THE PERILS OF EMPIRICAL LEGAL RESEARCH

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Bruce H. Mann. *Neighbors and Strangers: Law and Community in Early Connecticut.* (Chapel Hill: University of North Carolina Press, 1987). xi + 202 pp. Notes, appendices, bibliography, index. $27.50.

"Empirical" research is at once the most pressing item on the agenda of legal scholarship and the most terrifying prospect to legal academics. I can remember a seminar run by the American Bar Foundation to acquaint law professors with the opportunities for empirical research. One senior participant, much respected in his field of specialization, finally ventured a question: could the American Bar Foundation teach him how to do empirical research or pair him with someone who did? At another leading law school, a distinguished visiting professor, brought to the school to begin a program in "law and society" featuring empirical research, was refused tenure by the president of the university despite a large majority of the faculty approving the appointment. The victorious minority of the faculty had argued, *inter alia*, that no one had defined the scope or utility of empirical research, at least not to their satisfaction. Law schools teach legal history, the most empirical of all the social sciences, but more often than not the courses revolve about doctrine and imply that all law is autonomous, self-generating, and antithetic to empirical research. The "Wisconsin School," led at one time by James Willard Hurst, and now by such law-school luminaries as Lawrence Friedman and Harry Scheiber, urges empirical research, but the bulk of legal history remains doctrinal. It is a welcome event, then, for defenders of empirical studies in the law school that Bruce Mann, a professor of law at the University of Pennsylvania Law School, has produced so thoroughly empirical a study as *Neighbors and Strangers: Law and Community in Early Connecticut.*

Mann bases his account of the changing face of debt law in early Connecticut on 5,317 civil cases drawn from intervals of the sitting of the county courts of Hartford and New London. At the end of the seventeenth century, the overwhelming majority of these cases involved "book debt," a claim based on the plaintiff's own records of the defendant's indebtedness. These claims, brought to court, involved the jury in a quest for individualized

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factual evidence. By the middle of the next century, book debt was still heard, but increasingly debt actions were formal pleadings, managed by lawyers, going forward without juries, and depending upon specialized technical proceedings. Early in the book, Mann reveals that students of law and society must look not for a "perfect" correlation between the two, but for "patterns in how people used law" (p. 9), what they expected from going to law, and how that experience changed them. "Following [his] own advice," Mann discovered that "debt litigation became formalistic and unforgiving, pleading grew more technical and less expressive of the facts of individual disputes, and the civil jury faded from predominance to comparative insignificance" (p. 9). In all, the change "marked a transformation from a legal system that allowed litigants to address their grievances in ways that were essentially communal to one that elevated predictability and uniformity of legal relations over responsiveness to individual communities" (pp. 9–10). The change in society, the result of the change in law on the society, was profound: "By the middle of the eighteenth century, the formal legal system established its hegemony over the ways people resolved their differences" (p. 10). (The change in law, of course, was a response to the commercialization of the colony's economy, at least in the counties that Mann studies most intensely.)

Hartford and New London county court records may or may not have been typical (one wonders if the more agricultural and communal settlements of western Connecticut would have been quite so profoundly altered by legal formalism), but the story that they tell Mann fits into other scholars' findings. Mann is careful to abjure any knee-jerk adherence to the "modernization school" (n. 71, p. 41), but his evidence surely fits the general outlines of the theory. Though he concedes that the impulse may have been exogenous, increasing "formalization [of pleading] was a largely autonomous development that owed less to shifts in economy or society and more to the tendency of lawyers to treat law as a normative fact" (p. 164). There is little in the book on the lawyers of coastal Connecticut, but the argument is hardly novel or objectionable. In the context of Mann's simultaneous argument that "as changes in the economy and society drew people outside their towns, traditional patterns of social relations gave way in part to patterns that were organized on different principles" (p. 166)—in other words, legal formalism—it is hard to credit his disavowal of modernization theory. It may be that Mann is gun-shy, that he does not want the merits of the work missed in a furor over modernization. It may also be that Mann is trying to have his cake (law and society) and eat it (autonomy of legal processes).

As able as Mann's internalist argument appears to me, I do not think there is any way to avoid the modernization question. It plainly bothers Mann, and he struggles with it like Laocoön with

the serpent from the sea. Indeed, his prose, often supple and clear, becomes tangled when he tries to pull himself from the coils of modernization:

> The structure of a society shapes the way people handle their disputes. The expansion of the economy in the eighteenth century did not mean that all commercial dealings had become faceless and impersonal. That was never the case. However, population growth, migration, and economic development drew people beyond town and county boundaries and changed the way they did business with one another. Multilayered relations and the dealings appropriate to them did not disappear—the continued use of book accounts suggests that they persisted. But they now shared the stage with single-interest, instrumental relations *shaped by new patterns of economic behavior*. (p. 41, italics added)

If the legal behavior is shaped by economic conditions, even in part, it cannot be argued that the form of pleading was entirely a product of internal, autonomous impulses, or the work of the lawyers. One can put together the rise of a professional cadre of pleaders, the emergence of a layer of economic activity that was dominated by regional commerce, and new ways of thinking about dealings with strangers, and Mann, without wanting to fully concede the point, has done just that. All of the phenomena Mann identifies—economic and commercial expansion; formalization and depersonalization of economic, social, and legal relationships; and the professionalization of the legal culture—are part of what other scholars working in legal, economic, social, and geographic history have termed the modernization of society.

Mann relies on litigation to portray commercial practice. This is backward: litigation is commercial practice gone wrong. He assumes that the percentages of actions brought on book accounts or written instruments of total debt litigation are the same as the percentages of book accounts and notes actually relied upon in commercial practice. This is a dangerous assumption, since it is as reasonable to posit that newer or novel instruments of debt, actually used less frequently in day-to-day business dealings, might more readily lead to litigation, or that written instruments, though used less frequently, were relied upon in higher-risk transactions that necessarily led more frequently to litigated claims. Another interpretative flaw is the fact that Mann limits the bulk of his study to the three most commercial of largely agricultural Connecticut's counties. Thus changes in commercial debt litigation, if there were any, overstate the alleged trend toward impersonal commercialization of the law. That is, if one raises only one field of law to an unjustified central position, and a change is taking place in that one field, it does not mean that a similar or analogous change is sweeping all of the fields of law. The law of land survey and title, for example, may well have remained where it was in

the seventeenth century, a matter for neighbors to dispute in front of their neighbors, rather than like commercial debt, an increasingly impersonal process handled by lawyers using a specialized vocabulary in stylized proceedings far from the situs of the controversy.

In general, despite these reservations, Mann's empirical conclusions are plausible. A more troubling matter is his methodology. Mann is obviously uncomfortable with quantification. He notes in his introduction, when describing his methodology, that the core of his data is "a coded sample of 5,317 civil cases" and "several hundred other cases from elsewhere in the colony that *I left mercifully uncoded*" (p. 7, italics added). Coding is neither easy nor enjoyable. Mann could be absolved for leaving some cases uncoded were it not that this admission is only a partial confession. In addition to a lack of rigor in coding, he omits statistical measures of reliability and significance, though quantification of longitudinal hypotheses based upon a sample of cases should calculate and provide the slope, correlation coefficient, and level of significance for the data. In fact, Mann's numbers are often so small and his data points so few that that reliability of the "trend" he suggests would hardly pass statistical muster. Nevertheless, his thesis—that the type of commercial litigation, from a personal book debt transaction to an impersonal transaction based on a bond or note, changed over the course of the eighteenth century—relies on just such perceived trends.

A closer look at Table 1 in his Appendix (p. 171), in fact, tends to refute Mann's hypothesis. The actual number of cases (N) for the first two decades of the century are so small (35 and 87, respectively) that the percentages of each type of debt are nearly meaningless. If those decades are dropped and the remaining decades, each with more than several hundred cases, are examined, the percentage of book debt of all civil litigation fluctuated almost randomly, but it actually increased as a percentage of the total number of debt actions from the 1730s to 1760s. Mann's methods and presentation of data obscure this fact and instead stress the beginning and ending points, which are imperfect, deceptive measures. Indeed, in reality, Mann does not have longitudinal data at all. He confuses a series of cross sections of data, snapshots of a decade, with a true longitudinal series that provides a picture of change over time.

For quantitative methods to work, they must be a part of the original design of a project. The questions the researcher uses to choose a data base, assemble (and sample, if appropriate) the data, and finally display it, must all precede and inform the analysis of the data. Mann simply has not done this. His insistence on the autonomy of legal pleading—in the face of his own concessions that exogenous forces, like commercialization and increased personal

mobility directly affected the legal system—hint that he laid the quantitative design on top of a more traditional, doctrinal work.

Despite the fact that Mann's quantitative arguments are flawed, the trend he has uncovered gives support to the modernization thesis. His own copious citations from the file papers themselves, a myriad of detail of a world of communal economic and social relationships fragmenting into two worlds, the one coming to dominate the other in the courts of law, are an invaluable anecdotal addition to the growing literature of empirical legal history.

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