TAKING STOCK

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This collection, which brings together critical reviews of the literature on a variety of contemporary issues in law and social science, has its origins in two judgments about our field that may appear contradictory but in fact are complementary. On the one hand, the empirical data and generalizations produced during the last several decades seem to call for, and amply to deserve, compilation and summary of the kind often found in propositional inventories. But at the same time recent scholarship often seems stagnant, further documentation (or falsification) of a well-established (or generally discredited) hypothesis, one more entry in a sterile, and ultimately unresolvable, theoretical debate—a by-product of the demands of tenure-review committees rather than the expression of any real intellectual engagement. If this judgment is fair, reflective and critical reassessment of the field is essential to break the hold of existing preoccupations and to formulate new questions. These dual motives will be apparent throughout this volume. Here I will offer my view of the kind of reappraisal that is needed, explain how the topics and authors were selected, and suggest some of the ways in which the essays that follow contribute to the task.

Interdisciplinary studies of law in society have grown rapidly but unsystematically in recent years. Many measures attest to this growth: teaching in law schools and social science departments; the appearance of scholarly organizations in many countries and developing cooperation among them—the recent joint meeting of the Law and Society Association and

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the ISA Research Committee on Sociology of Law was attended by more than 400 people from 27 countries on 6 continents; the growing interest of government in applied research (evidenced by the Law Enforcement Assistance Administration and now the National Institute of Justice) as well as pure research (e.g., the Law and Social Science Program of the National Science Foundation, which supported this volume); and the creation of nonacademic, nongovernmental research institutions (like the National Center for State Courts and the American Bar Foundation). There has been a publishing explosion; numerous commercial publishers have launched book series (e.g., Sage, Academic, Lexington, Macmillan, Martin Robertson, Russell Sage, Plenum, Routledge & Kegan Paul) that augment the many books already published by university presses, and the number and circulation of journals have multiplied rapidly to include: this journal, *British Journal of Law and Society*, *International Journal for the Sociology of Law*, *European Yearbook of Law and Sociology*, *Law & Policy Quarterly*, *Contemporary Crises*, *Research in Law and Sociology*, *American Bar Foundation Research Journal*, *Journal of Legal Studies*, *Law and Human Behavior*, *Sociologia del diritto*, *Archivio Italiano di sociologia del diritto*, *Jahrbuch für Rechtssoziologie und Rechtstheorie*, *Zeitschrift für Rechtssoziologie*, *Verfassung und Recht in Übersee*, *Polish Sociology of Law Newsletter*, and *Anuario de Sociología y Psicología Jurídicas*.

Yet such rapid growth is attended by inevitable dangers. Geographic dispersion across continents and languages leads to difficulties in communication. The mere volume of scholarly production tends to produce balkanization by discipline (e.g., economics) or subject (e.g., plea bargaining or appellate court decision making), thereby diminishing a principal virtue of the field—its interdisciplinary character. Furthermore, the very speed with which research is being produced poses other dangers. Because the field is new, it does not have a well-developed body of theory. Instead, scholars borrow eclectically from many disciplines. This encourages originality, but it also threatens to render research noncumulative. It means that an investigator working within one tradition may be aware of relevant theory in another; Snyder's essay in the present volume illustrates well the persistent disregard by "law and development" scholars of the contemporaneous emergence of political-economic theories of underdevelopment and dependence; Danet documents the lack of communication

between anthropologists and sociolinguists in the study of dispute processing. Anyone who has read unpublished manuscripts or research grant proposals knows how frequently scholars are unaware of significant literature pertaining to their work. And many published articles seem to approach the literature survey as an irksome obligation, to be fulfilled perfunctorily after the real work has been completed. But even when the scholar gains access to the relevant literature, he may be unable to assimilate and use it because the theory remains implicit, or the framework too alien and obscure. In this field, as in so many others, social science largely ignores the mandate of its own epistemological canon that theoretical generalizations, inductively derived, should be used to generate hypotheses that are then tested empirically.

In sum, we are both blessed and cursed with a proliferation of empirical research, the value of which is seriously impaired, if not lost altogether, because too little effort is expended on synthesis. This is not to deny that valuable synthetic contributions have been made at various levels of abstraction. One is the interdisciplinary subfield: legal anthropology (Nader, 1965; Moore, 1969; Nader and Yngvesson, 1973; Collier, 1975; see also Pospisil, 1971; Moore, 1978; Nader and Todd, 1978), economic analysis of law (Posner, 1977; Hirsch, 1979), law and psychology (Tapp and Levine, 1977; Sales, 1977; Farrington et al., 1979), legal history (Gordon, 1975; Friedman, 1974). Yet such overviews tend to be most useful when the subdiscipline is still young; thereafter they must either be overly selective or degenerate into mere laundry lists of people and writings. Another approach is the review of a national literature in law and social science, e.g., Great Britain (Campbell and Wiles, 1976; Campbell, 1977), Italy (Baronti and Pitch, 1978), Scandinavia (Ziegert, 1978; Stjernquist, 1977), France (Arnaud, 1976, 1977), Spain (Treves, 1975; Diaz, 1978), Rumania (Popescu, 1977), Brazil (Souto, 1977), German Democratic Republic (Mollnau, 1977), and Poland (Kodjer, 1978). But once again comprehensive synthesis becomes less feasible and also, perhaps, less useful as these literatures become richer and more varied; it is hard to imagine a survey of American scholarship in law and social science, except in the form of anthologies intended for the beginning student (e.g., Schwartz and Skolnick, 1970; Friedman and Macaulay, 1977; Evan, 1980; see also Black and Mileski, 1973; Reasons and Rich, 1978; Campbell and Wiles, 1979).

A very different enterprise is the magisterial overview or grand theory. There have been a number of notable efforts by contemporary writers in recent years—Podgórecki (1974; see also Ziegert, 1977), Friedman (1975; see also Stinchcombe, 1977), Unger (1976; see also Eder, 1977; Parsons, 1977), Black (1976; see also Stinchcombe, 1977; Eder, 1977)—as well as restatements of the work of classic writers—Marx and Engels (see Cain and Hunt, 1979; see also Pashukanis, 1978; Beirne and Sharlet, 1979), Weber (see Trubek, 1972; Hunt, 1978; Beirne, 1979), Durkheim (see Hunt, 1978; Turkel, 1979; Lukes and Scull, 1980), Petrażycki (1955; see also Górecki, 1975; Podgórecki, 1974; Ziegert, 1977). The work of other European authors is still largely unknown in the United States because it has not been translated, e.g., Aubert (1968), Carbonnier (1972), Luhmann (1972), Treves (1977; but see Baronti and Pitch, 1978). But if such major theoretical restructuring has the greatest potential to renew the field, there is little a journal, an editor, or a funding agency can contribute, for such advances are the product of lonely and self-motivated work.

Perhaps the most fruitful synthesis, in terms of its capacity to consolidate isolated empirical studies and stimulate research in new directions, is the topical. *Law & Society Review* has published a number of literature surveys that seek to promote middle-level theorizing about such diverse subjects as extralegal attributes in sentencing (Hagan, 1974), the relevance of organization theory for the study of courts (Mohr, 1976), the structure of litigation (Galanter, 1974, 1975), dispute institutions (Abel, 1973), small claims courts (Yngvesson and Hennessey, 1975), American legal culture (Sarat, 1977), media ombudsmen (Palen, 1979), the delivery of legal services (*Law & Society Review*, 1976), and plea bargaining (*Law & Society Review*, 1979).

This last category of reviews inspired the present project, which proceeded as follows. I decided at the outset against any pretense of encyclopedic coverage, for I felt there were many areas neglected by researchers in which synthesis would be premature (e.g., the facilitative aspects of law) and others in which it would be redundant because of recent efforts of the kind described above.[1] I also wanted to avoid predetermining the ways in which topics were defined since such reviews often

[1] The Committee on Law and Social Science of the Social Science Research Council is currently engaged in producing a handbook whose approximately fourteen chapters will provide a more comprehensive picture of the field.

make their greatest contribution by conceptualizing subjects in new ways—Shapiro's essay in the present volume, analyzing appeals as mechanisms for political control and integration from the top down rather than devices for error correction from the bottom up is a good example of the insights to be gained by adopting a novel perspective.

I therefore felt it essential to allow the topics to be defined by the broadest possible community of scholars. To this end, I first sought the help of the trustees and officers of the Law and Society Association and the members of the Editorial Advisory Board of the *Review*—nearly 50 people—asking them to suggest subjects that were ripe for review. From the many responses, I drew up a long list of some fifty possible candidates. The trustees then appointed a committee of five to advise me on the ultimate selection of topics and authors: Malcolm Feeley (political science), Jack Ladinsky (sociology), Felice Levine (psychology), Samuel Krislov (political science), and David Trubek (law). We adopted the following criteria to guide our decisions:

> Has there been substantial and significant empirical research on the topic? Have reforms been implemented that permit quasi-experimental evaluations?
>
> Is there controversy over the conclusions to be drawn from that research?
>
> Can we anticipate theoretical advance from the synthesis, either because theory has hitherto been implicit or because competing theories have been advanced?
>
> Does the topic promise cross-fertilization between bodies of theory or subdisciplines that have not previously been seen as related?
>
> Would reinterpretation of the data have significance for contemporary social issues?

Using these criteria, our committee narrowed the original list to some twenty suggested topics, including most of those ultimately reviewed as well as subjects like legal regulation through the use of incentives; the relevance of empirical social research to substantive fields like consumer protection, family or juvenile law, and environmental law; white-collar crime; formalism in legal thought; victimization; popular participation in the legal process; and economic anthropology and the role of law in preindustrial societies. In order to avoid reliance on the old-boy network in selecting either topics or contributors, I sent out a call for participation to the broadest population that could readily be reached, the approximately 1,000 members of the Asociation. Their response constituted a strong endorsement of this method: more than sixty applications were completed, indicating a widespread feeling that this kind of theoretical synthesis was important; the broader community appreciated

being involved; and, most important, new topics were added to our original list and proposals received from many people who were not "the usual suspects."

The committee then had the task of selecting eight reviews, the number for which we thought we might realistically be able to secure financial support.[2] The high quality of the proposals made this enormously difficult. In making our decisions, we developed some additional criteria. First, we chose reviews that dealt with major institutions within the legal system: courts, criminal legislation, the police, the federal structure, language, and punishment. Second, we wanted reviews that looked at institutions in their entirety and would therefore be able to perceive linkages between specific problems—linkages that have thus far been overlooked—and ferret out the untoward consequences of apparently successful innovations. Third, we hoped each review would bring new theories to bear on vital problems where more traditional analysis appears to have led to a dead end. Thus Danet uses sociolinguistics to study the breakdown of communication in the courtroom, Snyder examines theories of underdevelopment to understand the inability of law to promote development, Scheiber seeks historical insight into contemporary tensions between centralization and decentralization, and Vidmar and Miller apply socialpsychology to the often overlooked retributive aspects of punishment. Fourth, we looked for comparative studies (e.g., Snyder, Shapiro, Hagan); indeed, several authors enrich this volume by drawing upon the scholarly traditions of the countries where they work—Canada, England, and Israel—in addition to reviewing the literature from the United States. Fifth, we sought to include a wide spectrum of disciplines: law, linguistics, sociology, political science, history, anthropology, criminology, psychology, and political economy. Finally, we wished to present younger scholars, whose work is not yet as well known in the larger community of law and social science, as well as those with established reputations.

Although the essays were not chosen for their thematic unity, they converge and complement one another in striking ways. First, they are in substantial agreement about important metatheoretical issues. Many adopt a macrosocial perspective, which has been relatively neglected in sociolegal studies.

[2] We commissioned two other essays that are not included in the present volume. Vernon Dibble died tragically before he could revise his analysis of the writings of the young Marx on law. David Bordua will soon complete his essay "Police in American Society," which we hope will be published in a future issue of the *Review*.

Scheiber studying federalism and Snyder underdevelpment both show clearly—though at the different levels of the nation and the world-system—that our understanding of law will remain incomplete as long as it is confined to smaller units of analysis. Hagan, similarly, seeks explanations for the content and chronology of criminal legislation in the broader context of the societies that enact those laws. And Shapiro demonstrates the insight to be gained by looking at an appellate structure not just from the perspective of the individual litigant seeking redress or the erroneous decision requiring correction but also from that of the overall political structure, in which appeals may constitute a mode of downward control. Yet a macrosocial perspective alone is also insufficient, for, as Vidmar and Miller show, there is constant tension between individual attitudes toward crime and punishment and the social institutions through which punishment is implemented.

The choice of a macrosocial perspective has two other metatheoretical consequences. All of the authors are more or less explicitly historical. This is not surprising in the case of Scheiber, a historian writing about change in federal relationships, but it is noteworthy in the other essays. Snyder, indeed, argues that the "developmental" view of law is flawed, ironically, precisely because it is *ahistorical*: it sees the parochial experience of western nations during the last century and a half of industrial capitalism as constituting the only possible historical path, one that leads inevitably to the single fixed endpoint of the present. Once a macrosocial perspective has been chosen, furthermore, intersocietal comparison is inescapable since hypotheses can only be generated and tested by contrasting the total societies that are the units of analysis. Thus, Danet urges historical inquiry into the emergence of a differentiated legal register and comparisons of the form and content of this register in various contemporary societies. Snyder argues that such comparison has the additional benefit of cross-fertilizing the perspectives of scholars from different environments and notes that the relationship between first- and third-world nations is pictured very differently by the scholars of each.

Finally, these essays depart from the existing corpus of research in the objects of their scrutiny. I will argue at the end of this volume that a serious failure in contemporary sociolegal studies has been the choice of subject matter: the preference for institutions that can be interpreted within a legalist paradigm (e.g., courts) and for marginal actors, rules, and

institutions (e.g., criminals, purely symbolic laws, the jury). Although the authors of these essays might not agree with all of my judgments, they appear to be responding to some of these same concerns. They consider lawmaking to be a social process just as important as application of law (e.g., Hagan). When they deal with crime, they do so less from the perspective of the criminal and more from that of the rest of society (e.g., Hagan, Vidmar and Miller). Similarly, they look at courts through the eyes of politics, not jurisprudence (Shapiro, Danet). And they redress the overemphasis on laws that regulate interpersonal behavior with studies of the regulation of fundamental economic institutions (Scheiber, Snyder).

Having made these choices of perspective and subject matter, what do the various authors find? If they share any conclusion, it is the very general one (common to social science, if not all scholarship) that things are not what they seem. In the vocabulary of sociology, they look for and find latent functions beneath the manifest. For if such ambiguity characterizes all social phenomena, it may be unusually pronounced within the law because legal rules, processes, and institutions carry official, public declarations of purpose. Thus Hagan notes that though early twentieth-century criminal-law reforms creating juvenile courts and adult probation were justified in the Progressive rhetoric of lenience, they actually increased the ambit of state control over deviant behavior. Vidmar and Miller contend that the criminal sanction cannot be adequately understood in terms of the deterrent or therapeutic language by which it is legitimated. They point to a number of empirical findings suggesting that the psychology of individual reactions to crime is in tension with these official ideals: the degree of responsibility attributed to an actor is proportional to the severity of harm his act causes; the depth of an individual's concern for crime is proportional to his belief in the efficacy of punishment; at the same time, concern for crime is inversely proportional to the perceived risk of victimization; and finally, belief in the necessity of punishment survives empirical disproof of its deterrent effect, simply shifting ground to a retributive justification. Thus, punishment must simultaneously satisfy the social ideas through which it is publicly legitimated and intraindividual needs; the two are often inconsistent.

Shapiro observes that appeal is described by lawyers as a means of correcting error and ensuring uniformity, yet that is

not an explanation but a folk image—a proclamation of the infallibility and incorruptibility of the regime that contributes to its legitimation. A more adequate account must view appeal as a mechanism for exercising control from the top down, justifying hierarchy, cooling out grievances, collecting information about the behavior of both the populace and lower officialdom and transmitting it upwards, and integrating the divergent elements of large nations and especially empires. The peculiar virtue of appeal for these purposes is its ambiguity: it appears to be initiated from the bottom up rather than the top down, and thus to be a form of democratic participation rather than authoritarian domination; and its superficial legalism disguises the political content inherent in national or supranational integration (as in the European Community). Danet demonstrates that though official dispute processing in contemporary western legal systems claims to be "fact" and "truth" oriented, it actually contains large and important elements of play and ritual.

Although Scheiber takes great pains to show that the manifest functions of federalism are not mere fictions, he too is concerned with the latent consequences of the federal structure: for instance, the much touted role of the states as laboratories for social experimentation often reduces, in practice, to competition in laxity when state officials are confronted with the economic centralization and power of the private sector; responsiveness to local interests can mean exploitation of the locally weak by the locally strong, as shown by the dismal record of most localities in the protection of civil rights and civil liberties. Snyder describes the way in which the rhetoric of development distracts attention from the process of underdevelopment; indeed, he goes further and suggests that law may be largely irrelevant to the manifest functions of social control and social change in whose name it is invoked.

One latent function emphasized by every author is self-aggrandizement by occupational specialists, particularly the officials and private professionals who operate or manipulate legal institutions. Hagan explains a great deal of criminal legislation in terms of the interests of the helping professions that staffed the new institutions of control and treatment (psychiatrists, psychologists, social workers, correction officers) as well as the interests of the mass media, which publicized crime and mounted reform campaigns at least in part to increase circulation. Vidmar and Miller also suggest

that the role of the media is central to any understanding of public attitudes toward crime and punishment. And the other essays all recognize as central the interests of institutional officials: federalism is, among other things, a political struggle between national, state, and local bureaucrats; appeals are a contest between higher and lower judges; and dependency in the third world is due, at least in part, to an alliance between the interests of state and capital in the first world and the growing bureaucratic bourgeoisie in the underdeveloped nations.

Danet is perhaps most explicit about the role of legal professionals in creating a distinctive legal register for the facilitative, regulatory, and dispute-processing functions of law. Legal English is not complex by necessity—the same content can be stated in linguistic forms that are much simpler and far more comprehensible to the lay public; it is lawyers, judges, legislators, and administrators who complicate the language. Furthermore, though legal professionals justify this complexity in the name of precision, careful analysis reveals numerous residues of deliberate ambiguity and stylistic refinements that serve no referential function. But whenever a legal phenomenon is interpreted in terms of the interests of officials or professionals it is essential to remember that neither the state nor the profession is wholly autonomous—it is particular clients, for instance, who use lawyers to complicate transactions; analysis, therefore, must continually search for more fundamental structures underlying the behavior of legal institutional actors.

I see another common issue in these essays, although some authors might not accept my reading. This is an increasing recognition of the need to deal explicitly with the political content of scholarship. I believe that this is a consequence of the breakdown of the liberal paradigm, as I will elaborate in my concluding essay. Here let me develop the point with reference to the subjects of these essays. The theoretical/descriptive model of liberal legalism and the social policies it advocates are under increasing attack from both left and right. No one seems satisfied with the criminal justice system. Conservatives criticize it for coddling criminals, ignoring just demands for retribution, being too permissive with respect to social behavior (but also too intrusive with respect to economic), and failing to ensure the safety of the citizen (or perhaps more to the point, the feeling of safety). Radicals condemn it as a system of class oppression that systematically overlooks the crimes of the

powerful; such critics see the campaign against permissiveness and the outcry for retribution as thinly disguised attempts to confuse and divide the oppressed along lines of race, gender, religion, sexual preference, and life-style. Other institutional structures and processes evoke little more confidence. Appellate courts, especially those at the pinnacle, are attacked for being either too political, too willing to engage in policy making, or too legalistic and cowardly to take a strong stand. Although these positions are sometimes associated with right and left, they tend to shift with the content of the particular actions being criticized. The structures of federalism have been at the center of political controversy ever since their creation, but the older lines between conservatives championing state rights and liberals favoring a strong central government have blurred: critics on both sides of the political spectrum now attack centralized power, mushrooming bureaucracy, and the lack of direct democratic control. The mystifications of legal language and ritual have few apologists outside the ranks of their acolytes. And faith in development through the transfer of western institutions (including law) to the third world is thoroughly shattered: conservatives and radicals alike decry foreign aid, the former as a waste of tax money, the latter because it creates relations of dependence, enriches the multinationals, and contributes to the formation of a comprador class.

If the liberal paradigm has collapsed, its demise may help explain why the essays in this volume are, indeed must be, macrosocial, historical, and comparative. Microsocial scholarship is possible only within an accepted model of law in society that determines both what should be studied and the overall structure, whose details, alone, are problematic. But the breakdown of the paradigm not only requires a major theoretical effort at a higher level of abstraction. It also compels scholars to choose sides and declare their political allegiances. This was unnecessary as long as there was general consensus that legal institutions worked reasonably well and should be preserved if they needed some limited reforms. But such agreement can no longer be taken for granted. Liberal legalism, for instance, is predicated on the belief that courts conforming to an ideal of formal rationality are both possible and essential to the good society. That belief is being challenged. It is therefore a political decision not only to study courts (rather than, say, poverty) but also to focus on their departure from formal rationality in order to propose marginal

corrections rather than view courts as so fundamentally flawed that we must ponder major structural changes.

In taking this position, I am disagreeing with Hagan, whose essay here attempts an empirical test of two competing views of criminal legislation that he denominates moral functionalism and moral Marxism (better known as, though not identical with, consensus and conflict theories). Hagan characterizes each view as "moral" in order to stress its inherent value content, a quality he deems unfortunate and remediable. I contend, on the other hand, that those words, and indeed all social science concepts, should and must have value content. I do not believe we can decide empirically whether criminal legislation expresses societal consensus or class domination. I would argue that it is possible to engage in "value-free" social science *only* at the microsocial level and *only* when there is consensus concerning the values supporting the paradigm within which the microanalysis occurs. In other words, research cannot be value free but only value implicit. It is ultimately a political decision whether to emphasize the shared values expressed in criminal legislation or analyze the ways in which it contributes to class formation and dominance by enhancing the power of professionals, expressing symbolic competition between status groups, impinging unequally on classes or races, or dividing the oppressed. Interpretations of the criminal sanction are also political. This is fairly obvious when sanctions are viewed as deterrent or rehabilitative: our notions of who is deterring whom from what kinds of acts, or who is undergoing treatment for being what kind of person, clearly influence our view of the legitimacy of punishment (and vice versa). But the significance of retribution also turns on value judgments: whether it is depicted as the essential ingredient in a Durkheimian normative consensus or as expressing the psychological needs of a dominant class—for instance, a middle class that sustains its own extraordinary level of self-control by punishing the indulgences of others and can vent this punitive urge precisely because those others are seen as fundamentally different by reason of class or race.

Snyder certainly shares my belief that views of the relationship between the first world and the third are strongly colored, indeed determined, by values. If the goal is a rising GNP and an optimum level of economic efficiency, technically conceived, then the relationship will be seen as flawed development; if the goal is increasing equality between nations and equality and political participation within them, then the

evidence will point to underdevelopment and dependence. Value judgments are implicit in the other essays as well, even if epistemological issues are not addressed directly. Federalism is only of interest, and can only be assessed, within the framework of views about the proper location of power, the importance of democracy, the value of civil rights and civil liberties, and the need for state control over the economy. Which of the manifold functions of appeal is emphasized will depend on whether the observer believes the important political problems are correcting error, ensuring legality, and equalizing access, or promoting political integration and exercising control over subordinates. And politics colors our view of language—whether we see legal discourse as referential and representational, though suffering from remedial ambiguity and complexity, or as a mechanism for the exercise of power, in which those qualities are essential, not adventitious.

I recognize that the issues just posed are ancient, difficult, and fundamental. I have not pretended to offer a full examination but only sought to pique the interest of readers as they peruse the essays that follow. Let me conclude with a text—not a classic of positivism or its critics, but rather a line from Lewis Carroll. In the words of the Red Queen, all social science, like law and indeed all human interaction, is "sentence first—verdict afterward."

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