law and Executive Director of the American Bar Foundation, in his article, "Limitations on the Uses of Behavioral Science in the Law," Case Western Reserve Law Review, 19:72 (1967).]

... social science can add valuable information and opinion to responsible judging--pragmatic, principled, or otherwise. Social science, therefore, is entitled to a respected place in the halls of justice. The study of people and their problems is a natural prerequisite of the legal decision of problems among people. [James B. McMillan, Judge, U.S. District Court, Western District of North Carolina, in his article "Social Science and the District Court: The Observations of a Journeyman Trial Judge", Law and Contemporary Problems, 39:163 (Winter 1975).].

I wonder how Dr. Ashenfelter or Dr. Laws would have reacted if they had read these quotations right after having completed their cross examinations.

8.3 Exalting Substance Over Form

It is the logical form of science, not the substantive content of its theories, that is responsible for limiting the utilization of both natural science and social science in the courtroom. In this regard, this dissertation stands apart from the literature, which takes the stance that social science will become better utilized as it becomes more like natural science. My position is that social science, no matter how predictive it may ever become, will continue to be stonewalled in the courtroom (just as natural science is now). I provide a few examples of the literature's position:

Limitations within the state of knowledge of the expert's own discipline are an obvious source of error ... In such cases, the law can aspire only to the "best" answer that the existing state of scientific knowledge permits.
[Harold L. Korn, Professor of Law, S.U.N.Y. at Buffalo, in his article "Law, Fact, and Science in the Courts," Columbia Law Review, 66:1092 (1966).]

A profound skepticism has crept into the relationship between law and the social sciences, a skepticism I share. For every judge who relies on policy research, there is another (or occasionally the same judge in a different case) who decries reliance on the vagaries of social science research in constitutional adjudications.

The source of the law's newfound sense of independence from the social sciences lies ... in the realization that the social sciences are also suffering from a crisis of legitimacy, one that perhaps runs deeper than that in the law. There are widespread doubts in the scientific, legal and political communities as to the objectivity, maturity, and relevance of the social sciences to the constitutional decisions rendered by the courts ... The social sciences, for all the advances they have made, are in many ways pre-sciences. Greater knowledge and more sophisticated research techniques do not yet lead to a convergence on particular paradigms as in the physical sciences; rather, they lead at best to complexity and the promulgation of conflicting paradigms ...

In the light of the shortcomings of modern social science, it would be convenient, both for jurists and for legal scholarship, if the ties between law and social science could be completely severed. [Edgar G. Epps, "The Impact of School Desegregation on the Self-Evaluation and Achievement Orientation of Minority Children", Law and Contemporary Problems, 42:71-73 (Autumn 1978).]

[Social Scientists] should not parade in the garb or language of the empiricism of the physical sciences. As Anthony Giddens is supposed to have remarked, "those who still wait for a Newton of the social sciences are not only waiting for a train that won't arrive, they're in the wrong station altogether." [James M. McPartland, "Desegregation of Elementary and Secondary Schools", Law and Contemporary Problems, 42:109 (Autumn 1978).]

I would not have the constitutional rights of the Negroes--or of other Americans--rest on any such flimsy foundation as some of the scientific demonstrations in these records ... [S]ince the behavioral sciences are so very young, imprecise, and changeful, their findings have an uncertain expectancy of life. Today's sanguine asseveration may be cancelled by tomorrow's new revelation--or new technical fad. [Cahn, Jurisprudence, 30 N.Y.U. Law Review 150:157, 167 (1955), as quoted by Betsy Levin and Philip Moise, "School Desegregation Litigation in the Seventies and the Use of Social Science Evidence: An Annotated Guide," Law and Contemporary Problems, 39:53 (Winter 1975).]

Justice Frankfurter: ... [We] are here in a domain which I do not yet regard as science in the sense of mathematical certainty. This is all opinion evidence.

Mr. Greenberg: That is true, your Honor.

Justice Frankfurter: I do not mean that I disregard it. I simply know its character. It can be a very different thing from, as I say, things that are weighed and measured and are fungible. We are dealing here with subtle things, very subtle testimony. ["Oral argument in Gebhart v. Belton, 347 U.S. 483 (1954)"] as quoted by Betsy Levin, "School Desegregation Remedies and the Role of Social Research," Law and Contemporary Problems, 42:1 (Autumn 1978).]

These quotations suggest a good hypothesis by which to test this study's theory of scientist-lawyer communication: if the theory is accurate, then we should not observe any improved utilization of social science in the courtroom as social science becomes more mature, more like physical science, less imprecise, less changeful and, of course, more fungible.

8.4 Questionable Statements About Science and Law

If Popper, Kuhn, Schutz, Nagel, Dewey, and Pound are to be believed (or if my interpretation of these scholars is to be believed), then the distinguishing trait between science and law is the way in which they proceed with justification. Premising this notion, I find some of the literature's characterizations of science and law to be questionable. First, consider the following two cases.

From the perspective of the science court, the principle difference between scientific and legal methodology is that a legal judgment is binding upon the parties. [Jeffrey N. Martin, J.D., Harvard Law School, "Procedures for Decisionmaking Under Conditions of Scientific Uncertainty: The Science Court Proposal," Harvard Journal on Legislation, 16:442, note 8 (1979).]

A scientific demonstration, to be accepted as such, requires an agreement of much the same kind as any other procedure for establishing a proposition of fact or of principle. If someone refuses to accept the evidence of the senses, or rejects the influences that might be drawn from it, there is no scientific way to overcome his refusal.

...

And if science requires, by its own terms, that its verdict be voluntarily accepted, then scientific method has little or no place in the direct processes of legal decisionmaking. This is a fundamental limitation on the uses of behavioral science in the law. [Geoffrey C. Hazard, Jr., pp. 74-75].

I believe that Martin and Hazard are mistaken. They both ignore the institutional forces that pervade the community of scientists. These forces help transmit the state-of-the-art theory from member to member in the community, sanctioning the theory's application in research and in teaching.

These institutional forces comprise the object of study of the discipline called the sociology of science. Popper refers to these institutional forces in his phrase, "mutual rational control" [emphasis added], and Kuhn explains scientific progress in terms of what the scientific group values, tolerates, and disdains. It is these institutional forces that render a scientific theory binding on scientists; the verdicts of science are hardly "voluntarily accepted" by scientists at all. Michael Polanyi has written about the binding influence of the institutional forces of science on himself, regarding one of his theories which the scientific community had rejected (but, also, which the community only later came to recognize as correct):

Even as professor of physical chemistry at the Victoria University of Manchester, I was unable to teach my theory. Undergraduates would have expected to be examined on it. But examinations were set and marked by a committee that included an external examiner and members of the teaching staff junior to myself. I could not undertake to force on them views totally opposed to generally accepted opinion. A system of collegiate examinations severely curtails the teaching of views that conflict with currently dominant scientific opinion.¹

The basic problem with the statements of Martin and Hazard is that they conceive of science primarily as a system of logic. They do not explicitly account for the necessary role played by the institutional forces, which render the system of logic into something that is really more like a language.

As a third example, consider:

... scientific reasoning relies primarily on induction ...

[Donald W. Large and Preston Michie, "Proving that the Strength of the British Navy Depends on the Number of Old Maids in England: A Comparison of Scientific Proof and Legal Proof," Environmental Law, 11:584 (1981).]

Large and Michie define induction as "reasoning from particular experiences to general truths", which is a practice that Popper and Kuhn would oppose. Writes Kuhn: "But neither Sir Karl nor I is an inductivist. We do not believe that there are rules for inducing correct theories from facts, or even that theories, correct or incorrect, are induced at all."² I believe that Large and Michie (who, incidentally, refer to R. Pirsig's Zen and the Art of Motorcycle Maintenance [1975] as their source for philosophy of science) are mistaken.

As a final example, consider:

To the extent that there is "a method" of reasoning in the law, it is the same as that in science--it is the rational method. [Emphasis in the original, Schriren, p. 198.]

How do I dare disagree with Schriren, a Professor of Philosophy at the University of California? I dare in a justifiable way: using my conception of law and science (a conception different from Schriren's), I had formulated a theory with falsifiable hypotheses, and I then refined the theory, but maintained its falsifiability while, hopefully, increasing its accuracy.

What ultimately determines whether my study is a valuable contribution to the literature will be, first, its ability to withstand attempts to refute it and, second, its ability to serve as the basis for an improved (but still falsifiable) theory, in the event that it happens to be refuted.

8.5 Parallel Efforts in the Literature

So far, the commentary on the literature may be giving the impression that the literature contains no insightful research. This impression is far from the actual case.

Other researchers have recognized, in ways that parallel this study, that there are differences between law and science. However, I have discerned in their work no counterpart to the notion of premise-oriented justification v. conclusion-oriented justification, or the notion of cultural and institutional forces. These articles are "Law and Science as Rival Systems" (Lee Loevinger, University of Florida Law Review, 21:530-551 [1966-67]); "The Scientist as Expert Witness: Why Lawyers and Scientists Can't Talk to Each Other" by James Friedman (Jurimetrics Journal, 18:99-106 [Winter 1977]); and "Understanding Legal Thinking" by Vilhelm Aubert (in Law and Society, ed. by _____, pp. 144-148).

Chapter 9 The Role of Knowledge in Policy-Making

The purpose of this chapter is simply to provide the reader with some perspective. I will answer the question, why did I choose to conduct the inquiry that I did?

9.1 Background and Motivating Interests:

My undergraduate education was in Engineering. My graduate education has been in planning and public policy. The motivation for the professional emphasis in my education has been a concern for problems and problem-solving.

Traditionally, the professional approach calls for the application of problem-solving methods while focusing one's attention on a substantive problem in nature or society, like environmental pollution or employment discrimination. While initially embracing the professional approach, I later shifted my focus from substantive problems to the act of problem-solving.

I believe that acts of problem-solving are not as well understood as the problems to which they are applied. As a result, problem-solving often becomes a substantive problem in itself, equally deserving as an object of inquiry and problem-solving activity.

I have been fascinated by the particular form of problem-solving that professional schools of planning, public policy, and management call "policy-making". The problem-solving method that these schools espouse is the scientific approach. The core of their curricula is economics, sociology

(usually in the form of organizational behavior), and statistics. Whether or not the scientific approach is appropriate for policy-making has remained a largely unquestioned assumption; however, in this study, I opened up this assumption to scrutiny.

9.2 The Rationalist Belief

Over the course of my education, I have generally encountered the fact that my colleagues (student and faculty alike) usually accept, as a matter of common sense, that there is an unrivaled value to the scientific approach in policy-making. (Invariably, their response to my concern, whenever I have worked up the courage to question their common sense, was that it was silly even to suggest an "unscientific" approach.) I have come to characterize these colleagues as devotees of "the rationalist belief". I have also sensed these devotees throughout the many fields of policy-making, whether they call it planning, decision-making, problem-solving, policy analysis, management, adjudication, or administration.

There are variations to the form the rationalist belief may take, but they are all built around the same kernel. The kernel is a depiction in which the rational application of social-science knowledge does, can, or should play a central role in policy-making.

According to the rationalist belief, there is a knowledge-base of facts, scientific findings, data, information, research, and rational judgments. Allegedly, successes in policy-making take place when such knowledge is applied to a problem, solving it. Failures in policy making result when such knowledge is applied improperly or is unavailable to be applied at all. Whether in explaining successes or failures, the rationalist belief gives scientific knowledge a central role in its depiction of how policy-making takes place.

I single out three positions to illustrate the rationalist belief.

1. Political decision making is irrational. According to this position, policy-making in the political arena could be made rational if political decision makers received social-science knowledge as the basis on which to design a course of action; as it currently stands, however, such knowledge is used instead as ammunition for defending or attacking a course of action that has already been decided on. This position holds that if social-science knowledge must be used in this way at all, it should at least be deployed to quell the irrational opposition, whereupon it could proceed with the job for which it is intended: providing a factual and logical basis for policy-making.

2. Insufficient knowledge causes policies to fail. Suppose there is a situation in which the attempted application of social-science knowledge does overcome political barriers. Suppose further, however, that the problem at hand persists. How would the rationalist belief explain this (for example, our society's inability to deal with the simultaneous problems of high inflation and high unemployment)? The rationalist belief would pin the blame on faultiness in the knowledge (Keynesian economics, it is claimed, is inadequate to today's economic woes) and would therefore recommend an overhaul of the knowledge base (hence, supply side economics).

3. More knowledge is better. Better predictive power will come to social science with continued research. Someday, social science will control the problems it currently finds unmanageable: crime, mental illness, discrimination, unemployment, inflation, productivity, housing, transportation, poverty. The position here is part and parcel of the more general notion that the scientific approach is capable of delivering us from the evils of the physical world: disease, starvation, pollution, scarcity of resources.

Scientific knowledge is technology for making things better. "The growing importance of the undisciplined problems facing public officials and social scientists is an argument for increasing efforts to use social science to cope with these problems, rather than dismissing the only technology that government has at hand."¹

These three positions and the rationalist belief in general are usually held to be true beyond all question. They are accepted as matters of common sense. What is there in the belief for me or anyone else to dispel?

I question the rationalist belief because I feel its depiction of policy-making to be lacking. I am not convinced that, as a general rule, social-science knowledge does, can, or even always should play a central role in how policy-making takes place. I have two basic objections.

First, the rationalist belief depicts policy-making as an activity that individuals perform through the exercise of their free wills. Such a depiction is not incorrect, but it is incomplete. It ignores the structure of defined opportunities and constraints which an individual policy-maker encounters as ready-made, and hence given to him, when he appears on the organizational scene. It also ignores the ingrained ways of thinking that the policy-maker, as a product of his profession's cultural forces, carries with him to the organizational setting. In short, the rationalist belief's depiction of policy-making underrates (if it acknowledges at all) the cultural and institutional forces that operate on a policy-maker's behavior. The rationalist belief thus has no explanation for the fact that judges and lawyers do not understand or apply inferential statistics in the way that scientists do, except possibly the explanation that these judges and lawyers

are being absurd and irrational.

Second, I object not just to the rationalist belief's exclusion of structural (that is, cultural and institutional) forces from its depiction of policy-making, but also to the manner of the exclusion. Proponents of the rationalist belief summarily dismiss these forces as "politics". I concede that this label identifies, in small part, the structural forces to which I refer, but I feel the label to be unjustly deprecatory.

It depicts the playing out of structural forces as if such a process were a third-rate alternative to the reasoned judgments of individuals. It brushes aside "political" forces as if they were incidental to the "real" process of policy-making, which it envisions as the rational application of scientific findings or other "objective" information.

Such a depiction, I believe, is a wholly inaccurate map of the territory--that is, the territory of how policy-making actually takes place. As the theory of scientist-lawyer communication reveals, structural forces are not incidentally bothersome to the social activity of policy-making, but constructively provide the very framework according to which it unfolds. In the light of this dissertation's analysis of judicial policy-making, I dare say that if anything is incidental, it is the role of social science, whose significance, if any, in policy-making is set up in large part by the structural forces. If we go so far as to equate the structural forces to the policy-making process itself, we might say that it is policy-making which makes the role of social science possible, rather than the role of social science which makes policy-making possible. To assert otherwise, as does the rationalist belief, would be to make the tail wag the dog.

At this point, it may be helpful to address a concern that many of my colleagues have raised: Does the courtroom pose a situation that is representative of the scientific approach in policy-making? After all, not all policy-makers are like lawyers and judges, and the role of scientific expertise in this setting is mediated by the rules and machinations unique to the courtroom.

Suppose I were to shift my focus away from the courtroom. On what would I be focusing then? Scientific and other types of knowledge, when present in the policy-making process, exist in one or another institutional setting. If not the courtroom setting, then what other setting?

To return to an often-used illustration in this study, suppose that a judge were interacting with a scientist not in a courtroom, but instead in a seminar of graduate students in the scientist's discipline, or in a staff meeting of scientists in a consulting firm. The institutional forces mediating the scientist-judge interaction in the courtroom would be merely replaced; the presence of institutional forces would not be eliminated. There is no "pure" relationship between science and policy-making; "pure", that is, in the sense of being free of institutional forces that mediate the role of scientific knowledge. This is true whether the policy-maker is a judge, planner, policy analyst, manager, architect, politician, social worker, or other professional. In my formulation, the role of science is always subject to the institutional forces (perhaps, the rules and machinations) unique to the policy-maker's institution. The courtroom setting merely illuminates, in the manner of a case study, the operation and the impact of these forces.

With regard to my motivating interest, judicial policy-making represents but one instance of policy-making in our society. Just as judges and lawyers speak their own language in their own culture, perhaps other professionals, like managers, planners, politicians, social workers, etc., do the same. If this turns out to be true, then what would be the practical significance of social-science knowledge? What would be the use of teaching economics, sociology (organizational behavior), and statistics in schools of planning, public policy, and management? If professionals do not use social-science knowledge, then what are the forms of knowledge that they do use? What are the conditions under which such knowledge is more effective or less effective in problem-solving? What, then, are the forms of knowledge that should be taught in schools of planning, public policy, and management? What is this artifact, called "knowledge", and what role does it play in this social activity, called "policy making"? What is the research design by which we, as observers, may formulate and test theories regarding these questions? These are the underlying questions that motivate my long-run research interests.

9.3 The Sociology of Policy-Making and the Policy of Policy-Making

The AT&T-EEOC litigation was but a single part of a larger process by which the two parties negotiated and then implemented the Consent Decree, which laid out the steps AT&T would take to promote equal employment opportunity for its female and minority employees. [See generally, Phyllis A. Wallace, Equal Employment Opportunity and the AT&T Case, M.I.T. Press, 1976.]

With regard to the role of knowledge in the policy-making process that produced and administered the Consent Decree, there was an organizational distribution of knowledge: the knowledge of scientists, submitted to the lawyers, was different from the knowledge of the lawyers, whose knowledge was not the same as the knowledge contained in the Consent Decree, which took on a different meaning to its implementors, practically none of whom were present for the Consent Decree's formulation. In this example, the organizational distribution of knowledge deals with who knows what, what is known, where in the organizational setting they are positioned, when they are occupying those positions, and how the factors of who, what, where, and when (in addition to the factors of the cultural and institutional forces) mediate the communication of knowledge from person to person.

The organizational distribution of knowledge is similar to the economic distribution of income; just as the income a person possesses may be explained by structural dimensions such as the person's race, sex, age, education and experience, I believe that the knowledge which an individual in the policy-making process possesses may be explained by the structural forces that define the policy-making situation: the person's profession,

the person's role or position in the organizational setting, the organization's formal and informal rules, etc.

Just as the economic distribution of income can be rigid, the organizational distribution of knowledge can be rigid. Poverty can be no more easily eradicated from society than the scientific rules of evidence may easily supplant the legal rules of evidence in the courtroom.

The theme that emerges from this brief discussion is that the role of knowledge (scientific or otherwise) in policy-making must be explained with reference to the social processes in which it is embedded. Only after establishing a sociology of policy-making (that is, a diagnosis of how policy-making in fact takes place) would it make sense to consider a policy of policy-making (that is, a prescription for how policy-making should take place.)

The rationalist belief amounts to a prescription that is not based on diagnosis. In professional schools of planning, public policy, and management, the rationalist belief prescribes the use of the scientific approach, as in the form of economics, sociology (organizational behavior) and statistics. I believe, however, that no diagnosis of the type I have been describing has every been performed to support such a prescription. If the cross examination of the expert witness provides a representative window onto the fate of scientific knowledge in the overall, policy-making process, then I would speculate that the curricula of these professional schools are inadequate to the task of preparing their graduates for multi-lingual, multi-cultural organizational settings.

The remarks in this concluding chapter are, for the most part, specula-

tions. Yet, they capture the puzzles and the problems that motivate my inquiry. My wish is simply for this study to make a contribution in the way that I mentioned at the beginning of the chapter: By making available a better understanding of the act of problem-solving, I hope to have the impact of facilitating the solving of actual, substantive problems.

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FOOTNOTES
(Author and title are
given. For full citation,
see bibliography.)

Chapter one

1. With regard to maps and territories: "In particular, our most recent examples show that paradigms provide scientists not only with a map but also with some of the directions essential for map-making." Thomas Kuhn, The Structure of Scientific Revolutions. In this study, we extend the map-territory metaphor to knowledge in general, not just scientific knowledge.

2. See Karl Popper, The Logic of Scientific Discovery, pp. 32-33.

3. Ibid., p. 29.

4. Ibid., pp. 32-33.

5. Thomas Kuhn, "Reflections on My Critics," in Criticism and the Growth of Knowledge, ed. by Imre Lakatos and Alan Musgrave, p. 245.

6. Ibid., p. 237.

7. Popper, p. 108.

8. Ibid., p. 109.

9. Ibid., p. 104.

10. Ibid., p. 105.

11. Ibid., p. 106.

12. Ibid., p. 111.

13. Ibid., p. 44.

14. Ibid., p. 44, footnote *1.

15. Thomas Kuhn, "Logic of Discovery or Psychology of Research?", in Lakatos and Musgrave, pp. 15, 19.

16. Kuhn, "Reflections on My Critics," pp. 237-238.

17. John Dewey, "Logical Method and Law."

18. Roscoe Pound, "The Theory of Judicial Decision, III." Oliver Wendell Holmes, The Common Law and Collected Legal Papers.
19. Dewey, pp. 21-22.
20. Roscoe Pound, "The Theory of Judicial Decision, I," p. 645.
21. See Lon Fuller, "The Forms and Limits of Adjudication."
22. Kocourek and Koven, "Renovation of the Common Law Through Stare Decisis," in Readings on Legal Method, ed. by W. Rose, p. 307.
23. James Kent, Commentaries on American Law, excerpted in W. Rose, p. 302.
24. Henry Campbell Black, Law of Judicial Precedents, excerpted in W. Rose, p. 303.
25. Dewey, p. 22.
26. Pound, p. 945.
27. Ibid., pp. 947-948.
28. Ibid., pp. 949-950.
29. Arthur L. Goodhart, "Determining the Ratio Decidendi of a Case," excerpted in W. Rose, pp. 319-322.
30. Pound, pp. 951-952.
31. We will define "cultural and institutional forces" later in the study. For additional illuminations of the social (perhaps, "sociological") processes operating in a courtroom, see Peter Nardulli, "The Caseload Controversy and the Study of the Criminal Courts"; Jerome H. Skolnick, "Social Control in the Adversary System"; and Suzann R. Buckle and Leonard G. Buckle, Bargaining for Justice.
32. Holmes, pp. 1-2.
33. Dewey, p. 26.
34. Pound, p. 654.
35. Alfred Schutz, "Concept and Theory Formation in the Social Sciences," in Alfred Schutz, Collected Papers, Volume I, ed. by Maurice Natanson, p. 54.

36. Maurice Natanson, "A Study in Philosophy and the Social Sciences," in Philosophy of the Social Sciences, ed. by Maurice Natanson, p. 278.

37. Fuller, pp. 365-366.

Chapter two

1. Thomas Kuhn, "Logic of Discovery or Psychology of Research?" in Criticism and the Growth of Knowledge, ed. by Imre Lakatos and Alan Musgrave, p. 4.

2. Rather than formulating new theories, the normal work of the natural scientist consists of the following, all of which involves premising the current theory: "... three classes of problems -- determination of significant fact, matching of facts with theory, and articulation of theory -- exhaust, I think, the literature of normal science, both empirical and theoretical." Thomas Kuhn, The Structure of Scientific Revolutions, p. 34.

3. Popper, The Logic of Scientific Discovery, p. 13.

4. Ibid., pp. 31-32.

5. Hans Reichenbach, The Rise of Scientific Philosophy, p. 231, as quoted by Lon Fuller, "An Afterword: Science and the Judicial Process," p. 1625.

6. Thomas Kuhn, "Second Thoughts on Paradigms," in The Essential Tension, pp. 293-294.

7. Alfred Schutz, "Concept and Theory Formation in the Social Sciences," in Alfred Schutz, Collected Papers, Volume I, ed. by Maurice Natanson, p. 59.

8. Lon Fuller, "Symposium--Natural Law."

9. Schutz, p. 59.

10. See Alfred Schutz, "Some Leading Concepts of Phenomenology," in Alfred Schutz ..., ed. by Maurice Natanson, pp. 112-113.

11. Van Maanen's reference is: H. Conklin, "Ethnography," in International Encyclopedia of the Social Sciences, ed. by D.L. Sills (New York 1968) 5:115-208.

12. John Van Maanen, "The Fact of Fiction in Organizational Ethnography," pp. 539-540.

13. See generally, Peggy Reeves Sanday, "The Ethnographic Paradigm(s)."
14. Frederick O. Gearing, The Face of the Fox, p. 128.
15. Sanday's reference is: Frake, as quoted by Harry Wolcott, "Criteria for an Ethnographic Approach to Research in Schools," Human Organization, 34:111-128.
16. Sanday, p. 529.
17. Schutz, pp. 63-65.
18. Alfred Schutz, "Common-Sense and Scientific Interpretation of Human Action," in Alfred Schutz ..., ed. by Maurice Natanson, p. 41.
19. Ibid., p. 43.
20. Schutz, "Concept and Theory Formation in the Social Sciences," p. 63.
21. Ibid., pp. 56-57.
22. Schutz, "Common-Sense and Scientific Interpretation of Human Action," pp. 43-44.
23. Sanday, p. 530.
24. Kuhn, "Logic of Discovery or Psychology of Research?", p. 12.
25. "Ideal type" is a Weberian formulation. "Typification", "kernel", "sedimentation", and "essence" are formulations found in the writings of Alfred Schutz; and in Peter Berger and Thomas Luckmann, The Social Construction of Reality.
26. Ernest Nagel, The Structure of Science, pp. 450-451.
27. Ibid., pp. 452-453.
28. Ibid., p. 452.
29. Abraham Kaplan, The Conduct of Inquiry, p. 7.
30. The progression to which I refer is described by Popper in the following way: "Indeed, as I have explained elsewhere, 'scientific knowledge' may be regarded as subjectless. It may be regarded as a system of theories on which we work as do masons on a cathedral. The aim is to find theories which, in the light of critical discussion, get nearer to the truth."

Thus the aim is the increase of the truth-content of our theories ... " [Emphasis added.] Karl Popper, "Normal Science and Its Dangers," in Lakatos and Musgrave, p. 57.

31. Popper, The Logic of Scientific Discovery, p. 33.

32. Lon Fuller, "An Afterword: Science and the Judicial Process," p. 1624.

33. Sanday, pp. 533-534.

Chapter three

1. Compare this to the statement: "The conclusions of a science may be both precise and binding without being fully derivable by logic from accepted premises." Thomas Kuhn, "Logic of Discovery or Psychology of Research?", in Criticism and the Growth of Knowledge, ed. by Imre Lakatos and Alan Musgrave, p. 9, footnote 1.

2. Orley Ashenfelter, "Telephone Rates in the Absense of Discrimination," pp. 3-4, Expert Witness Testimony, EEOC Exhibit 4, FCC Docket No. 19143.

3. Of course, the discussants are using the word "hypothesis" in a way different from this study's. The discussants use it synonymously with "assumption," which is a theory's premise. In this study, we use it to mean a theory's conclusion.

4. Strictly speaking, Boyle's Law is $PV=\text{constant}$. The equation of state for an ideal gas is $PV=nRT$, which is a more refined and generalized version of $PV=\text{constant}$.

5. Orley Ashenfelter and John Pencavel, "Estimating the Effects on Cost and Price of the Elimination of Sex Discrimination: The Case of Telephone Rates," in Equal Employment Opportunity and the AT&T Case, ed. by Phyllis A. Wallace.

6. Roscoe Pound, "The Theory of Judicial Decision, III," p. 947.

7. See generally, Peter Berger and Thomas Luckmann, The Social Construction of Reality.

8. See generally, Milton Singer, "The Concept of Culture," and Frederick O. Gearing, "The Face of the Fox."

9. "EEOC Reply to Opposition of Bell Company Respondents," p. 4, FCC Docket No. 19143.
10. See Black's Law Dictionary.
11. "EEOC Reply ... ," pp. 1-2.
12. Ibid., p. 5, footnote 5.
13. Ochoa v. Monsanto, 335 F. Supp. 53, quoted in "EEOC Reply ... ," p. 6.
14. "EEOC Reply ... ," pp. 35-37.
15. Ibid., p. 30.
16. Ibid., p. 20.
17. Ibid., p. 3.
18. Ibid., p. 7.
19. See also, "Reply of the Chief, Common Carrier Bureau, to the Opposition of Respondents to the Bureau's Motion to Strike the Testimony of Leona Tyler and Nathan Glazer" and "Opposition of the Bell Company Respondents to Motions to Strike Portions of Their Proffered Testimony," FCC Docket No. 19143.
20. "EEOC Reply ... ," p. 4.
21. "Reply of the Chief ... ," p. 2.
22. "EEOC Reply ... ," p. 2.
23. Wallace, p. 246.
24. Ashenfelter and Pencavel, p. 121.
25. See D. Campbell and J. Stanley, Experimental and Quasi-Experimental Designs for Research.
26. Strictly speaking, it is not Dr. Ashenfelter, but our scientist-puppet of Dr. Ashenfelter, whom we put forth as speaking science.
27. "Conclusion-oriented justification" goes by the simpler name of "hypothesis-testing" among members of the scientific culture.
28. Such re-creation also explains the existence of a heterogeneous distribution of knowledge over the population in an organizational setting. See chapter nine of this study.

29. "Though the lines are the same, the figures which emerge from them are not. That is why I call what separates us a gestalt switch rather than a disagreement and also why I am at once perplexed and intrigued about how to explore the separation." Kuhn, discussing his own views of science in comparison to Popper's, in "Logic of Discovery or Psychology of Research?", p. 3.

Chapter four

1. See Black's Law Dictionary.
2. See chapter six of this study for examples of additional documents that Mr. Powers looked up.
3. Interviews with individuals present at the hearing.
4. Judith Long Laws, "Causes and Effects of Sex Discrimination in the Bell System," p. 3, Expert Witness Testimony, EEOC Exhibit 4, FCC Docket No. 19143.
5. Ibid., p. 16.

Chapter five

1. Alfred Schutz, "Don Quixote and the Problem of Reality," in Alfred Schutz, Collected Papers, Volume II, ed. by Arvid Brodersen, p. 136.
2. Ibid., p. 136.
3. Ibid., p. 135.
4. Ibid., pp. 141-142.
5. Joseph Stein and Jerry Bock, Fiddler on the Roof.
6. Schutz, pp. 142-143.
7. Ibid., pp. 139-140.
8. Ibid., pp. 140-141.
9. Ibid., p. 140.
10. Ibid., pp. 143-144.
11. Ibid., p. 146.

11. Karl Popper, Conjectures and Refutations, pp. 34-35.
12. Schutz, pp. 140-141.

Chapter six

Quotations from the written testimony are taken from "Testimony of Andre Hellegers," Expert Witness Testimony, EEOC Exhibit 4, FCC Docket No. 19143.

1. Popper made this remark regarding another point in the philosophy of science. The Logic of Scientific Discovery, p. 93.
2. Ibid., p. 104.
3. Ibid., p. 105.
4. Ibid., p. 106.
5. Ibid., p. 111.
6. Ibid., p. 44.
7. Ibid., p. 44, footnote *1.
8. Ibid., p. 106.
9. Ibid., p. 104.

Chapter seven

1. Alfred Schutz, "Concept and Theory Formation in the Social Sciences," in Alfred Schutz, Collected Papers, Volume I, ed. by Maurice Natanson, p. 54.
2. Thomas Kuhn, "Reflections on My Critics," in Criticism and the Growth of Knowledge, ed. by Imre Lakatos and Alan Musgrave, pp. 237-238.
3. Roscoe Pound, "The Theory of Judicial Decision, I," p. 254.
4. John Dewey, "Logical Method and Law."
5. Schutz, p. 63.
6. See Martin Rein, "Values, Social Science and Social Policy," in Social Science and Public Policy, p. 136.

7. Kocourek and Koven, "Renovation of the Common Law Through Stare Decisis," excerpted in Readings on Legal Method, ed. by W. Rose, p. 149.

7. Stuart, Reginald. "Judge Overturns Arkansas Law on Creationism," p. B8.

Chapter eight

1. Michael Polanyi, "The Potential Theory of Adsorption," in Knowing and Being, ed. by Majorie Greene, p. 94.

2. Thomas Kuhn, "Logic of Discovery or Psychology of Research?", in Criticism and the Growth of Knowledge, ed. by Imre Lakatos and Alan Musgrave, p. 12.

Chapter nine

1. Richard Rose, "Disciplined Research and Undisciplined Problems," in Using Social Research in Public Policy Making, ed. by Carol Weiss, p. 35, as quoted by Charles E. Lindblom and David K. Cohen, Usable Knowledge, p. 29.