COMMENT ON SIMON AND LYNCH

KEEBET VON BENDA-BECKMANN

It is a curious sensation to read a review article on the main themes, problems, and gaps in one's own scientific field, the sociology of law, and yet hardly recognize it. This is precisely what happened to me upon reading Rita Simon and James Lynch's contribution to this issue. Since I do not suppose that they have consciously omitted some important parts, I began to wonder how this difference in perception was possible. Why should my own conception of the field be so different from theirs? Could it be that sociology of law in Europe has developed a tradition of its own which only shows remote resemblance to the field in the United States?

My task here is not to reconsider the American situation in the field of legal sociology, nor, for that matter, the whole field of legal sociology, nor all sociology of law that is being done in Europe. Only a few points will be made, therefore. The review article is almost exclusively concerned with an extremely narrow conception of the sociology of law. It deals basically with what is called "grand scale" theory and quantitative research within the United States. That is, it seems, in the eyes of the authors the real sociology of law. But even in this extremely limited view of the sociology of law the review omits important parts of the field, by not mentioning, for example, Cappelletti's *Access to Justice* (1978/79), with the results of the large Florence Ato project. It does not look beyond the traditional concern with adjudication, nor does it mention the interesting work on alternatives to adjudication, of which *No Access to Law*, edited by L. Nader (1980), and *The Politics of Informal Justice*, edited by Abel (1982), contain some of the major results.

The authors extensively and basically in agreement quote Friedman and Tremper, who state that the sociology of law has made no contribution to the understanding of law and society, that it is not taken seriously, and that there is hardly more than a series of sometimes useful case studies. Comparative studies are hardly available. There is no theoretical work of significance besides the work of Black, and what theoretical work there is has been done mainly in criminology. Lawyers do not take sociology of law seriously, in contrast to economics and law, and joint efforts between lawyers and social scientists have generally failed. I wonder whether the situation in the United States is as dismal as the

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authors picture it. It certainly is not the sociology of law I am familiar with in Europe.

As far as theoretical work is concerned, some serious omissions in the American literature were made. The two-volume *Toward a General Theory of Social Control*, edited by Black (1984), is not mentioned, although it does contain interesting theoretical work. In 1974 one of the most intriguing and encompassing theoretical works was published: Marc Galanter's highly influential article in the *Law and Society Review:* "Why the Haves Come Out Ahead: Speculations on the Limits of Social Change." This extremely densely written essay contains a descriptive theory of the legal system, which deals not only with problems of access to law, but also with the relationship between the official court system and "appended" and "private" systems of conflict management. It deals with the limits of changing the legal system through legislation, although this part is not fully developed. And it is a thought-provoking analysis of the relationship between legal centralism and pluralism. Galanter's other major theoretical contribution to the sociology of law, "Justice in Many Rooms," which appeared in the *Journal of Legal Pluralism* (1981), is equally stimulating. As such, it is far superior to Black's often-quoted poster theory, which reduces the whole field of law and society to the question of more or less law, without any distinction. Galanter's work deserved to be mentioned not only because of its ingenuity and its ability to encompass highly complex matters, but also because of its influence in Europe, not only on the few researchers in the field of the sociology of law, but also on legal education.

A second major theoretical trend that I did not find is that of the neo-evolutionists, among whom Unger (1976) is probably the most interesting and sophisticated. His book *Law in Modern Society* is a historical and comparative sociology of the development of Western law. Though his treatment of early forms of law and his comparisons to non-Western legal systems form a weak part of his work, and though I am not altogether happy with the neo-evolutionist tendency, the core of his work is a thoughtful and stimulating analysis of "modern" legal systems. The other neo-evolutionist authors, among whom are Teubner and Willke (1984), have started the debate on whether law is an "autopoietic," a self-referring system, and whether law in the modern Western world tends to become more "reflexive." I heartily disagree with their, to my mind, too rosy, speculations on the direction in which Western law develops, and their arguments about law as a self-referring system have now been sufficiently proven to be wrong by sociologists and by some lawyers as well. Yet the debate should be mentioned, if only because much of what is said is a direct if implicit reflection of neo-evolutionist ideas prevalent in current legal theory (see also Treiber, 1983; Luhmann, 1985, 1986).

One of the most interesting recent developments is the shift

away from adjudication and conflict management to other fields in which law plays a role: legislation, and rule development and application by administrative bodies. The authors are either unaware of this development or consider it uninteresting. It involves the discussion of the relationship between the state administration and the private sector in the process of legislation. This theme is central in the discussions on legalization and delegalization of social and economic relationships (Voigt, 1983a, 1983b). It appears also in another discussion: In an increasing number of fields the legislature is unable to make legislation on its own, but depends on (parts of) the private sector to do so. It is, in fact, the debate on neocorporatist trends, which is being discussed under various names, not only in political science, but also in the sociology of law, by authors such as Luhmann (1975, 1986), Habermas (1981), Offe (1973), Treiber (1973, 1983), and in the Netherlands Wassenberg (1982), and von Benda-Beckmann and Hoekema (1987), to name a few. In connection with this, some empirical work on legislation has been done as well, in the Netherlands for example by d'Anjou (1986), although not nearly as much as would be necessary and desirable.

The theoretical work in the Marxist tradition, such as that of Cain, de Sousa Santos, Mathiesen, and others, is not even mentioned, nor is the more or less closely related feminist work discussed. Yet the *Journal of Law and Society*, the *International Journal of the Sociology of Law*, and the *European Yearbook on the Sociology of Law* make this work easily accessible to authors who appear to read only English (or should I say American?).

To be sure, most of the theoretical work mentioned does not provide nice and clear-cut hypotheses with which we can measure the whole legal system. But that is precisely the point. Legal sociology is more than developing hypotheses to be measured in numbers by quantitative research, although we could use more of that too. Much of the more interesting work is concerned with describing processes and complex interdependencies, which cannot simply be reduced to the remark that "it all depends." The question is whether this provides us with less understanding than hard numbers (sociologists know only too well how soft these numbers often are). Many lawyers would perhaps prefer such numbers, and so would policymakers, because they are simple to handle—and easy to manipulate—and carry the look of exactness. The crude models upon which many so-called hard numbers are based suggest the same attractive neatness and exactness provided by economic theory. Policymakers and lawyers have shown a remarkable blindness to the lack of predictability of such models. And it could well be that some of the frustration of policymakers is a result of such undue expectations toward the sociology of law.

Another complaint of the authors is that so little comparative work has been done. This is a remarkable observation. Perhaps

the most famous comparative work has been written by Moore (1978) on the American garment industry and rule development among the Chagga in East Africa. She showed that garment industries develop their own law not in total isolation but in a semi-autonomous social field, in part depending on and restricted by state law. Macaulay's article (1963) on private norm development in the automobile industry is another example of legal pluralism in industrial societies, and so is Treiber's study (1983) on rule development and bargaining in the shadow of the law between the city administration and project developers in a West German town. Of course much more comparative work has been done in the anthropology of law, but that field, I understand, is being covered in another article.

Moore's work is a fine example of how legal anthropology and sociology of law have become increasingly integrated. Of course, American legal sociologists such as Galanter, Felstiner, Abel, and Trubeck, have all been greatly influenced by their own experience in other legal cultures. And so have many European legal sociologists, such as Ietswaart, Griffiths, F. and K. von Benda-Beckmann, Gessner, Roberts, and Snyder.

Much more and equally interesting comparative work has been done over the past decade or two: the comparative work on labor law between Great Britain, the Netherlands, and West Germany (by people like Falke, Höland, Blankenburg, Rogowski, Diekmann, Dickens, and Rhode, see *Rechtssociologische Studiën 2* and *Zeitschrift für Rechtssoziolgie* 1984/1; Simitis, 1985), comparative work on the use of state courts between Nordrhein Westfalen (West Germany), the Netherlands, Belgium, and France (by Blankenburg, Ietswaart, and others), between various states within West Germany on consumer problems, and between different European states on divorce are cases in point. Of course, some of this work has not been published in English, but much of it has, and at least some of the German publications are accessible to English readers through the English summaries in the *Zeitschrift für Rechtssoziologie*. Recently, an interesting comparative study in legal psychology has been started on tax-(non)paying behavior. Hessing and Elffers from the Netherlands, Weigel from the United States, and more recently a number of scholars from other European countries have been studying tax-paying and tax-evasion behavior and the influence of legal norms on such behavior from a legal psychological point of view (Hessing *et al.*, 1988).

Whereas the field of research in the sociology of law in the United States is reported to be limited to criminal matters, access to justice, and jury studies, the situation in Europe seems to be different. Perhaps in part because of a more developed welfare system, much more attention has been paid to studies of administrative organizations, such as labor offices and social security institutions, housing offices, and environment administration. In-

teresting work is being done on rule development and rule application by "street-level bureaucrats" in the Netherlands by Knegt (1986), Aalders (1984), and De Koning (1988), inspired by authors such as Jowell (1975), Kagan (1978), and Lipsky (1980). The Oxford project on compensation and support in cases of accidents has provided valuable insights. Recently, research has started on rule making and rule application in institutions of the European Community by Snyder.

My pretension is not to have presented the state of the art in Europe, for I have not written about the French sociology of law, nor of the Scandinavian contributions. Yet from the few comments I have been able to make here it is clear that the question arises whether indeed a different outlook, interest, and set of knowledge has been developed in Europe and the United States. The review article shows such a parochial preoccupation with the United States and such a narrow view of the field of the sociology of law that a fruitful comparison with Europe is hardly possible. But perhaps an important difference between Europe and the United States lies precisely in the perception of what the scope of the sociology of law is. Much of what falls under the heading sociology of law in Europe might well be labeled differently in the United States. Had the authors taken a wider perspective and had they taken the trouble of looking beyond the United States, the verdict of what sociology of law has brought would perhaps not have been as negative for the United States.

KEEBET VON BENDA-BECKMANN is Associate Professor in the sociology of law and social law at the Faculty of Law of Erasmus University Rotterdam. She has done legal-anthropological research in West Sumatra and Ambon, Indonesia. She wrote *The Broken Stairways to Consensus: Village Justice and State Courts in Minangkabau* and edited, together with Fons Strijbosch, *Anthropology of Law in the Netherlands* and, with F. von Benda-Beckmann and others, *Between Kinship and the State: Law and Social Security in Developing Countries*. She is currently carrying out research on emancipation of female Moluccan immigrants in the Netherlands.

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