THE CONSTRUCTION AND DECONSTRUCTION OF A DISPUTES-FOCUSED APPROACH: AN AFTERWORD

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> With each step forward, with each problem that we solve, we not only discover new and unsolved problems, but we also discover that where we believed we were standing on firm and safe ground, all things are, in truth, insecure and in a state of flux.
>
> Karl Popper
> (Popper, 1976: 87)

I. INTRODUCTION

The essays and comments in Part Two have dealt with a variety of topics ranging from technical matters like survey response rates to questions about the presuppositions of social research on law. Each paper is an individual contribution to knowledge, but all, to one degree or another, deal with a single issue: how can we best improve our knowledge of civil courts, dispute processing, and social conflict? It is to this broad question that I wish to address these final remarks. This special issue provides a rare opportunity to look systematically at a major area of law and society research. The discussion has brought to the surface questions about the focus, method, and purpose of research. Many of the CLRP papers were explicitly designed to outline an area or program of study, and all reflect a general orientation. The excellent comments by FitzGerald and Dickins, Lempert, and Kidder help us see just what that approach consists of, and the problems it presents.

* In the process of writing this paper, I benefited from discussions with the staff of the Civil Litigation Research Project, and my colleague, Mark Tushnet. An opportunity to discuss these matters at the Third Amsterdam Seminar on Legal Sociology, organized by Erhard Blankenberg, futher clarified my thinking. Richard Abel, Kristin Bumiller, William Felstiner, Heleen Ietswaart, and Austin Sarat made valuable suggestions which I have tried to incorporate. The ideas expressed here reflect my personal views and should not be attributed to the Civil Litigation Research Project or any of the other participants in it.

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The special issue has created a forum for the kind of self-conscious scrutiny of methods and purposes of research that any field needs to engage in from time to time (Abel, 1980). We all know that social knowledge is created and recreated. Concepts, ideas, theories, and even what we loosely call "empirical facts" are built, not found. Yet once we have built these systems, they become not merely a way of understanding some external reality, but also part of social reality itself (Geertz, 1973). The process of defining and developing a field of study is influenced by purposes, values, and presuppositions of which we are not always fully conscious. Yet once we build our sciences of society, they influence who we are and what we may become. The way we define a subject, the way we observe behavior, the things we choose to consider "facts," all may become part of what we subsequently think, feel, and do. The idea of social science as a neutral, technical machine, free of normative presuppositions or political significance, is a chimera. All we can hope is that practitioners make goals, values, and assumptions as transparent as possible, and that they are subject to criticism and debate (Myrdal, 1968). This issue of the *Review*, I believe, advances this essential task. It was designed to stimulate further work on dispute processing and civil litigation by bringing together some recent work in this area and soliciting the comments of leading scholars.

The CLRP papers explore the potential and limits of the "disputes-focused approach" to studying civil courts and civil litigation. This approach is not the only way to look at these phenomena, but it is one that has attracted substantial interest and influenced work by numerous scholars. Thus a general appraisal of the approach should be important to the field. The CLRP papers and the comments on them draw our attention to some basic issues about the effort to merge dispute processing research and the study of civil courts. Two seem most important: (a) does the dispute focus help us understand what is occurring in civil lawsuits; and (b) is the "dispute" the right "context," the proper way to conceptualize the relation between civil litigation and society?

II. UNDERSTANDING THE COURTS

The great virtue of the disputes approach is to throw open to question one important function of civil courts—disputes processing—and thus permit empirical inquiry into court activity. In this approach the researcher starts by saying: "We know that the courts are supposed to help parties resolve

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disputes they have been unable to settle otherwise. To what extent is this the case, and how it is done?" Furthermore, since courts are not the only institutions engaged in dispute processing, this approach should also allow us to compare judicial conflict resolution with arbitration, mediation, or other alternative institutions.

The Civil Litigation Research Project was built around these assumptions of the disputes approach. The CLRP papers take this for granted, and so, by and large, do two of the comments. Kidder, on the other hand, raises fundamental objections to the entire dispute focus. His criticisms are so basic that they deserve a more detailed analysis, which I shall take up in later sections. First, I want to examine a somewhat narrower range of problems arising from application of the disputes approach to the study of the courts as institutions. Although these problems are not explicitly raised by the preceding papers, they are implicit in the discussion.

Some of these issues have surfaced elsewhere in the literature. In a thoughtful paper, David Engel (1980) recently identified some of the problems involved in using the "dispute" as a prism to study courts. Analysis of Engel's points in light of the CLRP experience should help us more fully understand these issues. Engel explicitly questions whether it is useful to study courts as dispute processing institutions, and whether it is even possible to compare disputes in courts with those in other dispute processing settings. For Engel, these doubts arise for three reasons. First, he questions whether many lawsuits really involve what social scientists would call a "dispute." Second, he expresses doubts whether, in those lawsuits that really are disputes, courts actually engage in what can be called dispute "processing." Finally, he argues that those disputes that appear in court are, by virtue of that very fact, so different from disputes in other settings that comparison is really impossible. For these reasons, he fears that a comparative study of disputes in courts and other institutions will both mislead us about courts and fail to provide useful information on the difference between courts and other institutions.

Disputes in Courts

Engel's first point signals caution for the design and interpretation of studies of courts. It is widely recognized that much litigation does not involve genuine disputes. We know, for example, that many lawsuits are filed merely to record

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agreements reached outside the court, either because there never was a dispute in the sociological sense, or because the parties have resolved their conflict through other means (Friedman and Percival, 1976a). In such cases, of which the uncontested divorce is a good example, the lawsuit merely fulfills a legal requirement for judicial endorsement of a consensual agreement.

It is important to distinguish such situations from those lawsuits in which there is a genuine conflict. But rather than vitiating the disputes approach, this observation seems to confirm its utility. Researchers must be sensitive to the facts that not all lawsuits are disputes and that not all court activity is oriented toward the settlement of disputes. The disputes focus does not require us to assume that every lawsuit meets our social scientific criteria for a dispute. On the contrary, it provides a way for us to examine all lawsuits empirically, asking whether a given case is a dispute and separating the disputes from other lawsuits.

Of course, in the limiting case in which not a single lawsuit involved a real dispute, such an enterprise would be a vast waste of time. Engel's objections may rest on a belief that this limiting case is close to the actual situation in the civil courts. Our evidence suggests that this is not true. Preliminary results from the CLRP surveys indicate that parties frequently reach the court with substantial disagreements on normative and factual questions.[1] It will take more work on our data and further research by others to get an accurate estimate of the relative importance of disputes in the business of the courts. But the only way to do that is to assume that each lawsuit might be a dispute and examine it to see if that is the case.

Numerous methodological issues are connected with implementing this program. CLRP dealt with them in two ways. First, as Kritzer (1981a) notes, we excluded types of cases from our sample if, on *a priori* grounds, we felt that they would never involve disputes.[2] This makes it easier to compare court cases with disputes drawn from other sources, but it

[1] The CLRP surveys included questions designed to ascertain that a perceived problem had become a dispute and that the dispute was involved in any litigation that occurred. See Grossman *et al.* (1981b).

[2] Individual cases might be excluded from the CLRP sample for one of three reasons. Some cases were excluded because of over-sampling: in the New Mexico survey, for example, some student loan cases were excluded for this reason. Other cases were excluded on procedural grounds: cases that were remanded, ancillary, or transferred out of the sampled court. In addition, in the federal court sample, cases were excluded when terminated for lack of jurisdiction. A third group of cases were excluded because their subject matter was unlikely to involve a dispute—either in theory or in practice. This group

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limits what CLRP can say about the relation between disputes and non-disputes in courts. Second, in our surveys we sought to determine if there really were disputed issues between the parties.

If it is important to ask questions about the existence of disputes in courts, it is also essential that we have a way to determine whether what occurs in litigation can properly be called "processing." The second objection Engel makes to the disputes approach is that it seems to assume that courts, and particularly judges, are necessarily involved in processing disputes. We know that most cases filed in court are settled without a trial, and that in many cases judges play no role at all.[3] This does not, however, mean that the concept of "dispute processing" is not useful for studying the courts. Indeed, only by testing conventional assumptions about the role of the judiciary and other court personnel in disputes can we determine the nature and extent of official involvement in processing and resolution. CLRP approached this task in a number of ways. First, for each case in our sample we sought data on important events, including those involving judicial contact. Second, we asked attorneys whether the judge performed any informal role in seeking to settle cases. Through an analysis of this data and results of a parallel survey of judges from the same judicial districts from which our sample was drawn, we concluded that there is a significant amount of "processing," informal and formal, done by judges in civil cases (Kritzer, 1981b).

Comparing Courts and Other Institutions

Engel's third point about the limits of the dispute focus for the study of court is, however, of a very different order. He argues that even if courts do process disputes, what goes on in courts is by definition so different from what occurs in other settings that these two disputes are not the same thing, and cannot be compared. It is obvious that if this point were correct, it would raise doubts about the disputes focus, since such institutional comparisons are one of the more important benefits of using this approach.

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included matters such as probate, name changes, bankruptcy, garnishment, and prisoner petitions. See also Grossman *et al.* (1981a).

[3] The CLRP Lawyer Survey revealed that of those lawyers (N=1214) involved in pretrial settlements, 73 percent (N=887) indicated no judicial role, while 27 percent (N=327) indicated that the judge or hearing officer played some role in the negotiations.

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Engel's argument rests on the proposition that civil procedure necessarily narrows a dispute, while other approaches to processing do not. Thus all lawsuits are narrower than disputes emerging from similar circumstances that do not end up in court (Engel, 1980: 434). For that reason, comparing the one with the other is like comparing apples with oranges.

There are at least three things wrong with this position. First, it rules out the possibility that issues will be widened in the course of litigation. Anyone familiar with the debate on public policy litigation knows that individual cases can become major political issues and that broad questions of policy are sometimes injected into a lawsuit after it starts. Second, Engel assumes that if the *legal* issues are narrowed, the issues in the dispute will necessarily be narrowed as well. As Kidder points out, the issues in the lawsuit and those of the underlying dispute may, in fact, be very different, and parties may be able to keep them apart.

Finally, even if litigation often narrows issues in the dispute, this does not necessarily occur in all, or even most lawsuits. Engel's argument that litigation must limit the issues in a dispute assumes that since civil procedure is designed to refine legal and factual issues for purposes of adjudication, all cases reaching court will, necessarily, be subject to a "narrowing" process. Of course, if these techniques were fully employed, and the dispute resolved by adjudication, normally the controversy would be narrowed in Engel's sense. But it does not follow that most actual lawsuits are so "narrowed." The effect Engel predicts is most likely if the case follows all the procedural steps and terminates through a judicial decision. This is far from the typical situation in litigation. In the CLRP court sample, for example, about six percent of the cases filed went to trial. The truly representative civil lawsuit involves limited pretrial procedural activity and settlement negotiations which conclude the lawsuit (if not the dispute).[4] Now, we have no reason to believe that the issues ventilated in the settlement negotiations which typically terminate civil lawsuits are always narrow. Unfortunately, we know relatively little about settlement practices except that they are pervasive and important (but see Ross, 1970). Until we do, there is no reason to assume that all disputes which become lawsuits are

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[4] In the CLRP sample, only six percent of the cases went to trial. In 22 percent there were some pretrial motions. In most cases (N=78 percent) lawyers surveyed reported some settlement negotiations.

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narrowed, any more than we can automatically assume that disputes in other settings are not (see Abel, 1973). The disputes focus offers us a way to secure more useful information on these matters. By defining the dispute generally enough to encompass both narrow and broad conflicts, and by paying attention to the transformation of disputes over time (Mather and Yngvesson, 1981; Felstiner *et al.*, 1981), we should be able to make precisely the sorts of comparisons Engel rules out, and by doing this get empirical data on what he assumes to be an *a priori* difference between courts and other institutions. The disputes focus is the key to understanding if, how, and why different institutions have different effects on disputes.

III. RELATING "LAW" AND SOCIETY: DISPUTES, CONFLICT, AND COURTS

CLRP saw the disputes focus as a useful way to relate law and society. Throughout the history of law and society studies, there has been a constant effort to bring the insights of the social sciences to bear on issues previously studied only by lawyers. The adoption of the dispute focus is simply another example of this general phenomenon. Until recently, civil litigation has been studied primarily by experts on civil procedure and judicial administration. What empirical work has been done was largely inspired by the needs of court administrators. The disputes focus is a way to transcend the limits of this work and open the field to the theories and methods of the social sciences.

Critiques of the Disputes Focus

Not everyone agrees that the "dispute" is the proper link between legal and social phenomena. While CLRP saw the disputes focus as the highway to a richer, more empirical and contextual understanding of civil litigation, others see it as a by-road at best and at worst a dead end. The criticisms leveled at this strategy of research vary in extent and nature. Some see the disputes focus as a useful but partial way to explore "civil justice," while others contend that it is fundamentally flawed. Lempert seems to accept many elements of this approach, but suggests that its micro-level emphasis should be supplemented by more macro-sociological investigations. Kidder, on the other hand, sees behind the focus on specific disputes an implicit macro-sociology which he believes is both empirically biased and politically conservative.

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There are substantial differences among those who argue that the disputes focus distorts rather than improves our understanding of civil justice. Thus Kidder and Tushnet (1981) argue that this strategy leads us to underestimate the real level of conflict in society and misunderstand the true nature of social control in capitalist society. Engel, on the other hand, has suggested that an exclusive concentration on disputes or "trouble cases" will *overstate* the extent of conflict and focus attention away from functioning systems of normative integration (Engel, 1980).

It is clear that the decision to study litigation as dispute processing has mobilized substantial social scientific energy. Litigation studies have been dominated by such administrative concepts as "delay" and "effectiveness." The disputes approach reconceptualizes litigation as a social process embedded in social relationships, thus making it possible to draw on and develop a whole research tradition.

This does not mean that the disputes focus is fully adequate to explicate litigation as a social phenomena, or that current applications of this research strategy are flawless. Quite the contrary. In the first place, no approach or structure of thought can grasp social life in all its complexity: the greatest theories, the thickest descriptions must leave something out. Furthermore, the institutions we call courts do so many things, and the process we describe as litigation is so heterogeneous that it is hard to imagine any single functionally based approach which could fully account for everything in this domain (cf. Shapiro, 1980). Even those who endorse this approach recognize its limits; many of the CLRP papers themselves criticize aspects of the dispute focus. Finally, we are far from understanding the full potential and limits of the disputes approach. All research tools are imperfect; we are just beginning to learn the nature of this one.

While we all recognize the inherent imperfections of any conceptual approach and the underdeveloped state of disputes research, some have concluded that the flaws already apparent are so serious that they doom the effort in its entirety. Kidder's comments illustrate this line of thought; he wants us to abandon completely the path we have taken. Kidder's comments are important, for they do pinpoint serious problems. But I think his conclusion is just plain wrong.

There are four ways to approach this sort of criticism. One would be to show that many of the specific points are simply inaccurate. Another would be to say that when the critics

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chide dispute research for omitting things, they have imputed to it goals it never had. A third line of attack would be to argue that no criticism of a working method is valid unless it can produce some idea of what *alternative* paths should be taken, something Kidder fails to do. I believe that each one of these rejoinders could be developed, but what I want to show is that even when specific criticisms are valid, the critics' conclusion—that we should abandon the disputes focus—is nevertheless wrong. This fourth form of rejoinder incorporates elements of the first three but is also based on a very different view of social research than the critics employ. It is that concept of how research develops and progress made in social knowledge that is ultimately at stake here.

To demonstrate this, I shall take up three major issues concerning the disputes focus. These are raised by Kidder, but have also been discussed within CLRP and by others, including Tushnet (1980) and Lempert (1981). These are: (a) does this approach impose a fixed, individualistic, and over-legalized view of conflict; (b) does it cause us to overlook major dimensions of conflict; and (c) does it adequately account for the role of law, the legal profession, and particularly the courts in the resolution and repression of conflict?

Imposing A Biased Notion of Conflict

The "dispute" is a nominal category created by the researcher in the sense that the researcher decides what is to be labeled a dispute and what is not. This creates an obligation to be very clear about how and for what purposes labels are applied.[5]

If our goal is empirical knowledge about the incidence of grievances and disputes, and the processes of dispute transformation, labeling is unavoidable. Any approach must have a way to determine whether a given situation falls on one or the other side of the line. Yet lines are hard to draw. The simple picture of the dispute as a relationship emerging from an orderly transition through clearly delineated stages, each

[5] Of course, there is always an additional risk that the presence of the researcher will alter decisions the parties make. It is not hard to imagine situations in which inquiries by the researcher about dispute decisions could lead parties to reconsider and change their decisions. (Imagine if you ask disputants why they didn't use a lawyer, and this cues a decision to do so!) This problem of reactivity is always present, and must be guarded against. It is less serious in retrospective studies like those CLRP has conducted, in which the events studied are all in the past, and the disputes theoretically terminated, but it could become acute if we are able to initiate the kind of prospective "panel" studies suggested by several authors (Kritzer, Felstiner *et al.*, Coates-Penrod).

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fully understood by both parties, may have heuristic value, but it hardly represents the shifting, confused, and transitory relations "out there." It is helpful to see that disputes involve grievances, claims, and rejection, but as Kidder notes, it is hard to be sure what action by the parties fits these categories, or to be certain that these acts, once observed, have the subjective meaning and finality implied by the model. Obviously, there is a risk of imposing a rigid template on a shifting situation, and "observing" disputes that really are not there.

The second risk is that of imposing legal definitions on social relations. Note that CLRP did not just try to study "disputes"; in our surveys and sampling design we tried to limit our universe to something called "civil legal disputes." The use of this concept, admittedly never defined with great precision, signaled our desire to study those disputes which *might* conceivably end up in civil courts. At the same time we were looking at the courts through the prism of the dispute, we looked at the world of disputes through the perspective of the courts. This had two specific implications for our study. First, while we sought to include cases outside of courts, as well as lawsuits, in our sample, we made an effort to limit our non-court disputes to those that might conceivably have been brought to court, so that all disputes sampled would be comparable. Second, in our survey of the population, we used a method which defined eligible problems and thus disputes in terms derived from the sort of matters typically found in civil courts (Miller and Sarat, 1981: 534).

Given the purposes of our research, which included identifying "bilateral disputes"—those which might have gone to court but did not—as well as comparing courts and other institutions and bilateral with third-party dispute processing, these sampling and survey design decisions make sense. But they do restrict the universe of disputes. Someone—either one of the parties or us (or both)—had to classify disputes. Inevitably, these classifications are influenced by notions of what is justiciable. This not only imposes legalistic criteria on what is counted as a dispute; it also uses the current, conventional legal definition, rather than including within the boundary of the universe observed things that *might* become a valid claim through imaginative lawyering and doctrinal transformation.

Kidder is right to note these limits but wrong if he is really arguing that studies which adopt such methods are fundamentally flawed. All research has a specific purpose

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which shapes and limits the methods that can be used. We cannot observe the whole social universe at once: some limiting choices of the kind I have noted are an inevitable part of the enterprise. The issue is not whether some things are included and others excluded, but rather whether the criteria used are clear and related to the purpose of the inquiry, and whether the data are interpreted with full appreciation of their limits.

It would be useful to know what percentage of those disputes that might be handled by our courts under current notions of justiciability actually reach some civil tribunal. It is not easy to produce such data, as CLRP has learned. The survey reported by Miller and Sarat does provide some crude estimates of this aspect of dispute transformation for certain areas, but it is far from a comprehensive report of the baseline of currently justiciable conflicts. And there is no claim that we even tried to incorporate disputes that might, under altered ideas of substantive or procedural law, be handled by courts. But this is no reason to condemn a whole program of inquiry to oblivion because other things can and should *also* be studied. Nothing precludes us, or anyone else, from carrying this work further, finding better ways to measure what is currently cognizable in courts, or studying areas of social conflict which now lie beyond the legal realm. Nor does the approach we have taken rule out, in future work, research on the subtle relationship between peoples' views of what is legally cognizable and the way parties in conflict define and negotiate their relationships.

Omitting Dimensions of Conflict: The Problem of "False Consciousness" or the UnPIE

If there is a danger that empirical methods will identify disputes the parties had not thought were real conflicts and impose legal labels and artificial disputing "stages" on very unstable "folk" situations, there is the opposite possibility that these methods will fail to observe genuine conflict situations that the respondents do not perceive at all. The disputes focus tends to concentrate attention on those conflicts that have crystallized. This is especially true if one relies, as CLRP has, on retrospective survey techniques which measure only experiences that were salient to respondents. Kidder fears that a methodology limited to observation of individual conflicts which have crystallized through the perception of grievances and complaints by the aggrieved will be insensitive

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to the problem of "false consciousness." If we rely exclusively on the respondent's perception of events, we may fail to understand how inequalities are legitimated and collective struggles individualized in our society.

Imagine that my landlord raises my rent by 50 percent. If I do not consider this to be an injustice, this situation will not register as a dispute. Perhaps I have been lulled into believing that the present system of property relations and market pricing is just, and am therefore unhappy but not "aggrieved" by this act. If limited by theory and methodology to observing "perceived injurious experiences" and articulated grievances, the analyst of this event may fail to appreciate what many would see as an injustice. And to continue the example, even if I think that a 50 percent increase in my rent is unjust, I may, nevertheless, be fearful of complaining because eviction is more threatening than conflict. In this case, not only will there be no *dispute*, but also I may be unwilling even to tell the interviewer that I have a "grievance."

The CLRP surveys provided no way to tap this sort of information. Felstiner, Abel, and Sarat, however, recognized the need to observe what they call the "unperceived injurious experience," or unPIE and the important transition point of "naming"—i.e., becoming aware that one is injured. Indeed, their paper was in large measure inspired by our recognition that the original surveys were inadequate to deal with this dimension.

Few would deny the existence of important "latent" conflicts in society which are not perceived by the participants or are distorted in individual consciousness. And it is clear that a research method which is insensitive to such situations could be part of a social process that represses valid grievances. Yet it is not easy to develop empirical methods to explore such matters. Felstiner *et al.* make some suggestions, but to my knowledge, no one in our field has really figured out how to deal with this set of issues.

Some may see this problem as evidence of an absolute limit on all methods of empirical inquiry. Others will see it as a challenge to develop new tools which can get behind "false consciousness" and measure both perceived and unperceived injurious experiences. There are techniques which would help uncover suppressed views and tap more of the disputants' subjectivity than one can hope to observe through surveys.

It seems obvious that we need to put more emphasis on research that studies these matters from the potential

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grievant's viewpoint—"from the bottom up," as it were. We should also encourage more studies which employ techniques capable of assessing the complex subjective dimensions of naming, blaming, and claiming. Yet even those methods do not really get at the issue of "false consciousness," which assumes that there is a fundamental truth hidden from consciousness by ideology and thus not discoverable by any purely empirical technique (Adorno, 1976: 80).

This suggests that the issues Kidder raises are really much more complex than his brief comment suggests. It is interesting to note that while Felstiner, Abel, and Sarat recognize some of them, they, too, have not worked out the full implications of the idea of an "unperceived injurious experience." The principal example they give—the worker in an asbestos plant unaware of the existence, nature, or cause of a physical ailment—is of a very different order than what is suggested by the Marxist notion of false consciousness. The latter idea rests on Marx's concept of the fetishism of commodities, the distinction between use-value and exchange-value, and thus on a total critique of social relations in capitalism (Adorno, 1976). The asbestos example is an easy case: few would argue that the sick asbestos worker has not been injured, even if he or she is unaware of the injury. But how do we decide whether a 50 percent increase in my rent, allowed under existing law, which I do not object to on normative grounds, is an *injury*?

This issue, obviously, is that we have not squarely faced the problem of the relationship between research and the values of the researcher. As Richard Abel has noted (in a comment on a previous draft of this paper) the reason that the asbestosis case is unproblematic is that it relies on consensus to make the normative decision about injury. As the case is put, the experience is serious, harmful, and apparently preventable, so that everyone, including the worker, the researcher, and the reader, would agree that a bad thing has happened. The question of value, therefore, has been transformed into a matter of social fact: a consensus of valuation demonstrated by a form of discourse. Similarly, if in my example I had thought that my rent rise was a rip-off by a rapacious landlord, my valuation could be treated empirically as a fact. But where neither of these strategies will work, as they will not in the rent case I actually put, then we must squarely face the issue of values.

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The problem is not one of method, in the narrow sense of surveys versus observation, but one that reaches to the nature of social research itself. Numerous empirical methods (in-depth interviews, participant observation) promise greater insight into subjective perceptions than one can hope to gain by surveys. These approaches might uncover the tenant's sense that a rent rise was unjust, if it is there. But even they cannot provide it, if it is not accessible to the individual's own consciousness.

The strategy of defining injuries, grievances, and disputes in ways that avoid the issue of the researcher's values is not, paradoxically, value-free. For it makes inaccessible to thought those injuries and injustices for which there is no objective referent in existing law, consensus, or the consciousness of affected individuals. That a body of knowledge which excluded, because of a supposed methodological canon, information of this sort could be biased, I have no doubt. So we must be modest in our claims of what we have learned so far, and relentless in the search for ways to transcend the limits of existing work.

Explaining the Role of Courts in Conflict Resolution

Dispute processing research is based on the application of known methods of inquiry to explore a hypothesized function of civil law and the civil courts: conflict resolution. Perhaps we need to expand our notions of what conflict resolution is, and what "functions" courts play in it. This, at least, is what Kidder and Lempert seem to be suggesting. Both Kidder and Lempert fear that the dispute processing approach fails to explain adequately the relationship between the behavior of the courts and social conflict. They both feel the need for a more adequate "macro-social" approach to the study of dispute processing and civil litigation. Lempert feels that many dispute processing studies lack macro-sociological vision, while Kidder believes that they implicitly contain such views—"the pressure cooker" model—which he rejects.

There is a common point in Kidder's and Lempert's comments that seems especially important. This is the idea that we cannot understand what courts are doing if we merely look at the processing of individual cases or even a representative sample of individual cases. Lempert is very explicit on this point: he says we cannot understand the courts unless we can relate their activities not only to what happens in the individual case but also to the impact of these patterns

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on the legitimation of the social system. Kidder's criticism of the "pressure cooker" model, though not fully developed, is of similar import. Something more is going on here, both commentators seem to say, than just a lot of judges deciding (or not deciding) if X or Y should win a case.

It is commonplace that courts do more than settle the individual case before them: frequently they also define rules and set precedents which influence other actors, setting the standards for Engel's working normative systems and conditioning the bargaining that goes on among parties in many settings (Galanter, 1979, Mnookin and Kornhauser, 1979). They also enter into complex policy debates, make law, and do a host of other things (Shapiro, 1980). Even the strongest proponent of the disputes focus would not claim that resolution of private disputes is the sole, most important, or even most interesting thing that courts do. Clearly, we need several foci and different approaches to gain a full understanding of courts as institutions in society.

But this is not what seems to be at issue here. I think the critics want to draw our attention to ways in which the very existence of the civil law and its machinery of dispute processing affect underlying definitions of self, society, and justice, and thus determine the limits within which civil conflicts will appear as possible and legitimate.

In this view, suggested but not developed in their comments, one must examine the whole structure of civil law and civil courts in order to understand how they function in society. In this sense, the individual dispute is part of the "context," but only part. Rather, one must see the whole system in operation, a system which includes the substantive law; the structure, attitudes, and performance of the bar; and the nature of the civil courts and civil procedure.

Let me try to sketch what such an account might involve.[6] The first thing to note is that a complete account will require that we look at the emergence of disputes in specific areas of social relations, rather than generally, as Miller and Sarat have done. Their study tells us that naming, blaming, and claiming vary from area to area, and suggests that only by fully understanding the social relations from which disputes do—or

[6] This preliminary and admittedly inadequate sketch of conflict resolution and repression in the landlord-tenant area represents an effort to integrate insights from critical legal theory and the sociology of law. It draws on ideas developed by Arnaud (1973), Balbus (1973), Gabel (1980), Hay (1975), Horwitz (1977), Kennedy (1976; 1979; 1980), Klare (1978), Macaulay (1979), Simon (1978), and Stone (1981).

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do not—emerge will we be able to explain these patterns. A second point is that we now start with quantitative data about individual disputes between participants in these various relationships, even if that is not the end point of inquiry. Since I have already introduced the problem of studying disputes between landlords and tenants, I will continue this example. It is, of course, important to study disputes between specific landlords and tenants. But one cannot understand the social meaning of these disputes unless they, too, are put in context.

We can start with Miller and Sarat's data on individual landlord-tenant disputes. Their survey suggests that tenants perceive grievances against landlords relatively frequently. As Table 2 (p. 537) indicates, over 17 percent of tenants report a grievance with their landlord. Not only is this the highest grievance rate for any "problem" surveyed: there is reason to believe that tenants may, relative to other potential grievants, underreport injuries. This is because it is relatively difficult for tenants to get redress if they do complain: tenant claims have one of the lowest scores on Miller-Sarat's "success" scale. If we believe that the likelihood of redress increases the propensity to name and blame—as seems likely—then the tenant grievance rate of 17 percent may understate injuries.

But to understand this data fully, and to see how disputing is structured in this field, we need to expand the context even further. Most important, we have to look at the disputes that do not occur, and understand why some conflict that might occur does not. Specifically, we must pay more attention to the role of "law," in the broadest sense, in this process. From this point of view we may conclude that the reported 17 percent grievance rate is merely the tip of an iceberg of potential, but repressed, conflict.

The most elementary aspect of this expanded context is the brute reality of the economic and social relations between many landlords and tenants, marked as they often are by economic inequality and substantial disparities of power. Overlaying such unequal and hierarchic relationships is a complex structure of law and institutions which regulate and mediate the underlying relationships. It is important that we understand all the aspects of this structure. What the critics seem to be saying is that we have only *begun* to identify the relevant dimensions.

The first element is the substantive law of landlord and tenant. Sometimes we overlook substantive law when we study disputes. Yet the law is as much a part of the context as the

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lawyers, the courts, the relations of the parties. The substantive law of landlord and tenant both regulates the relationship and legitimates its basic structure. Legal rules, concepts, and doctrine become part of the social reality of landlord-tenant relations. The law reflects but also mediates the reality of economic inequality.

The law itself is one of the filters that determines what disputes will emerge. It helps define what is perceived to be an injury and what counts as a dispute (cf. Gabel, 1980). There is a wide range of possible forms that landlord-tenant conflict might take, from direct challenges to basic property relationships like squatting, holding over, and unauthorized rent strikes, through the articulation of limited but accepted legal defenses like constructive eviction and habitability. The very concepts which form the law help determine which of these options will be chosen.

The law is both a direct and an indirect filter. Not only does it bar certain possibilities, or at least discourage their use, but it also influences other aspects of the filtering system. We know that dispute possibilities are largely defined by the lawyers, who influence the types of cases that are or are not brought. In their decision to encourage or discourage disputants, lawyers are influenced by economic incentives (Johnson, 1981), as well as by their own social relations and definitions of society (Macaulay, 1979). Legal rules clearly affect the chances of success in various situations, thus affecting lawyers' cost/benefit calculations. They also can affect how the lawyer perceives the world and thus defines his or her relationship to clients or potential clients. The lawyer tends to accept the property relations which are constituted by the legal rules, and thus adds support to a system which encourages the tenant to see only those possibilities defined by law.

In this way, the very concept of society and implicit notions of justice built into legal doctrine become part of the behavioral system of landlord-tenant disputes. All these factors and filters limit the disputes that emerge, and structure the actual transformations that occur. And the constrained possibilities that remain further reinforce the system. For the system not only transforms the various individual conflicts: in so doing it "transforms," so to speak, a raw conflict of interest into a social process with limited possibilities. The disputes that do emerge are those in which basic economic relationships are not challenged: all other possibilities are filtered out. Thus the

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existence of some disputes and repression of others serves, as Lempert notes, to legitimate underlying social relationships. In this way, dispute transformation serves to limit social transformation.[7]

Such an expanded vision of civil disputes, civil law, and society is, as Lempert argues, clearly desirable. There is no doubt that to develop it, we need to go beyond the limits of existing approaches to dispute processing. We need to get away from the idea that the social role of courts and law can be explained exclusively by looking at the disputes that do arise, and by studying what courts do and do not do in these situations. We need as well to look at the disputes that are not there and to understand how courts, dispute processing options, and legal doctrine itself affect both the emergence and the suppression of disputes.[8]

The critics have done us a great service. They have made clear that we need to define the context in which to understand civil litigation more broadly than the isolated dispute.[9] Completion of this program will obviously take us all beyond what has been achieved so far. But this does not mean we have to abandon the whole line of inquiry into civil litigation and disputes processing reflected in this issue. Quite the contrary; if my brief effort to sketch an expanded view of conflict "resolution" in the landlord-tenant area has any persuasive force, it should be obvious that progress lies in correcting and expanding what has been started—not rejecting it. Of course, as this account also suggests, we may have to go far beyond available quantitative data and develop trans-empirical analyses to make sense of what our measuring instruments can record. But these efforts build from, and should complement, the data gathering CLRP and others have begun.

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[7] The most comprehensive account of the way the law and the profession constrain dispute possibilities is contained in Macaulay's (1979) study of lawyers and consumer protection law. Stone's (1981) study of the relationship between labor law doctrine and dispute processing is similarly suggestive.

[8] What Kidder seems to find objectionable in the pressure cooker model, therefore, is that each individual is treated as an isolated atom which may or may not collide with other atoms and create a dispute. When these collisions occur with sufficient frequency, the pressure is relieved by social processes of dispute resolution. Problems exist only when the valve is too tight to relieve the pressure; then we need more courts, more efficient systems for dispute resolution, etc. The disputes focus, say the critics, looks at the number of collisions and compares them with the capacity of the valve. But it fails to see that the valve itself determines the number and nature of the collisions that may occur.

[9] For an effort to relate developments in the processing of individual disputes to broader social and political phenomena, see Ietswaart (1981).

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IV. CONCLUSION: MEETING THE DEMANDS OF AN EVER-EXPANDING "CONTEXT"

The lesson to be learned from this analysis of some of the major criticisms of current work on dispute processing and civil litigation is clear. A valid beginning has been made, but much remains to be done. I have argued in the previous section that progress lies in constant reappraisal of the presuppositions of our approach, continued scrutiny of methods, development and refinement of research techniques, and expansion of the perspectives we employ to conceptualize litigation and conflict resolution.

A Different View of Social Inquiry

This means that I explicitly reject a notion which seems to underlie Kidder's call to abandon the disputes focus. It seems to me that his comments are based on a hope that we can find some new, total insight that will sweep away the limits of our conceptual schemes, wipe out the errors of the past, and overcome all the limits of our disciplines, so that we can stand on some sunny plain of purified knowledge. I think that such hopes are as illusory and corrosive as unthinking acceptance of the present state of things is complacent and conservative. Social inquiry demands that all aspects of the research endeavor, except the commitment to truth, always be open to scrutiny, that all methods be seen as imperfect, and that all findings and interpretations be treated as provisional. But this does not mean that when we recognize errors and flaws in our studies we can, or should, reject them in their entirety. Of course, there are "revolutions" in social studies. But even when they occur, these major shifts involve the substitution of one set of partial concepts and methods for another. Most of the time our work involves modest corrections to, and augmentation of, the kinds of imperfect tools that we are given and must work with at all times.

Nevertheless, there is a danger that the cumulative effect of inevitable errors and omissions could seriously bias our results. To do him justice, one might read Kidder as expressing the fear that such cumulative effects could vitiate whatever is positive in the disputes focus. Critics like Kidder could be read as suggesting that the elements of the disputes focus are so interrelated that their inevitable distortions are mutually reinforcing. By putting individual disputes in the foreground, they suggest, we could deflect attention from the collective dimension that lies behind them and the structural inequalities

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they involve. By looking only at the crystallized grievance, we may distract attention from the unperceived injurious experience and false consciousness. By using legally derived definitions of disputes, we could accentuate the tendency to individualize collective conflict, since law itself defines things in this way.[10]

All this, the argument would run, could support a distorting ideology of "access" as justice. In this way of looking at the world, we would tend to equate justice with the availability of mechanisms, informal or formal, to process and resolve disputes. Disputes would be seen merely as conflicts between isolated, equal parties, and not *also* as expressions of actual or potential collective struggle. Implicit in this view is the further notion that there exists a neutral method, be it adjudication or something else, that will properly resolve the dispute, and institutions to apply that method. The only question left to ask in such a one-dimensional view is whether people have access to these mechanisms. If so, the issue of justice is solved: if not, its solution is a mere question of social engineering.

If that is what the critics are really saying, then we should pay close attention to their arguments, for such charges are not to be treated lightly. But as I look at the rich body of material presented in this issue, the tradition these studies draw on, and the new directions that are suggested, I do not think that our community is in much danger of being dominated by any such one-dimensional thought.

Demands on the Law and Society Community

These observations bring me to a final point, one raised most clearly by FitzGerald and Dickins. While they applaud the effort to expand the study of courts to include disputes and the whole process of dispute transformation, these commentators worry whether this expansion of focus will somehow carry us beyond the limits of "the sociology of law" (1981: 682).

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[10] Of course, there is another critique of the "gestalt" emerging from disputes studies that is symmetrical with Kidder's yet leads to radically different conclusions. Look at David Engel's views on the overall issue of disputes and conflict. Engel criticizes disputes research for focusing, as the anthropologists have, on the "trouble case." These, he says, are the situations in which social relationships have broken down. By looking at these to the exclusion of working social relations, Engel says, the disputes focus highlights what is unusual and aberrant, not what is typical and normal. By doing this, we tend to picture society as isolated atoms in collision, rather than seeing it as a whole, integrated by working normative systems which maintain harmony. This approach, he suggests, makes society seem more conflicted than it is, and grossly overstates the role of formal law and courts in maintaining order.

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While it is easy to reject their concern as unduly parochial, they do raise an important issue. Of course, it is hard to describe the disputes field as "sociology" in any sense. CLRP, for example, has obviously drawn on many disciplines, for better or worse. No one would describe Johnson's paper as sociology, yet it seems to be central to the phenomenon we want to explain. But FitzGerald and Dickins are really not concerned about boundaries of *disciplines*, as much as boundaries of *subject matter*. That is, they recognize that once one gets into areas like Felstiner, Abel, and Sarat's "Naming, Blaming, Claiming," and especially into higher mysteries of false consciousness and unPIE, one may have to move beyond the subject matter usually discussed in these pages. One wonders what they would say about the even wider context which Kidder and Lempert have pointed us toward!

It seems to me that this kind of expansion of focus is inevitable and ultimately desirable. But it is also very difficult to do well, as FitzGerald and Dickins rightly observe. It seems to me we must move in three directions. First, we have to learn a lot more about the workings of civil procedure, the reality of settlement, and the disputes that do not get to court. At the same time, we have to secure a much better idea of how varied "contexts" affect the emergence and transformation of disputes. For if we have learned anything so far, it is that explicating family or landlord-tenant disputes involves understanding families and property relations in our society, as much as that of an arbitrarily defined generic phenomenon called the "dispute." Finally, we must supplement our empirical work with social theory. By this I mean more than efforts to find causal relations among operational variables. We must additionally be able to imagine alternative ways things might be organized in society, so that we can see what is not occurring and why. These are the lessons I have taken from my own work in the Civil Litigation Research Project. That it is hard to ride all these horses at once, I have no doubt. But that is where we are, and it is really too late to jump off!

For references cited in this article, see p. 883.

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