LOWERED HORIZONS: IMPLEMENTATION RESEARCH IN A POST-CLS WORLD

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The Critical Legal Studies movement is often perceived as being unsympathetic to the use of empirical research generally, and in particular to "implementation research"—that is, research that employs empirical techniques to determine the changes in behavior associated with changes in legal rules. In this Essay, Professor William Whitford examines the major criticisms of implementation research that have been or could be derived from CLS writings: the critique of determinism, the indeterminacy hypothesis, the conceptualism critique, and Mark Kelman's criticism of empiricism as inherently conservative. Professor Whitford answers these criticisms, arguing that implementation research can be helpful in making educated guesses about how changes in legal rules have and will affect human behavior. Educated guesses are needed by those who seek to develop practical strategies for achieving social change through use of law. Professor Whitford also maintains that the CLS criticism has been valuable in suggesting ways implementation research can limit methodological bias.

THE OTHER TIGER

I think of a tiger. The half-light enhances
the vast and painstaking library
and seems to set the bookshelves at a distance;
strong, innocent, new-made, bloodstained,
it will move through its jungle and its morning,
and leave its track across the muddy
edge of a river, unknown, nameless
(in its world, there are no names, nor past, nor future
only the sureness of the passing moment)
and it will cross the wilderness of distance
and sniff out in the woven labyrinth
of smells the smell peculiar to morning
and the scent of deer, delectable.
Among the slivers of bamboo, I notice
its stripes, and I have an inkling of the skeleton
under the magnificence of the skin, which quivers.
In vain, the convex oceans and the deserts
spread themselves across the earth between us;
from this one house in a remote lost seaport

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in South America, I dream you, follow you,
oh tiger on the fringes of the Ganges.

Afternoon creeps in my spirit and I keep thinking
that the tiger I am conjuring in my poem
is a tiger made of symbols and of shadows,
a sequence of prosodic measures,
scraps remembered from encyclopedias,
and not the deadly tiger, the luckless jewel
which in the sun or the deceptive moonlight
follows its paths, in Bengal or Sumatra,
of love, of indolence, of dying.
Against the symbolic tiger, I have planted
the real one, it whose blood runs hotly,
and today, 1959, the third of August,
a slow shadow spreads across the prairie,
but still, the act of naming it, of guessing
what is its nature and its circumstances
creates a fiction, not a living creature,
not one of those who wander on the earth.

Let us look for a third tiger. This one
will be a form in my dream like all the others,
a system and arrangement of human language,
and not the tiger of the vertebrae
which, out of reach of all mythology,
paces the earth. I know all this, but something
drives me to this ancient and vague adventure,
unreasonable, and still I keep on looking
throughout the afternoon for the other tiger,
the other tiger which is not in this poem.

Jorge Luis Borges
A PERSONAL ANTHOLOGY (1967)

I. INTRODUCTION

One form of empirical research that has proven popular among legal scholars—some would say it has been the bread and butter of the law and society movement—is what is commonly referred to as implementation research. Although a precise definition of implementation research remains elusive,[1] in general implementation research seeks to learn about the effects of a legal initiative—a new statute, administrative regulation, or judicial decision establishing a new precedent—by empirical means. Legal scholars frequently resort to implementation research in order to facilitate the design of social change through the manipulation of legal rules—a process sometimes known as social engi-

1. *See* Clune & Lindquist, *What "Implementation" Isn't: Toward a General Framework for Implementation Research*, 1981 WIS. L. REV. 1044.

neering. Unless the context otherwise indicates, in this Essay, I use the term implementation research to refer to studies for such purposes.[2]

There is a widespread impression that the teachings and followers of Critical Legal Studies (hereinafter CLS) debunk empirical studies about law.[3] However, CLS texts[4] rarely discuss explicitly the legitimacy of implementation research or even of empirical research generally. The principal exception is Mark Kelman's article, *Trashing*.[5] Kelman does not expressly state whether he objects to either implementation or empirical research as epistemologically inadequate.[6] But Kelman clearly believes that, in light of alternative methods of intellectual inquiry, devoting time and energy (a scarce resource) to empiricism reflects a poor sense of priorities—it is politically incorrect, one might say.

The absence of authoritative texts notwithstanding, the widespread impression that mainstream CLS commentators object to the epistemological validity of implementation research is not completely unfounded. Few involved with CLS believe that Kelman is alone in questioning at least the political correctness of empirical work; such questioning has been a frequent topic at CLS meetings.[7] Moreover, by building on the standard arguments of "deconstruction" replete in CLS literature, one can construct an argument that implementation research is epistemologically invalid. The unarticulated sense that this argument exists, I believe, accounts for the widespread impression that it has been made.

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2. There are other possible purposes for research that could be described as implementation research in a more general sense. For example, studies about the impact of a statute might be used to show that the statutory initiative did not work as represented, thus making it easier to persuade the members of a group thought to be favored by the legislation to join some political organization. All forms of implementation research are subject to what I shall call in subsequent discussion the critique of conceptualism, but implementation research for the purpose of social engineering is subject to the greatest questioning based on Critical Legal Studies precepts. Hence, I focus on this form of implementation research.

3. *See* White, *From Realism to Critical Studies: A Truncated Intellectual History*, 40 SW. L.J. 819 (1986); Trubek, *Where the Action Is: Critical Legal Studies and Empiricism*, 36 STAN. L. REV. 575, 576 (1984).

4. There is no consensus about what qualifies as a CLS "text". For a history of Critical Legal Studies, suggesting that it is not possible to define a particular creed, see Schlegel, *Notes Toward an Intimate, Opinionated, and Affectionate History of the Conference on Critical Legal Studies*, 36 STAN. L. REV. 391 (1984). For my purposes, a CLS text is simply one written by a person who has been associated with the Conference on Critical Legal Studies. A bibliography of these writings, now somewhat dated, appears in Kennedy & Klare, *A Bibliography of Critical Legal Studies*, 94 YALE L.J. 461 (1984).

5. Kelman, *Trashing*, 36 STAN. L. REV. 293 (1984).

6. Implementation and empirical research would be epistemologically inadequate, as I use the term, if by these methods one could never learn anything that might be considered valid.

7. *See* Schlegel, *supra* note 4, at 409 n.53. In a recently published article, White provides an interpretation of the split between the Law and Society movement and CLS that centers on the latter's criticism of the work of the former. White, *supra* note 3, at 832-36.

In this Essay, I will construct the arguments against the validity of implementation research, and to a lesser extent against empirical work generally, by using the standard deconstruction hypotheses. I will indicate why I believe that implementation research can be justified nonetheless.[8] I recognize that this exercise has all the characteristics of building a strawman, only to knock it down, but I hope it will prove useful nonetheless.

I will next discuss Mark Kelman's critiques of implementation research. Some of Kelman's arguments can be understood as questioning the epistemological validity of empirical research altogether. But even assuming that Kelman's critique states only a pragmatic argument against engaging in such research, I will contend that implementation research can be useful, and in that sense "politically correct," in particular circumstances. I will then discuss the relation of my perspectives to those advanced by others who have written on CLS and empirical research, primarily my colleague, David Trubek. I will conclude by commenting on some implications I believe CLS writings have had for the form of implementation research.

II. THE IMPLICATIONS OF DECONSTRUCTIONIST HYPOTHESES

A. The Critique of Determinism

It is standard in CLS work to attack determinism. Although the word "determinism" means many things, I use it here to refer to the philosophy that there are inherent "laws" of social order which explain how certain events are causally linked to other events. Much social science as practiced today appears to presuppose determinism, given its concentration on cause-and-effect propositions based on inductive reasoning from empirical findings.[9]

The CLS response to determinism is to deny the timelessness of any social practice by affirming the power of humans to create their social institutions by choice.[10] If social practices result from choice, and are not the ineluctable product of some unchangeable set of circumstances, it follows that those practices can be changed at any time.

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8. In the interests of full disclosure, much of my own work qualifies as implementation research, as I have defined the term. *See, e.g.*, Whitford, *The Small Case Procedure of the United States Tax Court: A Successful Small Claims Court*, 1984 AM. B. FOUND. RESEARCH J. 797; Whitford, *Structuring Consumer Protection Legislation to Maximize Effectiveness*, 1981 WIS. L. REV. 1018; Grau & Whitford, *The Impact of Judicializing Repossession: The Wisconsin Consumer Act Revisited*, 1978 WIS. L. REV. 983.

9. I shamelessly steal my notion of determinism from Dave Trubek's concept of "behaviorism". See Trubek, *supra* note 3, at 600-03.

10. *See* Gordon, *New Developments in Legal Theory*, in THE POLITICS OF LAW 281, 287-90 (D. Kairys ed. 1982).

Implementation research seeks to determine what changes in social behavior, if any, are caused (at least in part) by legal initiative. The use of implementation research to improve social engineering capacity implies that we seek lessons that might be applied in the future. The critique of determinism suggests, however, that certain prediction about future events in the social world is not possible. The existence of a correlation between law and behavioral change in the present day cannot prove that the same relationship will be repeated in the future, when the social practices on which the relationship exists may have changed.[11]

One possible implication of the critique of determinism is that epistemologically valid research must take a strictly descriptive approach, avoiding statements about possible cause and effect relationships between two observed phenomena, or predictions about the recurrence of the phenomena in the future. This is the tradition of thick description or phenomenonology, which certainly has it adherents within the CLS tradition.[12] Implementation research would still be possible to the extent that it simply describes a legal change and behaviorial change that occur at approximately the same time. But implementation research could provide no guide for the future under this view.

In my judgment the critique of determinism itself does not require such an extreme response. I am prepared to accept the critique to the extent that it rejects a timeless, natural law of the social order. It is quite another matter, however, to assert that there are no time-bounded, momentary consistencies in human social behavior that can be anticipated with considerable reliability.[13] The emphasis in so much CLS work on consciousness, or world views, presupposes the same. This work explains behavior as influenced by consciousness, and proposes effort to change consciousness as probably the most efficacious form of political practice.[14] It is a fundamental premise of CLS work that a consciousness is never determined (e.g., by one's social class), and therefore can be changed by an act of will. Nonetheless, I contend that beliefs fundamental enough to be described as a consciousness, although they are always changing in detail, usually retain a stability for long periods of time. The existence of these "structures of thought," with their substan-

11. *See* Gordon, *Historicism in Legal Scholarship*, 90 YALE L.J. 1017 (1981). The same idea is contained in Leff, *Law and*, 87 YALE L.J. 989 (1978). Though Leff is not eommonly identified as a CLS scholar, there is great commonality between the ideas he was working with in the latter parts of his life and the ideas that have absorbed CLS. Of course, few have expressed those ideas as well or as entertainingly as Leff did.

12. *See* Kennedy, *Spring Break*, 63 TEX. L. REV. 1377 (1985). Leff devoted much of his last years to work on the legal dictionary—the ultimate descriptive practiee. *See* Leff, *The Leff Dictionary of Law: A Fragment*, 94 YALE L.J. 1855 (1985).

13. *See* Gordon, *Critical Legal Histories*, 36 STAN. L. REV. 57, 125 (1984).

14. *See* Klare, *Law Making as Praxis*, 40 TELOS 123 (1979).

tial influence over behavior, link legal initiatives to later events in a causative sense, even given the rejection of determinism.

Not all CLS scholars would accept the foregoing argument. Sensitive to the inconsistency between structuralism and the subjectivism that underlies much CLS thought, a number of writers have questioned the idea that there are relatively stable concepts about how society is organized that enable an observer to predict future behavior reliably.[15] In developing a case for implementation research for the purpose of social engineering, I side with that group within CLS that has taken a more structuralist view of the social order. From that perspective the goal is to discover valid propositions about the relationship between legal initiatives and social behavior that exist, not in all times and places, but within a given time and place during and at which particular world views or consciousnesses prevail.

B. The Indeterminacy Hypothesis

Though the critique of determinism is not fatal, in my judgment, to the integrity of implementation research, two other fundamental CLS propositions pose a more serious challenge. The first is the famous indeterminacy hypothesis, beyond doubt the most discussed aspect of CLS work.[16] In CLS work these days, everything is indeterminate—rules, policies, structures of thought, and so on—but for purposes of this Essay I will limit my discussion to the "indeterminacy of rules" hypothesis. Closely related to the work of the legal realists, the rules indeterminacy hypothesis asserts that, in any given conflict situation, there will exist several results for which there are formally correct rationalizations. What determines the result of a case, therefore, is not the rules. And even given a particular result, there will often be a choice between several formal rationalizations for it.

The rules indeterminacy hypothesis would seem to imply that simply changing the rules—through legislation, regulation or judicial decision—can have no predictable consequence on conflict resolution. If this is so, then it is difficult to imagine a rules change that could have a predictable effect on any other behavior: hence there is nothing for implementation research to study.

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15. James Boyle details this conflict in CLS thought most clearly. Boyle, *The Politics of Reason: Critical Legal Theory and Local Social Thought*, 133 U. PA. L. REV. 685 (1985). *See also* Kennedy, *supra* note 12; Heller, *Structuralism and Critique*, 36 STAN. L. REV. 127 (1984).

16. The indeterminacy hypothesis is commonly traced to Duncan Kennedy's early works. Kennedy, *Legal Formality*, 2 J. LEGAL STUD. 351 (1973); Kennedy, *Form and Substance in Private Law Adjudication*, 89 HARV. L. REV. 1685 (1976). For more contemporary statements of the hypothesis, see Altman, *Legal Realism, Critical Legal Studies, and Dworkin*, 15 PHIL. & PUB. AFF. 205 (1986); Spann, *Deconstructing the Legislative Veto*, 68 MINN. L. REV. 473 (1984); Singer, *The Player and the Cards: Nihilism and Legal Theory*, 94 YALE L.J. 1 (1984).

Before assessing this implication for implementation research, I must first explore more deeply the nature of the rules indeterminacy hypothesis. CLS literature has suggested at least two distinct groundings for the rules indeterminacy hypothesis. One form of the hypothesis attributes indeterminacy to the omnipresence of the "fundamental contradiction"—the idea that rules reflect an unresolved ambivalence, which we all feel within us, between a desire for freedom and individuality and a desire for security and relations with others.[17] The other form of the rules indeterminacy hypothesis rests upon the inherent ambiguity of language and draws heavily on European schools of critical thought.[18]

The implications of the rules indeterminacy hypothesis depend partly on the grounding one assigns for the hypothesis. If the hypothesis rests on the fundamental contradiction, it is easy enough to argue that not all rules implicate that contradiction. I find it difficult to argue, for example, that the fundamental contradiction is implicated by the rule defining the minimum age qualification for the American presidency.[19] Some implementation research could be justified, therefore, as focusing on rules that do not implicate the fundamental contradiction, since those rules are not necessarily ambiguous because of our inability to resolve that contradiction.[20]

The apparent implications of the rules indeterminacy hypothesis for implementation research is not so easily avoided if that hypothesis rests on the inherent ambiguity of language. All rules are expressed in language. If language is always ambiguous, it must follow then that all rules are likewise ambiguous. Ambiguity is often enhanced by the existence of canons of construction, such as the admonition to apply a rule according to its purpose. These canons are frequently invoked to justify ignoring what is characterized as a rule's "plain meaning," though perhaps just as frequently a court expresses an obligation to apply a "plain meaning." The result of all this ambiguity, in the CLS view, is that in

17. The origin of the idea of the fundamental contradiction is usually attributed to Kennedy, *The Structure of Blackstone's Commentaries*, 28 BUFFALO L. REV. 205 (1979), but its author has since recanted it—Gabel & Kennedy, *Roll Over Beethoven*, 36 STAN. L. REV. 1, 15-16 (1984)—much to the dismay of some. *See* Hunt, *The Theory of Critical Legal Studies*, 6 OXFORD J. LEGAL STUD. 1, 24-28 (1986).

18. Gary Peller has recently published a sophisticated defense of indeterminacy from this perspective, citing to European sources. Peller, *The Metaphysics of American Law*, 73 CALIF. L. REV. 1151 (1985).

19. *See* Hegland, *Goodbye to Deconstruction*, 58 S. CAL. L. REV. 1203, 1207-10 (1985).

20. Under this view, in doing implementation research one might not bother to assess or predict the effects of vague equitable rules, such as the unconscionability doctrine, because they obviously implicate the fundamental contradiction. On the other hand, the adoption of even a completely ambiguous rule can have symbolic effects, which might be useful to study. *See* notes 29-30 *infra* and accompanying text.

any potential dispute a clever lawyer can articulate arguments, not fallible on logical grounds, for at least two alternative positions.[21]

For purposes of this argument, I am prepared to accept the more radical CLS view that all rules are logically ambiguous. And certainly I believe that enough rules are logically or analytically ambiguous to render meaningless any attempt to justify an implementation research program by concentrating on whatever few unambiguous rules exist. Yet I share with others, particularly lawyers, the sense that the outcome of a case can be predicted, albeit with less than complete accuracy. A seasoned observer can be correct much more often than not in predicting the outcome of many kinds of cases.[22] Furthermore, I believe, along with others, that knowledge of the rules can increase the accuracy of this observer's predictions.[23] If I am right on this point, it follows that a change in rules can alter the observer's predictions.

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21. Hegland, *supra* note 19—like others before him, *see* Carrington, *Of Law and the River*, 34 J. LEGAL EDUC. 222 (1984)—is uncomfortable with the indeterminacy hypothesis because it suggests that judges are not accountable to some objective standard in their decisionmaking. As it is sometimes stated, the distinction between law and politics is drawn into question. This in turn calls into question some of the usual assumptions about what distinguishes democracy from tyranny—such as the common saying that we are a "government of laws, not of men." *See* Michelman, *Justification (and Justifiability) of Law in a Contradictory World*, in JUSTIFICATION 71 (J. Pennock & J. Chapman ed. 1986).

Motivated by this discomfort, Hegland, *supra* note 19, attempts a frontal assault on the indeterminacy position. In the end, however, because of the canon that a rule must be applied according to its purpose, coupled with a lack of any determinate or logical grounding for discovering a rule's purpose, even Hegland admits that judgment must be exercised in deciding a case. He argues that this judgment is not always "subjective," apparently because there will often be widespread consensus about what is a good and a bad argument. I interpret his position as not fundamentally different from the position I articulate subsequently for why case outcomes are sometimes predictable.

In illustrating that at least some rules are reasonably unambiguous, Hegland uses the example (which he borrows from Spann, *supra* note 16), of the minimum age qualification for the presidency. Numbers are, of course, generally conceded to be the least ambiguous of the symbols we use in speech, though because of the need to interpret a rule according to its purpose even here Hegland admits to the need for "judgment." In exploring this example, Hegland concentrates too much on the lack of ambiguity in an age. He should have been concerned with what it means to be "president." Suppose for example an underage aspirant for the position were to put forward as a candidate an aged person with whom our aspirant has entered a contract granting her the powers of a guardian, including the powers to exercise the aged person's official powers as president. Would such a contract be enforceable? Does the constitutional age restrictions on who may be president bar such an arrangement, whether or not the contract is enforceable? Surely Hegland must admit some doubt as to these questions, and for precisely that reason should an underage person ever seek the presidency that is the form in which the questions are most likely to be put.

Stick, *Can Nihilism Be Pragmatic?*, 100 HARV. L. REV. 332 (1986), published as this Essay was in page proofs, also attempts a defense of the objectivity of judicial decisionmaking. The timing of its publication prevents me from commenting on it here.

22. My good friend, Joel Handler, has frequently defended this proposition in conversation by referring to the routine handling of welfare administration complaints by the bureaucracy.

23. *See* Yablon, *The Indeterminancy of the Law: Critical Legal Studies and the Problem of Legal Explanation*, 6 CARDOZO L. REV. 917 (1985).

This conclusion creates a paradox: a change in rules is analytically indeterminate because of the ambiguity of language, yet it can have predictable effects on behavior, particularly in the conflict resolution context. The resolution I suggest for this paradox borrows on the idea of speech communities—groups that tend to share common understandings of words, despite their inherent ambiguity from a logical perspective. Let me suppose that rules often create expectations about how cases will come out among law consumers (practitioners, judges, even law professors). Those expectations often influence case outcome, and, more importantly, they also influence settlement practices. In addition, expectations affect perceptions about whether a controversy is worth litigating. Consequently, if a change in rules alters expectations about case outcome, it is likely also to alter social behavior related to litigation, even though formally correct rationalizations exist for other than the expected results—perhaps even for the same outcomes as were reached before the legal initiative was taken.

By explaining the effects of a rule change in terms of the altered expectations about case outcome that the change induces, I can account for the interest of most law trained people in the circumstances of the new rule's creation—its legislative history, the precedent overruled, and so on. If these circumstances are known, one can look beyond the actual verbal formulation of the rule and guess at the purposes of the enacters of the new rule. There have even grown up certain conventions (which we call canons of statutory construction) concerning what inferences can appropriately be drawn about the purposes of the rule enactors from the circumstances surrounding the rule's enactment. On the assumption that law appliers will want to implement those purposes (an assumption usually made by law consumers, though not always valid), we then have more information with which to predict how the rule change will affect common expectations about future case outcomes. Stated in a different way, knowledge of a new rule is more complete if one knows not only its formal verbalization but also the circumstances of its enactment. With such knowledge one can predict the effects of the new rule with greater accuracy.

To illustrate this point, consider the case of section 1202 of the Internal Revenue Code, which was repealed effective January 1, 1987. Section 1202 established preferential taxation of long term capital gain income by allowing a sixty percent deduction of what the tax code defines as net capital gain. The repeal of this section will not necessarily change any one person's tax return. A person might choose to ignore the statutory change, and her return may not be audited. Moreover, the tax code is sufficiently vague that it is entirely possible for a taxpayer, by relying on different tax principles, to come up with the same bottom line

(i.e., taxes owed) as was permitted by utilizing the sixty percent deduction for net capital gain. Yet many observers, including myself, believe that repeal of section 1202 will affect behavior, in partly predictable ways. This belief has been sufficiently strong that over the years many people spent huge sums of money to lobby against the repeal of section 1202.[24] Thus, despite the fact that the change in section 1202 need not change the bottom line for any particular taxpayer, many of us believe that the repeal will increase the bottom line for many taxpayers, and that it will induce some changes in the investment behavior of others (those who now are careful to structure their investments to take advantage of the 1202 tax break). Note, however, that even these judgments are not a certainty. For example, a court might declare the legislation repealing section 1202 unconstitutional, thus restoring the status quo.[25] To be epistemologically valid, however, implementation research need not predict the effects of a legal change to a certainty. It is enough to be able to make valid probabilistic judgments.

A view of the indeterminacy hypothesis similar to the one expressed here is reflected in Duncan Kennedy's soon to be published manuscript on the phenomenology of judicial decision-making.[26] Kennedy's hypothetical judge faces a request for an injunction to prevent striking bus drivers from lying in front of buses driven by scabs. Kennedy's judge is a staunch believer in the indeterminacy hypothesis, but she also believes it is necessary to rationalize her decisions in a conventional manner. She believes that as a logical matter she could decide the lie-in case almost any way she wishes, but she recognizes that some results are easier to justify than others. The more difficult alternatives require greater intellectual energy to develop formal justifying rationalizations (mostly, I suggest, because these "offbeat" results are less expected and hence less discussed among law consumers). Kennedy's example of a difficult-to-justify result is one that characterizes the lie-in as protected first amendment behavior, a result that would require some reinterpretation of precedent to characterize the lie-in behavior as "speech" rather than "action." Another reason unexpected results are

24. The history of § 1202 is lively. For many years it permitted a taxpayer to deduct 50% of net capital gain in calculating taxable gain. Doubt about the policy justification for this tax break led to changes in 1969 and 1976 that effectively increased the rate of taxation of capital gain income. In 1978, however, a heavily lobbied statutory change reversed the 1969 and 1976 statutory innovations and put into place the recently repealed 60% deduction. This established the most favorable treatment of capital gain income, relative to ordinary income, in the post-war period. Now Congress has done an about-face—it has virtually eliminated the distinction between capital gain and ordinary income.

25. Tax experts will recognize the analogy to the abortive § 1023, which would have changed the stepped-up basis rule of § 1014, only to be repealed before it ever became effective. *See* M. CHIRELSTEIN, FEDERAL INCOME TAXATION 60 (4th ed. 1985).

26. Kennedy, *Freedom and Constraint in Adjudication: Toward a Critical Phenomenology of the Rule of Law*, ___ J. LEGAL EDUC. ___ (forthcoming, 1986).

difficult for Kennedy's judge is because to reach them requires the expenditure of what I call "legitimacy chips." When rationalized in a formally correct manner, the law consuming public can accept unexpected results, or at least can accept the judge's behavior as not outside a socially acceptable role definition. But even if properly rationalized, a judge can only come to so many unexpected, or radical, results before the judge's overall behavior comes to be viewed as inappropriate. This consequence can have several negative effects. For example, the judge may lose some of her capacity to convince other judges to respect her opinions, and her decisions will lose whatever precedential effect they might otherwise have. In other words, reaching radical results requires the expenditure of legitimacy chips, and each judge has only so many to spend.

If one accepts Kennedy's description of the judicial decision-making process, one can see how a rule change can alter case outcomes. If a rule change alters what comes to be expected, a judge wanting to reach the same result as was previously customary will need to expend both legitimacy chips and the energy needed to come up with an appropriate rationalization. Because both legitimacy chips and energy are scarce resources, however, in the great mass of cases a change in the pattern of decisions can be predicted. Yet this change in the great mass of cases is not a certainty; our hypothetical judge may render a precedent-establishing decision that will change expectations about future decisions and thereby restore the status quo.

This Part began with an inquiry into the implications of the rules indeterminacy hypothesis for implementation research. So far I have defended only the proposition that despite the logical or analytic indeterminacy of rules, useful predictions can be made about case outcomes and other behavior directly associated with litigation.[27] Anyone familiar with the tradition of law and society research knows that changes in litigation related behavior do not necessarily imply the existence of changes in the underlying social behavior that the litigation concerns. For example, changes in rules concerning consumer warranties may alter the general pattern of case decisions concerning such warranties, yet have little effect on manufacturer-consumer relations beyond those few instances that get into litigation.[28] Yet the idea of social engineering is to alter that underlying social behavior through manipulation of legal rules.

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27. For example, about what cases will be settled, or viewed sufficiently meritorious to justify litigation.

28. *See, e.g.*, Braucher, *An Informal Resolution Model of Consumer Product Warranty Law*, 1985 WIS. L. REV. 1405.

This analysis indicates that there is still another indeterminacy problem to be discussed. The rules indeterminacy hypothesis questions the causative relation between rule and case outcome. A different indeterminacy hypothesis questions the necessary connection between change in either rule or case outcome and underlying social behavior. Although I will not detail my argument with respect to this indeterminacy hypothesis, my conclusion would be similar. Though there is no logically or analytically necessary connection between changes in rules or case outcome and non-litigation related behavior, changes in rules sometimes do stimulate change in such behavior. I believe it is possible sometimes to predict, albeit with less than complete accuracy, when such changes will occur and what they will be.[29] The job of implementation research is to provide the basis for making such predictions.

Before concluding this discussion of the rules indeterminacy hypothesis, I must return once again to the different potential groundings for that hypothesis. The justification for implementation research developed above responds to the more radical form of that hypothesis—the form that asserts that all rules are indeterminate because of the inherent ambiguity of language and meaning. I believe that some rules can be accurately described as more uncertain, or more indeterminate, than others. I have in mind such legal rules as the unconscionability doctrine from contract law, or the "substantial evidence on the record as a whole" standard of judicial review in federal administrative law. These rules are sufficiently vague that within the law consuming public there will often be no consensus about a case outcome. This vagueness usually stems from an attempt to bridge conflicts in values that remain essentially unresolved, including Duncan Kennedy's "fundamental contradiction."[30] It does not follow, however, from the fact that it is difficult to predict the effect of rule change on case outcome that it is impossible to offer predictions about the relation between rule change and non-litigation related social behavior. Certainly one variant of the body of law and society work concerning so-called "symbolic" legislation concerns rule changes that have little litigation-related effect but nonetheless have substantial political effects—typically relieving the political pressure for other rule changes that might have more substantial effects on litigation.[31]

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29. For an example of an attempt to generate hypotheses about when legal change will stimulate change in underlying social behavior in the consumer area, see Whitford, *Structuring Consumer Protection Legislation to Maximize Effectiveness*, 1981 WIS. L. REV. 1018.

30. *See supra* note 17 and accompanying text.

31. Professor Murray Edelman's works are commonly considered to have established this branch of law and society research. *See, e.g.*, M. EDELMAN, THE SYMBOLIC USES OF POLITICS (1964).

C. The Conceptualism Critique

Of the CLS deconstructionist hypotheses, the critique of conceptualism creates the greatest difficulties for a defense of implementation research. The critique of conceptualism takes the indeterminacy idea and extends it from rules to the social categories and concepts that we use to describe what we observe. Consider, for example, a study about whether tenants are "better off" because of a housing code, an implied warranty of habitability, or the like.[32] The concept of "better off" is inherently ambiguous. Most people would agree that it includes decreases in rents or reduced risk of injury due to unsafe stairs, but there would be less consensus whether the category includes increased capacity of tenants to organize politically—because issues of housing code enforcement are now possible organizing centerpieces. Nor is it sufficient to ask the tenants themselves as a way of seeking a determinate, unambiguous method of defining "better off." Anybody familiar with survey research knows that the answers obtained to such a question will be influenced by the way the question is worded, about what subjects are raised by the interviewer before the ultimate question is asked, and so forth.

It is not just my hypothetical "better off" category but all categories used in describing observations of human behavior that are at least partly ambiguous, and hence must be applied subjectively. Consider, for example, such standard concepts as whether there is a "dispute," or whether somebody "knows" something. In each instance subjective judgments must be made to determine whether particular behavior is described by those words. Is there a "dispute" if a consumer experiences a product failure, discovers that the retailer has gone out of business, and concludes there is no remedy that can be pursued, unaware of a possible remedy against the manufacturer?[33] Does a consumer "know" the risks she undertakes in entering a loan contract if she has never been through bankruptcy and experienced the material and psychic harms that can result?[34]

Although the social categories we use in our research are inevitably ambiguous (indeterminate, in CLS lexicology), we continue to use them, and in applying these categories our judgments are not random. Values, usually those of the researcher, inform the way in which these

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32. *See* Ackerman, *Regulating Slum Housing Markets on Behalf of the Poor: Of Housing Codes, Housing Subsidies, and Income Redistributive Policy*, 80 YALE L.J. 1093 (1971).

33. *See* Felstiner, Abel & Sarat, *The Emergence and Transformation of Disputes: Naming, Blaming, Claiming . . .*, 15 LAW & SOC'Y REV. 631 (1981).

34. If the risks one is willing to undertake change with experience (as they surely do), is that because one did not "know" enough before the experience, or is that because one's preferences (e.g., for risk) have changed? *See generally* Kelman, *Choice and Utility*, 1979 WIS. L. REV. 769.

concepts are applied (i.e., in what is considered to make a tenant "better off" or a consumer "knowledgeable").[35] These biases may be conscious, but often they go unrecognized, by the researcher and her reader alike. Thus the inevitably value-laden character of the concepts applied in social research means that social science research can never be value-free.[36]

The conceptualism critique is destructive of the epistemological validity of implementation research if one believes the goal of implementation research should be the discovery of objective truth. Let me assume that the concepts of "dispute" and "knowledge" refer to a reality that exists. That is, I assume that there are meaningful differences between circumstances accurately described as a dispute and others in which a dispute does not exist, and further that some people know more about particular phenomena than others do. Yet the critique of conceptualism suggests that because of the limitations of human language, and perhaps of the structure of our mentality as well, we cannot know for certain what that reality is. Because of the inevitably value-laden character of the social categories we use, we may be able to know only what the researcher wants that reality to be.

A common CLS reponse to the conceptualism critique is to turn to phenomenology, or thick description, as a mode of scholarship.[37] Though there is much to be said for phenomenology, it does not avoid the validity problem suggested by the conceptualism critique. Any useful description about legal matters must utilize social categories and concepts. Furthermore, all language is value-laden, and yet one can describe what is happening only with words. This point is acknowledged

35. There is no necessary inconsistency in maintaining that concepts are logically indeterminate, because ambiguous, yet are used regularly in a biased way—and hence in a predictable way—by particular individuals. Within CLS there has been a raging controversy whether the widespread use of particular concepts within a culture does in fact have predictable effects on social practices (i.e., on the politics of that culture). For a detailed argument that concepts, though logically indeterminate, often have a political "tilt," by a leading proponent of that position, see Horwitz, *Santa Clara Revisited: The Development of Corporate Theory*, 88 W. VA. L. REV. 173 (1985). It follows from my argument that rules, though logically indeterminate, can have predictable effects—*see supra* notes 22-27 and accompanying text—that I side with Horwitz on the question of the "tilt" of concepts as well.

36. For a clearly written and sophisticated account of the case that social science research cannot be value-free—the aspirations of what is called "positivism" notwithstanding—see D. TRUBEK, MAX WEBER'S TRAGIC MODERNISM AND THE STUDY OF LAW IN SOCIETY, (Institute for Legal Studies, University of Wisconsin, Working Paper No. 3, Dec., 1985).

37. A particularly strong statement of the conceptualism critique can be found in Leff, *Law and*, 87 YALE L.J. 989 (1978). Leff followed this statement by devoting his efforts largely to preparation of a legal dictionary. *See supra* note 12.

by two of the high priests of CLS, Peter Gabel and Duncan Kennedy, in their famous and provocative article *Roll Over Beethoven.*[38]

The only possible conclusion, therefore, is that there is no objective way to discover and describe reality. That being the case, the only meaningful question for evaluating a method of inquiry, I would like to suggest, is whether the method is instrumental—does it help the inquirer accomplish some goal she has set?[39] Suppose that my goal is to improve the world according to my values, and that I would like to discover the most effective means of accomplishing this goal. I contend that empirical observation, and in particular implementation research, can assist in discovering which means are most effective. My ultimate justification for this proposition is my experience, but it is an experience I believe we all have shared. In conversation I have often made this point in the following manner. I believe twenty years of involvement in faculty meetings at the University of Wisconsin Law School has increased my ability to predict the effects of a particular tactical maneuver in that endless game we call faculty politics. In making this point, I use the term "empirical" in the broadest possible sense, so that it encompasses any way of collecting information by direct or indirect observation of human behavior and its effects. It can include such "hard" data as court filing statistics as well as such relatively "soft" information as observing settlement discussions (or even talking to lawyers about settlement discussions in which they have participated).

The argument that empirical observation aids instrumental planning may seem terribly obvious, even pedestrian. Novelists, after all, generally want to immerse themselves in their subject matter at some point before pulling back and writing about it. Politicians usually seek empirical information about what people are thinking before planning political appeals. Yet when it comes to implementation research this key point is infrequently acknowledged. For example, a student note in the *Harvard Law Review*,[40] written in the CLS tradition, criticized what it called the "liberal approach" to the regulation of contractual relations, for relying on "ad hoc empirical . . . judgments to limit exceptions to the free contract rule." The author expressed concern that "empirical analysis that elucidates the complexity and variability of human behavior disables the analyst from making meaningful generalizations . . . (and is) impractical as a basis for social ordering and understanding."

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38. Gabel & Kennedy, *supra* note 17. The passages most in point are in the first ten pages of the article. Gabel is the defender of the need to use concepts in order to communicate, and Kennedy, extremely sensitive to the dangers of conceptualism, reluctantly agrees. *See id.* at 10.

39. The philosophy I am attempting to state here is rooted in the philosophy of John Dewey. *See* J. DEWEY, THEORY OF VALUATION (1939).

40. Note, *Efficiency and a Rule of "Free Contract": A Critique of Two Models of Law and Economics*, 97 HARV. L. REV. 978 (1984).

The note concludes by appealing to the need for theory if we are to have fundamental change.

The need for theory, and not just more facts, is a common topic of conversation at CLS events. This concern for theory reflects two related ideas, one relating to the nature of "ends," the other to the CLS critique of conceptualism. The first concern is illustrated by my attempt, in the foregoing pages, to justify implementation research as a tool for discovering the best available means to achieve a predetermined end. Ends, however, are neither predetermined nor given. I share the belief with many other critical scholars that ends are socially constituted—that is, they reflect, in part, the past experiences of the person seeking the ends. More importantly, implicit in CLS' rejection of determinism[41] is a belief that ends are not only socially determined but can be changed by act of will, including a choice informed by intellectual activity. Many critical scholars would argue that theoretical work, even armchair theoretical work, is an efficient way to gain the insights needed to change one's values by act of will.

The second reason that theory is important relates to the CLS critique of conceptualism—that concepts such as "better off," "dispute," and "knowledge," are informed by our values. Despite the fact that such terms are value-laden, social research requires that they be treated, at least provisionally, as "facts." Over time, perhaps because of such use, we come to treat the concepts as facts in the classic positivist sense—as reflecting objective phenomenon that all can observe through the senses.[42] As concepts become objectified in this way they constrain our thinking, either about possible changes in our values or about the best ways to achieve existing values. In this sense our concepts, like language itself, become the "prison houses" of thought.[43] As an example, if we have only one concept of what kinds of social interactions should be called a "dispute," we hinder our ability to make some useful distinctions. Some behavior described as a dispute may be very socially beneficial (e.g., a tenant strike) and such "disputing" behavior should be encouraged, while other "disputing" behavior (perhaps routine automobile accident litigation) should be discouraged.

Breaking out of our prison houses and imagining new concepts with which to describe the social world should be a very important item on any research agenda. New concepts can lead us to discover new means for achieving long-standing, widely accepted goals. Even more

41. *See supra* notes 9-15 and accompanying text.

42. When this happens the concepts are sometimes said to be "reified." *See* Gabel, *Reification in Legal Reasoning*, 3 RESEARCH IN L. & SOC. 25 (1980).

43. *See* F. JAMESON, THE PRISON HOUSE OF LANGUAGE (1972).

importantly, new concepts can help us rethink our ends, leading ultimately to new ideas about what goals we should seek to achieve.

Theoretical work is one effective way to reimagine existing concepts for ordering the social world. But it seems to me that empirical work also has considerable potential to stimulate the transcendent experience of seeing things in different ways. Let me provide a couple of examples, one justifiably famous, the other personal.

Stewart Macaulay's famous study of contractual relations in business is commonly credited with fundamentally altering prevailing views about the relation of contract law to business practice.[44] Macaulay assures me that he did not embark on this empirical project convinced that contract law was quite so irrelevant to the daily conduct of business as he ultimately concluded it was. A suggestion made to him by others to the effect that contract law was far less important than usually supposed—whetted his appetite for the research. But it was the empirical observation that convinced him that new ways of looking at the world were more accurate.

In my own case, a few years ago I conducted an empirical study of the small case division of the United States Tax Court.[45] I expected to find that the division, which is essentially a small claims court for tax disputes, served pro se litigants in the same inadequate way that general small claims courts are universally reported to function.[46] I did the study nonetheless because there were a few indications that this court was different, though I did not expect these indications to stand up to close scrutiny. To my surprise, after observing the court function and analyzing a variety of data respecting its functioning, I concluded that it was a "small claims court that works." My reified assumption that adversary justice was never an efficacious way of routinely protecting the interests of the little guy had to be discarded.[47]

In conclusion, while it may be meaningful to consider theorizing and empirical work as distinct activities, the significance of the distinction is too often exaggerated. Since both must use language, the epistemological integrity of each is implicated by the critique of conceptual-

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44. Macaulay, *Non-contractual Relations in Business: A Preliminary Study*, 28 AM. SOC. REV. 55 (1963). This study is not implementation research in the sense that it had a social engineering objective. On the other hand, in fundamentally altering the way we think of the relation between the law-in-the-books and the law-in-action in the contracts area, it must constantly be taken into account by contemporary social engineers.

45. Whitford, *The Small Case Procedure of the United States Tax Court: A Small Claims Court That Works*, 4 1984 AM. B. FOUND. RESEARCH J. 797.

46. *See* Yngvesson & Hennessey, *Small Claims, Complex Disputes: A Review of the Small Claims Literature*, 9 LAW & SOC'Y REV. 219 (1975).

47. Unfortunately, I was forced to conclude that pro se litigation worked for a variety of reasons probably peculiar to tax litigation. Whitford, *supra* note 45, at 818-25. The Tax Court's procedure may not be successfully transferable to other contexts.

ism.[48] Necessity, or at least a desire to avoid political impotence, requires that we push ahead nonetheless. In pushing ahead we seek both to reexamine our values and to devise new means for implementing ends. Theory and empiricism are each useful for both purposes. Indeed, it is widely accepted that empiricism is impossible without theory.[49] Theory, of what is commonly called the armchair variety, might be able to exist without empiricism, but in my experience empiricism has passed the test of usefulness. Empirical observation, broadly defined, makes me much more confident in my judgments about how best to implement preestablished goals. It can also provide the stimulus leading to a reconceptualization of my values or of the concepts that I use to describe the reality around me.

D. Other CLS Objections to Empirical Research

In this Part, I respond to Mark Kelman's criticisms of empirical work on law, as explained in his article *Trashing*.[50] Kelman advances two closely related arguments against the use of empirical research, one of which relates to methodology, the other to its conservative bias. Kelman's first objection is that we lack the necessary methodological and other skills to acquire any useful information through empirical techniques. To quote:

> [T]he . . . attempt to understand the effects of some isolated institutional shift is, I fear, farther beyond our technical skills than we like to believe. Although I am highly sympathetic to the aims of the "Law and Society" people, I can't honestly say I've ever read anything in the Law and Society literature that persuaded me beyond something far less demanding than a reasonable doubt that they really know what went on out there.[51]

It is not clear whether Kelman's argument invokes the indeterminacy and conceptualism concerns discussed in the previous section, or whether he is instead lamenting the low level of technical skills used in law and society empirical work, with a resultant low productivity (measured in useful knowledge) for the effort devoted to such an activity. Assuming the latter, my basic response to Kelman is that his test for

48. So, of course, is phenomenological description. *See supra* text accompanying note 38.

49. Without theory, there is no basis for the empiricist to know what of the multitude of phenomena about her to describe. This ultimately is why not even phenomenology can claim the value free label.

50. Kelman, *supra* note 5.

51. *Id.* at 338 (italics omitted).

empirical work is too demanding. To know "what really went on out there" is one hell of a test. In discussing the conceptualism critique, I suggested that we cannot really know "what really went on out there," in the sense of objective truth. From an instrumental perspective, empirical work is worth doing if it helps me to be a little more comfortable in my guesses about what strategies will be most effective in achieving desired ends, or what ends will seem satisfactory once achieved. And it has been my experience that empirical observation has helped in that respect.[52]

Kelman's second objection is that empirical work is inherently conservative. In part because of limited "technical skills," the argument goes, those engaged in empirical work tend to focus on relatively small problems. What is needed, if the goal is a radical transformation of social existence, are new visions about how we might order our activities or how we might bring about change in our society's present systems for ordering activities. "In essence empiricism . . . serve[s] far more as a very necessary limitation on vision than as a significant spur to it."[53] The implication is that armchair theorizing is a much more productive form of intellectual existence for any true radical.

Kelman's concern sounds much like an argument I discussed previously:[54] that the empirical researcher must use preexisting concepts to order and describe our social world, even though real progress is only possible if we develop new concepts that will enable us to think about the world in different ways. I respond to Kelman's second argument by noting that, while implementation or empirical research is not the only true road to an improved existence, neither should anyone attuned to CLS' embrace of indeterminacy maintain that only armchair theory can help us break out of our present confined existence. There is no one true strategy for imagining new forms of existence. No doubt immersion in the details of empirical work sometimes diverts attention away from new ways of thinking about things. But sometimes empirical work—for example, by directing attention to gaps in the theory—can be a source of inspiration for new theory. Empirical work can also con-

52. Kelman discusses specifically my own empirical studies concerning self-help repossession of automobiles. *Id.* at 339. *See* Grau & Whitford, *supra* note 8. Kelman notes that I was unable to learn much of anything about the effects of the consumer reforms under study on interest rates. He also implies that my co-author and I could find nothing definite about the long-term effects of the reform on the incidence of repossession itself, but he cites only to the earlier of my studies on the topic. Whitford & Laufer, *The Impact of Denying Self-Help Repossession of Automobiles: A Case Study of the Wisconsin Consumer Act*, 1975 WIS. L. REV. 607. A later study offered strong evidence that the reform yielded a decline in the incidence of repossession. Grau & Whitford, *supra* note 8, at 988-96. In fact, I would offer this study as support for the proposition that empirical research can help in the design of an effective social engineering strategy—in this case, reducing the incidence of repossession (the end) by judicializing repossession (the means).

53. Kelman, *supra* note 5, at 340.

54. *See supra* text accompanying note 38.

firm the importance of some new idea. This, I believe, was Stewart Macaulay's experience in the aforementioned study of the contracting practices among the business community.

I would also respond to Kelman's second argument by noting that it is not necessary to devote all our energy to the development of new ways of thinking about things. Kelman's arguments sound too much like the old refrain that reform is dangerous, because it only diverts us from the true task of revolution. At times, of course, reform may be inherently conservative because it forecloses the possibility of more radical change. Today, however, few on the left in this country oppose all efforts at reform for this reason. The problem, instead, is to know when reform is progressive and when it is not.[55] Implementation research can help design a very pragmatic program to achieve reform. The challenge is to find the proper mix of theory and observation, and of revolution and reform.

III. Other Articles on CLS and Empirical Research

My colleague, David Trubek, has written a very well-known article on CLS and empiricism, on which I have drawn greatly.[56] I view this paper as detailing primarily a kind of empirical research that Trubek there called "pragmatism."[57] Trubek's principal emphasis, however, was not on this kind of research. CLS literature frequently attributes to legal doctrine the important function of legitimating prevailing consciousnesses, or structures of thought. A political tactic that critical scholars often advocate is to expose the inconsistencies of doctrine (i.e., its indeterminacy) as a way of calling attention to the contradictions and limitations of prevailing ideologies. Fundamental to such hypotheses and programs for political change is the assumption that law exerts an influence over consciousness.[58] Hence changing legal rules, or more commonly perceptions about what the legal rules are, can alter consciousness, and in particular can alter attitudes about the legitimacy of established ways of doing things. Trubek called for research on when and how legal rules affect these consciousnesses.[59] Very sensibly, I be-

55. It has been suggested to me verbally that implementation research is politically incorrect because in today's situation it can only help a repressive government better achieve its goals. It is true, of course, that as a generic category implementation research can be used for good and bad purposes alike—but who would doubt that one could not say the same about theorizing, or any other form of intellectual activity? I do not believe that in this country at this time state power is so in the control of the "forces of evil" that all hope for progressive reform need be eschewed.

56. Trubek, *supra* note 3.

57. *Id.* at 580-82.

58. *See* Hunt, *supra* note 17, at 37-43.

59. A similar call for research of this type is made by Munger & Seron, *Critical Legal Studies versus Critical Legal Theory: A Comment on Method*, 6 Law & Pol'y 257 (1984).

lieve, he suggested that empirical research could reveal whether different kinds of rules have different effects on attitudes, and whether legal rules impact differently on elite and non-elite consciousnesses.

The research I have advocated—what I have called implementation research—differs from that which Trubek has suggested in that it attempts to link legal rules directly to behavior, rather than to consciousness. In developing an answer to the paradox that a change in legal rules can be logically indeterminate, yet have a predictable effect on behavior, I suggested that legal rules may have a predictable impact on the expectations and, therefore, on the behavior of law consumers.[60] If this hypothesis is correct, a phenomenological account of implementation would attempt to trace the effects of a legal change to a change in belief system, and from there to a change in behavior. While I support such research, in this Essay, I also advocate research that bypasses the consciousness stage and attempts directly to correlate changes in rules to changes in behavior. While such research can never fully describe the process of implementation, it can help design effective strategies for legal change.

Implementation research as I have defined it differs from the research proposed by Trubek in another respect as well. The primary emphasis in Trubek's proposal, as in so much other CLS work, is on deconstruction. In essence, Trubek and other deconstructionists propose to delegitimatize existing legal rules and existing consciousnesses by exposing their latent contradictions and ambiguities.[61] While many deconstructionists hope that something better will take the place of existing belief structures as they are discarded,[62] the research itself is not directly focused on what that substitute should be. Implementation research, on the other hand, is mostly reconstructive in outlook. As used by social engineers, it helps discover how best to replace the existing way of doing things with another order.

There is no necessary correlation between the two respects in which implementation research differs from the empirical research proposed by Trubek. Research on legal rules can link those rules to either consciousness or directly to behavior, and either type of research can be deconstructive or reconstructive. Indeed, one can find examples of all

60. *See supra* note 23 and accompanying text.

61. Klare, *Judicial Deradicalization of the Wagner Act and the Origins of Modern Legal Consciousness, 1937-1941*, 62 MINN. L. REV. 265 (1978) is commonly cited as a classic of this genre.

62. *See* Singer, *supra* note 16. Others in CLS have come to question whether reconstruction is possible. To some, the only epistemologically legitimate activity is perpetual criticism, or constant deconstruction. *See* Tushnet, *Some Current Controversies in Critical Legal Studies*, (May, 1986) (unpublished paper delivered at meetings of Law & Society Association, Chicago, Ill.) (on file at the Wisconsin Law Review).

four kinds of research, though not always empirical research, in the literature, as I illustrate in the following chart:[63]

	LINKING RULES TO:	
	CONSCIOUSNESS	BEHAVIOR
DECONSTRUCTION	Klare and delegitimation	Macaulay and business deals
RECONSTRUCTION	Unger and deviationist doctrine	implementation studies

Nothing in this Part is meant to imply that Trubek would object to any of the types of research represented on the above chart. Indeed, he appears to approve of them all, though emphasizing the need for empirical research linking rules to consciousness. As discussed earlier, the epistemological validity of even that type of research must cope with the implications of CLS' critique of conceptualism.[64] Indeed, empirical research on conciousness may face even greater methodological problems than measuring changes in behavior. Rules, after all, may influence consciousness, but they are not the only influences. With so much going on all the time, it is very difficult to ascertain the extent to which a measured change in attitudes was caused by a change in rules.[65] Furthermore, measuring change in consciousness typically requires asking people questions about what they believe. As anybody who follows the polling business knows, to ask such questions in ways that do not bias the responses is very difficult. Measuring changes in behavior often requires less subjectivity.

IV. THE FORM OF LEGAL IMPLEMENTATION RESEARCH

I do not mean to imply that legal implementation research should continue unchanged, as if CLS had never happened. My personal encounter with CLS has led me to reinterpret the place of implementation research in the world. Improved social engineering remains its primary function, but gone are any pretensions that the results are building

63. The references in the chart are to the following works: Klare, *supra* note 61; Unger, *The Critical Legal Studies Movement*, 96 HARV. L. REV. 561 (1983); Macaulay, *supra* note 44. There are numerous examples of implementation studies, of course, including my own work cited in note 8 *supra*.

64. *See supra* note 36 and accompanying text.

65. For example, many studies have attempted to ascertain the effects of the Supreme Court's school prayer decisions on attitudes about the state and religion. The problem is determining the extent to which any changes in attitudes would have occurred even if the Supreme Court had not decided the cases, or had decided them differently. Of course, a similar problem exists in research linking rules to behavior change, but arguably the problems are somewhat less, in part because there are fewer obvious alternatives to rule change to explain the observed behavior change.

blocks towards an objective theory of society. I have become a structuralist, with a keen awareness of the importance of consciousness, or structures of thought, in understanding human behavior. Implementation research, in its narrowest form of linking specific rules to behavioral change, is only valid within the framework of a particular consciousness. The consciousness of societies change, and as they do so, previous findings of implementation research become outmoded.

The critique of conceptualism has greatly enhanced my awareness of possible biases in "data." This sensitivity has two primary implications for the form, or methodology, of legal implementation research. One implication concerns what might be called the "hardness" of the research design. By "hardness," I mean the extent to which the research is designed to test, and only to test, predetermined hypotheses by methods that appear to minimize the opportunities for subjective judgment by the researcher, and that permit enough observations to make statistical manipulations of data possible. The other implication concerns the generality of the propositions subjected to study. In both instances the critique of conceptualism suggests new strategies to avoid unrecognized bias in social science work.

A. The Hard-Soft Dimension

There is what passes for a scientific method in social science research. It requires the formulation of hypotheses, and the testing of them in some predesigned way. It is very important that the results be reproducible—that is, that another researcher following the same research design be able to obtain the same results. To be avoided, because the results are not reproducible, is research designed to allow the researcher to seek inspiration after immersion in data—commonly labeled a "fishing expedition," a derisive term in academic circles. And it is very desirable that the research design permit enough observations to make possible statistical tests, in order to better insure that the results describe "what is truly going on out there."

The traditional emphasis on hard methodologies has reflected a belief that it is possible in a value free way to observe and describe objective reality. One who has accepted the implications of the critique of conceptualism must reject this view. At the same time I believe that there is value in unmasking hidden and perhaps unrecognized bias. At a minimum, it promotes honesty in communication between the observer and her audience. And if biases can be brought into the open, it makes implementation research more useful, by enabling potential users to pick research with biases they share.

Hard methodologies sometimes have value in unmasking bias. Statistical tests can help the observer understand when initially perceived

regularities (probably ones the researcher wants to exist) are merely the product of chance. On the other hand, hard methodologies must necessarily rely on concepts that indicate what is to be measured (such as the "better off" test). The bias inevitably built into these concepts is often difficult to recognize. Soft methodologies (e.g., participant observation) rest heavily on the subjective judgments of the observer, but it is often easier to detect what those biases are.

Soft methodologies also have the advantage of being able, for a given expenditure of resources, to take account of so many more phenomena, including previously unanticipated ones. These unanticipated observations are often the ones with the greatest potential to stimulate us to see the world in radically different ways, and thus to escape from our current prison houses.[66]

B. The General-Particular Dimension

The sociology of law bias exhibited a tendency to push towards ever more general propositions. Propositions about the nature of dispute settlement attract attention and earn rave reviews, while very specific propositions about the operation of a small claims court for landlord-tenant disputes within a particular jurisdiction go relatively unnoticed. Two characteristics can render a study more general than particular. First, a general study may focus on propositions possibly valid for a vast range of behaviors (e.g., all dispute settlement). Alternatively, though focusing on a very limited range of phenomena (e.g., landlord-tenant cases in small claims courts), a general study can strive for a description that fully accounts for the observed behavior (e.g., a regression equation that accounts for all variance observed).

The emphasis on generality in the sociology of law reflects the lingering influence of determinism—the view that there are a few innate human characteristics that determine law-related behavior. After all, if that were the case, the payoff for learning them would be great indeed. Substituting a structuralist for a determinist perspective, as much CLS work does, negates neither the (possible) validity nor the utility of relatively general propositions. General propositions sometimes can be made about a particular culture. And if the objective of implementation research is to abet social engineering, general propositions, if valid, are likely to be more useful than particular ones.

Nonetheless, I believe there are valid reasons for implementation researchers to focus more frequently on "particular" studies. In the first place, general studies tend to oversimplify the world. They often account for behavior by reference to a single cause, when in fact any par-

66. *See supra* notes 42-47 and accompanying text.

ticular behavior is invariably the product of many causes,[67] the alteration of any one of which would be sufficient to change the result.[68] A focus on particular studies, where the goal is only to identify factors that influence events rather than to account fully for those events, at least reduces the temptation to oversimplify in an effort to come up with valid results at a general level of explanation.[69]

It seems probable that there is a direct correlation between the generality of the propositions sought to be validated and the use of abstract concepts and categories, such as whether the parties to a dispute settlement are "satisfied." Such abstract concepts frequently hide implicit value judgments.[70] The critique of conceptualism suggests that particular studies—those that attempt to describe only a very limited number of phenomena from a variety of perspectives—also use concepts that reflect value choices. But I would contend that the reader of the very particular study will frequently find it easier to discern these hidden value choices. Hence the goal of unmasking hidden bias also favors particular over general studies.

I conclude, therefore, that CLS has taught me not to privilege any particular methodology, whether hard or soft, general or particular, over another. By the same token, it is inappropriate to trash any particular methodology, or even empirical research in toto, a position sometimes, though I believe wrongfully, attributed to CLS.

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67. I assume here that it is meaningful to talk about behavior as caused by external forces, even though humans possess the capacity to control their behavior by force of will. This reflects my embrace of structuralism. *See supra* notes 13-15 and accompanying text.

68. In thinking about this problem, I have found very helpful Charles Yablon's discussion of the problem of overdetermination in historical and legal explanation. Yablon, *supra* note 23.

69. It is essentially this conflict that underlies my interchange a few years ago with Professor George Priest (whom nobody would identify with CLS). *See* Priest, *A Theory of the Consumer Product Warranty*, 90 YALE L.J. 1297 (1981); Whitford, *Comment on a Theory of the Consumer Product Warranty*, 91 YALE L.J. 1371 (1982); Priest, *The Best Evidence of the Effect of Products Liability Law on the Accident Rate*, 91 YALE L.J. 1386 (1982). For a different view of the interchange, advocating general explanations but in the name of CLS, see Note, *supra* note 40.

70. For example, to ask whether parties to a dispute settlement are "satisfied" ignores the possibility that particular groups of disputants do not understand what they might have obtained from the dispute resolution. More particular studies, that focus on describing what groups of disputants obtain, rather than their general attitudes about the process, substantially obviate this problem. For an example of what I mean by a more particular study in the dispute settlement context, see Ross & Littlefield, *Complaint as a Problem-Solving Mechanism*, 12 LAW & SOC'Y REV. 199 (1978).