American Legal Realism and Empirical Social Science: From the Yale Experience

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American Legal Realism and Empirical Social Science: From the Yale Experience*

John Henry Schlegel **

We regard the facts as the prerequisite of reform.

Charles E. Clark & Robert M. Hutchins †

As a coherent intellectual force in American legal thought American Legal Realism simply ran itself into the sand. If proof of this assertion be needed one has only to ask a group of law school faculty members what American Legal Realism was and what it accomplished. If one gets any but the most cursory of responses, the answers will range from "a naive attempt to do empirical social science that floundered because of its crude empiricism," through "a movement in jurisprudence that quickly played itself out because it really had no technical competence and little

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1. Professor Duncan Kennedy supplied me with this felicitous image of the decline of Realism.
to say,” to “a group of scholars who did much to destroy the 19th Century doctrinal universe but left nothing in its place.” Each of these answers is both wrong and essentially partial. The answers are wrong because first, the Realists’ social scientific research died out because of the impermanence of the institutionalized circumstances in which it was undertaken, the peculiarities of the personalities of the leaders of the undertaking, and the difficulties in matching the impulse to do such research with the social science of the time; second, their jurisprudential activities gave out when, faced with the implications of their own constructions, the protagonists lost their nerve; and third, their destruction of the 19th century legal universe left behind enduring achievements in commercial law, corporations, and procedure that point toward the largely legislative legal universe in which we live. The answers are notably partial in that they ignore, if not suppress, both the political coherence of the movement and the more directly political activities of many of its key members, as well as the extent to which it reflected, albeit belatedly, similar movements in American academic thought generally—the rise of the social sciences, the destruction of the general formalist universe, and the decline of “pragmatic” political and social thought.

Given these deficiencies in the common legal understanding of the Realist movement, it is at least curious that each partial understanding is invariably accompanied by an implicit, fatherly assertion: “We are all realists now, don’t worry about these questions.” While one may speak prose without knowing it, and similarly put forth ideas without knowing their lineage, it is more

2. I see no reason to document these attitudes towards Realism in great detail for they are ubiquitous. Gilmore, Legal Realism: Its Cause and Cure, 70 YALE L.J. 1037 (1961) expresses all of them, especially the third. Llewellyn, On What Makes Legal Research Worth While, 8 J. LEGAL EDUC. 899, 400-01 (1956) expresses the first with his usual degree of overstatement. H.L.A. HART, The Concept of Law 132-44 (1961) expresses the second with some reserve. H. REUSCHLEIN, Jurisprudence—Its American Prophets (1951) is an unusually vituperative presentation of all three.

3. I do not wish to document these assertions here; the series of articles that I plan to write, of which this is the first, see note 13 infra, will provide all the necessary documentation.


5. See generally M. White, Social Thought in America: The Revolt Against Formalism (new ed. 1957).

difficult to have learned a lesson—to no longer be that young and foolish, but rather to have grown up, as it were—without having a rudimentary understanding of the something about which the lesson was learned. No less curious is the fact that historians, who have generally paid little attention to narrow legal matters, have better understood the movement. Not only have they pointed out the previously enumerated similarities between Realism and other intellectual movements in the twenties and thirties, but also they have explicitly noted that the intellectual roots of the movement can be found in the progressive politics of the pre-war period.7

These twin curiosities—the failure of understanding by the lawyers and the relative success of the historians—can be explained, in part, if one remembers that the common understandings of Realism are an integral part of the intellectual world of its successors, either the more conservative harvardian ones or the less conservative las-dougalian ones. For the harvardians, deprivation of the certainty of doctrine by the destruction of the formalist universe, and of fact by the failure of empirical social science provided the justification for seeking legitimacy in orderly process;8 for the las-dougalians, the same two deprivations provided the justification for seeking legitimacy in a policy analysis based on assertedly democratic values and the soft facts of experts' opinions.9 And for both groups, the failure of Realist jurisprudence provided the occasion for their theorizing. Given the successors' stake in a particular understanding of the Realist movement, and their near total success in dominating post-Realist legal theory in America, it is not surprising that the understanding of the movement necessary to post-Realist theorizing is the understanding generally held in the legal community. Nor is it surprising that the historians who have a less immediate stake in the fortunes of post-Realist theory have managed to understand the movement more fully.

To know that the intellectual world of the successors of Realism has shaped our understanding of that movement is not to know how to respond to that insight, however. And how to re-

8. Ackerman, Law and the Modern Mind by Jerome Frank, 103 Daedalus 119, 123 (1974) provides a general list of the members of this school, although I do not agree with all his choices. The classic work is H. Hart & A. Sacks, The Legal Process: Basic Problems in the Making and Application of Law (1958).
spond is an important question, for it has become an increasingly pervasive perception that post-Realist legal theory has about run its course headlong into a dead end. The harvardian search for legitimacy in orderly process has not succeeded in avoiding troublesome questions of value, including those raised by the process itself, anymore than the formalist universe had or empirical social science might have been thought capable. The las-dougalian search for legitimacy in policy analysis has not succeeded in avoiding troublesome questions about the source and beneficiaries of the values assumed and the independence of the experts' opinions from those values. And so, the question of whose values legal rules serve, a question highlighted in the Realists' destruction of the formalist universe and in their attempts at legislative law-making, and believed to have been put to rest by post-Realist legal thought, has re-emerged, exactly where it was found forty years ago.

One might respond to the "dead end" feeling accompanying this re-emergence in several different ways. One that comes immediately to mind, because of its current popularity, is the attempt to return to the mainstream of European, specifically continental European, social thought after nearly 200 years of largely ignoring that intellectual force. However, the juxtaposition of the feeling of being at a dead end and the knowledge that the intellectual world of the successors of Realism has shaped our understanding of that movement in such a way as to suggest that, "We might all not be Realists now," counsels a different, though related, enterprise. One might attempt to recover an understanding of what

10. Wechsler, Toward Neutral Principles of Constitutional Law, 73 HARV. L. REV. 1 (1959) makes the deficiencies obvious even as it attempts to argue for the primacy of "process."

11. One might simply ignore the feeling and build a castle at the dead end as does B. Ackerman, Private Property and the Constitution (1977). Ackerman, who from his footnotes, though not his other writing, seems to answer the question "What was American Legal Realism and what did it accomplish?" with an "American Legal Who?" seems to be struggling to reunite harvardian and las-dougalian thought by stiffening orderly process with a spine of formalist welfare economics and wrapping up the result in the banner of scientific policymaking. The echo of Langdell in this use science, see note 34 infra, is surely no accident, anymore than is the echo of las-dougalian assertedly democratic values. See, e.g., B. Ackerman, supra, at 229 n.3. Of course, Ackerman is not the only contemporary practitioner of the castle builder's art. See, e.g., R. Dworkin, Taking Rights Seriously (1977); R. Posner, Economic Analysis of Law (2d ed. 1977).

Realism was and how and why it ran itself into the sand. Such an endeavor could be undertaken in order both to learn what went wrong, if anything, in that particular revolt against legal orthodoxy and to regain the mainstream of American legal thought at some point before it turned into its present dead end, all in the hope if not to learn from the mistakes of the past, at least to better understand the way in which the choices of the past inform the world, and thus the options, of the present.

What follows is the first installment of an attempt to undertake the second enterprise and thereby recover an understanding of what Realism was.\(^\text{13}\) At this level it is simply an excursion into why and how empirical legal research first came to be done by some of the Realists at Yale and then died out.\(^\text{14}\) At the same time this story is not simply about Realism, but has two other prospects, both aspects of the rise of science to intellectual hegemony in, and with, the 20th century American university. First, it is an indirect attempt to capture what it is like to engage in a large multi-disciplinary research project that must meet and satisfy numerous divergent single-disciplinary constituencies. It is thus an example of the recurrent story about the attempts, largely successful, by each of the many individual, balkanized intellectual subunits of the modern intellectual community to dictate the terms on which it will "recognize" research. Second, it is an episode in the continuing confrontation between law and the social sciences over the past fifty years. As such it is an example of a recurrent pattern of confrontation between the "scientific ideal"\(^\text{15}\) and any of the lib-

13. The following outline of the entire project as presently conceived may perhaps present a better sense of the relationship of part to whole, and of the significance of this part:

I. The Intellectual Roots of Realism

II. Realism and Empirical Social Science
   A. The Yale Experience
   B. The Singular Case of Underhill Moore
   C. The Johns Hopkins Institute of Law

III. Realism and the Politics of Law Reform
   A. Economic Life and the New Deal
   B. The Reform of Federal Procedure
   C. The Belated Reform of Commercial Law

IV. The Implicit Jurisprudence of Realism

V. The Intellectual Legacy of Realism

Because of the vagaries of research this is the first part of the study to be completed.

14. This aspect of Realism is treated inadequately in both of the major reconsiderations of the movement to date: W. Rumble, American Legal Realism (1968); W. Twining, Karl Llewellyn and the Realist Movement (1973). The same is true of G. Tarello, Il Realismo Giuridico Americano (1962).

15. I have stolen the notion of the "scientific ideal" in American thought from David Hollinger who is at work on a book on the growth of that ideal in 20th century America. See D. Hollinger, Morris R. Cohen and the Scientific Ideal (1976).
eral arts or, on occasion, part of one of the social sciences. But these are sub-themes, themes for the reader to pick out largely on his own. It is the main theme to which I wish to turn, that of the short rise and gentle fall of empirical legal research at Yale as a part of the Realist enterprise generally.

I. GETTING A LAW SCHOOL MOVING

It is perhaps ironic that the once sleepy Yale Law School should ever have been in the center of any intellectual movement. It was a marginal enterprise, at best, for nearly the first century of its existence, and, as the story is often told, did not fully join the mainstream of legal education until 1916 when, with the appointment of Thomas W. Swan, it acquired a Harvard Law School graduate as its dean to complement its recent adoption of the case method of teaching. Although the transformation began earlier—Arthur L. Corbin, who began teaching there in 1903, adopted a version of case method teaching soon after, and Wesley N. Hohfeld came in 1914—Swan’s deanship marked the end of Yale’s career as a backwater law school. Starting from a nucleus of these two men and Walter Wheeler Cook, who also arrived in 1916, Swan had by 1923 assembled a faculty that included Corbin and Cook; Edwin M. Borchard, the father of the declaratory judgment; Edward C. Clark, soon to be a respected scholar in real


19. G. Pierson, Yale: The University College 1921-1937, at 259 (1955). This is the standard history of Yale for the period.


property and civil procedure;\textsuperscript{22} Karl N. Llewellyn, later, while at Columbia, the father of the Uniform Commercial Code; Ernest Lorenzen, a noted scholar in the conflict of laws;\textsuperscript{23} Edmund M. Morgan, who became a famous evidence scholar at Harvard;\textsuperscript{24} Edward S. Thurston, later a famous torts scholar at Harvard;\textsuperscript{25} William R. Vance, an authority on insurance law;\textsuperscript{26} and George E. Woodbine, an eminent legal historian.\textsuperscript{27} He had thus created a respectable and even promising law school attached to what was otherwise an unintellectual, overgrown college.\textsuperscript{28}

As a practicing lawyer, Swan was perhaps the perfect person to turn Yale into a copy of Harvard, still a revolutionary thing to do at staid old Yale in 1916. But, although a missionary from Harvard, Swan was neither a scholar\textsuperscript{29} nor an academic, in the sense of having devoted his life to teaching, as was each member of the Yale faculty. Thus, under Swan's guidance Yale had come as far as the Harvard of almost thirty years before.\textsuperscript{30} What remained was for Yale to become thoroughly academic just as had Harvard when it selected Ames, a devoted scholar who had never

\begin{itemize}
\item \textsuperscript{23} b. 1876. Ph.D. 1898, LL.B. 1899, Cornell. Prof. 1903, Maine; prof. 1904, dean 1910, George Washington; prof. 1911, Wisconsin; prof. 1914, Minnesota; prof. 1917, Yale. d. 1951.
\item \textsuperscript{26} b. 1870. A.B. 1892, M.A. 1893, Ph.D. 1895, LL.B. 1897, Washington and Lee. Asst. prof. 1897, prof. 1898, dean 1900, Washington and Lee; prof. 1903, dean 1905, George Washington; prof. 1910, Yale; dean 1912, Minnesota; prof. 1920, Yale. \textit{VANCE ON INSURANCE (1904) and CASES AND OTHER MATERIALS ON THE LAW OF INSURANCE (1914) are his major publications}. d. 1940.
\item \textsuperscript{27} b. 1876. B.A. 1903, Ph.D. 1909, LL.B. 1919, Yale. Instr. (history) 1906, asst. prof. (history) 1912, asst. prof. (law) 1919, assoc. prof. (law) 1924, prof. (history) 1927, Yale. His most important work was editing \textit{BRACON, DELEGIBUT EX CONSUETUDINibus ANGLIDE (trans. BRACON ON THE LAWS AND CUSTOMS OF ENGLAND vols. I & II (1923)). d. 1953.}
\item \textsuperscript{28} For a general understanding of Yale in the period, see G. PIERSON, \textit{supra} note 19. B. KELLEY, \textit{YALE: A HISTORY 376-86 (1974)} indirectly demonstrates how the college still dominated Yale at this time, even though the university had already produced several notable scholars, when he notes how much effort had to be put into improving graduate and professional education. \textit{VEYSEY, THE EMERGENCE OF THE AMERICAN UNIVERSITY (1965) provides further support.}
\item \textsuperscript{29} In the ten years of his deanship, Swan published but one article, and that article was the reprint of a speech. Swan, \textit{Reconstruction and the Legal Profession}, 28 \textit{YALE L.J.} 784 (1919).
practiced, to succeed Langdell. But how Yale would take this last symbolic step and what that step would ultimately mean for legal thought were by no means evident in 1923.

A. A New Dean

If the truth be known, no part of Yale’s future was particularly evident in 1923. The return of Cook from his sojourn at Columbia was heartening, but little else worthy of note was occurring. Some movement might have been detected in the university as a whole, for Yale had recently acquired a new president—James Rowland Angell. Angell, the only president of Yale since the American Revolution who had not been graduated from the College, had been a professor of psychology, dean, and, for a time, acting president at the University of Chicago, and then president of the Carnegie Foundation before moving to New Haven. A scientist by trade—Angell worked in the psychological tradition of Wundt—he attempted from the start to turn Yale into the university it pretended to be. As part of this attempt, he stressed the importance of research, especially scientific research, the need for strong graduate and professional schools, and the desirability of interdisciplinary studies. He was understood to be less than favorably impressed by the law school, or at least its faculty thought so. On the other hand, he was understood to be favorably impressed by Robert Maynard Hutchins, secretary to the Yale Corporation.

31. See A. SUTHERLAND, supra note 30, at 190-91.
32. B. KELLEY, supra note 28, at 370-71, has a nice thumbnail portrait of Angell: G. PIERSON, supra note 19, at 16-19, 508-33, has a more complete but less incisive portrait of the man. There is capsule explanation of Angell’s quite significant contributions in psychology as the leading “functionalist” in L. ZUSNE, NAMES IN THE HISTORY OF PSYCHOLOGY 276 (1975).
33. Interview with Leon Green, June 19, 1975; interview with Robert M. Hutchins, June 20, 1975. The reasons for this displeasure are unclear. Perhaps it was because the langdellian science of law—a rationalist, historical and, to the outside world at least, deductive derivation of principles—was surely not the same as Angell’s own science of experimental psychology. Or it may have been his exposure as president of the Carnegie Foundation to the criticisms of legal education made in the first two reports of the Foundation’s study of the subject. See J. REDLICH, THE COMMON LAW AND THE CASE METHOD (1914); A. REED, TRAINING FOR THE PUBLIC PROFESSION OF LAW (1921). These criticisms are summarized in Stevens, supra note 17, at 405, 441-53 (1971). A hint of Angell’s attitude can be found in his gentle criticism of law schools for ignoring their academic, university responsibilities while pursuing their vocational, professional responsibilities in an address to the Association of American Law Schools, The University and the School of Law, [1927] HANDBOOK OF THE ASSOCIATION OF AMERICAN LAW SCHOOLS 40 [hereinafter cited as A.A.L.S. HANDBOOK, without cross-reference].
Being impressed by Hutchins hardly distinguished Yale's president from many other people at Yale. Hutchins, tall, boyishly good looking, extremely bright and witty, and the son of a religious academic of certifiable liberal credentials, had been hired by the Corporation in January 1923. He had spent the year and one-half following his graduation from the College teaching English and history at a school for "rich juvenile delinquents." The twenty-three year old Hutchins had jumped when the job was offered to him, ostensibly because he was almost the only member of his class "who had gone into education." Why he jumped is less clear because the job, largely that of vice-president for public and alumni affairs and chief administrative officer of the university, came nowhere near to occupying his enormous energy.

Hutchins used some of his spare energy to finish his law degree, begun out of boredom while in his last year as an undergraduate. Taking his courses in the early mornings, late afternoons, and summers, and working for credit as the equivalent of an unpaid research assistant for Charles E. Clark, he finished in two and one-half years. During this time he and Clark became friends,

35. There is no biography of Hutchins. The following facts have been collected from his senior class book, History of the Class of 1921; Who's Who in America (1974); N.Y. Times, May 16, 1977, at 1, col. 1 (Obituary); and an interview with Robert M. Hutchins, June 20, 1975. Hutchins was born June 17, 1899. His father, William J., was a Presbyterian minister who in 1907 became a professor of religion at Oberlin Theological Seminary and in 1920 became president of Berea College, Berea, Kentucky. Hutchins' uncle was a lawyer. Hutchins' college education, begun at Oberlin in 1915, was interrupted when he enlisted in the Army Ambulance Corps in an attempt to square his desire to serve in the war with his essential pacifism. When he returned to college in 1919 he transferred to Yale, from which he was graduated Phi Beta Kappa in 1921.

36. The characterization is that of Hutchins. Interview with Robert M. Hutchins, June 20, 1975.


38. For a description of the job, created because of the general administrative ineptitude of Angell's predecessor and then vacated by its first occupant when he was passed over for the presidency, see B. Kelley, supra note 28, at 324, a description confirmed by Hutchins. Interview with Robert M. Hutchins, June 20, 1975. Since the job required a great amount of public speaking, especially to alumni, Hutchins' achievements as a college orator contributed to Yale's interest in him. Angell wanted Hutchins' services badly enough to go to the trouble of finding a person to take up Hutchins' teaching job so he could start in mid-semester. See James R. Angell to Wilber Cross, Nov. 20, 1922, on file in the James R. Angell presidential papers at Sterling Memorial Library, Yale University [hereinafter cited as Angell papers, without cross-reference].

39. Interview with Robert M. Hutchins, June 20, 1975. Partial confirmation is found in Robert M. Hutchins to James R. Angell, Sept. 30, 1924, on file in the Robert M. Hutchins papers at the Regenstein Library, University of Chicago [hereinafter cited as Hutchins papers, without cross-reference]. The faculty did not particularly approve of this method of earning credits. Once when approving credit for some of his work with Clark, the faculty recommended that Hutchins take "some of the fundamental subjects given in the regular course." Minutes of the Faculty of Yale Law School, Mar. 17, 1924 [hereinafter cited as Yale Minutes, without cross-reference].
and the two jointly published some of Hutchins' research.40 Other faculty members seem to have liked this brash young man too; in 1924 the faculty decided to try him out as a part-time lecturer the following fall.41

It was not long after Hutchins' arrival on the faculty that the law school began to feel his presence. In January 1926, Hutchins and Clark suggested offering honors courses, a proposal designed to institutionalize Hutchins' own legal education.42 A month later the two presented a different kind of proposal: to "perform distinguished public service by assisting in the solution of the most pressing problem in the law by scientific study of all procedure in its functional, comparative, and historical aspects."43 When they found it necessary to justify their proposal, they began by lamenting the low estate to which the administration of the law had fallen, harking back to the criticisms of Bentham and Dickens as well as noting similar complaints of contemporary leaders of the bar.44 Then, after recounting the unsuccessful efforts at procedural reform and detailing contemporary efforts, including those of the American Law Institute, they concluded: "The reformers have failed, we believe, because the necessary basic research has been lacking. . . . We regard facts as the prerequisite of reform."45 Their prescription followed directly from their diagnosis: "We believe that the way to escape from the morass into which law administration has fallen lies through study. This study should be directed to discovering the working in practice of our present rules. It should be correlated with the study of allied subjects outside the law."46 To carry out the necessary studies they proposed to establish an Institute of Procedure, which would examine civil and criminal procedure and evidence.47

41. Yale Minutes, Nov. 13, 1924. Hutchins was appointed Lecturer in Public Service Law and Trade Regulation.
42. Yale Minutes, Jan. 21, 1926. An undated copy of the proposal entitled "Plan for honors courses in the Yale School of Law" is in the Hutchins papers. The proposal was to allow "men of Law Journal rank" to do all or most of their third year in directed research. Id.
43. Yale Minutes, Feb. 25, 1926. An updated, untitled copy of the proposal is in the Hutchins papers. The quotation is from the opening of that document.
44. A Program of Research in the Administration of the Law, App. A, at 1 (n.d. Summer 1926), Hutchins papers. This document is a proposal directed to the General Education Board, a Rockefeller philanthropy, for a grant of approximately $125,000 for a five-year pilot program.
45. Id. at 2.
46. Id. at 3.
47. In each field they proposed studies of the rules in force and the actual operation of the rules, and comparative studies of both. A Program of Research in the Administr-
The source of these ideas of Hutchins and Clark is unclear.\textsuperscript{48} Llewellyn, teaching at Columbia but still dabbling in Yale Law School politics, probably had his hand in their formulation,\textsuperscript{40} but that fact only broadens the question to include his sources also. In one sense the ideas are too commonplace to have clear roots as the reference to Bentham and Dickens might suggest. At least since the 19th century, liberal intellectuals, and especially liberal intellectual lawyers, have argued that the delays and uncertainties in litigation are an evil crucially in need of reform. The lineage of this idea can easily be traced, as it was by Hutchins and Clark,\textsuperscript{50} through such events as the various English and American pleading and practice reforms—the Hillary Rules and the Field Code, for example; Roscoe Pound's St. Paul speech to the American Bar Association on "The Causes of Popular Dissatisfaction with the Administration of Justice";\textsuperscript{51} and the founding of various legal "reform" organizations, such as the American Judicative Society, the American Law Institute, and the National Crime Commission. The plausibility of this notion about the importance of procedural reform and its equation with law reform generally is beside the point;\textsuperscript{52} it was at hand and easily available to teachers of evidence.

\textsuperscript{48} The occasion of their formulation was a train ride by Hutchins and Clark in December 1925. Clark had complained about the school and indicated his desire to leave. Hutchins chided him about leaving without first trying to change anything. Clark agreed and they thereupon drew up plans for the honors courses, the Institute of Procedure, and limitations on enrollment. "We stood absolutely alone on all these matters and were opposed at every step more or less actively by the Dean." Robert M. Hutchins to Samuel H. Fisher (member of Yale Corporation), Feb. 24, 1927, on file in the Samuel H. Fisher papers at the Sterling Memorial Library, Yale University [hereinafter cited as Fisher papers, without cross-reference].

\textsuperscript{49} See Karl N. Llewellyn to Robert M. Hutchins, May 6, 1926, Hutchins papers. See also Robert M. Hutchins to Karl N. Llewellyn, May 17, 1926; Robert M. Hutchins to Karl N. Llewellyn, May 21, 1926, Hutchins papers.

\textsuperscript{50} A Program of Research in the Administration of the Law, \textit{supra} note 44, App. A, at 1-2.

\textsuperscript{51} [1906] \textbf{REPORT OF THE AMERICAN BAR ASSOCIATION} 400.

\textsuperscript{52} The proposition that the reform of procedural law (together with the simplification of substantive law) is law reform, must be counted as one of the stranger notions of the bar. Intoned with monotonous regularity in its poundian mirror image—the administration of justice has brought disrespect upon the law—the proposition is notable for the fact that evidence in its support, beyond the existence of dissatisfaction (also assumed) and the assertion that the procedural deficiencies of interest to the speaker are its cause, is virtually always lacking. Equally notable is the fact that such dissatisfaction with the
and procedure such as Hutchins and Clark. Yet their explicit criticism of the "reform" tradition for centering its attention "on the presentation and adequacy of an ideal system" rather than on "the working in practice of our present rules" and on "progress in allied fields" is a distinct variation on the "reform" tradition of which the proposal was a quite self-conscious part. Here, where roots are an important matter, the proposal offers no insight. Of course, there are echoes of Deweyan ethics, of the investigative commissions that were a part of the strategy of countless progressive reform movements of the 1910's and earlier, and of a general interest in, and perhaps even wonderment at, the scholarly resources of the new university in which the two men found themselves; but evidence of a conscious drawing on any of these sources is lacking. Apparently the sources, such as they were, were "in the air," and the two men just grabbed them.

The faculty eventually adopted both the proposal to offer honors courses and that for the Institute of Procedure, but the real impact of doing so on the activities of the school was no more than marginal, as the short-term fate of the Institute demonstrates. Although willing to approve the proposal, when asked to fund it the Yale Corporation politely said, "No," as did the General Edu-

administration of justice as there may be is always seen as "popular," and not professional. Professor Marc Galanter has brought to my attention an excellent example of this confusion of popular and professional dissatisfaction: the recent conference called by Mr. Chief Justice Burger to commemorate the seventieth anniversary of Pound's address. See 70 F.R.D. 79 (1976).

Although I have no difficulty in saying that this misidentification has allowed slightly left-of-center lawyers to make their work simpler while supporting both "reform" and the economic and political "status quo," see J. AVERBACH, UNEQUAL JUSTICE (1976), the lineage and function of this notion about what constitutes "law reform" is unimportant here. What is important at this point is that the notion was available to Hutchins and Clark and that they took it and used it, but in a different way than it had been used in the past.

55. Id.
56. Tracing the intellectual roots of realist thought is made difficult because the Realists did not usually identify the sources of their ideas. Where such identification is made, as in Llewellyn, The Effect of Legal Institutions upon Economics, 15 Am. Econ. Rev. 665 (1925), the citations tend to be general and to uncritically lump together rather disparate thinkers, as in Llewellyn's list of "Summer, Holmes, Veblen, Commons and Pound."
57. Yale Minutes, Feb. 11, 1926. The proposal was, however, watered down so as to make honors work generally more available. See Hutchins, The Yale Law School in 1928, 2 Conn. B.J. 1, 3-5 (1928).
58. Yale Minutes, Mar. 11, 1926.
59. Minutes of Yale Corporation, June 14, 1926.
cation Board. But the psychological impact of the two proposals and of Hutchins' arrival generally was significant, as can be seen by comparing such a mundane document as the dean's annual report for academic year 1925-1926 with any of Dean Swan's earlier reports. Although earlier reports had done nothing more than note the comings and goings of faculty and students and lament the low estate of the law school's quarters, this time Swan's report suggested that the place was humming, largely with Hutchins' inspired proposals and more importantly with a new sense of purpose: law improvement. For example, after beginning with the "general recognition . . . that the rules of law and the machinery for administering them need simplifying and adaptation to modern conditions," Dean Swan noted that the founding of the American Law Institute and of committees to study defects in criminal law and procedure as well as the attempt of a "sister university" to acquire a large endowment for legal research were "familiar evidences" of the generally acknowledged need for law improvement. He then triumphantly concluded that Yale, too, knew what was generally recognized and was going to do something about this problem: establish an Institute of Procedure.

Dean Swan's report not only shows a change in the mood at Yale that followed Hutchins' arrival but also suggests some of the motivation for the change. The academic wing of the American Law Institute's organizing committee contained all the Harvard heavyweights—Beale, Pound and Williston—as well as the recently arrived Edmund Morgan. Only Corbin represented Yale. The pioneer study of criminal law and procedure, the Cleveland Crime Survey, had been run by Pound and Felix Frankfurter. Harvard had also recently sought an endowment for research in an amount so large that it intimidated even the Columbia Law School.

60. I infer this from the fact that a proposal was submitted, see note 44 supra, and to the best of my knowledge, never funded.

61. Report of the School of Law to the President and Fellows of Yale University, 1925-26, at 110 [hereinafter cited as Report of Dean, without cross-reference].


63. See R. Pound, Criminal Justice in the American City—A Summary (1929). This is one of seven separate studies collected as Criminal Justice in Cleveland (R. Pound & F. Frankfurter eds. 1922). A short account of the project appears in A. Sutherland, supra note 30, at 271-72 (1967).

64. See A. Sutherland, supra note 30, at 262-70. The Columbia Curriculum Study was moved in faculty on April 15, 1926, one day after The New Republic printed an editorial in support of the Harvard endowment drive: Socializing Legal Education, The New Republic 211 (1926) (the style suggests that Felix Frankfurter was the author). See Young B. Smith?, Memorandum for the President (n.d.) on file in the papers of Underhill
Thus, a note of inter-university warfare was hinted at in Dean Swan's announcement, and indeed explicitly found in Hutchins' and Clark's proposal for the Institute of Procedure—a note not surprising in an up-and-coming law school. At the same time the honors work, really the authorization of credit for independent, directed research, was but another step toward improving the quality of legal education at Yale, a step ahead, this time, of Columbia and Harvard.

Fall 1926 brought continued institutional rivalry and efforts at the academization of legal education. This time Hutchins and Clark proposed to abolish the privilege, which Hutchins had exercised, of Yale College men to begin legal studies in their senior year. After approving this proposal the faculty finally adopted an earlier Hutchins and Clark proposal to limit Yale's enrollment, an action designed to improve the quality of the student body and directed squarely at Harvard and Columbia. Then, in

Moore at Sterling Library, Yale University [hereinafter cited as Moore papers, without cross-reference] (explicit acknowledgement of stimulus). This study has now been chronicled four times, Foundation for Research in Legal History, A History of the School of Law, Columbia University 297-303 (1955); W. Twining, supra note 14, at 45-52; Currie, The Materials of Law Study, pt. 2, 8 J. Legal Educ. 1 (1955) (most comprehensive); Stevens, supra note 17, at 471-75; see text accompanying notes 91-93 infra.

65. Undated, untitled copy of proposal for an Institute of Procedure, Hutchins papers. ("Purpose ... 5. To maintain and develop the present leading reputation of Yale in the field of procedure, and in general to establish Yale as the pioneer in the practical, coherent, unified study of the most chaotic and maladjusted branch of the law. (Must be done to counteract Harvard's 6 million ... ."") Karl N. Llewellyn to Robert M. Hutchins, May 6, 1926; Robert M. Hutchins to Karl N. Llewellyn, May 7, 1927, Hutchins papers.

66. Columbia followed suit in 1928 by allowing third year students to enroll in seminars previously reserved for graduate students and, in 1929, by providing for directed research. Foundation for Research in Legal History, supra note 64, at 338. Harvard did the same in 1939. A. Sutherland, supra note 30, at 340-41.

67. Yale Minutes, Nov. 18, 1926.

68. However, the Educational Policy Committee of the Yale Corporation refused to approve this change in policy (Minutes of Committee on Educational Policy, Jan. 8, 1927) and a compromise was struck allowing "properly qualified" seniors in the college to begin law studies. See Hutchins, Connecticut and the Yale Law School, 2 Conn. B.J. 162 (1928) (details terms of admission).

69. Yale Minutes, Dec. 4, 1926. The attempt to limit enrollment dated at least to 1923, when Dean Swan recommended against any limit on the ground that relying too heavily on grades would bring in students of "foreign" rather than "old American parent-age," just the opposite of those Yale should be attracting, with the result that Yale would have an "inferior student body, ethically and socially." Yale Minutes, Dec. 20, 1923. Hutchins and Clark resurrected the proposal in 1926 as part of response to the Harvard endowment drive. They proposed to turn Yale into an "honors or research law school" of 300 students who would be trained "to discover the actual operation of the law rather than to memorize its rules." Yale Minutes, May 6, 1926; Robert M. Hutchins to Karl N. Llewellyn, May 7, 1926, Hutchins papers. Two weeks later the proposal was effectively tabled, Yale Minutes, May 20, 1926; Robert M. Hutchins to Karl N. Llewellyn, May 21, 1926, Hutchins papers. The reason for the change in outcome seems to have been that Corbin, who had been undecided in May, became a supporter of the idea in November,
late December, Dean Swan was appointed to the United States Court of Appeals for the Second Circuit.\textsuperscript{70}

The only real candidates to succeed Swan as dean were Cook, thought by many to be too cantankerous to be an effective dean over any long term, Clark, Thurston, and perhaps Vance, none of whom had sufficient support to achieve the consensus that the faculty desired.\textsuperscript{71} Part of the faculty had, however, reached some agreement; the untenured people announced that they supported only candidates with "the views and policies with which Mr. Cook has been identified."\textsuperscript{72} Sensing that the faculty was deeply divided, Corbin, already its grand old man, proposed that Hutchins, who apparently had already been suggested as associate dean, be appointed acting dean.\textsuperscript{73} The tenured faculty agreed to the nomination and so did the corporation.\textsuperscript{74}

Whether Hutchins was supposed to be a caretaker or whether a permanent compromise had already been struck by the law faculty is unclear. He thought that the truth was somewhere in

\begin{footnotesize}
\textsuperscript{70} See Yale Minutes, Nov. 24, 1926. From the beginning, Hutchins and Llewellyn recognized that Corbin’s support was essential. See Karl N. Llewellyn to Robert M. Hutchins, May 6, 1926; Robert M. Hutchins to Karl N. Llewellyn, May 7, 1926; May 14, 1926, Hutchins papers.

\textsuperscript{71} The limit of 100 students to an entering class was a trifle fraudulent since Yale had not regularly enrolled more than that amount. T. Arnold, Fair Fights and Foul 35 (1965). However, the decision was successful in increasing interest in Yale and thus indirectly in improving the quality of the student body. Interview with Robert M. Hutchins, June 20, 1975.

\textsuperscript{72} See Yale Minutes, Dec. 23, 1926.

\textsuperscript{73} See Karl N. Llewellyn to Robert M. Hutchins, May 6, 1926; Robert M. Hutchins to Karl N. Llewellyn, May 7, 1926; May 14, 1926, Hutchins papers.

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\textsuperscript{74} See Yale Minutes, Dec. 23, 1926.

\textsuperscript{75} See Karl N. Llewellyn to Robert M. Hutchins, May 6, 1926; Robert M. Hutchins to Karl N. Llewellyn, May 7, 1926; May 14, 1926, Hutchins papers. The limit of 100 students to an entering class was a trifle fraudulent since Yale had not regularly enrolled more than that amount. T. Arnold, Fair Fights and Foul 35 (1965). However, the decision was successful in increasing interest in Yale and thus indirectly in improving the quality of the student body. Interview with Robert M. Hutchins, June 20, 1975.

\textsuperscript{76} See Yale Minutes, Dec. 23, 1926.

\textsuperscript{77} See Yale Minutes, Jan. 12, 1927. Although the faculty had sworn to Angell that there was no division of opinion on the questions of educational policy underlying the choice of dean, "except ones of degree and some of details," Yale Minutes, Jan. 6, 1927, a subsequent faculty meeting at which the question of new appointments was discussed had disclosed exactly how divided the faculty was. Yale Minutes, Jan. 19, 1927. Robert M. Hutchins to William J. Hutchins (father), Jan. 18, 1927, Hutchins papers ("The situation . . . is growing more and more complicated and more and more tense."). Hutchins predicted that with "24 of the 25 candidates mentioned" for dean, his becoming assistant "would be certain to prove fatal within 48 hours." The one exception was apparently Cook, whom Hutchins supported. Id. Robert M. Hutchins to Walter W. Cook, Feb. 9, 1927, Hutchins papers ("I wish we had found it possible to put you over because I think the School will require the kind of leadership that only you can supply.").

\textsuperscript{78} Minutes of the Yale Corporation, Feb. 12, 1927. Not that approval of the nomination was a cinch. The Educational Policy Committee of the Corporation approved the nomination by one vote, a tie-breaking vote cast by the Comptroller of the University. Minutes of the Educational Policy Committee of the Yale Corporation, Feb. 12, 1927.
\end{footnotesize}
What is clear however, and what suggests that the first possibility is not the appropriate one, is that with its vote the faculty got a whirlwind. One week later Hutchins created a committee that consisted of himself, Clark and Corbin to schedule classes and prepare the catalog. Two weeks later this committee reported on appointments! And where two weeks before the faculty had debated the relative merits of such standard scholars as Bigelow and Seavey, now it was in succession asked to appoint Richard J. Smith, a recent graduate interested in Public Utilities, Hutchins' own course; Donald Slesinger, a psychologist interested in the law of evidence, another Hutchins course; and Bronislaw Malinowski, for a course in legal anthropology. At the same time the report of a commercial law committee, headed by Wesley Sturges, signaled the onset of curriculum reform just like Columbia's, and this endeavor was urged on with a pep talk given by two of Columbia's more dedicated reformers. Thus, if drive and activity were relevant qualities for a dean, whether Hutchins had been intended as a caretaker or in fact as a compromise candidate, when it came time for the search committee to act it would have been hard for it to select anyone else. Not that the faculty was of a mind to do so, although some of its members were less than ecstatic with Hutchins; at least one prominent legal scholar, John Henry Wigmore, thought the idea preposterous, and promise...
ent alumni favored aged practitioners. Thus, in late May 1927, when the search committee summarized its work, it supported its nominee by stressing the youth of Ames when he was appointed dean at Harvard, the need for a candidate who was both a legal educator and a Yale College graduate, and the desirability of continuing the "scientific" Hohfeldian tradition at Yale. President

evidence of unfitness because devotion to extreme "behaviorism in psychology" like that of "the free silver craze in economics" raised "suspicion of . . . [a lack of] balance," because a "jaunty and witty but irresponsible dismissal of the recorded experiences of lawyers and judges" indicated "an unscientific and unsafe attitude towards the law," and because the attitude underlying the paper "would tend to unsettle the minds of young men in these days when they are already too much inclined to cast away . . . all prior experience." Angell sent a copy of the letter to Hutchins with the following note: "I get the impression that you must have stepped on some of his most sensitive corns." James R. Angell to Robert M. Hutchins, Apr. 8, 1927, Angell papers. No copy of Hutchins' paper seems to have survived. See note 101 supra.

84. Thomas W. Thatcher to James R. Angell, Jan. 28, 1927 (Charles P. Howland, Lansing P. Reed); William Howard Taft to James R. Angell, Jan. 27, 1927 (Morris Hadley), Angell papers.

85. The report, untitled, is found with Yale Minutes, Apr. 13, 1927.

86. Wesley N. Hohfeld had come from Stanford in 1914 after impressing the Yale faculty with a paper entitled Some Fundamental Legal Conceptions as Applied in Judicial Reasoning, 23 YALE L.J. 16 (1913). See W. TWINING, supra note 14, at 34. In Fundamental Legal Conceptions, Hohfeld attempted to show the analytical relationship between the concepts of right, privilege, power, and immunity and their opposites. As even this short description suggests, his work was "scientific" only in the older sense of that word, meaning scholarly or rationally derived. The article was well enough received that Hohfeld was invited to give a paper at the 1914 meeting of the Association of American Law Schools. The product, A Vital School of Jurisprudence and Law, 1914 A.A.L.S. HANDBOOK 76, is an equally rationalist analytic presentation of an ideal school for training jurists, practitioners, and citizens. The popularity of Hohfeldian analysis and the contemporary understanding that it was "scientific" seems quaint today, especially since the analytic system bears some of the hallmarks of what might uncharitably be called crank legal philosophy. The popularity and the scientific appellation might both be explained in part by the intellectual currents of the 1910's in the law schools.

While the law teachers were developing their professional identity, the major intellectual novelty among them was the spread of continental legal learning through the Modern Legal Philosophy series, the Continental Legal History series, and the beginning of comparative law studies. This exposure brought considerable respect, if not envy, for the achievements of German legal academics in rationalizing German customary law before the adoption of the German Civil Code in 1896. Such work had been seen as scientific in Europe and called such by Langdell; a similar scientific role for the new American professionals seemed desirable. Hohfeld's Vital School suggested such a role, id. at 86-96, and the dean at Wisconsin made a similar suggestion in 1915. Richards, A Survey of the Progress in Legal Education, 1915 A.A.L.S. HANDBOOK 60, 75-76. These two items are the immediate roots of the American Law Institute's Restatement Project. See W. TWINING, supra note 14, at 458 n. 19.

At the same time Hohfeld's role in these events should not be overestimated. Analytic schemes, like those presented by Hohfeld, designed to bring system and order to the common law were numerous at the time. See, e.g., Beale, The Necessity for the Study of Legal System, 1914 A.A.L.S. HANDBOOK 31, 42-43. And, projects virtually identical to the Restatement had surfaced earlier. See Alexander, Memorandum in re Corpus Juris, 22 GREEN BAG 59 (1910).

Given that Hohfeld was well known for his system, it is not surprising that when appointing Hutchins, the Yale faculty would advert to the only glorious bit of its then recent past, even though that evocation meant equating Hutchins' science with Hohfeld's
Angell approved the nomination although he cautioned that "it might be desirable to defer making the appointment" until some later date. Hutchins was still a month shy of his twenty-eighth birthday.

In a sense, how Robert Maynard Hutchins became dean of the Yale Law School is neither distinctive nor unusually auspicious. Many a law school feeling itself on the threshold of something special has hired a bright, dynamic young man to be its dean. The sameness of American law schools provides an adequate obituary for most of their high hopes. Yet, under Hutchins something special did develop, if only for a while. Thus, what Yale thought it was doing when it hired its bright, dynamic young man should be remembered, if only as a baseline against which to view the decade to follow during which American Legal Realism meant, in large measure, the Yale Law School.

Yale's deanship search committee stressed three things in its report: Ames and Harvard, legal educator and Yale, and science and Hohfeld. This list is a good clue to the meaning of Hutchins' election to the Yale faculty. Hutchins was the first unpracticed, purely scholar-lawyer dean at Yale just as Ames had been at Harvard, and much of Hutchins' early efforts were directed toward improving the law school's academic program. These efforts were and thus misunderstanding the nature of Hohfeldian science. Curiously, Hohfeld would not have made the same mistake; at one point in *A Vital School*, Hohfeld actually refers to empirical studies and distinguishes them from his own scientific pursuits. 1914 A.A.L.S. HANDBOOK 76, 110.

87. Yale Minutes, May 26, 1927. Angell was extremely cautious in dealing with the nomination, which was made pursuant to a power held by the faculty and not by him or the Corporation. He moved first for Hutchins' appointment as a full professor, then, four months later, for his reappointment as acting dean, and finally, two months after that, for his as dean. Minutes of the Yale Corporation, June 11, 1927; Oct. 8, 1927; Dec. 10, 1927. Each time, the appointment was presented to the Corporation from outside normal bureaucratic channels.

88. Here Hutchins' idea of academic improvement and Swan's should be contrasted. Swan improved Yale by buying established faculty wholesale—in four years he acquired half the faculty at Minnesota: Lorenzen, Morgan, Thurston and Vance—and by promoting good local talent—Clark, Llewellyn, Tulin, and Woodbine. Almost instantaneously Yale was a good law school in the by then traditional model. Hutchins eschewed this mode of development. His suggested appointments were anything but traditional and his projects for academic improvement squarely challenged the traditional model, for they suggested that improvement would come not by doing things better but by doing things differently in both teaching and research. All of which is not to say that Hutchins lacked bridges to the tradition of academic reform in law schools. The proposal to eliminate the ability of Yale College seniors to begin their law studies a year early is directly within the pattern of reform through the raising of formal standards that Stevens, note 17 supra, chronicles so thoroughly. But its companion, the proposal to formally limit the number of students admitted was not, and was contested because it conflicted with that tradition; it threatened to cut out of law school the "C" student, not just the poor immigrants. See note 68 supra; Robert M. Hutchins to Charles E. Clark, May 7, 1926; May 14, 1926; May 21, 1926, Hutchins' papers.
important to the Yale faculty because, given the academic politics of the time, unless it was thoroughly academic Yale could not say that it had become a truly first-class law school, and because being as good as or better than Harvard was part of the inter-university rivalry that accompanied an advance to the front rank. At the same time, the mention of science and Hohfeld served a dual purpose. The only glorious, independent tradition at Yale that might set it apart from Harvard and Columbia was Hohfeldian and “scientific,” and Hutchins’ actions with respect to both the Institute of Procedure and the proposed appointments fit into that tradition. His appointment was thus useful as a part of the existing rivalry and as a statement on the part of a law faculty worried about its degree of support from Yale’s scientist-president James Rowland Angell. The Yale Law School, too, was going to be scientific, whatever that meant, as well as academic. Whether the faculty’s worry was well-founded is unimportant. Given its worry, Hutchins was desirable because he would be doubly attractive to Angell. He was a friend and former employee as well as bearer of an academic, scientific mission that seemed to suit the president’s biases. And thus as a personal, if not political compromise, Hutchins was a hard candidate to top.

Academic respectability, scientific distinctiveness, competitiveness, and a certain notoriety (Hutchins was the youngest member of the Yale law faculty and more than twenty years younger than was Ames when elected dean at Harvard) were all bought by Yale when it nominated Hutchins. Whether it also bought or merely accepted Hutchins’ style of academic leadership is, however, another matter.

B. A Pattern of Activity

The whirlwind did not stop with Robert Hutchins’ formal nomination as dean or with his belated election to that post by the Yale Corporation. Indeed, if anything it grew stronger, for it reflected Hutchins’ personal style.

69. There was another tradition available at Yale, that deriving from Simeon E. Baldwin, professor at Yale from 1872 to 1919, Judge of Connecticut Court of Errors & Appeals from 1893-1910, Governor of Connecticut from 1910-1914, and founder of the American Bar Association. But that tradition was simply unavailable both academically and politically. Baldwin stood for the “Yale method” of instruction that was discarded in the 1910’s when Yale adopted the case method and for conservative politics to the right of even the right wing of the Yale faculty in 1926. See F. Jackson, Simeon Eben Baldwin: Lawyer, Social Scientist, Statesman (1955); Leon Green to John Henry Schlegel, June 4, 1975.

90. See note 87 supra.
A good place to start an examination of Hutchins' style is one of the first areas he turned to upon becoming acting dean: curriculum reform. In spring 1926, responding to internal pressures from Herman Oliphant and Underhill Moore and also to Harvard's drive for an increase in endowment, the Columbia Law School faculty decided to restudy its curriculum in order to rearrange the substance of the law into functional categories "in terms of the human relations dealt with," and, not incidentally, thus to achieve a distinctiveness apart from Harvard. This major undertaking, begun in spring 1927 and actively participated in by the entire faculty, was carried on as a faculty seminar, led by Leon C. Marshall, a personal friend of Oliphant from the University of Chicago. Marshall was an economist and former business school dean. As an expert on business education, he brought to the reform effort an acute understanding of functional organization of a curriculum, a system pioneered in the business schools. Counting

91. On curriculum reform at Columbia, see the sources cited at note 64 supra. Oliphant began agitating for curriculum reform in the fall of 1923. Oliphant, The Revision of the Law School Curriculum, Oct. 29, 1923, Moore papers (memo to the Curriculum Committee?). The quoted material is found at id., at 8. The explicit attempt to be different from Harvard is found at id., at 3-4, 5. See also Young B. Smith ?, Memorandum for the President 2 (n.d.), Moore papers (justifying Marshall's appointment). Oliphant sent this memo to Columbia's President Nicholas Murray Butler and not a long letter as reported in Foundation for Research in Legal History, note 64 supra, at 299. Cf. Herman Oliphant to the Committee on Curriculum, Nov.?, 1923, Moore papers (detailing the response of former Columbia Dean Harlan F. Stone to the same document). For Moore's participation, see Underhill Moore to James C. Bonbright, Apr. 28, 1924; James C. Bonbright to Underhill Moore, Apr. 5, 1924; Huger W. Jervey (Dean) to Underhill Moore and Herman Oliphant, Apr. 2, 1926 (appointment to committee to make arrangements for Marshall's seminar), Moore papers.

92. See Currie, supra note 64, at 1, 9 n.22. Marshall's appointment was justified to Columbia's president only in part by his leadership of the seminar. His appointment was primarily justified as the second step of a general plan to integrate "law and the allied sciences, such as economics and sociology." Thus he was supposed to coordinate the work in commercial law with "underlying economic problems" and "the actual structure and working of . . . business life"—apparently the work he did during the latter half of his one and one-half year appointment. Young B. Smith ?, Memorandum for the President 1, 4 (n.d.), Moore papers. The first step of the plan was said to have been the appointment of Alexander M. Kidd, a teacher of criminal law and criminology at the Law School of the University of California, Berkeley. See Foundation for Research in Legal History, note 64 supra, at 286-87. Kidd came in fall 1926, apparently on a two-year appointment. He left as scheduled. Cf. Alexander M. Kidd to Underhill Moore, June 10, 1929, Moore papers ("I would not have missed my two years there even if I had known in the beginning that it was going to fail at Columbia.").

the time necessary for the production of course materials, the project was sustained for about five years and parts of its revision of the curriculum are still generally accepted fifty years later.

In contrast, no one would call Yale's attempts at curriculum reform either sustained or major. The work of the commercial law group, headed by Wesley Sturges and announced in spring 1927, just three months after the start of the Columbia seminar,94 seems never to have been reported back to the faculty. Yet the following fall similar groups in procedure and criminal law were established.95 While the procedure group, chaired by Charles Clark, easily accomplished its limited business of creating an advanced procedure course out of common law pleading and equity, the criminal law committee—Hutchins and Leon Tulin—managed to come to a faculty meeting essentially unprepared, so that its proposal had to be rescued on the spot by, of all people, Arthur Corbin.96 Then, in spring 1928, Hutchins announced that the curriculum "correlation" was not going to work if current procedures were followed and that he was going to appoint a special curriculum committee consisting of Walton H. Hamilton, Donald Slesinger, Arthur Goodhardt, a visiting professor from Oxford, and Charles P. Howland of Yale's Department of Economics, Sociology and Government.97 Although support for the proposal was registered, apparently a committee of two nonlawyers and two outsiders was too much even for Yale, and the following week the faculty decided to proceed with weekly reports, group by group, course by course, starting with business courses.98 But, after a quick start with reports on agency, an old nonfunctional course, and credit transactions, a new, functional course, the program lagged

Marshall, The Collegiate School of Business at Erewhon, supra at 170 (emphasis added). At Columbia the study was broken up into finance, labor, risk and risk bearing, marketing, form of business unit, law administration, criminal law, family and familial property, legislation, and historical and comparative jurisprudence. Marshall picked up form of business unit as a junior college prerequisite. Id. at 170 n.9. Oliphant had picked up these five divisions in his first memo. Herman Oliphant to Committee on Curriculum, Oct. 29, 1923, Moore papers, as well as law administration, criminal law, and family and familial property.

94. See text accompanying note 91 supra. The commercial law group had been divided up, exactly like that at Columbia: form of business organization, finance, marketing, risk, and personnel. Yale Minutes, Apr. 21, 1927.


96. Yale Minutes, Nov. 10, 1927; Nov. 17, 1927.


98. Yale Minutes, Mar. 22, 1928 (support from Leon Green and Wesley A. Sturges); Mar. 29, 1928.
and with a report on corporations was never heard from again.\textsuperscript{99} In the end, curriculum reform at Yale proceeded in the time-honored way with the acquisition of new faculty members who brought new courses with them and the departure of old faculty members who took their old courses away.

Hutchins' attempt to understand the psychology of the law of evidence is an equally revealing example of his style of leadership. In December 1926, Hutchins, who had just begun teaching evidence for the first time (although he had previously published a rather devastating case note on a crucial evidentiary point in the Sacco-Vanzetti case)\textsuperscript{100} read a paper on evidence at an A.A.L.S. round table in which, according to Wigmore, he took an extreme behaviorist position.\textsuperscript{101} The following spring, Hutchins, apparently feeling that he needed some technical advice on the subject of psychology, brought to Yale Donald Slesinger,\textsuperscript{102} a psychologist with a background in testing, whom Hutchins had discovered while looking for money for another project.\textsuperscript{103} Slesinger tried to talk Hutchins out of the proposition, implicit in his offer of a

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\textsuperscript{99} Yale Minutes, Apr. 12, 1928; Apr. 19, 1928; May 3, 1928.

\textsuperscript{100} Note, Cross-Examination to Impeach, 36 YALE L.J. 384 (1926). This note and other activity on behalf of Sacco and Vanzetti brought forth a protest from Chief Justice Taft. William Howard Taft to James R. Angell, May 1, 1927, Angell papers. (Letter containing the complaint that Felix Frankfurter seems to be “closely in touch with every Bolshevistic communist movement in this country”).

\textsuperscript{101} The topic of the round table was “Psychology, Deception Tests, and the Law of Evidence.” 1926 HANDBOOK AND PROCEEDINGS OF ASSOCIATION OF AMERICAN LAW SCHOOLS 92. Wigmore's characterization comes from John H. Wigmore to James R. Angell, Apr. 1, 1927, Angell papers. From an examination of the article that “grew out of” the paper, Hutchins & Slesinger, Some Observations on the Law of Evidence—Memory, 41 HARV. L. REV. 860 (1928), and from Hutchins & Slesinger, Some Observations on the Law of Evidence—The Competency of Witnesses, 37 YALE L.J. 1017 (1928), I suspect that the paper was not behaviorist in the sense of specifically following the psychology of John B. Watson, but rather in the much looser sense of using experimental studies of the psychology of human perception and intelligence. Cf. Hutchins & Slesinger, Some Observations on the Law of Evidence—Consciousness of Guilt, 77 U. PA. L. REV. 725 (1929) (unusual eclecticism). Thus Hutchins' error was probably more that of disagreeing with Dean Wigmore than anything else. See John H. Wigmore to Robert M. Hutchins, Dec. 9, 1926, Hutchins papers (“As to the empiric psychology of testimonial error . . . the question of material is shown in my Principles of Judicial Proof and in my . . . reply to Professor Munsterberg.”); Robert M. Hutchins to Justin Miller (chairman of round table), Apr. 12, 1926, Hutchins papers (“At the time of Wardman's [sic] reply to Munsterberg the statements of the learned dean of Northwestern were probably correct. I doubt if they are today.”). The relevant cites are H. Munsterberg, ON THE WITNESS STAND (1908); Wigmore, Professor Munsterberg and the Psychology of Testimony, 3 ILL. L. REV. 39 (1908).


\textsuperscript{103} Interview with Donald Slesinger, July 8, 1975. Slesinger, who was working in criminology at the Laura Spellman Rockefeller Memorial, came to Yale in the fall 1927.
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job, that psychology could contribute to an understanding of the law of evidence.\textsuperscript{104} But he came to Yale nonetheless, first on a fellowship, then as a research associate. Hutchins set to work on his case law research; Slesinger, on finding what little in the psychology literature was relevant to the evidentiary problems they had chosen to discuss. Subsequently, Hutchins learned from Charles K. Ogden that Bertrand Russell thought Mortimer Adler, then in the psychology department at Columbia, was doing the best work in philosophy in America and that Adler was interested in the law of evidence.\textsuperscript{105} Hutchins sought out Adler and, after meeting him and his collaborator, Jerome Michael of the Columbia Law School, apparently became concerned that Adler's work with Michael might turn out to be better than his own work with Slesinger.\textsuperscript{106} Hutchins proposed to work jointly with them and secured a grant for the purpose from the Commonwealth Fund.\textsuperscript{107} Little joint work was produced, however, though it is hard to believe that anyone thought such work might have been done given the divergence of the two groups' initial interests.\textsuperscript{108} Hutchins and Slesinger published their series of articles between 1928 and 1929.\textsuperscript{109} Each was an examination and criticism of a classic prob-

\textsuperscript{104} Interview with Donald Slesinger, July 8, 1975.

\textsuperscript{105} Interview with Robert M. Hutchins, June 20, 1975. Several readers have suggested that this story is dubious, at least to the extent of reporting Russell's actual opinion of Adler's work. Although Donald Slesinger could neither confirm nor deny the story, I have no reason to doubt that this was Hutchins' understanding. M. ADLER, PHILOSOPHER AT LARGE 107-08 (1977) while not supportive is consistent. Hutchins would surely have been amused at this hearsay problem.

\textsuperscript{106} Interview with Donald Slesinger, July 8, 1975.

\textsuperscript{107} Report of Dean Hutchins, 1927-28, at 114. See Yale Minutes, Jan. 15, 1928. All of this must have happened in summer and fall 1927. Adler met Hutchins in July, M. ADLER, supra note 105, at 108; Michael did not start at Columbia until fall; Hutchins and Slesinger began publishing their articles noting the collaboration in spring 1928. Adler's story (M. ADLER, supra note 105, at 111) that Hutchins "engaged" Slesinger to work with him after Adler informed Hutchins that he could not come to Yale seems unlikely. See Yale Minutes, Mar. 10, 1927 (Slesinger's appointment).

\textsuperscript{108} From the beginning, Michael and Adler emphasized their interest in logic. Although they talked of doing work like that of Hutchins and Slesinger, they started with a comparative study of the "logical structure of trial and experimental procedure" and hoped "to state the law of evidence as logical doctrine, in geometrical form." CURRENT RESEARCH IN LAW, 1928-29, at 71 (M. Harron ed.). (This short-lived serial, issued by the Johns Hopkins Institute of Law, gives an unusually complete picture of the range of legal research both within and without the law schools.)

lem in the law of evidence—for example, the use of a recorded past recollection to "refresh" present memory or the use of past statements of intention to prove the doing of the act intended—first through a review of the relevant case law and commentary and then through a review of the relevant psychological literature. The articles were of a generally high quality, although their effectiveness varied directly with respect to the quality and relevance of the underlying psychological literature: where good quantitative, behavioral studies were available, the articles were crisp and their criticisms effective;110 where an older, introspective psychology or new freudian psychology provided the studies, the articles tended to be less well focused and their criticisms weak.111 Michael's and Adler's contribution to the enterprise did not appear until 1931.112 And although Hutchins planned "experimental work" to test out the theories set out in the articles and although Slesinger left Yale to follow Hutchins to Chicago in order to do that work, no experimental work was ever done, largely because Hutchins and Adler headed off in wholly different directions.113

Better than by curriculum reform or in the evidence studies, the style of the Hutchins deanship is captured by the story of the founding of the Institute of Human Relations. That story begins not with Hutchins but with a man named Milton C. Winternitz, a "steam engine in pants"114 and another one of President Angell's bright young men.


113. Report of Dean Hutchins, 1928-29, at 8; Interview with Donald Slesinger, July 8, 1975. For Hutchins' later attitude, see The Autobiography of an Ex-Law Student, 1 U. Chi. L. Rev. 511, 513 (1934), a typically overdrawn Hutchins presentation. It is perhaps only curious that Hutchins' turn to the "rational sciences of Ethics and Politics," id. at 517, was in part a return to the Hohfeldian "scientific" tradition so prominently noted when Hutchins was elected dean. See note 86 supra.

114. G. Pierson, supra note 19, at 260 (quoting James R. Angell). Winternitz was born in 1885. He was graduated in 1907 with an M.D. degree from Johns Hopkins where he taught and did research in pathology until going to Yale. When he resigned as dean in 1935, he remained at Yale and he returned to teaching and research. He retired in 1950 and died in 1959. There is a short, uncritical biography of him: C. Winslow, Dean Winternitz and the Yale School of Medicine (1935) (address on Winternitz's resignation as dean).
Winternitz was appointed dean of the Yale Medical School in 1921, just before Angell took office. At that time the school was in desperate straits, if it had not in fact collapsed.\textsuperscript{116} By the time Winternitz resigned fifteen years later, the school was outstanding.\textsuperscript{116} In between, Winternitz faced numerous problems, only one of which is relevant for present purposes: Yale had no psychiatry department.

In the mid-twenties Winternitz and Angell set out to do something about that deficiency. After much study and one false start, they proposed to create an Institute of Human Behavior, bringing "into geographic continuity, probably in the same building, the various sciences associated with behavior, chiefly human behavior."\textsuperscript{117} In the fall of 1927, a proposal was made to one of the Rockefeller philanthropies for a grant of $4.3 million to begin this enterprise.\textsuperscript{118} All the money requested was for the primary benefit of the psychiatry department, except for the office space in the Institute's building needed for other participants—the department of psychology and the Institute of Experimental Psychology, and the department of research in child hygiene.\textsuperscript{119}

For some reason this plan did not excite foundation interest. But rather than cut back the scope of the proposal, during winter 1927-28, Winternitz and his good friend Hutchins created an

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\item[115.] B. Kelley, supra note 28, at 380. Through resignations the school had dropped to an effective faculty of four.
\item[116.] Id. at 380-81.
\item[117.] Report of Dean Winternitz, 1925-26, at 79. In 1925, Angell announced that the necessary funds had been secured. What actually materialized from the Commonwealth Foundation was a small grant for personnel that was used to bring in consultants to get work in psychiatry started. See id. at 85. In 1926, one of the Rockefeller Foundations was approached unsuccessfully. James R. Angell to Wickliff Rose (General Education Board), Oct. 26, 1926, Angell papers (Angell and Hutchins' predecessor as Secretary to the Yale Corporation, Anson Phelps Stokes, sat on the board of this Rockefeller philanthropy).
\item[118.] Id. at 85. In 1927, Angell papers. Real progress was not made until two months later. James R. Angell to George E. Vincent (Rockefeller Foundation), Jan. 5, 1928, Angell papers. The document submitted was apparently a memorandum by Winternitz, dated March 1927, to be found as Appendix A in A Program for an Institute of Human Relations at Yale University, May 20, 1928, Moore papers [hereinafter cited as A Program].
\item[119.] The Institute of Experimental Psychology was established in 1924 with a gift from the Laura Spellman Rockefeller Foundation as part of a plan to separate psychology from the control of the philosophy department, a plan that finally succeeded in 1928. This Institute brought to Yale three famous scholars: Raymond Dodge, a psychologist, Clark Wissler, an anthropologist, and Robert M. Yerkes, the anthropoid psychologist. See James R. Angell to Lawrence K. Frank (Rockefeller Foundation), Nov. 13, 1928, Angell papers.
\item[119.] The department of research in child hygiene was the private preserve of Arnold Gesell, one of the pioneer American scholars in the study of child development. Since 1911 it had been supported by grants from the Laura Spellman Rockefeller Foundation. See A. Gesell, Atlas of Infant Behavior (1934).
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They proposed to establish an "Institute" located across town from the main campus, near the medical school, to house the sciences interested in human behavior, both individual and social, and at the same time to erect a new law school building adjacent to the Institute. What was to hold these two pieces of an idea together was the notion that the three units would form a center for the study of human behavior in both its theoretical (Institute) and applied (law and medicine) aspects.

The major change in the financial details of the proposal for the new "Institute," now renamed the Institute of Human Relations, was the addition of money for research in the social sciences; money for the law school's new building was to come from elsewhere. Thus, under the new proposal the medical school would still get its psychiatry department, but the law school would get only the right to make a claim, one of several, on a fund for research. Moreover, any benefits that would accrue to the law school from the fact of its location in the center would depend on its securing construction of a new building at the appropriate place. All other things being equal, that might have been a relatively easy task; however, all other things were not equal. The law school had already secured a pledge of funds for a new building on a different site. And what would happen to the plan if this

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120. Winternitz and Hutchins had been meeting regularly for lunch at least since spring 1926. See Robert M. Hutchins to Karl Llewellyn, May 4, 1926, Hutchins papers ("The experience of the Medical School points the way that the Law School should go."). Hutchins later said the two men were good friends. Interview with Robert M. Hutchins, June 20, 1975. They corresponded regularly, although only socially, while Hutchins was at the University of Chicago. All available sources suggest that Hutchins and Winternitz thought up the idea. G. Pierson, supra note 19, at 590 n.12, traces the idea back to January 1928. The first general mention of the plan to the law school's faculty is in Yale Minutes, Feb. 20, 1928, a special faculty meeting.


123. Why the Institute's name was changed is mysterious. Hutchins said it was changed because "Human Behavior" had too behaviorist an overtone for Howell Cheney, an influential member of the Corporation. Interview with Robert M. Hutchins, June 20, 1975. In truth Cheney disapproved of the idea generally because it was too behaviorist and favored students who did not intend to practice. Memorandum of Meeting with Howell Cheney, Mar. 7, 1928, Fisher papers. Another participant in the enterprise said the objector was Angell, who had had Watson as a graduate student and disliked behaviorism. Interview with Mark A. May, June 9, 1975. Neither explanation is plausible since both men had passed on the original proposal a year earlier. I think the change probably reflects the addition of law and the social sciences, disciplines in which the relations of individuals are important kinds of behavior.


125. Minutes of Yale Corporation, May 8, 1926 (original site selected); James R. Angell to Howell Cheney, Mar. 5, 1928, Angell papers.
donor refused to permit a change in the site of the new building apparently never crossed anyone's mind.

In March, soon after the project had been thought up, the Institute proposal was presented to the members of the Yale Corporation, who, given the educational ramifications of the plan, had to approve any approach for foundation support. Although this presentation to the Corporation was hopelessly confused, after a month of work Hutchins and Angell won its approval of the proposal. However, dealing with the trustees of the estate of John W. Sterling, who had agreed to finance construction of a new law school building, was not so easy.

Hutchins and Angell asked the trustees to do two things: first, to move the site of the new and badly needed law school to a location near the medical school and the proposed Institute building, and, second, to change the building's architectural style from Gothic to Georgian, to reduce its size, and use the money saved by this reduction to endow several research professorships in the law school. The trustees requested an opinion from their counsel, John A. Garver. Mr. Garver was unalterably opposed to the idea, as he knew Mr. Sterling, "a hard-headed, practical business lawyer," would have been. Among his reasons were the

126. Minutes of the Yale Corporation, Mar. 10, 1928. This was before either the provost (chief academic officer) of the university or the dean of the graduate school had even had a chance to formally comment on the idea. Charles Seymour (provost) to James R. Angell, Mar. 15, 1928; Wilbur Cross (dean) to James R. Angell, Mar. 15, 1928, Angell papers. Yale Minutes, Feb. 23, 1928, report Hutchins' first meeting with these two officers. Hutchins had first broached the subject to the law faculty at a special meeting three days earlier, Yale Minutes, Feb. 20, 1928, a meeting held, in all likelihood, so that the law faculty would not be the last to know.

127. For an example of the confusion, see Francis Parsons (member of Yale Corporation) to James R. Angell, Mar. 12, 1928; James R. Angell to Francis Parsons, Mar. 16, 1928, Angell papers. Angell calmed Corporation members and Hutchins drummed up outside support. See Robert M. Hutchins to James R. Angell, Apr. 3, 1928, Angell papers (enclosing letter from Benjamin N. Cardozo); Yale Minutes, Apr. 27, 1928 (Taft and Hughes). The Corporation approved the plan in March. Minutes of the Yale Corporation, Mar. 14, 1928.

128. Sterling, who had died in 1918, was a graduate of Yale College, B.A. 1864. He was the co-founder of Shearman & Sterling, the New York law firm. Capsule portraits of Sterling can be found in W. Earle, Mr. Shearman and Mr. Sterling and How They Grew 54-57 (1963), G. Martin, Causes and Conflicts 194-95 (1970), and 19 Nat'l Cyclopaedia Am. Biog. 36 (1926).

129. Yale Minutes, Mar. 20, 1928; A Program, supra note 118, App. B, at 21 (statement by Hutchins); Memorandum of statement made to George H. Church by James R. Angell on behalf of the Corporation of Yale University, Mar. 22, 1928, Fisher papers.

130. Garver was a partner of the decedent. A biography of Garver can be found in 39 Nat'l Cyclopaedia Am. Biog. 429 (1959).

131. James R. Angell to Samuel H. Fisher, Mar. 23, 1928, Fisher papers; John A. Garver to George H. Church & Farmers Loan & Trust Co., Apr. 3, 1928, Angell papers (Mr. Sterling "had an ill-concealed impatience of theoretical research as bearing upon the
usual opposition of the average lawyer to anything smelling of social science. But behind his stated reasons was another: he and the trustees had planned a three-building memorial to the decedent at one edge of the campus. If the site of the law school were moved, that memorial would have to be scrapped. Mr. Garver was not about to scrap it, especially to make way for social science. Moreover, memorial aside, he plainly felt that the suggestion to erect a less expensive building in order to save money to endow research that neither the decedent nor his partner would approve of was simply out of the question, especially when the decedent's estate had more than doubled under the careful tutelage of the estate's counsel and could easily supply money for such research should the trustees so choose. Following the advice of counsel on this obscure question of the testator's intent, the Sterling trustees indicated their intention to deny Hutchins' requests, although they agreed to postpone any decision in order to allow an influential member of the Corporation a chance to educate Mr. Garver.

Despite the negative response of the Sterling trustees, a request for $7.8 million was submitted to the Rockefeller Foundation. The grant application consisted of a four page summary of two documents that followed: Winternitz's original proposal for the Institute of Human Behavior and a new proposal, written by Hutchins, explaining the law school's participation in the new, joint enterprise. This summary, written by President Angell, emphasized cooperative investigations into human relations and

activities of the ordinary legal practitioner.

132. Otto Bannard (member of the Yale Corporation) to James R. Angell, Apr. 19, 1928, Angell papers (report of meeting with Garver and trustees). At this location a "T" intersection made it possible to erect a little triptych of modern "Gothic piles" with an appropriately impressive tower rising at the cross of the "T" to provide a real focus for the piece. Construction of one part of this memorial, the library, had already begun; the architectural plans for the second part, the law school, were virtually complete; and the trustees, who for some reason were incapable of doing two things at once, were about to consider the third part of the monument, a home for the graduate school.


134. James R. Angell to John V. Farwell (member of the Yale Corporation), May 9, 1928, Angell papers.

135. James R. Angell to George E. Vincent (Rockefeller Foundation), May 28, 1928, Angell papers (enclosing A Program, supra note 118). The $3.8 million increase in the size of the grant requested is deceiving. Only $1 million of that was for endowment to support the social science programs. The balance was for endowment of existing programs and a modest increase of $250,000 to cover the cost of a slightly larger building.
painted a novel picture of an Institute housing psychiatry, psychology, child development, and "the social science group" as a literal connecting link between the medical school "studying the applied phases of individual behavior" and the law school "studying the applied phases of social behavior." Yet exactly what cooperative investigations were planned and how, if at all, law school or medical school personnel would participate in these investigations was unspecified. And nowhere was there any indication that the Sterling trustees were not inclined to help the project along. Hutchins' statement on behalf of the law school made up for neither of these deficiencies. He began by emphasizing the school's recent actions "tending toward studies in the social sciences." Then he noted that by contributing to the education of the law school's faculty and by getting away from "departmental organization," the planned Institute would overcome the twin difficulties of "bringing in as law teachers" more than a few persons "trained in the social sciences" and of creating "effective cooperation" with the department of economics, sociology and government. He never managed, however, to articulate the relation of these rather limited problems of the law school to its wholly contingent contributions to the plan, much less the relation of either to Angell's fleeting reference to the possibility of cooperative investigations of human behavior, or even to the original psychiatry/psychology side of the Institute.

In fall 1928, the Rockefeller Foundation began to show some interest in the project. President Angell worked to answer the Foundation's questions, while Hutchins and Winternitz attempted to shore up their position on the location of the law school by securing the agreement of the deans of the graduate and divinity schools for the establishment of a graduate/professional center in the area around the medical school, the Institute, and the proposed new law school building. Some tentative support for this idea

136. A Program, supra note 118, at 1. The Yale Corporation's resolution approving the Institute had envisioned "psychology in the broadest sense of the term" as the connecting link. Minutes of the Yale Corporation, Apr. 14, 1928.
137. A Program, supra note 118, at 16.
138. Id. at 18. Both Provost Seymour and Dean Cross had earlier cautioned that establishing the Institute would create problems with the department of economics, sociology and government, an antique creation of William Graham Sumner. See letters cited note 126 supra.
140. Angell had given Vincent a memo restating the plan for the Institute. See untitled, undated memo beginning "More than a year has elapsed . . . ", Angell papers. Angell also helped Hutchins deal with law alumni who did not favor the idea. See e.g
was garnered, but, in early December, the Sterling trustees formally indicated their refusal to consider changing the site of the school.\textsuperscript{141} The law faculty protested the trustees' decision, and its dean petulantly demanded that the trustees provide him with the endowment that the school would not now receive.\textsuperscript{142} The Corporation bowed to the trustees' wishes.\textsuperscript{143} In the meantime, before anyone could revise the plan for the Institute, now that it was clear that the law school would be over half a mile away from it and lack the resources that were to have provided for participation in the Institute's activities, the Rockefeller Foundation, responding to a new and concise formulation of the plan, prepared by President Angell,\textsuperscript{144} began the new year by contributing $4.5 million to found the Institute, a sum that included extra funds for the social sciences.\textsuperscript{145} Thereafter little, if anything, was heard of the plan for the graduate/professional center.\textsuperscript{146}

Years later when asked about his tenure as dean, Hutchins dismissed it as nothing special, as simply a part of the activities of

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\item James R. Angell to Charles H. Sherrill, Nov. 26, 1928, Angell papers. On the attempt to secure the support and participation of the graduate and divinity schools, see E. Furniss, \textit{The Graduate School of Yale 55} (1966); Charles Seymour to James R. Angell, Oct. 26, 1928 (reporting meeting at divinity school at which "Winternitz and Hutchins spoke with their usual persuasiveness"); James R. Angell to George E. Vincent, Oct. 24, 1928, Angell papers.
\item Minutes of the Yale Corporation, Dec. 8, 1928, reporting letter of Dec. 6, 1928.
\item Minutes of the Yale Corporation, Jan. 12, 1929.
\item For Angell's restatement, see untitled, undated memo, \textit{supra} note 140. He emphasized an intention "to direct a comprehensive, well articulated attack on human behavior," yet the document still did not hide the fact that the law school and the social sciences were largely the tail on the dog.
\item The Rockefeller Foundation knew of the decision of the Sterling trustees before it made the grant. \textit{See} Robert M. Hutchins to Samuel H. Fisher, Dec. 12, 1928, Fisher papers.
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The purpose of the Rockefeller gift of Jan. 3, 1929, was to support "a concerted effort to bridge the gap which heretofore has separated the consideration of the intellectual, emotional and personality aspects of the individual from the purely medical on the one side and from the social on the other." A copy of the resolution accompanying the gift may be found in the Records of the Institute of Human Relations, Sterling Library, Yale University [hereinafter cited as IHR files, without cross-reference].

The foregoing discussion is an alteration of the generally accepted understanding of the establishment of the Institute of Human Relations given by B. Kelley, \textit{supra} note 28, at 381-82, and presented to me by almost all of my interviewees. This understanding ignores the roots of the Institute proposal in the search for a psychiatry department and the previous action by the Sterling trustees and sees the participation of the graduate and divinity schools as central to the plan. By thus compressing the events these accounts fail to see the crucial, though opportunistic, role Hutchins played, and they render the law school's relation to the resulting entity (\textit{see} text accompanying notes 445-519 \textit{infra}) quite unintelligible. Hutchins, who recounted the standard story in his interview, came close to getting the story right as far as the psychiatry department was concerned in Hutchins, \textit{An Institute of Human Relations}, 35 Am. J. Soc. 187 (1929).
a group of men trying to make sense out of courses, out of law, and out of a professional school's connection to a university that "had to" amount to more than simply "sharing the same heating plant." Putting aside the question of how "special" the tenure was, the observation captures the substance of Hutchins' activities at Yale. Whether the subject was honors courses or evidence exams, appointments or programs, his aim seems to have been the same: to make the professional study of law more rigorous and more acceptable as a part of the academic university. In that effort he was meeting one of the needs of the law school as it had articulated them when he was named its dean. Similarly, both in his own research and in his grander plans for the law school, there was a continuing attempt to explore the resources of the university—particularly in the social sciences—in an effort to learn what they might offer to the study of law and thus to learn if they might provide permanent links between the university and the professional law school. This activity also fit the law school's perception of its needs. But however well Hutchins' summary captures the substance of his deanship, it captures nothing of its style, except perhaps in its striking of a lingering grace note of boyish irreverence.

In considering Hutchins' career at the law school one can almost hear him yell, "Do something!" And his style reflects that command. The pace was frenetic as he constantly pushed, jostled, and probed both law in general and legal education in particular for ways to make them better, more sensible, more reputably a subject of academic inquiry. At times, as in curriculum reform toward which he made three starts in little over a year, the style could verge on a kind of educational guerrilla warfare. Then, movement, keeping the enemy—old, tired ways of thinking and teaching—off balance, became more important than the careful planning that may be essential to any success at the endeavor, if the Columbia experience may be taken as a guide. But guerrilla warfare or not, at the very least the style put a premium on starting and little on following through, on creating opportunities but not on working with the opportunities created, on coming up with

147. Interview with Robert M. Hutchins, June 20, 1975.
148. Hutchins once proposed to give a true-false mid-term exam in his course in evidence and to drop anyone who could not attain a score of 55 or better. Yale Minutes, Dec. 9, 1926.
149. Which is not to say that Hutchins invariably knew an opportunity when he was presented with one. A. A. Berle came to Hutchins seeking an academic connection with
ideas but not on working them through. Thus the evidence studies, which were a good idea, were dropped when it became apparent that they would require years of primary research with little immediate impact and that something more interesting, namely Adler's ideas on education, had come along. All of which is not to belittle the ultimate importance of that choice for Hutchins' later career. Knowing when to cut losses is an art too seldom found in academia, and Hutchins' subsequent collaboration with Adler was provocative. Nor is it to suggest that this aspect of the style was peculiar to Hutchins; on the contrary, many of the Realists shared it. But for a dean, especially one as persuasive as Hutchins, the style carried with it the risk that those left behind to work out the details would find that the pieces of Hutchins' abandoned projects made up something no one would have wanted in the first place. Even worse was the risk that the heirs to these projects would lack both the ability to create opportunities from those pieces and the good sense to know when and how to exercise the art of cutting losses.

Here the story of the founding of the Institute of Human Relations is instructive. The opportunity was extraordinary. Hutchins was convinced that academic improvement in the law school was hindered by two things: the "mephitic" atmosphere of Yale College and the lack of a social science perspective to the study of law. The first was a conclusion he had drawn from his personal experience at Yale as both student and administrator; the second was more a matter of faith. The obvious way to eliminate both hindrances was to get away from the college and get access to resources. The Institute plan reflects precisely these two objectives—move the site of the law school, and use the money saved in the process as endowment with which to buy resources. However, by just listing Hutchins' objectives it becomes plain that

Yale in order to fulfill one of the conditions on a grant Berle had received from the Social Science Research Council to support work on a study of corporation law. Hutchins said no, so Berle took his money to the Columbia Law School instead. The product of the research was A. BERLE & G. MEANS, THE MODERN CORPORATION AND PRIVATE PROPERTY (1932). See NAVIGATING THE RAPlms 1918-1971, at 20-21 (B. Berle & T. Jacobs eds. 1973).


151. Most notably Thurman Arnold, but also to a lesser extent William O. Douglas and Karl N. Llewellyn. I do not mean to suggest that Hutchins was their model for I doubt whether that is the case.

152. See James R. Angell to Howell Cheney, Mar. 5, 1928, Angell papers (quoting Hutchins). The Yale Corporation agreed that the college atmosphere was at least distracting. Minutes of the Yale Corporation, Apr. 14, 1928.
as an entity for cooperative research the Institute was little more than a vaguely appropriate opportunity that Hutchins seized as it was passing by. That opportunity was lost when the Sterling trustees would not be moved from their initial resolve. Yet, in seizing the opportunity, Hutchins so changed the Institute that what remained was a queer derelict, professing concern for interdisciplinary research over a broad range of individual and group behavior, but consisting largely of a collection of existing or proposed highly specialized research projects. As for the law school, after a year of trying to garner resources Hutchins had merely created a different opportunity, this time one not for employing social scientists but for financing such social science research as otherwise employed members of the law faculty would propose to do. Had the two social scientists—Walton H. Hamilton and Walter F. Dodd—that Hutchins managed to get appointed to the faculty eagerly taken the lead in using these funds, the task of realizing on this opportunity might have been simple. But it turned out neither was interested. So that task, just as so many others that the thirty-year-old Hutchins left behind when he left Yale to become President of the University of Chicago, fell largely into the hands of Hutchins’ good friend and teacher, Charles E. Clark.

II. KEEPING A LAW SCHOOL MOVING

In April 1929, Robert Hutchins turned over the deanship to a somewhat surprised Charles E. Clark, in what was surely the quickest changing of the guard any major law school has ever seen. Clark, ten years Hutchins’ senior and an honors graduate of both Yale College and Yale Law School, had been teaching at

153. One of the two, Walter F. Dodd, a political scientist from Chicago, came from his practice in Chicago in spring 1927. Two and one half years later he left to begin a study of the administration of workmen’s compensation statutes under a grant from the Commonwealth Fund. The resulting work, W. DODD, ADMINISTRATION OF WORKMEN’S COMPENSATION (1936), was one of the “interview the experts” variety. While completing it Dodd returned to practice and never taught again.

The other, Walton Hale Hamilton, a prominent institutional economist who had taught in the field since 1910, came to Yale from the Brookings Institution in 1928. For reasons that are unclear he did no work in economics from the time of his appointment to the Yale faculty until about 1940, and he actively opposed empirical research in law. Interview with Dorothy Swaine Thomas, June 3, 1975.

154. Clark was elected the day after Hutchins announced his resignation. Yale minutes, Apr. 25, 1929; Apr. 26, 1929. Hutchins’ announcement was not a total surprise; the corridors had been full of gossip for several weeks. Interview with Leon Green, June 19, 1975.
Yale since 1919. His fields were procedure and property. Although by no means the whirlwind, either personal or intellectual, that his predecessor was, Clark, a collaborator on the reforms at the law school and a supporter of Hutchins' activities generally, willingly assumed the task of picking up where Hutchins had left off.

The law school inherited by Clark was, in Hutchins' words, in "the best condition in its history" and one of the best and liveliest in the country by any standard. It was a law school significantly changed from the one in which Hutchins had begun his teaching career only four years before and yet, at the same time, one remarkably the same. Clark captured both the change and the sameness rather well when, in his first annual report, he noted:

The School of Law is now a complex and colorful organization. It is still possible for a student to obtain at it a more or less orthodox legal education. The activities of the school are, however, so many and so varied that he is not likely to choose such a course.

The balance of the report was replete with similar assertions that Yale had arrived, and in style. Yet the past of the School of Law was also there in the continuing concern of the parvenu for both "being there" and being out of the shadow of those who got "there" first. Clark noted that at Yale the standard of professional training was as high as that at Harvard, and suggested as well that Yale had moved beyond the Harvard standard by having "begun to study the law anew in the light of [changes in] modern life."

The new note that Clark's report struck—a note of having arrived—is, given the evident continuity of his report, somewhat difficult to account for. Nothing magical happened with the change in dean, or even during the four years that Hutchins was on the faculty. True, Hutchins had added three important new faculty members—William O. Douglas, Walton H. Hamilton, and Underhill Moore—and he had begun all sorts of important projects; but Hutchins' reports did not express either the confidence or the sense of having arrived that Clark's did. Yet, Clark made only one important senior faculty addition—Thurman Arnold—and began only one important research project that did not have its roots in the Hutchins' deanship. However, concentration on events—the

157. Id. at 4.
comings and goings of people, the beginnings and endings of projects—can obscure the cumulative effect of those events on their participants and does obscure the effect of such events during the Hutchins' years on the "feelings" of the Yale faculty.

Objectively, Hutchins built rather modestly; indeed, he did much starting and little building; but the cumulative effect of his limited accomplishments in pushing his school in a particular direction made the actual growth in the institution seem enormous to the faculty. With this sense of growth came the development of notions of what made Yale special—that is, different from Harvard and, to a lesser extent, from Columbia—and what made the faculty's enterprise a common one. Only one of these notions is important for present purposes: Yale was special because of the interest of its faculty in empirical research.

The idea that an interest in empirical research made the Yale Law School distinctive and its faculty's enterprise common becomes fully formed about the time of the change in deans, and its development can be traced quite accurately in the dean's annual reports. When adverting to the proposed Institute of Procedure, Swan's last report spoke of the Yale faculty's commitment to what in late 19th century style was interchangeably described as "legal research" and "scientific investigation." A year later, Hutchins' first report, when describing the first projects to be undertaken as an outgrowth of the procedure proposal, spoke of the faculty's having undertaken an "experiment" designed to learn "how the rules of law are working." With the arrival of the first results from the procedure work and the beginning of the work in psychology and evidence, Hutchins' report moved past "experiments" and discussed the expectation of learning "how the judicial system actually works [and] how it is affecting the community" and the hope of learning how that system "may be altered to attain more readily the objects for which it has been developed." Finally, Hutchins' third and last report, when announcing the funding of the Institute of Human Relations, spoke of the Institute as an expression of the faculty's need "to make clear the part of the law in the prediction and control of behavior." Thus, when Dean Clark capped the opening of his first annual report with a rhetorical question and answer—"Has not law as the expression of com-

munity standards of social conduct actually felt and reacted to the impact of . . . forces [of change]? This the School has now set itself to the task of ascertaining." 162—he was both summing up an old trend and establishing a new base line. Whether they recognized it at the time, Hutchins had brought his faculty (and himself) from the glimmerings of a naive empiricism, through the general but largely unfulfilled research program of Pound's "sociological jurisprudence," and into the mainstream of the empirical social science of the time, and Clark's statement was strong evidence of that change in consciousness.

Of course, the point can be overdrawn. A few faculty members were not interested in empirical legal research, 163 and others who were interested, or at least sympathetic, were not interested enough to try to do some. 164 And the old ways surely received their due; the first occupant of the Sterling Research Professorship—a year at full salary, free of classes—was Corbin, who worked on his Treatise. 165 But the dominant elements of the faculty, and more importantly most of those faculty members who are usually considered to have been Realists, talked and acted as if empirical legal research was one of the things that made Yale a special place. Here Clark's actions are truly symbolic. He began his first report


163. Of the faculty Clark inherited, Walton H. Hamilton is the most notable; he actually opposed doing such research. Interview with Dorothy Swaine Thomas, June 3, 1975. Arthur Corbin should probably be counted in this group too; see Karl N. Llewellyn to Robert M. Hutchins, May 6, 1936, Hutchins papers; and surely Ernest G. Lorenzen and William R. Vance. Alexander Hamilton Frey was skeptical of social science generally, see Frey, Some Thoughts on Law Teaching and the Social Sciences, 82 U. PA. L. Rev. 405, 409 (1934), although he once held a Social Science Research Council fellowship to study the economic consequences of no-par and nonvoting shares.

164. George Dession (Interview with Dorothy Swaine Thomas, June 3, 1975; Interview with Emma Corstvet Llewellyn, Aug. 19, 1975), Walter F. Dodd (Yale Minutes, May 19, 1927; May 31, 1927), Leon Green (Yale Minutes, Nov. 17, 1927), Richard Joyce Smith (Interview with Richard Joyce Smith, May 17, 1976), and Wesley A. Sturges (Interview with Robert M. Hutchins, June 20, 1975). Difficult to catalog are (1) Edwin M. Borchard, who did something close to empirical research in his book Convicting the Innocent (1932), but who nonetheless seems to have opposed the doing of such research by others, Yale Minutes, May 31, 1927; (2) Roscoe Turner Steffen, who did a little bit, see Turner, A Factual Analysis of Certain Amendments to the N.I.L., 38 Yale L.J. 1047 (1928) (opinion poll), but opposed developments at Yale generally, Interview with Leon Green, June 19, 1975; cf. Charles E. Clark to Roscoe T. Steffen, June 18, 1947, on file in Charles E. Clark papers, Sterling Law Library, Yale University [hereinafter cited as Clark papers, Law, without cross-reference]; and (3) Leon Tulin who left for Columbia in 1929 after three years at Yale and died a few years later. I have been unable to learn anything about the attitudes of Roger S. Foster, Ashbel Gulliver and Walter Nelles.

on the research activities of his faculty as no dean before or since; he divided all research into field research and library research, and then showed where his heart was with loving descriptions of the field research and perfunctory treatment of the library research.

Given the law school's understanding of what made it special, Clark's job as dean was to keep his school moving in directions both he and it approved of. At the outset of his tenure that task may have seemed easy; as things turned out, it was not. The projects that Clark picked up from Hutchins turned out to be difficult to complete and the opportunities he inherited turned out to be difficult to capitalize on. To understand why that was so and thus why empirical research died out at Yale, one must examine four topics: studies in procedure, business failures and auto accidents, the Institute of Human Behavior, and the bar survey.

A. Studies in Procedure

Of the projects that Clark had to pick up, the one he was most familiar with was the research in procedure whose conception dated back to the original proposal for an Institute of Procedure that he and Hutchins had made in February 1926. The Institute proposal had remained unfunded for over a year until, on the same day that he was elected dean by the law faculty, Hutchins, with the help of two new faculty members, secured the money for Clark to hire four research assistants and thus begin the work of the projected Institute. Although one faculty member objected to the decision to fund Clark's project on the ground, not particularly inaccurate, that no specific plan of research was proposed, in

167. Clark had a coherent idea of field research that was coextensive with the contemporary notion of empirical research. In his list he included all the projects then underway that are described in this article, as well as the work of Underhill Moore. He excluded the one “sexy” project that was purely work at assembling already published sources, Clark & Douglas, Law and Legal Institutions, in Research Committee on Social Trends, Inc., Social Trends in the United States (1933), a report generally discussed in Karl, Presidential Planning and Social Science Research: Mr. Hoover’s Experts, 3 Persp. Am. Hist. 347 (1969). His inclusion in the class of “field” research of a study that when published turned out to consist largely of a review of secondary literature and expert interviews—W. Dodd, Administration of Workmen’s Compensation (1926)—can perhaps be explained as a lack of knowledge, since Dodd was in Chicago on leave of absence when he did the research.
168. See text accompanying notes 43-60 supra.
169. Yale Minutes, May 31, 1927. The new faculty members were Leon Green and Walter F. Dodd.
due course precise plans were formulated.\textsuperscript{170} The work was to follow generally from the Pound-Frankfurter Cleveland Crime Survey, adapting the techniques first developed there—largely a census of a year or more's caseload—to civil suits in state trial courts.\textsuperscript{171} At the time, Hutchins described the endeavor with remarkable candor and without the sense of excitement one might have expected at the outset of such a new enterprise:

The distinction between law in action and law on the books, and the great relative importance of the former, have frequently been emphasized. Little work has been done, however, which gives any indication of how practicable investigation into operation of legal rules may be. An experiment in this type of research has therefore been initiated.\textsuperscript{172}

The actual project description, probably written by Clark, was more positive when it proposed "to take to the field in Connecticut in the effort to discover how the administration of justice is working. . . . The actual effect of procedural devices on the progress of litigation will be studied in detail."\textsuperscript{173}

Clark must have "taken" to the field with some relish, or at least put his research assistants right to work and worked them hard, for in July 1928, less than a year after the assistants were hired, he published his initial findings, based on but five months of work.\textsuperscript{174} The scope of the research on which the article was based was comparatively small—one or two years of cases in the upper trial courts of the three largest counties in the state and three years of federal district court cases, a total of about 9,300 cases—but the amount of effort must have been prodigious. No

\textsuperscript{170} The objector was Edwin M. Boxchard. Yale Minutes, May 31, 1927. He had presented a competing research proposal, as had both Green and Dodd. Yale Minutes, May 26, 1927.

\textsuperscript{171} For an explanation of the original methodology of the Cleveland Crime Survey, see A. Bettman, Prosecution (1921). Little is known about this singular example of Pound's sociological jurisprudence in action. Pound's biographer tells just enough to suggest that the topic needs exploration. See D. Wigdor, Roscoe Pound: Philosopher of Law 242-45 (1974). Frankfurter's biographer barely mentions the project. Lash, A Brahmin of the Law: A Biographical Essay in From the Diaries of Felix Frankfurter 32 (J. Lash ed. 1975).

\textsuperscript{172} Report of Acting Dean Hutchins, 1926-27, at 118-19.

\textsuperscript{173} Id. at 119.

\textsuperscript{174} Clark, Fact Research in Law Administration, 2 Conn. B.J. 211 (1928), 1 Miss. L.J. 324 (1929). Clark milked his study for all it was worth; he also published what was substantially the same article as Clark, Some of the Facts of Law Administration in Connecticut, 3 Conn. B.J. 161 (1929); Clark, New Types of Legal Research, 1 N.Y. St. B. Ass'n Bull. 394 (1929); Clark, Methods of Legal Reform, 36 W. Va. L.Q. 106 (1930); Clark & King, Statistical Method in Legal Research, 5 Yale Sci. Mag. 15 (1930). A preliminary version appeared as Clark, An Experiment in Studying the Business of Courts of a State, 18 A.B.A.J. 318 (1928).
sampling techniques were used, and all the counting was done by hand.\textsuperscript{175} Considering the relatively primitive technique, it is not surprising that the results were not elaborate. Following the Cleveland model, Clark created a simple table showing disposition by type of action and also took a stab at determining the frequency of the use of jury trial, prejudgment attachment, and various dilatory pleas. He was somewhat tentative in his interpretation of the data. Examination of the data suggested that, given the preponderance of uncontested divorces and foreclosures, settled automobile negligence claims, and simple debt collections, the largely administrative nature of state court civil litigation had already emerged in urban Connecticut by 1925.\textsuperscript{176} Clark was content to state the findings specifically and wonder about the appropriateness of using complex judicial machinery to resolve such apparently simple disputes.\textsuperscript{177} He also presented evidence that jury trials were infrequent,\textsuperscript{178} that prejudgment attachment, especially of large sums, was an effective way to promote settlement of contract disputes,\textsuperscript{179} and that most often motions directed at the pleadings in a case were effective only as a delaying tactic, but all without much comment.\textsuperscript{180} However, he was less cautious in describing his understanding of the value of his enterprise:

\begin{quote}
It is believed, and experience so far shows, that this, although almost a virgin field to the social scientist, is one of the most fruitful for this type of investigation.
\end{quote}

These records are capable of use for at least two important purposes. They may be used to illustrate and to test the efficacy of our rules of procedure and our general methods of administering justice. And they may be used, second, as starting points for the

\begin{footnotes}
\item[175] \textsc{C. Clark & H. Shulman, Law Administration in Connecticut} 4, 206 (1937) [hereinafter cited as \textit{Law Administration}]. However, it is not clear that sampling techniques would even have been known to Clark or anyone he might have talked with about the design of the study. \textsc{Stepan, History of the Uses of Modern Sampling Procedures, 43 Statistical A.J.} 12, 18, 23 (1948), suggests that although isolated examples of what might be called sampling occurred in studies as early as 1914, modern techniques were not generally used in the United States until after 1932 when they were disseminated to the academic community as a part of the New Deal social programs. \textit{But see text accompanying note 204 infra.}

\item[176] \textsc{Clark, Fact Research in Law Administration, 2 Conn. B.J.} 211, 218-19.

\item[177] \textit{Id.} at 213.

\item[178] \textit{Id.} at 224-27. The data also showed that verdicts in jury trials were more often favorable to defendants than was the case in bench trials and that verdicts in jury trials were more likely to be appealed.

\item[179] \textit{Id.} at 227-30.

\item[180] \textit{Id.} at 230-33.
\end{footnotes}
further detailed investigation of social problems of many and
varied kinds.\textsuperscript{181}

And his conclusion was enthusiastic:

It is felt that the limits of possible investigation of this kind are
only set by the capacities of the investigators. Thus it may be pos-
sible eventually to go behind the court records and to trace some-
what the potential law suits which never come to court.\textsuperscript{182}

Apparently, a rush job with simple results and a glorious
vision was exactly what was called for, for as soon as results were
available, the Laura Spellman Rockefeller Foundation made a
grant of $55,000 to extend the study for five more years.\textsuperscript{183} Thus
fortified, Clark set his research assistants upon the necessary but
time-consuming and hardly glamorous job of refining their ques-
tionnaires and expanding the scope of their study to include more
years, different states' courts, and criminal cases.\textsuperscript{184}

While Clark and his assistants worked away largely un-
noticed, America elected a new President—Herbert Hoover. When
Hoover took office as President he indicated not only his support
for the eighteenth amendment but also his near outrage at the
nationwide failure to enforce prohibition.\textsuperscript{185} Since the liquor
problem and its near twin—the crime problem—had been issues in
the preceding campaign, the President's position was not much of
a surprise. Nor was that of Congress which, in response to a re-
quest made in his inaugural address on that same day, appro-
priated monies for "[a] thorough inquiry into the problem of the
enforcement of prohibition under the provisions of the Eighteenth
Amendment . . . together with the enforcement of other

\textsuperscript{181.} Id. at 212.

\textsuperscript{182.} Id. at 233. Clark's research assistants collected data that led to two more articles,
Clark and O'Connell, \textit{The Working of the Hartford Small Claims Court}, 3 CONN. B.J. 123
(1929) (survey of cases filed and dispositions) and Clark, \textit{Should Pleadings Be Filed
Promptly?}, 3 CONN. B.J. 69 (1929) (comparison of timeliness of filing responsive plead-
ing in Connecticut and Massachusetts). Harris, \textit{Joiner of Parties and Causes}, 36 W. VA.
L.Q. 192 (1930) (objections to joinder); and Harris, \textit{Is the Jury Vanishing?}, 4 CONN. B.J. 73
(1930) (comparison of use of jury in Connecticut, Massachusetts, and New York; very
difficult interpretive problem) use figures developed in this initial research of Clark's.


\textsuperscript{184.} \textsc{Law Administration, supra} note 175, at 4. Some work was done in Massachusetts:
Clark, \textit{Should Pleadings Be Filed Promptly?}, 3 CONN. B.J. 69 (1929); New York: Harris,
\textit{Joiner of Parties and Causes}, 36 W. VA. L.Q. 192, 194 (1930); Ohio: \textsc{Law Administration,}
\textit{supra} note 175 at 5, n.20; and West Virginia: Arnold, \textit{The Collection of Judicial Statistics
in West Virginia}, 36 W. VA. L.Q. 184 (1930).

\textsuperscript{185.} Hoover, \textit{Inaugural Address}, in \textsc{Public Papers of the Presidents of the United
laws.” And thus was spawned the National Commission on Law Observance and Enforcement, a prestigious body headed by George W. Wickersham, former Attorney General of the United States, and including Ada Comstock, president of Radcliffe College, and Roscoe Pound.

Despite the existence of the Wickersham Commission, as the National Commission on Law Observance and Enforcement became known, President Hoover was not without “volunteers” who offered to help with investigations of the problem of the administration of justice. Among them were Robert Hutchins and Charles Clark who, but two weeks after the inauguration, met with Hoover and other federal officials to propose a $250,000 study of the operation of the federal courts patterned on the Connecticut courts study. Hoover’s response to the proposal is not recorded, but eight months later Wickersham asked Clark, who was by then dean, to meet with him about conducting one of the Commission’s studies of law enforcement. The meeting was successful; Wickersham liked the old Hutchins-Clark proposal. So after he conferred with the two members in charge of the Commission’s inquiry

187. Other members of the commission were Newton D. Baker, a former Secretary of War; William S. Kenyon, Paul J. McCormick, and William I. Grubb, all federal judges; Kenneth Mackintosh, Judge of the Supreme Court of Washington; and Henry W. Anderson, Monte M. Lemann, and Frank J. Lorsch, all prominent attorneys.
188. Yale Minutes, Mar. 21, 1929. ALI STUDY OF THE BUSINESS OF THE FEDERAL COURTS PART I, CRIMINAL CASES 21 (1934) [hereinafter cited as ALI STUDY: CRIMINAL]; Robert M. Hutchins to Henry Stimson, Mar. 18, 1929, on file in Charles E. Clark papers, Beinecke Library, Yale University [hereinafter cited as Clark papers, Beinecke, without cross-reference]. Connecticut Senator Hiram Bingham set up the meetings that included presentations to Chief Justice Taft, Justice Stone, Attorney General William D. Mitchell and Idaho Senator William E. Borah. From an undated, untitled copy of the “budget” for the project that can be found in the Clark papers, Beinecke, it can be inferred that the proposal contemplated use of the major university law schools as centers for the decentralized collection of data. Eighty percent of the budget was for the employment of an army of field workers. See also Charles E. Clark to Frederick C. Hicks, Jan. 14, 1940, Clark papers, Law (Hoover thought the whole problem was delay; Taft endorsed the project).
189. George W. Wickersham to Charles E. Clark, Nov. 21, 1929, Clark papers, Beinecke. The intermediaries apparently were Felix Frankfurter and his friend Max Lowenthal, the Commission’s secretary, who were trying to outflank Frankfurter’s colleague and Commission member, Roscoe Pound, who proposed to do the work himself. See Charles E. Clark to Felix Frankfurter, Nov. 30, 1929; Felix Frankfurter to Charles E. Clark, Dec. 2, 1929 (reporting Wickersham’s enthusiasm for the plan); Charles E. Clark to Felix Frankfurter, Dec. 18, 1929, Clark papers, Beinecke (“[Y]our distinguished colleague and leader had been willing to conduct the whole investigation.” Clark “understood” the intention of Frankfurter and Lowenthal “to guard against the very contingency which has happened.”). But see Robert M. Hutchins to Charles E. Clark, Dec. 18, 1929, Hutchins papers (speculating that Attorney General Mitchell had given the papers from the original Hutchins-Clark presentation to Wickersham, and by inference suggesting that Frankfurter was claiming credit where none was due).
into the courts and with potential members of an advisory committee to be set up to aid the project, and after President Angell's blessing was secured, Clark was hired in January 1930 as a consultant to the Commission.

Clark, who had been told that only $3,000 to $4,000 was available for the preliminary organization of his study and that it might be difficult for the Commission to obtain any future appropriations from a Congress generally uninterested in any of the Commission's work other than on prohibition, began work immediately. Or, more accurately, William O. Douglas did, for he was to design the forms on which the research assistants were to record their data. In this task he had the help of Charles Ulysses Samenow, Clark's primary research assistant on the Connecticut courts study, who was to occupy the same position with respect to this project. Working at a feverish pace, the two men began by creating the forms for collecting data on criminal cases, since those cases were the ones the Commission was most interested in, as well as the ones with which the Connecticut courts study had had the least experience. Once the forms were ready they were pretested and then reworked, then pretested again and again revised, each time after consultation with the advisory committee. Simultaneously, Douglas and Samenow developed and pretested

190. William I. Grubb, district court judge in Alabama and a Yale grad, and Monte M. Lemann, a prominent New Orleans attorney and law school classmate and friend of Frankfurter.
191. Charles E. Clark to George W. Wickersham, Dec. 30, 1929, Clark papers, Beinecke. (Meetings with Orrin K. McMurray, Dean at University of California, Berkeley, Prof. Edmund Morgan, Owen J. Roberts, then in practice in Philadelphia, later Justice of the United States Supreme Court, and Lemann). Frankfurter was also present, though not an official advisor. William I. Grubb to Charles E. Clark, Dec. 24, 1929, Clark papers, Beinecke. The other member of the advisory committee was Robert M. Hutchins. ALI STUDY: CRIMINAL, supra note 188, at 20.
193. George W. Wickersham to Charles E. Clark, Jan. 9, 1930, Clark papers, Beinecke.
194. Id.; William I. Grubb to Charles E. Clark, Dec. 24, 1929, Clark papers, Beinecke.
195. Charles E. Clark to Max Lowenthal, Mar. 2, 1930; Charles E. Clark to Charles H. Willard (Lowenthal's assistant), Mar. 25, 1930, Clark papers, Beinecke.
196. LAW ADMINISTRATION, supra note 175, at 6; ALI STUDY: CRIMINAL, supra note 188, at 22. Samenow was a 1929 graduate of the Law School. He had previously published some of his research for Clark as Clark & Samenow, The Summary Judgment, 38 YALE L.J. 423 (1930).
197. Douglas & Clark, Interim Report of the Committee on the Study of Law Administration in the Federal Courts 3 (May 11, 1930), Clark papers, Beinecke [hereinafter cited as Interim Report]. Pretesting was begun in Connecticut, moved to the Southern District of New York, and thereafter continued in Louisiana, Ohio and West Virginia. The member of the advisory committee most regularly consulted was Edmund Morgan. See, e.g., Charles E. Clark to Edmund Morgan, Feb. 18, 1930, Clark papers, Beinecke.
a form covering civil cases. Meanwhile, Clark began to assemble a collection of law school deans and faculty members who were to participate in the study by securing and nominally supervising local research assistants.

When in May 1930 Clark and Douglas paused to help the Commission obtain further funds by explaining exactly the purpose of their research, they suggested that they wished to “collect concrete factual, statistical information” in order to “illustrate and test the efficiency of our rules of procedure and our general methods of administering justice” in the federal courts. They intended to secure “actual figures bearing on congestion,” on the “types of business” in the federal courts, and on “bargain days” and other aspects of the so-called “breakdowns” in the system. All were quite topical, practical inquiries, given that President Hoover had asserted that these “problems” were leading to a general lawlessness of which the lack of enforcement of prohibition was only a single example.

Time for thought about purposes was, however, limited.

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198. Interim Report, supra note 197, at 3. This form, based on that used in the Connecticut Courts study was pretested only in the Southern District of New York.

199. See, e.g., Charles E. Clark to Herschel Arant (Dean, Ohio State), May 11, 1930 (Arant was a Law School graduate who had taught at Yale 1920-22, just after Clark had begun teaching); Charles E. Clark to Rufus Harris (Dean, Tulane), May 11, 1930 (Harris was a former student, LL.B. 1923, and graduate student, J.D. 1924, at Yale), Clark papers, Beinecke. After running out of friends and former students Clark filled in to fit the need for a cross section of district courts.


201. Id. (“While public hysteria and professional criticism will not be motivating causes in the study, much of the subject matter of these will be dealt with.”)

202. Clark and Douglas did manage to find the time in late summer 1930 to attend a Conference on Legal Research sponsored by the Social Science Research Council and organized by Henry M. Bates, Dean at Michigan. The conference was designed to explore the existing pattern of research, principle lines of future development, available personnel and potential for social science contributions. R. Lynd, Tentative Agenda for Social Science Research Council Conference on Legal Research, Hanover [New Hampshire] Aug. 29-Sept. 2, on file at Social Science Research Council, New York, New York. Other participants included Karl N. Llewellyn from Columbia, Felix Frankfurter and Joseph H. Beale (!) from Harvard and Hessel E. Yntema of the Institute of Law at Johns Hopkins. Id. See also Charles E. Clark to Henry M. Bates, Oct. 8, 1930 (good conference; group should be expanded); Henry M. Bates to Charles E. Clark, Oct. 23, 1930 (rather keep group small, adding perhaps Northwestern and Ohio. Beale should not be included next time; “regrettable incident” brought him this time); Charles E. Clark to Henry M. Bates, Oct. 27, 1930 (Frankfurter “interesting and stimulating” but too “chauvinistic” to be a good “conference or committee man”), Clark papers, Beinecke. “Next time” turned out to be never. Commenting on the conference twenty-five years later, Llewellyn suggested that Harvard and Michigan “found themselves unready for research competition . . . and . . . effectively killed off the [Social Science Research] Council’s interest in aiding any law school at all in any research looking toward integration of the disciplines.” Llewellyn, On What Makes Legal Research Worth While, 8 J. Legal Educ. 399, 401 (1956). Frankfurter and Pound had attended a similar conference to the Council in 1926, this time on criminal studies. B. Karl, Charles E. Merriam and the Study of Politics 135 (1975).
As soon as the Commission’s appropriation was renewed, Clark finished lining up law schools to help with the research. As fast as Clark lined up law schools, Douglas and Samenow set to work what was soon to be a small army of field workers whose task was enormous. In each of the thirteen districts to be studied, all civil cases and all criminal cases terminated in the five years ending June 30, 1930 except for prohibition violations were to be examined and coded; for prohibition cases, a ten percent sample was to be taken in the seven most populous districts, while all cases were to be examined and coded in the less populous districts.

At the outset it was estimated that it would take two years to gather and process the data Clark sought. Why such a vast project was planned when funding was known to be precarious is unclear, but soon problems with the original timetable appeared. The civil forms proved to need more testing than expected and the examination and coding of cases was very slow. Some change in plans was surely indicated. Exactly how serious a change did not become apparent until early January 1931 when the Commission filed its prohibition report, which satisfied neither wets nor drys because it both supported prohibition and suggested that...
if more vigorous enforcement of existing criminal statutes proved to be unsuccessful in reducing the general prevalence of violations, revision of the eighteenth amendment would be appropriate.\(^{208}\) It immediately became clear that Congress would not appropriate more money to extend the life of the Commission just to learn about the "crime problem" after it had learned nothing it wanted to know about the "liquor problem."\(^{209}\) So that January, Clark, interested Commission members, and other advisors thrashed out a new, more limited goal in many letters and several conferences.\(^{210}\) A new target for accumulating data was established: three years' worth of criminal cases and one of civil in each district. None of these data would be processed except the data on criminal cases in Connecticut, which would form the basis for a progress report to the Commission.\(^{211}\) Meanwhile, Chairman Wick- ersham, working with materials furnished by Clark, would seek the foundation support necessary to finance completion of the study.\(^{212}\)

While the search for funds went forward, field workers poured the results of their labors into New Haven where Douglas and Samenow, aided by Thurman Arnold, newly added to the faculty, and others,\(^{213}\) struggled to check, run, and analyze the Connecticut criminal cases. Their job was made doubly difficult by the need to first process materials on juveniles that they had agreed to collect for another commission consultant many months before.\(^{214}\) But by May 1, although the New Haven staff, which had been "working nights for the last week or so," was "used up," the progress report was done.\(^{215}\)

What the report showed was a bit of a surprise. It noted that although the literature suggested the existence of numerous obstacles to effective law enforcement, "technicalities, delays and continuances, irrational juries, a cumbersome grand jury system,


\(^{209}\) Claire Wilcox (Commission Research Director) to Charles E. Clark, Jan. 8, 1931, Clark papers, Beinecke.

\(^{210}\) See, e.g., Charles E. Clark to William I. Grubb, Jan. 3, 1931; Monte Lemann to Charles E. Clark, Jan. 14, 1931, Clark papers, Beinecke.

\(^{211}\) Charles E. Clark to George W. Wickersham, Mar. 31, 1931, Clark papers, Beinecke.

\(^{212}\) Charles E. Clark to George W. Wickersham, Mar. 31, 1931; Apr. 1, 1931; George W. Wickersham to Charles E. Clark, Apr. 14, 1931 (reporting contacts with Rockefeller Foundation), Clark papers, Beinecke.

\(^{213}\) Most notably Dorothy Swaine Thomas, see note 303 infra.

\(^{214}\) Charles E. Clark to Monte Lemann, Oct. 25, 1930; Charles E. Clark to George W. Wickersham, Apr. 31, 1931, Clark papers, Beinecke.

\(^{215}\) Charles E. Clark to Claire Wilcox, May 1, 1931, Clark papers, Beinecke.
long trials, appeals on obsolete doctrinal points, and, in general, the widely advertised results of what is generally called ‘the sporting theory of justice,’” in fact, nothing of the sort appeared to be happening. Seventy percent of the defendants pled guilty when arraigned, most of those on the day the indictment or information was filed. Ultimately ninety percent of the defendants pled guilty and only one percent had a jury trial, generally lasting less than a day. Sixty percent of all cases were disposed of with a fine (eighty percent of the prohibition cases and twenty-five percent of the balance), with the amount “of the fine so nicely adjusted that in three years only five defendants were committed to jail for failure to pay.” Eighty-five percent of the cases were disposed of in two months. Clark, Douglas and Samenow had discovered modern federal criminal procedure.

The three men were slightly bewildered by their discovery, which for them raised doubts about the administration of justice because

> [the system] seems almost too efficient; because it presents the spectacle of a long line of orderly offenders, few of whom it is necessary to commit to jail either before or after trial, pleading guilty with systematic regularity . . . , raising no technical objections and so far as the records show, complaining about no invasions of their constitutional or other privileges.

But they proposed to stick with their figures, which to them suggested that the absence of delay was due to careful selection of the prosecutions brought, with an eye to eliminating or prosecuting under less serious charges possibly contested cases.

At least one member of the Advisory Committee was enthusiastic about the limited results. Unfortunately, the Commission was not. After nearly a month of sitting on the report,

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217. PROGRESS REPORT, supra note 205, at 18, 22-23, 32.
218. Id. at 18, 22-23, 58.
219. Id. at 18, 23-29.
220. Id. at 18, 32.
221. As distinguished from contemporary federal criminal procedure in which by the addition of lawyers for indigents and the creation of several new constitutional defenses, the legal system seems to have kept the same ultimate results but lengthened the time for disposition.
223. PROGRESS REPORT, supra note 205, at 19. The inference was correct. See John A. Danaher (U.S. Atty. for Conn.) to Charles E. Clark, June 10, 1931, Clark papers, Beinecke.
224. Owen Roberts to Charles E. Clark, May 16, 1931, Clark papers, Beinecke.
Wickersham, probably acting at Pound's request, asked for deletion of the conclusion that no delay in the disposition of cases had been found because "the statement is at variance with conclusions which were reached in other reports."225 Although the other reports were virtually free of any data suggesting court congestion and delay, a slightly bitter Charles Clark agreed.226

Why Clark agreed is uncertain. Perhaps it was the realization that even with Wickersham's deletions, the report was a strong statement. It noted "the complete absence of procedural delays," a "negligible" incidence of contested cases and jury trials, a "negligible" amount of time required for the disposition of cases, and the prevalence of "minor offenses" for which "small sentences" were imposed.227 These findings and the inference of selective prosecution228 negated the suggestion that the system was overrun with congestion and delay, at least for anyone who could read.

But whatever Clark's reason for agreeing to Wickersham's deletions, it was probably not any hope for future favors. Wickersham had generated a grant of $25,000 from the Rockefeller Foundation to the American Law Institute that might have financed completion of the study, but that grant was contingent on securing $25,000 more from other sources, and Wickersham had already run through his short list of alternative sources of funds with no success.229 So in June 1931, as the fiscal year drew to a close, the field workers worked madly to finish their counting and everyone awaited the inevitable termination of the study.

When funding terminated on July 1, the staff of trained field workers disappeared. Samenow, who according to Clark suffered from a personality that was not "pleasing" and was thus precluded from consideration for a permanent faculty appointment,
left for practice.\textsuperscript{230} Everyone expected that Douglas would complete the exodus by accepting Hutchins' seemingly magnificent offer to go to Chicago, but instead he stayed on as a visiting professor at a school he had never left.\textsuperscript{231} Nevertheless, the excitement was over.

In October, Clark and Wickersham again approached the Rockefeller Foundation in an effort not to finish the original project, but just to process and tabulate data already collected.\textsuperscript{232} Their effort succeeded and, as a result, Samenow was rehired to begin work on the accumulated data.\textsuperscript{233} But with the small work force that could be afforded, the job went slowly. The cards were not all punched until summer 1932; Samenow's preliminary draft of the report on criminal cases was not finished until fall; and

\textsuperscript{230} Charles E. Clark to Charles Seymour (Yale Provost) Sept. 22, 1931 (very valuable but unfair to encourage about permanent position), Angell papers; Charles E. Clark to George W. Wickersham, Oct. 2, 1931, Clark papers, Beinecke. Samenow was described to me as "brilliant, tactless and intolerant of lesser minds." Interview with David Kammerman (friend, assistant on both Connecticut and Federal Courts studies), June 20, 1975.

Samenow may well have sensed the way in which he was an outsider. A 1932 picture of the faculty shows him defiantly seated with his arms crossed and his fashionable white bucks, the only shoes of that kind in the picture, prominently displayed. He was absolutely essential to the functioning of both courts studies. See, e.g., William O. Douglas to Charles E. Clark, Mar. 21, 1931, Clark papers, Beinecke. For an example of his technical expertise, see Samenow, Judicial Statistics in General in Practical Applications of the Punched Card Method 319 (G. Bachne ed. 1935).

\textsuperscript{231} W. DOUGLAS, GO EAST, YOUNG MAN 163-64 (1974). Why Douglas did not go to Chicago is difficult to say. Hutchins claimed not to know. Interview with Robert M. Hutchins, June 20, 1975. Douglas formally told Hutchins that he had "a number of things" that he had to spend the next year "trying up and completing." William O. Douglas to Robert M. Hutchins, June 10, 1931, Hutchins papers. Clark speculated that Douglas was unhappy that Hutchins seemed to be delegating to his dean of the social sciences everything about the proposed research project "in the field of finance"—the main attraction for Douglas—and that in general Douglas seemed concerned about Hutchins' ability to deliver on his promises. Charles E. Clark to James R. Angell, May 29, 1931, Angell papers. The visiting professorship was a way for Douglas to finesse the issue of his concerns about Hutchins' promises. Id. Local scuttlebut had it that Douglas was upset that Hutchins could not deliver the $25,000 salary he had promised. Interview with David Kammerman, June 2, 1975. While it is clear that Hutchins could not pay that salary (William O. Douglas to Robert M. Hutchins, Apr. 15, 1932, Hutchins papers), the $20,000 per year salary he could deliver was substantially above the best Clark could offer (Charles E. Clark to James R. Angell, Feb. 18, 1932, Angell papers). Thus money cannot have been a great factor. I suspect that although Douglas talked as if things at Yale were not going well (William O. Douglas to Robert M. Hutchins, Apr. 15, 1932, Hutchins papers), Yale looked more attractive than Chicago where the law school would not support his work, especially since by the time Douglas really had to decide, he had made the contacts with the Harvard Business School that were to absorb him for the next few years. W. DOUGLAS, supra, at 172-73. See also note 434 infra.

\textsuperscript{232} Charles E. Clark to George W. Wickersham, Oct. 2, 1931; George W. Wickersham to E. E. Day (Rockefeller Foundation), Oct. 6, 1931; Charles E. Clark to E. E. Day, Oct. 31, 1931, Clark papers, Beinecke.

\textsuperscript{233} Norma S. Thompson (Rockefeller Foundation) to George W. Wickersham, Nov. 13, 1931; Charles E. Clark to William Draper Lewis (Director, American Law Institute), Jan. 6, 1933, Clark papers, Beinecke.
Douglas' redraft languished until just before Christmas. While Douglas worked, Clark negotiated a complicated agreement with the American Law Institute to publish the two reports. The agreement required approval first by a committee of the ALI Council consisting of two members of the Commission and Judge Learned Hand, then by the Council, and finally by the membership—a process so full of potential traps that it plainly left Clark worried. The report on civil cases was not ready until the end of summer 1933, at which time Samenow left for good. ALI approval of both reports did not come until May 1934, and publication was delayed until the following fall. Counting from the Hutchins and Clark meeting with Hoover, it had taken over five years to publish about one year's worth of research.

Despite the setbacks and the enormous effort, Clark remained remarkably good-humored, as can be seen from an incident that took place at the very end of the study. Wickersham had been asked to write an introduction to the published volumes; his draft cut squarely into the representativeness of the study. His reason for so doing was simple. Clark had already published an article in the American Bar Association Journal highlighting one of the study's most topical discoveries. He had noted that the diversity

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235. See William Draper Lewis to Charles E. Clark, Sept. 15, 1932; Charles E. Clark to William Draper Lewis, Sept. 19, 1932; William Draper Lewis to Charles E. Clark, Sept. 23, 1932; Charles E. Clark to William Draper Lewis, Sept. 27, 1932, Oct. 21, 1932, Clark papers, Beinecke. Clark was worried that since he could not get agreement on the Progress Report from the eleven member Wickersham Commission it would be impossible to get agreement from the larger ALI council, much less the entire membership of that organization. See Charles E. Clark to Learned Hand, Feb. 4, 1933; Learned Hand to Charles E. Clark, Feb. 15, 1933; William Draper Lewis to Charles E. Clark, Mar. 1, 1933; Charles E. Clark to William Draper Lewis, Mar. 2, 1933, Mar. 10, 1933, Clark papers, Beinecke.

236. Charles E. Clark to William Draper Lewis, Sept. 21, 1933, Clark papers, Beinecke. Samenow went briefly into practice and then did a study for Herman Oliphant, by then General Counsel of the Treasury Department, on the Customs Court in New York City. C. SAMENOW, REPORT OF PROTEST LITIGATION (1936). The report lists Clark and Fleming James, Jr., as sponsors of the study as well as a committee of the New York Law Society, a survival from Oliphant's work while at the Institute of Law at Johns Hopkins. See James & Stockman, Work of the New York Law Society, 27 Geo. L.J. 680 (1939). After completing the study, Samenow went to Washington, first to the United States Housing Authority and then to the Rural Electrification Administration as a special assistant to the Administrator until his retirement years later in 1968. He died in 1974. Interview with David Kammerman, June 2, 1975; interview with Mrs. Charles U. Samenow, June 2, 1975.

237. ALI STUDY: CRIMINAL, supra note 185, at 22.

238. See George W. Wickersham to Charles E. Clark, Oct. 1, 1934, Clark papers, Beinecke.

jurisdiction had been so swallowed up by the federal question jurisdiction and by cases brought by or against the United States that diversity cases represented less than twenty percent of filed cases.\(^{240}\) Moreover, of those diversity cases filed, eighty-five percent were simple contract or tort claims, and most of these were claims involving foreign corporations doing business in the forum state.\(^{241}\) Clark suggested that these findings supported a pending bill to treat a foreign corporation doing business in a state as a citizen of that state for purposes of the diversity jurisdiction.\(^{242}\) Wickersham strongly opposed the bill\(^{243}\) and so wrote his introduction to undercut Clark's conclusion. Yet, in the face of this rather direct attack on his scholarship, Clark, while seeking a change in the introduction, was able to remark that he thought he had adequately emphasized that the conclusions drawn in the study were his own.\(^{244}\) He continued: "Perhaps this thought is not important, but I have in mind to make it always clear that the figures are available to all commentators, whatever side of pending questions they take."\(^{245}\) Such equanimity was a long way from the optimism of the days of the early reports on the Connecticut courts study. Yet the reasons for Clark's change of tone, if not obvious from the story of his enterprise, are relatively easily isolated: experience and a bit of bewilderment.

Clark's experience had been like virtually no other law teacher's. His Connecticut courts study was the first of its kind, although it admittedly had antecedents in the Cleveland crime survey. In it Clark had developed a variation on the staple methodology of the crime survey—the mortality table. While the mortality table looked at how cases entering the system dropped out, in an effort to learn if criminals were being allowed to escape from justice, Clark's method in Connecticut courts focused on completed cases in an effort to learn what the system as a whole looked like over a given period of time. This methodological variant did exactly what it was designed to do; it generated what we now know to be a remarkably accurate picture of an urban

\(^{240}\) Id. at 500.
\(^{241}\) Id. at 501-02.
\(^{242}\) Id. at 503.
\(^{243}\) George W. Wickersham to Charles E. Clark, July 17, 1933, Clark papers, Beinecke (responding to Clark's article).
\(^{244}\) Charles E. Clark to George W. Wickersham, Oct. 4, 1934, Clark papers, Beinecke.
\(^{245}\) Charles E. Clark to George W. Wickersham, Oct. 4, 1934, Clark papers, Beinecke. This time Wickersham backed down and deleted the offending comments. George W. Wickersham to Charles E. Clark, Oct. 5, 1934, Clark papers, Beinecke.
court system. In supervising the development of this methodology, and then further refining it in the federal courts study, Clark had learned enough to have acquired the concerns of the newer social scientists about control of observation and their preference for the analysis of primary data. He worried about observer bias, prided himself on the fact that all his data were collected by investigators under his control, and even did some error estimation. Clark had also absorbed some of the then current social science cant about the separation of data collection and data analysis. And he was even a bit of a missionary bringing social science to the more provincial law schools. Yet as he absorbed bits of the traditions of academic social science, somehow the early excitement derived from participation in a new and scientific inquiry was lost.

The published results of the Connecticut courts study hint at the reason for the loss of enthusiasm. After the first flurry of articles, virtual silence set in, just when there was little else to absorb Clark's time. Eight years later, further findings were published, largely because there was little else to do with the money left in the grant secured to support the research. The total number of cases examined was tripled, data collection measurably refined, and machine processing of data begun. But when Clark stepped back from the technology and looked at the results, he discovered that they were basically the same as in the original rush study done to secure the grant. True, some new items of significance were disclosed—most obviously the development of a specialized personal injury bar and the tendency of more jury verdicts to be appealed and appealed successfully more often than nonjury verdicts. But the basic story was the same. Essentially uncontested matters—collections, divorces, and foreclosures—to-

246. See Clark, Fact Research in Law Administration, 2 CONN. B.J. 211 (1928); Charles E. Clark to Felix Frankfurter, Jan. 13, 1930; Charles E. Clark to George W. Wickersham, Feb. 4, 1930; Charles E. Clark to William F. Berry (successor to Lowenthal as Commissioner Secretary), Dec. 4, 1930 (use of secondary data); Charles E. Clark to William I. Grubb, Jan. 3, 1931, Clark papers, Beinecke.

247. See, e.g., Charles E. Clark to William O. Douglas, Oct. 26, 1932, Clark papers, Beinecke. See also text accompanying notes 404-05 infra.


250. Here one should contrast Clark's experience with the Federal courts study in which it also turned out that the initial report and the ultimate report disclosed the same general pattern of largely bargained pleas, but that the situation in Connecticut was variant within that pattern. See ALL STUDY: CRIMINAL, supra note 188, at 115 (detailed Table 7).

251. LAW ADMINISTRATION, supra note 175, at 42-51, 78.
gether with the largely settled matters—primarily tort cases—domi-
nated the docket. In so duplicating the results of the initial
foray into the subject, the larger study exposed the routine nature
of most scientific inquiry.

Routine inquiry need not be a problem. Indeed, fifty years
later the fact that three times the effort was not likely to even
double the results is not startling. But as the silence and resigna-
tion show, to Clark and his co-workers that fact was doubly de-
pressing. It was depressing, first, because in contrast to the initial
high expectations it seemed as if the field of investigation chosen
by Clark had not been "one of the most fruitful" for scientific re-
search, for the results did not "test the efficacy of our rules of pro-
cedure," nor did they suggest any "further detailed investigations
of social problems" that might be undertaken, much less ways to
trace "the potential law suits which never come to court." It
was depressing, second, because from the beginning—the Hutchins-
Clark proposal for an Institute of Procedure—the courts studies
were supposed to reveal the knowledge that would fuel the progress
of reform, specifically the reform of the technical rules of pro-
cedure. Yet the increased effort by Clark and his co-workers had
merely generated more evidence of the same conditions that
seemed by and large irrelevant to the cause of reform as they knew
it, since in a system where most cases are uncontested or settled
technical procedure played little part and thus to expend effort
at its reform made little sense.

Taken together the failure of the study to live up to the
initial high expectations and the failure to provide fuel for the
progress of reform suggested that the aims of the project, already
reduced from solving the problems of the administration of jus-
tice to "providing valuable information . . . to all those
interested in the processes of law administration and in its im-
provement," ought to be further limited to gaining "experience
. . . in the operation of an extensive survey of civil statistics" and
to providing a stimulus for setting up "a permanent machinery
to supply" "statistical data on judicial administration." Such

252. Id. at 2.
253. See text accompanying notes 181-82 supra.
254. See text accompanying notes 45-46 supra.
256. PROGRESS REPORT, supra note 205, at 4.
257. ALI, A STUDY OF THE BUSINESS OF THE FEDERAL COURTS: PART II, CIVIL CASES 17
(1934) [hereinafter cited as ALI STUDY: CIVIL].
258. LAW ADMINISTRATION, supra note 175, at 201.
an aim was a long way from reform and although Clark, who well knew exactly what his enterprise was worth as a piece of scientific research, accepted that reduction in aim, he surely did not revel in it. And so in a real sense his experience was thus his undoing; it sapped the impetus for the doing.

By thus limiting his objectives Clark may have succeeded in compensating for the debilitating effects of the limited results of his science. But limited objectives were no defense against the bewildering demands on one's science made by friendly insiders and by outsiders, friendly or not.

Of the insiders Samenow and Douglas were the workhorses of the operation. They apparently did their tasks without quarrel, maybe even with a bit of relish—indeed both worked on more than one such research project at a time. The nearly two years of never quite knowing whether Douglas would leave for the University of Chicago were perhaps unsettling, but the prolonged indecision seemed to have no appreciable effect on his work. Thurman Arnold, however, was a quite different kind of co-worker. He had been brought to New Haven in part because of his ostensible interest in both the state and federal courts studies. After his arrival in New Haven, Arnold was in fact of some help, but as the project dragged on and he found new and more...
interesting things to do, his participation became more problematic. Clark had difficulty in keeping him at his work, and Arnold began to question the legitimacy of collecting "mass statistics." 

Arnold's drift away from the enterprise could perhaps be rationalized by looking at the exciting new things he was doing. Outsiders, on the other hand, though less personally troublesome, were more intellectually so, as can be seen from their comments on the federal courts study's report on criminal litigation. Some were, of course, simply supportive, though whether reflexively or reflectively it is often hard to say. In contrast, Learned Hand was thoroughly, but gently, skeptical about the enterprise; though he suggested that the results were "scarcely worth the extraordinary amount of intelligence and time they have cost," he knew his role and willingly bestowed his "unorthodox blessing" on the re-

Business of the Federal Courts, Apr. 30, 1931 (draft); Charles E. Clark to William D. Lewis, Oct. 21, 1932; Edson R. Sunderland (University of Michigan) to Charles E. Clark, June 20, 1933, Clark papers, Beinecke. Before coming to Yale, while dean at West Virginia, Arnold did some work for the expanded Connecticut courts study and supervised local pretesting of the criminal forms for the federal courts study. See Arnold, The Collection of Judicial Statistics in West Virginia, 36 W. Va. L.Q. 184, 186-87 (1930); Arnold, Review of the Work of the College of Law, 36 W. Va. L.Q. 319, 322-23, 324 (1930).


264. See Charles E. Clark to Sam Bass Warner, Jan. 25, 1933; Charles E. Clark to Edmund Morgan, Feb. 6, 1933; Charles E. Clark to Edson R. Sunderland, Feb. 17, 1933; Charles E. Clark to Thurman Arnold, July 14, 1933 (draft of proposed Law Review symposium, Arnold to begin with "indictment of mass statistics"); Clark papers, Beinecke. Cf. Law Administration, supra note 175, at 200 ("Many persons became interested—some genuinely, others momentarily.").

Questions about the genuineness of Arnold's commitment to the courts studies have been raised, for example, by Fleming James, Jr., Interview, June 11, 1975. Arnold was an incredible opportunist, and there seems to have been some opportunism in his move to Yale. However, an alternative explanation fits the facts of Arnold's participation in the courts studies at least as well as does one that stresses his opportunism. A quick review of his biography suggests that Arnold's main intellectual characteristic was possession of the attention span of a two-year-old. Once he left practice he changed interests every two or three years. Ultimately, however, he returned to practice, for only there did problems change fast enough. Thus, I rather credit Arnold's profession of interest in the courts studies, at least at the outset.

265. See, e.g., William I. Grubb to Charles E. Clark, Feb. 18, 1933; Edwin Sutherland (University of Chicago sociologist and author of study of the federal courts) to Charles E. Clark, Jan. 17, 1933, Clark papers, Beinecke. From some came backhanded consolation, Edson R. Sunderland to Charles E. Clark, Feb. 21, 1933, Clark papers, Beinecke ("If it had been known in advance to what extent statistical methods would lead to important affirmative conclusions, a study of that scope would never have been undertaken.").
The Harvard establishment presented a more disconcerting problem, however. To some extent Clark had created his problems at Harvard. True to an ideal of objective social science and also a bit gun-shy because of the Commission’s objections to the conclusions in the preliminary report, he sent out the draft of the criminal report virtually devoid of any conclusions or interpretive material. This action had the support of William Draper Lewis, executive director of the ALI, who was as worried as Clark about the problems of getting any conclusions approved by his diverse membership. Many of the most thoughtful readers were not at all bothered by this mode of presentation. Mr. Justice Roberts, for example, was pleased with the “great discretion and fairness of appraisement,” and Thomas E. Atkinson, a professor at Kansas, found the report “interesting” and “of great value.” But Clark’s Harvard friends were not so charitable.

Monte Lemann, for example, found that the final report fell short of fulfilling the expectations created by the preliminary one. Sam Bass Warner, who had done similar work himself, found the report a “jumble of figures without any special meaning or significance,” suggested that Clark or Arnold or “some other genius... dream about it for the next six months and... completely rewrite it with a view to emphasizing the nuggets discovered,” and then delivered the unkindest blow of all by suggesting it was “no better than a good Johns Hopkins report.”

His colleague Edmund Morgan agreed. Felix Frankfurter suggested that since the study presented neither “vivid illumination of the workings” of the lower federal courts nor “exploration of a new technique for securing such illumination,” “scholarship would

266. Learned Hand to Charles E. Clark, Feb. 3, 1933, Clark papers, Beinecke. (I sit “silent before the authority of statisticians, those modern magicians, who would enslave us all, except for their own benign internecine warfare.”).
267. See note 235 supra & accompanying text.
268. Owen D. Roberts to Charles E. Clark, Jan. 20, 1933; Thomas E. Atkinson to Charles E. Clark, Jan. 25, 1933 (“dry” Kansas had “wet” judges), Clark papers, Beinecke.
269. Monte Lemann to Charles E. Clark, Feb. 13, 1933 (question whether the report as a whole adds enough to existing information to justify the money and work); Feb. 16, 1933, Clark papers, Beinecke.
270. Sam Bass Warner to Charles E. Clark, Jan. 25, 1933, Clark papers, Beinecke; see S. WARNER, CRIME AND CRIMINAL STATISTICS IN BOSTON (1934); Wahrer, Survey of Criminal Statistics in the United States, in NATIONAL COMMISSION ON LAW OBSERVANCE AND ENFORCEMENT, REPORT ON CRIMINAL STATISTICS 19 (1931).
271. Edmund Morgan to Charles E. Clark, Jan. 24, 1933, Clark papers, Beinecke.
not be advanced" by publication of the report. In support of his conclusion, he presented a memo written by an unnamed "professional friend," in reality Henry Hart, then in his first year of teaching, that was an outrageous hatchet job done with little understanding of the project.

Clark was both unhappy with the criticism and slightly liberated by it. Exasperated, he complained to Warner, "A few years ago the cry was all for collecting many figures. Now it is to collect hardly any and interpret." Yet he admitted that the cry for interpretation "clears the air and prepares the way for a report and a set of conclusions which we have for some time felt we would like to make, but which, in view of the institutional nature of the study and its backing from diverse sources, we did not want to send out with the first draft." Of course, when the conclusions arrived, some of the same parties complained—for example, Morgan. He argued that although the study disclosed no real obstacles to law enforcement, such as "technicalities, delays, . . . irrational juries [and] appeals on obsolete doctrinal points," neither did it "indicate their absence"; thus, it did not negate the possibility that the "long line of offenders pleading guilty with great regularity" was evidence that "the regular system of examining into the merits" was "so unsatisfactory that both the prosecution and defense sought compromise rather than trial." Clark, by

272. Felix Frankfurter to Charles E. Clark, Apr. 3, 1933, Clark papers, Beinecke.

273. Clark identifies Hart's authorship in Charles E. Clark to Max Lowenthal, Sept. 19, 1933, Clark papers, Beinecke. The Hart memo was untitled and undated. It began with the objection that "on almost no point have we" concrete factual, statistical information "in the sense of data establishing beyond peradventure a conclusion," as well as that the number of districts surveyed and the time period covered were limited. Hart objected as well to the failure to disaggregate cases by type when examining time between commencement of prosecution and disposition. These criticisms are well beside the mark. Establishing anything "beyond peradventure" with the kind of data in question is virtually impossible. The defect in sampling is not the number of districts nor the number of years but the weak scientific basis for the sample chosen, and the figures for anything but the shortest durations are so small that disaggregation would have robbed them of what little significance they had. Perhaps Hart knew no better. If so, then comments like "One wonders why when detailed study was made of so many trivial matters, conclusions as important as this one [duration by type of case] were left to guessing" or "Many times one gains the strong feeling that things have simply been counted indiscriminately . . . as if all facts were free and equal" were simply out of line. Twenty-five years later Clark was still haunted by Hart's last assertion, which a careful reading of the report in the light of the accepted reform proposals of the day would dispel. See Charles E. Clark to Filmer S. C. Northrup, Jan. 10, 1948, Clark papers, Law.

274. Charles E. Clark to Sam Bass Warner, Jan. 25, 1933, Clark papers, Beinecke. Challenged, Warner backed down with an admission that it took much time to complete his own study that was almost exclusively of secondary data. Sam Bass Warner to Charles E. Clark, Feb. 3, 1933, Clark papers, Beinecke.


276. Edmund Morgan to Charles E. Clark, May 1, 1933, Clark papers, Beinecke.
this time virtually shell-shocked, was ready to change his tune and not only to argue against appending conclusions to his study, but also to maintain that with the coming of the New Deal the prevailing practice had changed from collection of fact to "action without even looking at the consequences," and that this change had done the study in.\textsuperscript{277}

Clark's difficulties with his external audience were a bit bewildering, especially when one considers the content of the report. He had managed to examine 70,000 cases in thirteen well-selected district courts.\textsuperscript{278} Presentation was clear, if not expeditious. The pattern that emerged from the figures was again much like that of modern criminal procedure; most cases were disposed of on guilty pleas with relatively lenient sentences.\textsuperscript{279} As a result indictments were at times dispensed with, especially in liquor cases; few motions were filed; few trials were held; those trials held were of short duration; verdicts were appealed only when imprisonment was the sentence; and appeals were not often successful.\textsuperscript{280} Interestingly, the report concluded that plea bargaining in the federal courts had begun about 1916 with the rise of federal criminal liability for what would otherwise be thought of as local law enforcement problems, such as auto theft, prostitution and narcotics, and suggested that the practice be recognized and regularized.\textsuperscript{281} All in all the report was, and still is, quite informative and useful.

Given the negative reaction to the careful work, based on rather extensive data, in the criminal report, Clark must have sent out the draft of the civil report with trepidation. Here Clark had nearly 10,000 cases collected in haste during the last few months of the study.\textsuperscript{282} And presentation of what data there were was complicated by the existence of separate law, admiralty and equity

\textsuperscript{277} Charles E. Clark to Edmund Morgan, May 2, 1933, Clark papers, Beinecke. Why Clark took Morgan seriously after Morgan had admitted that he had "no burning enthusiasm" for the study as he was "not born an artist" and that appointment to Clark's advisory committee "did not light the necessary fire" within him, "perhaps because there was no ignitable material," is not clear. Edmund Morgan to Charles E. Clark, Feb. 4, 1933, Clark papers, Beinecke. Perhaps Clark valued the apparent, if not actual, disinterest.

\textsuperscript{278} ALI STUDY: CRIMINAL, \textit{supra} note 188, at 109.

\textsuperscript{279} \textit{Id.} at 115 (detailed Table 7).

\textsuperscript{280} \textit{Id.} at 59-67, 71, 81-83, 104-05.

\textsuperscript{281} \textit{Id.} at 13. This recommendation supported one recently aired, Miller, \textit{Compromise of Criminal Cases}, 1 S. CAL. L. REV. 1 (1927). At least one of Clark's readers opposed the idea, Orin K. McMurry to Charles E. Clark, June 19, 1933, Clark papers, Beinecke.

\textsuperscript{282} ALI STUDY: CIVIL, \textit{supra} note 257, at 115.
dockets, each with its own procedural vocabulary, and by the overlay of federal civil actions for forfeitures and penalties. Even more important, but for the materials on the use of the diversity jurisdiction, precious little about the activities of the federal courts emerged beyond the prevalence of settlements, the limited incidence of jury trials, and the relative expedition with which cases, especially those permitting nonjury trials, were disposed of. A very careful reader might have noticed the relative unwillingness of the federal government to settle cases, but that was about all.

This time, however, except of course for a continuingly skeptical Learned Hand, the reviewers, especially those from Harvard, found the report thoroughly satisfactory. Frankfurter commented, “Not only have you posed important problems—you have gone a long way towards shedding much light on them”; he even claimed to have gotten Clark started in the business of studying courts. Lemann found the report both interesting and useful, and lamented that time and a lack of money had limited its scope; Warner found it both very good and understated. Clark intuited a reason for the change in tune. In between circulating drafts of the first and second reports, he had published his piece on the diversity jurisdiction. As that piece supported the frankfurterian-liberal reformist position on the legislation to limit corporate access to the diversity jurisdiction, Frankfurter and his friends supported Clark’s research; it was all as predictable as Wickersham’s action had been. However, the existence of such a reason was hardly comforting and, taken together with the earlier objections, showed at best a perverse preference for the results of a poor

283. Compare id. at 65-69, 86-92, with note 176 supra & accompanying text.
284. ALI Study: Civil, supra note 257, at 66.
285. See, e.g., Monte Lemann to Charles E. Clark, Dec. 6, 1933 (interesting and useful; regrets that time and money limited scope); Learned Hand to Charles E. Clark, Oct. 3, 1933 (“I do not now see what advantage will be got out of this extremely careful and meritorious collection.”), Clark papers, Beinecke.

Friendly people said the usual friendly things. See, e.g., Thomas E. Atkinson to Charles E. Clark, Sept. 21, 1933 (excellent); William I. Grubb to Charles E. Clark, Sept. 15, 1933 (“fine work”); Max Lowenthal to Charles E. Clark, Sept. 9, 1933 (“thoroughly worthwhile job”; now try an observational study); Edwin H. Sutherland to Charles E. Clark, Sept. 25, 1933, Clark papers, Beinecke.

286. Felix Frankfurter to Charles E. Clark, Sept. 20, 1933, Clark papers, Beinecke.
287. Monte Lemann to Charles E. Clark, Dec. 6, 1933; Sam Bass Warner to Charles E. Clark, Oct. 9, 1933, Clark papers, Beinecke.
job over those of a good one that conflicted with Clark's newly acquired scientific values.

However bewildering to Clark, the outsiders' reaction to the Study of the Business of the federal courts nevertheless illuminates the understanding of empirical legal research held by Clark and his contemporaries. Clark had spoken of the knowledge that was prerequisite to reform. In so doing, he spoke from within a tradition of "progressive" reform through the gathering of "the facts" that dated back through Brandeis' brief in *Muller v. Oregon* at least as far as the Sanitary Commission's investigations during the civil war. While Clark had altered that tradition, at least in its legal branch, by yoking it more firmly with the emerging quantitative social science of the time, he had at the same time affirmed it in two important ways. First, he affirmed the activist aspect of the tradition that saw and focused scientific inquiry in terms of its usefulness in securing desired reforms. Second, he affirmed its model of the way the world to be inquired into was structured—congested courts, procedural shenanigans, and all—

289. See text accompanying note 45 supra.

290. Thus it is no accident that the initial piece on the Connecticut courts study was entitled "Fact Research in Law Administration." See note 174 supra (emphasis added).

291. I recognize that my concept of "the progressive reform tradition" needs further definition. In particular it is no longer clear that the participants in it were in any sense "progressive" or their objective "reform." Indeed one might quite cogently argue that these individuals were slightly "reactionary" and their objective the further entrenchment of privilege. See, e.g., T. Haskell, *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth Century Crisis of Authority* (1977), R. Wiebe, *The Search for Order 1877-1920* (1967). But to resolve all of the questions would further balloon this already overly long presentation. I plan to attack the problem after completing an article on the work of Underhill Moore. In the meantime the concept can serve as a convenient shorthand for the thought and activities of a group of individuals active in what has been traditionally seen as liberal politics between the end of the Civil War and the First World War.

Professor Robert Gordon has suggested that what I have described as a "tradition" might aptly be termed a kuhnian "paradigm." See T. Kuhn, *The Structure of Scientific Revolutions* (1962). While the progressive reform tradition, which continues to this day, is durable enough to qualify as, and has other aspects of, a paradigm, especially in the way that disconfirming evidence is either ignored as unintelligible or imported into the tradition in an ad hoc fashion, I have chosen not to use Kuhn's concept. I am convinced that the progressive reform tradition is not so fully formed to qualify as a paradigm and I doubt whether constructs of social reality change in the way Kuhn suggests that scientific paradigms do, although social constructs surely do not change in the manner dictated by the scientific rationalism of Kuhn's opponents. Moreover, the alternative tradition in which Clark moved, that of academic social science, is even less fully formed in the twenties and thirties, since at that time structural functionalism was by no means fully entrenched. Thus, in order to deal with this tradition I would have to develop a notion of quasi or emergent paradigm. For me these concerns counsel against further stretching Kuhn's already badly battered concept. On the other hand, if the resonance from this well known concept aids the reader's understanding, I surely have no objection to equating "tradition" and "paradigm."
as well as its prescription of technical procedural reform as the way to eliminate the perceived defects in that structure. But, as noted earlier, Clark acquired part of a different tradition, that of the new academic social science. In his work he affirmed aspects of that tradition as well, especially the young social scientists' concern for method that in part replaced the old model of the way the world to be inquired into was structured, and their recognition of a more attenuated relationship between scientific inquiry and desired reforms.

Clark was confused by the intersection of these two traditions, as can be seen from his reaction to the later Connecticut Courts' work. There the results did not fit with the received understanding of how the legal world was structured, and moreover suggested that the reforms that the research was done to support were largely irrelevant. Faced with this discontinuity between the expectations generated through his participation in the progressive reform tradition and the results of his research, Clark hesitated a while, but ultimately published the work anyway. In doing so he drew on his commitment to the traditions of the newer academic social science, although his analysis never fully broke free of the other tradition.

In contrast, the troublesome outsiders who so bewildered Clark by their perverse preference for the bad over the good were

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292. See text accompanying notes 246-47 supra.

293. It would be a mistake to see these two traditions as somehow "competing" for Clark's soul. They were not in any real sense competing at all. First, they shared the same slightly left-of-center politics and a view of reform related to that politics. Second, the social science tradition largely lacked an agreed model of the way the world to be inquired into was structured. What took its place was a rather virulent positivism. If a metaphorical description of their claims on Clark's attention is needed, the best is "divergent."

294. For example, although the federal courts study managed to dispel the myth of the congestion of the criminal courts, when reporting on civil cases in Connecticut, the problem of congestion, especially in negligence actions, reappeared though it was evidenced only by moderate delay in disposition and that delay was greater for contract than for negligence actions. Compare Law Administration, supra note 175, at 35, 168-70, with id. at 37, 39 (Table XV). Similarly, Clark's evaluation of the jury, id. at 79, went far beyond his data, id. at 59-78, particularly with respect to the use of Clark's pet procedural device, the summary judgment. Here, as I suspect is often the case, Clark's academic interests and commitments hobbled his understanding in ways that his collection of everyday views of criminal law did not hobble his understanding of criminal proceedings in the federal courts.

It is wonderfully ironic, as Professor Fred Konefsky has pointed out to me, that Clark's search for a better technique of fact finding to support activist reform led him to a tradition that, through its own primary concerns with being scientific, acted as a drag on the very activism that led Clark to seek it out.
not confused at all. They were squarely within but one tradition, that of progressive reform, and their reaction to the federal courts study proved it. When, as in the criminal report, the study simply had knowledge to supply, when it was irrelevant, if not potentially undermining, to the cause of reform as the liberal reformers conceived of it, they were hostile; when, as in the civil report, it supported liberal reform proposals, they were pleased.295 In so doing they reacted quite similarly to the way Clark had initially reacted to the enlarged Connecticut courts study, but they could not, and would not, react as Clark had ultimately done. They were completely outside the social science tradition; thus, the fact that Clark's fact gathering was a "scientific" enterprise made no difference. Fact gathering that did not advance an immediate reform objective was scholarship not worth publishing, just as fact gathering that did not fit their model of how the world was structured was an "irrelevant jumble of figures." They would give or withhold their support for the newer empirical research in law just as they had for the older research.

Thus, the origins of empirical legal research in the progressive tradition of searching for the facts that would provide the basis for reform partly impeded the research enterprise it had generated. Because the heirs of the old progressive reformers wished the newer research to continue to fuel reform as the old research had done, by providing the facts indicated by their model of the way the world was structured, they refused to extend plainly expected and needed support to the newer research, unless their wishes were met. Deprived of support from these obvious allies, Clark and others who wished to do empirical legal research might have turned to the emerging social scientific community and its tradition for support; however, an attempt to gain support from that community presented its own problems.

B. Business Failures and Auto Accidents

The establishment of the Institute of Human Relations brought a small influx of social scientists to Yale. Each was at-

295. Further evidence of this proposition, if any be needed, can be found in Edmund Morgan's review of the Connecticut courts study, which began with the observation that the "results can hardly be called startling or even of prime importance" and finished with the assertion that, as there was no "interpretation" of the data, the "chief contribution" of the book was as an "exposition of a method of investigation," which showed its "deficiencies and excellencies." Morgan, Book Review, 51 HARv. L. Rev. 1133 (1938).
attached to a regular academic unit in the university as a result of a decision, designed to wed the university's schools and departments more closely to the Institute, that no one, not even persons whose major occupation would be Institute-sponsored research, would be solely a member of the Institute's staff. Two of these social scientists—Dorothy Swaine Thomas and Emma Corstvet—were attached to the law school. Both were recruited by Donald Slesinger, who acted as "Hutchins' headhunter" and secretary of the Institute. Formally both would seem to have been more at home intellectually in the department of economics, sociology and government. Unfortunately, that department would have no part of the Institute or its personnel, despite the fact that one of the department's members had been put on the Institute's executive committee in order to foster departmental cooperation, so they were assigned to the law school. Yet even in such circumstances both were genuinely quite pleased with their new associations. They lived together with two other similarly minded women and were together known as the "four graces" or the "71 Dwight Street crowd"—so named for their residence, a gathering place for junior faculty. All were fascinated by statistics; on the basis of their training and the emerging statistical ethos in many areas of social science, they both advocated and participated in the statistically quantifiable social science that was plainly the new wave of the time. There, however, the similarity ended.

296. May, A Retrospective View of the Institute of Human Relations at Yale, 6 BEHAVIOR SCI. NOTES 141, 143 (1971). This piece, written by the long-time director of the Institute, is the only history of it. It is strongest in showing what the Institute accomplished, but weakest when depicting how difficult accomplishment was.

297. The phrase is that of Dorothy Thomas, interview, June 3, 1975. Emma Corstvet concurs that this was Slesinger's role, interview Aug. 19, 1975. The three people are connected by their work at the Laura Spellman Rockefeller Memorial. See note 102 supra; notes 303 infra.

298. Interview with Robert M. Hutchins, June 20, 1975; Charles Seymour to James Rowland Angell, Mar. 15, 1928, Angell papers. The department was the private preserve of the shade of William Graham Sumner and run by his protege A. G. Keller, both of whom thought empirical social research of a quantifiable nature was somehow anathema. See Oberschall, The Institutionalization of American Sociology, in The Establishment of Empirical Sociology, 187, 222 (A. Oberschall ed. 1972). Cf. Interview with Mark May, June 9, 1975 ("Hutchins would have taken a two bit whore.").


300. Interview with Donald Slesinger, July 8, 1975.


Of the two, Dorothy Thomas was the more formally educated. She came with a reputation as a methodologist of real sophistication, based largely on her Ph.D. dissertation, a statistical inquiry into the social consequences of business cycles that she did under a pioneer English statistician, but also based on her subsequent attempt to establish techniques for increasing the reliability of observations of human behavior. Emma Corstvet, on the other hand, came with impeccable progressive credentials and a vital interest in social problems, accompanied by a then still fashionable anti-degree bias and decent training in statistics. Thomas was the first hired; she came as a consultant in summer 1929 to help decide what the Institute should be doing and stayed on as a part-time research associate and then as a full-time associate professor, starting in fall 1930. Corstvet, lacking equiva-


304. Interview with Emma Corstvet Llewellyn, Aug. 19, 1975. Emma Corstvet Llewellyn, b. 1898. B.A. 1918, University of Wisconsin, study at Bryn Mawr, 1918-19 (economics, statistics), University of London, School of Economics, 1923-24. While an undergraduate she took courses from Commons and Tawney and worked for the Wisconsin Industrial Commission. As a result of both experiences she became interested in the causes of poverty, a subject she pursued in graduate school. In between her two stints of what she termed "desultory" graduate study, she worked in Wisconsin and then tried Paris for a while. After returning from London, she worked for the Laura Spellman Rockefeller Memorial, first as a translator and later in criminological studies. From there, in late 1927, she went to Peking with a job as a teacher in a school that had closed by the time she arrived. In order to remain in China she worked on a newspaper until 1929, when after another stay in Paris, she returned to the United States.

305. Institute of Human Relations, Executive Committee, Minutes, June 11, 1929, Angell papers (she will map out "existing statistical data" and preview "methodological difficulties" in studies). Her report, Thomas, A Survey of Some Materials Relevant to the Development of a Social Science Program, Sept. 15, 1929, Angell papers, acknowledged the "tremendous value" of "path-finding studies, depending on the analysis of behavior records, life histories, etc.," but strongly pushed the use of statistical methods, especially new ones designed to get "fundamental quantitative data on behavior and social milieu."

306. Clark had a bit of trouble securing faculty approval for the appointment because Thomas was female. See Charles E. Clark to Robert M. Hutchins, Oct. 4, 1929, Hutchins papers.
lent degrees, was first hired in late fall 1929 as a full-time research associate. Each was put to work on an existing project: Thomas on William O. Douglas' business failures project, Corstvet on Charles E. Clark's study of the compensation received by auto accident victims. The work on these two projects by these two women illustrates the problems faced by Clark and others when seeking support for the doing of empirical legal research by engaging in overtly collaborative efforts with members of the social science community.

Douglas' project was older and farther along when social science assistance in the form of Dorothy Thomas arrived.\(^7\) Douglas, who had taught bankruptcy while he was a part-time instructor at Columbia, thought up the project in summer 1928 right after he moved from Columbia to Yale.\(^8\) The original conception, announced in September 1928, was grand; its object was taken to be an inquiry into the "functioning of the whole credit system of the country."\(^9\) This inquiry was to take as its "point of departure administration of the bankruptcy laws."\(^10\) Douglas isolated two initial targets for the inquiry: the efficiency of the various methods of liquidating and salvaging a business and the extent of fraudulent practices in bankruptcy.\(^11\) Although naming these two targets was the only specification Douglas could give of "the actualities of bankruptcy administration which in the past have been engulfed in so much misunderstanding and doubt as to bring the whole system at times under suspicion and disrespect,"\(^12\)

\(^7\) Douglas' investigations have been briefly chronicled once before. Hopkirk, *The Influence of Legal Realism on William O. Douglas*, in *ESSAYS ON THE AMERICAN CONSTITUTION* 69-75 (G. Dietz ed. 1964).

\(^8\) On Douglas' move from Columbia to Yale, see W. DOUGLAS, *Go East, Young Man* 161-63 (1974), an account that reflects a certain amount of license in its composition while it quite accurately captures the flavor of events. For example, Douglas suggests that he was offered a job at Yale the morning after he first met Hutchins, that his proposed salary at Chicago was to be $25,000, and that he was offered the Deanship of the Yale Law School in 1937. *Id.* at 163, 164, 281. All are possible but none are likely. The first is doubtful, see Yale Minutes, May 10, 1928, May 18, 1928, May 31, 1928; Robert M. Hutchins to James R. Angell, May 24, 1928, Angell papers. The second is equally so, see note 231 supra. The third is even more doubtful since the deanship wasn't open at the time, as Clark was reappointed to a second five-year term in spring 1934, and the faculty minutes contain no evidence of any such offers having been made. On the other hand, Douglas' stories about the intoxicated Thurman Arnold or a rather stuffy Charles Clark dealing with a student caught with a girl in his room, whether accurate or not, capture their subjects beautifully. W. DOUGLAS, *supra*, at 167, 171.

\(^9\) Address by W. O. Douglas, 3 J. NAT'L A. REF. BANKR. 48, 49 (1928).

\(^10\) *Id.*

\(^11\) *Id.* at 49.

\(^12\) *Id.* at 50. Douglas gave no more evidence for the assertion that "the whole system" had at times been "under suspicion and disrespect" than Hutchins and Clark had given two years before, see text accompanying note 46 *supra*, or the auto accident investigators would give three years later, see note 378 *infra* & accompanying text.
he did know what resources would be required in order to make his investigation: three years and $60,000 to pay for the expenses of two roving teams— one of economists "looking at the facts from the economic, business and social angle" and one of lawyers "looking at them from the legal, administrative angle."  

Where Douglas got the idea for the project is unclear, although it bears certain family resemblances to the original Hutchins-Clark Connecticut courts proposal first funded not six months earlier, and a hint of an idea first put forth in the Columbia curriculum study. For some reason, funding was expected from the Social Science Research Council for work that was to have begun in the Southern District of New York. The funding, however, did not appear. What did appear was the United States Department of Commerce, which expressed interest in Douglas' project, apparently because it fit into the Department's own "national retail credit survey," begun in 1928 at the "request of an organization of retail merchants" and already in progress in two cities. As a result of the Department's interest, the business failures project began not in New York, but in Philadelphia, with a study of the causes of failure of retail grocers.

The size of the Philadelphia inquiry made in spring 1929 was small—35 grocers. Law students acting as dollar-a-year special agents of the Department of Commerce did the work of examining court records and interviewing bankrupts. Given the size of the unscientifically chosen sample and the rather cavalier use of even such rudimentary notions as an "average," only in the loosest sense was the study scientific. Yet from an examination of

313. Douglas, supra note 309, at 50.
314. See text accompanying notes 43-46 supra.
315. Underhill Moore, Douglas, and Douglas' friend Carrol Shanks, acting as the Committee on Business Unit, had suggested that a group consisting of "a statistician, an accountant, several specialists in business, and a number in law" do general research on the subject of business associations, a course that was apparently to include bankruptcy. Currie, The Materials of Law Study (pt. 3), 8 J. LEGAL EDUC. I, 23 (1955).
317. W. PLUMMER & P. RITTER, U.S. DEP'T OF COMMERCE, TRADE INFORMATION BULL. No. 700, CREDIT EXTENSION AND CAUSES OF FAILURE AMONG PHILADELPHIA GROCERS ii (1929). Earlier a study had been begun in Louisville. See U.S. DEP'T OF COMMERCE, TRADE INFORMATION BULL. No. 627, CREDIT EXTENSIONS AND BUSINESS FAILURES (1929). Paul O. Ritter, one of the joint authors of this study, had worked on it for credit as one of Douglas' research assistants during spring 1929, the last semester of his third year of law school. Paul O. Ritter to John Henry Schlegel, June 5, 1977.
this sample the investigators concluded that credit losses had contributed to the failure of only a few of these businesses, while "unscientific business practices" and "losses in real estate investment and speculation" had contributed to failure in many more of the cases.\(^{320}\) They also found, with some surprise, that despite adherence to a principle of creditor control of insolvency proceedings, most of the bankruptcies returned little to the creditors, that attorneys' fees ate up much of the limited assets available for payments to creditors, and that the entire process took a relatively long time.\(^{321}\) It was a start.

While the Philadelphia study was underway, Douglas' project got the first of two unexpected boosts when in spring 1929 a bankruptcy scandal broke out in the Southern District of New York.\(^{322}\) Several lawyers and some district court personnel were indicted for bribery, subornation of perjury, and conspiracy to defraud creditors in a grand jury investigation that had disclosed widespread filing of collusive or solicited voluntary and involuntary bankruptcies.\(^{323}\) The grand jury report accompanying the indictments suggested that a thorough investigation of bankruptcy administration in the district be made, and the United States Attorney, acting on that suggestion, petitioned the District Court to institute such an investigation.\(^{324}\) The District Court chose one of its members, Thomas D. Thatcher, to hold the investigation, and he invited the major local bar associations to participate.\(^{325}\) With the consent of the United States Attorney, the associations were ordered to conduct the investigation under the direction of "their counsel," Col. William J. Donovan.\(^{326}\) From the beginning, the investigation was directed not only at the corrupt practices discovered in the grand jury investigations, but also at reform of

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321. Id. at 10.
323. Donovan, supra note 322, at 79-81.
324. Id. at 64-65 (Order of Judge Thatcher, Mar. 22, 1929).
bankruptcy administration generally, and thus of the Bankruptcy Act. As part of this effort at bankruptcy law reform, Douglas was asked to prepare a comparative study of bankruptcy administration in the United States, England, Canada, France, and Germany, and an analysis of the various rules adopted by district courts for administering the Bankruptcy Act. Douglas read most of the study as his "testimony" before Judge Thatcher. There were two principal findings of his study. First, English and Canadian practice often limited discharge when American practice did not, particularly in cases of the use of business funds for speculative or other nonbusiness purposes, and in cases of failure to keep adequate business records. Second, English and Canadian practice seemed on the whole more efficient, more businesslike, and less creditor-controlled than American practice.

Although the purest of library research, Douglas' study for the Donovan investigation advanced the business failures project in two ways. First, its principal findings fit in nicely with the findings in the Philadelphia grocers study. In Philadelphia, Douglas discovered the existence of objectionable practices by debtors that the English and Canadian statutes suggested might be corrected with legal controls. Similarly the inefficiencies in bankruptcy administration that Douglas found seemed to be avoided in England and Canada. Second, Douglas' research gained him the appreciation of Col. Donovan and a certain amount of public recognition among those persons interested in bankruptcy reform. Among those persons was William Clark, a United States District Judge for the District of New Jersey. He gave the business failures project its second boost when he convinced Douglas and the Department of Commerce to move their cooperative investigation to Newark and to expand it from grocers to all bankrupts, including wage earners.

While Douglas' project was getting these twin boosts, he began another branch of his projected study of the efficiency of the


328. Record, supra note 327, at 28, 33-36.
329. Id. at 38-39, 48-49, 52-54, 59-63.
various devices for liquidating or salvaging a business, with an investigation of the court records of the equity receiverships brought in the Federal District Court in Connecticut. This project and the New Jersey project, together with the results of the Philadelphia study, were sufficient to secure the agreement of the Institute of Human Relations to fund the business failures project generally, even though "as a matter of policy it was thought unwise to take over a school or department project and put it on the Institute budget." After a year’s worth of work, Douglas had finally secured relatively stable financing.

With financing thus secured, Douglas took time to re-examine his conception of the project as a whole as he prepared his first written report on it. This time he abandoned the notion that his project was in any sense a study of the whole credit system of the country; it was a study of business failures pure and simple. And with a year's experience he could specify a little better what he meant by a study of business failures. He contemplated three kinds of studies: one of the causes of business failures, the Newark study; another of "the efficiency of the administrative machinery employed in reorganizing or liquidating" a business, the equity receivership study plus parts of the Newark study; and a third of "the incidences of the [business] failure as measured by the effect on the owners, the creditors, the employees and other groups in the community," a study not yet begun.

Douglas accompanied this restatement of his project with a long report on a completed portion of the enterprise, a study of the forty-four equity receiverships instituted in the United States District Court for the District of Connecticut in the previous ten years. The report drew no real conclusions but emphasized the high cost of fees paid to a seemingly inordinate number of officials—receivers, attorneys, appraisers, and the like—participat-

332. Institute of Human Relations, Executive Committee, Minutes, Oct. 7, 1929, IHR files. How early the contemporary world of university research funding does appear!
333. Equity Receiverships, supra note 331, at 1.
334. Id. at 2.
335. Id. at 3.
336. I detect here the influence of Dorothy Thomas. The tone of the entire report, except for one item discussed at note 339 infra, is the same as that of the earlier Philadelphia study, but no conclusions are drawn. Douglas had met Thomas in early summer 1929 (Donald Slesinger to James A. Angell, June 7, 1929, IHR files); she worked part-time at Yale during fall 1929.
ing in the proceedings and the tendency of these officials, other than those attorneys acting as such, to be untrained for their duties.\textsuperscript{337} He also noted that although the object of an equity receivership was to avoid the liquidation of the business that would occur in bankruptcy,\textsuperscript{338} most of the receiverships undertaken had headed or seemed to be headed toward liquidation anyway.\textsuperscript{339}

For some reason Douglas never followed up on the equity receiverships study directly;\textsuperscript{340} instead he turned his energies toward the Newark study of the "causes" of business failures. The key to this study was to be a "clinic" at which by court order all bankrupts would have to appear and be examined by project investigators administering a loosely structured questionnaire, "evolved through conferences with judges, lawyers, economists, psychologists, sociologists and physicians," that was designed to isolate what commonsensically might be called the "causes" of business failures—what bankrupts' business practices had been and how they had used available funds before insolvency.\textsuperscript{341}

At this point Dorothy Thomas began working at Yale. When she surveyed the project she concluded that methodologically it was "all mixed up" and set to work to "clean up" the project and "cut out" the ineffective parts.\textsuperscript{342} Her initial efforts in this direction can be seen in an article that she wrote with Judge Clark and Douglas.\textsuperscript{343} The article outlined the procedures that were to be followed in the Newark study and then flatly stated that the effort could be seen only as "an immediate path-finding approach to an

\textsuperscript{337} Equity Receiverships, supra note 331, at 12-25.
\textsuperscript{338} Provisions allowing the reorganization of a corporation in bankruptcy were not added to the Bankruptcy Act until 1934. Act of June 7, 1934, Pub. L. No. 296, 48 Stat. 911.
\textsuperscript{339} Equity Receiverships, supra note 331, at 4-8. Douglas did hazard one observation that went beyond the suggestions based on his data. Speaking of the decision to reorganize or to liquidate, he stated:
Another factor would be the effect of immediate liquidation on employees as well as on creditors and stockholders. To be sure the employees do not have an investment in the business in the legal or popular sense. Yet their association with the business has given them a prospective income from the business as measured by the labor turnover which is as certain as the bondholders' prospective interest or the stockholders' prospect of dividends.
\textit{Id.} at 8-9. That idea has yet to find explicit recognition in the bankruptcy law although at times, as in the Lockheed bailout or the Penn Central reorganization, the interest of employees does seem to surface in decisions made outside of statutory confines.
\textsuperscript{340} The study of the activities of investors protective committees in reorganizations, \textit{see note 442 infra}, was an indirect outgrowth of the kind of concerns Douglas had voiced in this study.
\textsuperscript{341} Business Failures I, supra note 330, at 1015, 1016.
\textsuperscript{342} Interview with Dorothy Swaine Thomas, June 3, 1975.
\textsuperscript{343} Business Failures I, supra note 330. The article reads as if she were the major author.
untouched field” designed to “indicate possible causative factors.” Thereafter effort would be needed to develop “more adequate techniques for controlling errors” and to produce data that would permit “inferences as to the causal connection of these various factors with bankruptcy.” The study was merely preparatory for two reasons. First, it was only going to collect data from and about bankrupts, yet causation could be inferred only on the basis of comparable data about bankrupts and nonbankrupts. Second, although the method chosen for administering the questionnaire minimized several known problems with questionnaire studies generally, the survey would produce only a very limited amount of information of high reliability, and the method chosen to check the accuracy and completeness of much of the data obtained—cross examination of the debtor by the investigator—would not likely yield data suitable for statistical treatment.

Unluckily, after only fifty-eight bankrupts were interviewed, “political problems” forced the closing of the bankruptcy clinic. Rather than abandon the work in Newark, Douglas chose to collect his data by relying on some personal interviews with bankrupts and on questionnaires mailed to them with a cover letter either from the court or from the investigators. He thus secured responses to his questionnaire from about half of a pool of nearly 1,300 bankrupts in the district. From a methodological point of view, however, the results he obtained were pure chaos. The only thing that could be salvaged was material helpful for “the development of a new [questionnaire] . . . in which most of the obvious sources of unreliability have been eliminated.”

344. Id. at 1019.
345. Id.
346. Id. at 1019-20.
347. Id. at 1020-24.
348. Thomas, Some Aspects of Socio-Legal Research at Yale, 37 Am. J. Soc. 213, 217 (1931). Dorothy Thomas did not remember what those problems had been (Interview with Dorothy Swaine Thomas, June 3, 1975); nor did Paul O. Ritter or Saul Richard Gamer, two of Douglas’ assistants. Persistent rumors suggesting that the questionnaire was outrageous, for example, that it included questions on the toilet training of bankrupts, are false, as an examination of copies of the questionnaire in the Angell papers shows. I suspect that efforts to begin to develop physical and psychiatric examinations of bankrupts and to measure their intelligence—see Douglas and Thomas, The Business Failures Project II; An Analysis of Methods of Investigation, 40 Yale L.J. 1034, 1036 (1931) [hereinafter cited as Business Failures II]—made the two investigators too “hot” for Judge Clark to handle.
349. Business Failures II, supra note 348, at 1035. When the study was completed Douglas had collected 91 personal interviews, 359 responses to the court’s letter, and 90 to the investigator’s letter. Id.
350. Id.
Thus Douglas and Thomas produced an article, admittedly "somewhat pedantic," that examined two things in considerable detail.\textsuperscript{351} First, it described the refinement of the questionnaire in order to assure the "definiteness and completeness" of answers given and thereby ensure their representativeness;\textsuperscript{352} second, it described the variations in the data secured by the various methods in order to "indicate the extent to which the [questionnaire]... itself was at fault in its failure to elicit definite replies."\textsuperscript{353} After a second year of work Douglas had finally developed a reliable research procedure, although it was not one designed to find out what he wanted to know—the causes of bankruptcy.\textsuperscript{354}

In fall 1930 the revised questionnaire was used in Boston "because of the interest and cooperation of the Hon. Thomas D. Thatcher," newly resigned from the district court bench and appointed Solicitor General and director of a special Department of Justice study of bankruptcy administration.\textsuperscript{355} This time the questionnaire was administered by the bankruptcy referee in the regular examination of about 910 out of some 2,900 bankrupts filing petitions during the fiscal year ending June 30, 1931.\textsuperscript{356} Preliminary results indicated that under the new procedures the revised questionnaire was working significantly better than the one used in Newark; it was producing "definite and complete answers to an extent that [would]... make frequency tables more truly representative of the groups studied."\textsuperscript{357} Progress was being made.

Although the Boston study was as incapable of pinpointing the "causes" of bankruptcy as the Newark study, the results from the joint effort of Douglas and Thomas to refine the question-
naire were not conceived of as wholly methodological. The two expected that the data generated would supply material for additional articles: one based on the data from both Newark and Boston, "indicating the frequency of certain presumably causal factors in the production of bankruptcy," and another describing an attempt to develop "a method of investigating these factors in non-bankrupt cases."\footnote{Douglas worked on the first of these articles during the summer and fall of 1931. When he was finished he had two articles, not one.} Curiously neither of Douglas' articles bore much relation to what might have been expected on the basis of the announced joint expectations. True, something like frequency tables appeared in both articles, and in neither did Douglas explicitly make the elementary mistake that Thomas had cautioned against—inferring causation from a study that did not include a control group—but the articles were only incidentally about the studies. They were about bankruptcy law reform, a topic that had heated up in the summer of 1930 with President Hoover's message on the subject announcing the beginning of Thatcher's study, which was in fact being run by Lloyd K. Garrison, one of Col. Donovan's assistants in the earlier New York investigation in which Douglas had testified.\footnote{In each article Douglas' argument was the same basic moral one: there are circumstances—such as inadequate accounting records, speculation, gambling, gross extravagance, or previous failures—that actually occur, as shown by the raw data from the Newark and Boston studies—in which discharge should not be allowed or in which the bankruptcy court should have the discretionary power, unfettered by notions of creditor control, to condition or suspend discharge, presumably in order to force the debtor to pay his debts out of after-acquired earnings.} Similarly, there are actual practices—such as lack of interest in the election of the trustee and inadequate examination of either bankrupts or applications for discharge—that are inconsistent with the assumed pur-
pose of the Bankruptcy Act. In each article the remedy for those evils was to look to the English bankruptcy act that Douglas had analyzed for the Donovan investigation a year or so earlier. And so Douglas proposed a series of technical amendments to the Bankruptcy Act, designed to better effectuate its assumed purposes and to deal directly with the occasions for granting of discharge when he thought circumstances made an absolute discharge undesirable.

Even as phrased, Douglas' argument was at least implicitly causal. It implied that a change in the statute would cause a defined change in the behavior of businessmen and officials, unfortunately a causality that the very limited data Douglas chose to present on the operation of the English bankruptcy act seemed to belie. But the argument was at least more sophisticated than the arguments made in the Philadelphia grocers study. That too was progress.

In two years' time Dorothy Thomas had made some small impact on the way empirical legal research was done at Yale. Similarly Douglas' arguments made some impact on the movement for bankruptcy law reform. When the attorney general submitted his bill for the reform of the bankruptcy laws, based in part on the study done by Solicitor General Thatcher, the accompanying report thanked Douglas for his help, and the bill included most of Douglas' suggested changes. But despite this progress

362. See Factual Study, supra note 359, at 25-26, 36-37 (examples).
365. See Functional Aspects, supra note 356, at 343-52; Factual Study, supra note 359, at 37. The articles together suggest that the ability to condition discharge in cases of speculation and gambling does not lead to a decline in those kinds of behavior, for Douglas' American bankrupts seemed to engage in less objectionable behavior than the more carefully regulated English bankrupts.
366. See STRENGTHENING OF PROCEDURE IN THE JUDICIAL SYSTEM, S. Doc. No. 65, 72d Cong., 1st Sess. 4, 45 (1932) (thanking Yale Law School and William O. Douglas). Thatcher advocated conditional and suspended discharges in a provision tied directly to the objectionable actions by bankrupts that Douglas had isolated, compulsory examination of bankrupts, and compulsory hearings on discharge applications. Less related to the Newark and Boston studies were proposals to limit creditors' control in the selection of the trustee, to validate and facilitate certain kinds of assignments for the benefit of creditors, and to provide for amortization of the debts of wage earners.

A proposal to establish a mechanism for corporate reorganizations outside the traditional equity receivership was obviously related to the study of equity receiverships in Connecticut. Throughout the report there were references to the data gathered by Douglas in Newark and Boston. See, e.g., id. at 101, 157. In one sense Douglas' articles on the Boston study were presented in support of Thatcher's proposed reforms since they were published after Thatcher's report was issued, though they do not mention that report.
and despite published promises of more articles based on further studies\textsuperscript{367} and private assurances that a book was forthcoming,\textsuperscript{368} no further studies were made by Douglas,\textsuperscript{369} no book written. The business failures project was over.\textsuperscript{370} Of its stated subjects of inquiry, the first, the causes of failure, had progressed only as far as learning the incidence of some of the purported causes of failure; the second, the efficiency of administration, had proceeded only far enough to make some simple observations based on the original forty-four equity receiverships, and on the indications of creditor interest disclosed in Newark and Boston; and the third, the effect of bankruptcy on others, had never even been started.\textsuperscript{371}

Like the business failures project, the auto compensation study can be traced back to Robert Hutchins, who announced in February 1929 that he had been talking about doing a study of the extent and impact of injuries from auto accidents with members of the Committee to Study Compensation for Auto Accidents.\textsuperscript{372} This committee, chaired by Arthur Ballantine\textsuperscript{373} and including several prominent New York and Philadelphia lawyers and judges, was organized in late fall 1928 by Ballantine and

\textsuperscript{367} Business Failures II, supra note 348, at 1035.
\textsuperscript{368} William O. Douglas to Robert M. Hutchins, Apr. 15, 1932; Feb. 2, 1933, Hutchins papers.
\textsuperscript{369} One related study was made at a later date, though not by Douglas. See Corstvet, Inadequate Bookkeeping as a Factor in Business Failure, 45 Yale L.J. 1201 (1936), discussed in text accompanying notes 416-18 infra.
\textsuperscript{370} The Department of Commerce ultimately published results of the Boston Study. V. SADD & R. WILLIAMS, U.S. DEP'T OF COMMERCE, CAUSES OF COMMERCIAL BANKRUPTCIES (Domestic Commerce Series No. 69, 1932); V. SADD & R. WILLIAMS, U.S. DEP'T OF COMMERCE, CAUSES OF BANKRUPTCIES AMONG CONSUMERS (Domestic Commerce Series No. 82, 1935) (most frequent causes of bankruptcies are "extravagance" and "evasion" of judgment debts, including those on foreclosed real estate). Douglas used some of the Newark and Boston results in Wage Earner Bankruptcies--State vs. Federal Control, 42 Yale L.J. 591 (1933), an article parallel in its use of the data to Functional Aspects, supra note 356, and Factual Study, supra note 359. See text accompanying notes 428-34 infra.
\textsuperscript{371} The following articles, related to the business failures project, were done by students of Douglas' at Yale. Gamer, On Comparing "Friendly Adjustment" and Bankruptcy, 16 Cornell L.Q. 35 (1930) (using New Jersey data); Furth, The Critical Period Before Bankruptcy, 41 Yale L.J. 853 (1932) (using New Jersey and Boston data to suggest that most bankrupts delay filing until well past the time of bankruptcy and in the interim dissipate their assets; includes interesting speculations on the reasons for variations in data). Douglas was the only one at Yale who managed to generate such student participation in the research enterprise (see articles cited note 432 infra) as is so common in contemporary social science, although Clark did something similar when publishing the early data from the Connecticut courts study. See articles cited note 182 supra.
\textsuperscript{372} Yale Minutes, Feb. 21, 1929. Douglas and Leon Green had suggested the desirability of such a study the previous fall. Id.
Shippen Lewis, who was to be director of the study.\textsuperscript{374} The faculty promptly voted to undertake "the principal work of investigation" on the subject, provided that money for it could be found.\textsuperscript{376}

When Clark picked up the project in summer 1929, after Hutchins had left for Chicago, the funding problem had been solved with a grant of $72,000 given to the committee by the Rockefeller Foundation, through the Columbia University Council for Research in the Social Sciences.\textsuperscript{376} Clark learned that Yale was to be a principal investigator; he also learned that he was to be a member of the committee, as well as of its executive committee.\textsuperscript{377} In that role he soon found out that the dominant members of the committee were convinced that a system of statutory compensation, like that under the workmen's compensation laws, was the only solution to the problems of personal hardship and court congestion caused by lawsuits involving the by then increasingly common automobile.\textsuperscript{378} The committee had already decided that its study was to have four parts: case studies of the nature and impact of auto accidents on individuals; court record studies of auto accident litigation; legal studies, largely directed at the constitutionality of a compensation system; and insurance studies designed to flesh out the details of the compensation system. Most of the work was split between Columbia and Yale.\textsuperscript{379} Columbia professors executed or supervised the third and fourth parts of the study. Clark supervised the court records studies—not an onerous task, given his experience in such enterprises. Who was do-

\textsuperscript{374} Committee to Study Compensation for Automobile Accidents, Report 2 (1932) [hereinafter cited as Auto Accidents]. In addition to Ballantine the New York Bar was represented by Henry W. Taft; Philadelphia, by Henry S. Drinker, Jr., and William A. Schnader.

\textsuperscript{375} Yale Minutes, Feb. 21, 1929.

\textsuperscript{376} Walter F. Dodd (a member of the committee) to Barry C. Smith (Commonwealth Foundation), June 1, 1929, Clark papers, Beinecke. Dodd was studying workmen's compensation for the Commonwealth Foundation, see note 153 supra.

\textsuperscript{377} The balance of the executive committee was Ballantine, Joseph P. Chamberlain and Noel T. Dowling, both of the Columbia Law School's faculty, and William Draper Lewis, executive director of the American Law Institute. During the progress of the study Dowling resigned, apparently concerned with the potential for conflict of interest since he was doing a study of the constitutionality of the legislation proposed by the committee.

\textsuperscript{378} The committee began its study in June, 1929. Auto Accidents, supra note 374, at 2. At that time its objective was to suggest the desirability of adopting a compensation system. Walter F. Dodd to Barry C. Smith, June 1, 1929, Clark papers, Beinecke. By December the committee was already beginning to make decisions about the scope of the compensation plan. Committee to Study Compensation for Automobile Accidents, Minutes of Executive Committee, Dec. 13, 1929, Clark papers, Beinecke.

\textsuperscript{379} Auto Accidents, supra note 374, at 5-5.
ing the “principal investigation” was thus hard to say, especially since Shippen Lewis personally supervised the case studies of injured individuals.

The various studies were begun in late fall 1929. Work on the court records studies was not time-consuming, at least in New Haven. Clark used previously compiled data from the Connecticut courts study; he did not bother to update or augment that data to include additional information collected for this study by workers in other jurisdictions. It cannot have been difficult at any of the other locations either, since the data sought—number of cases, number of plaintiff victories at trial, dollar amount of verdicts, and time from filing to trial—were quite limited and rudimentary.

Work on the case studies of injured persons initially presented no particular problems either. The general plan for this part of the research was to use personal interviews in order to develop illustrative case studies as well as statistical measures of the severity of injury, the amount of loss, and the amount and timing of compensation. The first studies were done in Philadelphia; others followed in Connecticut, a state with a financial responsibility law, and Massachusetts, a state with a compulsory insurance law. No single form of questionnaire was ever agreed upon. Rather, each researcher was told the general objective and the specific data needed and was left to develop his or her own questionnaire and data collection technique.

Unlike Dorothy Thomas who found Douglas’ study already going when she arrived, Emma Corstvet, hired just about the time work was to begin, was able to design her study from the start. In doing so she worked primarily with Shippen Lewis, not Clark. Given the object and the method of the study, she saw her problems much as had Dorothy Thomas: ensuring the accuracy and internal consistency of the data collected. Thus, great effort was spent in developing a questionnaire “that really worked” and in careful cross-checking of answers with other available sources of information such as hospitals, employers, attorneys, and charities. With the job of questionnaire development done,
Corstvet and her three assistants began field work in December 1929, which continued through September 1930. The case studies were obtained, without sampling, from the Department of Motor Vehicles records. When finished, they had together examined over 2,300 cases and made complete investigations of almost 1,200.

The results did not surprise any of the participants. Only about sixty percent of the victims received any compensation, although about thirty percent of them received compensation in excess of total expenses, including lost wages. Some slight tendency for the adequacy of compensation to be inversely related to the severity of the injury was also noted. Careful research disclosed that deficiencies in the amount and delays in the timing of compensation were met largely by postponing or defaulting on payment of medical expenses or by using savings, and that in severe cases, when family income declined significantly, substantial debts were incurred for current living expenses.

When the results of the various studies were compared it appeared that the results from Boston, located in the only state with a compulsory insurance law, were significantly different from those elsewhere. A victim in Boston appeared to have a better chance of receiving compensation equal to or substantially in excess of losses than victims elsewhere, even victims of insured drivers. Clark was excited; while advising caution, because figures were "limited," and after careful checking to avoid "grievous error," he noted that the committee was "at the threshold of perhaps the most important conclusions to be drawn from [its] . . . case studies," for as a matter of "first impression . . . Massachusetts law causes the difference." On the other hand, Corstvet, seconded by Dorothy Thomas, warned that worker bias or discrepancies in worker understanding of the object of research was likely to be a significant source of the differences. Indeed, upon

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386. Auto Accidents, supra note 374, at 256.
387. Emma Corstvet to Charles E. Clark, May 19, 1930, Clark papers, Beinecke.
388. Auto Accidents, supra note 374, at 257.
389. Id. at 76-90.
390. Compare id. at 80, with id. at 85. The results are more clearly set out in Corstvet, The Uncompensated Accident and its Consequences, 3 Law & Contemp. Prob. 466, 470 (1936).
393. Id.
394. Emma Corstvet to Charles E. Clark, Oct. 28, 1930; Dorothy Swaine Thomas to Shippen Lewis, Oct. 28, 1930, Clark papers, Beinecke. Dorothy Thomas acted as a statistical advisor for the committee. Auto Accidents, supra note 374, at 3.
careful inquiry it turned out that the worker in Boston had simply decided to include in her study only cases of "serious" injury.\footnote{395} So the Boston study had to be rerun in Worcester, where the results turned out to be different from those elsewhere, although not as extreme as in the initial study.\footnote{396}

While the social scientists were gathering their data and debating the reasons for their differing results, the lawyers were planning their report and worrying about money. From the beginning, despite the known preferences of other committee members, Clark advocated making the committee's product "a scientific investigation rather than a brief for a particular point of view."\footnote{397} Lewis, who was initially interested in using the case studies in a "fact brief of the Brandeis type," found that Clark's argument changed his mind; at least he agreed that the facts had to be "strong enough to speak for themselves."\footnote{398} However, Clark's opinion, even as strengthened by Lewis', did not carry much weight with the committee, which decided to put the statistical tables in an appendix and to present a Brandeis brief drawn from the case studies in a chapter in the text preceding the chapter interpreting the data in the appendix.\footnote{399}

At the same time Clark was pushing for a real "scientific investigation," the committee was running out of money. Within a year of receipt of the initial grant, Lewis was predicting that expenses would be $7,000 in excess of the $72,000 grant; five months later the estimated deficit had doubled.\footnote{400} Since the legal consultants had worked on a fixed fee basis, a significant portion of this shortage was due to the case studies and much of that could be attributed to Yale. Corstvet had caught the error that had

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\footnote{395. Shippen Lewis to Charles E. Clark, Nov. 6, 1930, Clark papers, Beinecke.}
\footnote{396. Auto Accidents, supra note 374, at 261, 273-75.}
\footnote{397. Charles E. Clark to Shippen Lewis, Mar. 8, 1930, Clark papers Beinecke. (Three quarters of the entire bulk of the book would be devoted to presenting facts without much in the way of interpretation.)}
\footnote{398. Shippen Lewis to the Executive Committee, Feb. 28, 1930; Shippen Lewis to Charles E. Clark, Mar. 10, 1930, Clark papers, Beinecke.}
\footnote{399. Committee to Study Compensation for Automobile Accidents, Minutes of the Executive Committee, Mar. 6, 1931, Clark papers, Beinecke.}
\footnote{400. Shippen Lewis to the Executive Committee, April 13, 1930; Oct. 14, 1930, Clark papers, Beinecke. The rather quick decline in the committee's financial fortunes and the tone of the entire enterprise is shown by the nature of and change in the physical format of the official record of its operations. In December 1929, its meetings were run from a printed agenda, complete with draft resolutions; its minutes were also printed. While the nature of the agenda and minutes remained the same, by May 1930, both were being typed.}
necessitated another study in Massachusetts, and her study had cost anywhere from fifty percent to six hundred percent more per case than any other. Although the Rockefeller Foundation picked up the deficit, Clark was both apologetic about Yale’s part in creating the deficit and a bit bitter because of the nature of the report that the committee finally chose to issue. In a letter to Lewis he defensively suggested that Yale had “tried to do this work thoroughly and carefully” and then observed that the cost had not been “excessive” when “compared with other research projects we have undertaken.” He noted that there had been a bit of conflict because at Yale “we have been most interested in the social and methodological implications of the study, while you have been most interested in the support which it gives to the Committee’s thesis.” And then he lamented that, as most sociological data would have to be left out of the report and would appear badly cut even in the appendix to which it was relegated, he and the Yale staff felt “as though we had worked hard in mining coal only to have most of it left at the mouth of the mine.” Lewis was hardly comforting when, in reply, he suggested that the effort had been worthwhile, for it showed that “a very careful study will reveal conditions similar to those revealed by less careful studies.”

The committee’s report was completed in December 1931 and published two months later. After being “extensively discussed,” it was forgotten to such an extent that a Yale Law School faculty member, hired only a year and a half after its publication and teaching in the field of torts, did not even know that Yale had had a major part in the research. Meanwhile, the social

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401. Curiously, Clark had indirectly warned Lewis about the problem that actually arose. Shippen Lewis to Charles E. Clark, Jan. 17, 1930 (we want to study “hardship cases”); Charles E. Clark to Shippen Lewis, Jan. 18, 1930 (going to be hard to decide what those are), Clark papers, Beinecke. Lewis nonetheless went ahead and used the term without further defining it.

402. Committee to Study Compensation for Automobile Accidents, Agenda, Feb. 18, 1931, Clark papers, Beinecke. Costs ranged from 85 cents per reported case in Indiana to $2.50 in Philadelphia, $4.00 in New York and $5.50 in Connecticut.

403. Id.


405. Shippen Lewis to Charles E. Clark, Oct. 16, 1931, Clark papers, Beinecke.

406. Committee to Study Compensation for Automobile Accidents, Minutes, Dec. 21, 1931, Clark papers, Beinecke. Clark did not bother to attend although up until this time he had attended regularly.


408. Charles E. Clark to Fleming James, Jr., Aug. 28, 1959, Clark papers, Law.
scientists, who felt that the effort at Yale had "laid the foundation for... a detailed social study," began that study at the Institute of Human Relations.\textsuperscript{409} Apparently it showed nothing that the earlier study had not also shown, for it was never published and thus could not even be forgotten.\textsuperscript{410}

Years later when asked why she had stopped working with members of Yale's law faculty, Dorothy Thomas, who at first was extremely happy with her new association,\textsuperscript{411} replied that it was because "nothing interesting" was going on.\textsuperscript{412} That was, of course, not the only reason,\textsuperscript{413} but within a year after the termination of the business failures project she severed all but her paper ties to the law school by giving up the seminar in empirical legal research that she taught there.\textsuperscript{414} Her reasons for this decision are significant; she felt that she was getting too few students, each of whom had too little time for research, and that she was presenting ideas about the importance of careful, statistical research for an understanding of social and legal institutions that were "clearly against certain of the prevailing dominant attitudes in the law school."\textsuperscript{415}

Emma Corstvet stayed active and interested in empirical legal research for several more years. During the year following the completion of the auto accident compensation study, she did an elegant study of the adequacy of accounting records kept by bankrupt

\textsuperscript{409} Charles E. Clark to Shippen Lewis, Oct. 15, 1931 (quoting Dorothy Thomas); Charles E. Clark to Emma Corstvet, Mar. 3, 1931 (start of IHR study), Clark papers, Beinecke. For details about this study, see T. Neely, A Study of Error in the Interview 119-40 (n.d.) (unpublished thesis in Butler Library, Columbia University).

\textsuperscript{410} One of the field workers on the initial study did manage to use the data from both to enrich a Ph.D. thesis that was otherwise drawn from secondary sources. See T. Neely, supra note 409. Several years later Emma Corstvet took the committee's results and turned them into a more poignant and effective plea for a compensation scheme than the committee had ever managed to make. See Corstvet, The Uncompensated Accident and Its Problems, 3 LAW & CONTEMP. PROB. 466 (1936).

\textsuperscript{411} Dorothy Swaine Thomas to James R. Angell, Nov. 25, 1930, Angell papers.

\textsuperscript{412} Interview with Dorothy Swaine Thomas, June 8, 1975.

\textsuperscript{413} About the time Dorothy Thomas came to Yale she met the Gunnar Myrdals and began a long term study of population migration and business cycles in Sweden. Interview with Dorothy Swaine Thomas, June 3, 1975. This work was a continuation of her Ph.D. thesis work on the same subject done with data on Britain, and the beginning of the major work of a career largely devoted to population studies. Thus, in retrospect, her work at Yale and at Columbia before that was really a detour from what proved to be her major interest. Indeed, in an autobiographical essay, Thomas, Contribution to the Herman Wold Festschrift in Scientists at Work: Festschrift in Honour of Herman Wold 216-23 (T. Dalenius, G. Karlsson & S. Malmquist eds. 1970), she does not even mention her connection with the law school.

\textsuperscript{414} Dorothy Swaine Thomas to Charles E. Clark, Mar. 9, 1933 ("I'll be glad to participate in other seminars, particularly those of Underhill Moore..."), Moore papers.

\textsuperscript{415} Id.
and going concerns.\textsuperscript{416} This was just the kind of study that Thomas thought should be the next part of the business failures project,\textsuperscript{417} but that Douglas never got around to doing. The results were hardly conclusive, but given the difficulty of defining and measuring adequacy and the limitations of the available statistical tools, Corstvet nevertheless managed to show that more going businesses than bankrupt ones had adequate records.\textsuperscript{418} After completing this study she became research assistant to Underhill Moore. She worked on Moore’s studies for nearly three years until family responsibilities, acquired when she married Karl Llewellyn who was still teaching at Columbia, necessitated her moving to New York City.\textsuperscript{419} Although for a while thereafter she commuted to New Haven for a few days a week, she too was through with empirical legal research.

Thomas turned her attention to population studies, an outgrowth of her interest in business cycles and her training in statistics; Corstvet turned hers to institutional economics, an outgrowth of her Wisconsin progressive education.\textsuperscript{420} Of the two, Corstvet had had the better experience in working with the lawyers, largely because she did some research herself and because Moore, the lawyer she worked most closely with, was more interested in scientific research than the rest of the Yale Realists. But, neither social scientist really looked back, and the Realists’ interest in empirical legal research was never to receive significant support from the social scientific community again.

As Dorothy Thomas and Emma Corstvet grew away from participation in empirical research at Yale, surely neither thought that she had been helped to leave her former activities by ten-
sions generated from the interaction of the social scientific tradition and the progressive reform tradition. Each simply found that other professional and personal interests and commitments were more satisfying and followed them. In personal terms that perception was correct, but here personal terms are misleading.

Both Thomas and Corstvet were squarely within the emerging social science tradition. As such, each had needs or objectives common to most of the young social scientists interested in empirical research at the time—accuracy of data collection, niceties of questionnaire design, and basic scientific predispositions. More important, though perhaps less obvious at the time, each also accepted the lengthening of the reform horizon that accompanied the growth of academic social science in America. In contrast, the lawyers in the supposedly joint research enterprise had an interest in procedural and remedial reform that was straight out of the progressive reform tradition. Ballantine’s committee acted out of a “felt need” to reform the mechanism for compensating the victims of auto accidents. That Douglas acted from a similar motive can be seen in his continued attempts to refine his notions about what he was going to investigate: he felt the need, but that was about all. In particular, both the committee and Douglas shared the same model of the world that their inquiry into the facts was directed at—court congestion, delay, procedural complexity, popular disrespect, and all that goes with the progressive notion of law reform.

Except for Clark, all of Ballantine’s committee was probably a part of the progressive law reform tradition alone. For the relationship of these men to the social science tradition, “tension” is probably too weak a word. By the early thirties the two traditions, historically offshoots of the same root, were simply discontinuous. The lawyers on Ballantine’s committee were not interested in, if they were even more than dimly aware of, the campaign to make social inquiry a science like the physical sciences. If the use of the best current methods—“careful selection of sampling, careful rules of questionnaires, for formal decisions on substitutions when parts of the sample were unavailable”

421. Id.
422. See text accompanying notes 50-52 & 290-91 supra.
brought good results, that was fine; but to be hobbled by method or the limits of available data, that was a different matter. Carefully collected data were less important than effective data. Facts were facts; for these lawyers, once enough of them were collected or the need for them had passed the enterprise was over. But for social scientists like Thomas and Corstvet, turning social inquiry into a science was a very important part of a developing professional identity that each participated in. Facts were not facts; some were meaningful and others doubtful. If the lawyers would let the exigencies of short-term campaigns for discrete reforms determine the nature and scope of social inquiry, if they would not attenuate their perception of the relationship between such inquiry and desired reforms, then there was no reason to continue the association. There were many other things to investigate, things that because of their susceptibility to methodological sophistication were truly more interesting.

Douglas and Clark presented a less clear-cut problem for Thomas and Corstvet. Clark sat insecurely in both traditions. Douglas' position was more ambiguous. His activities in the Federal Courts study suggest that he participated in, if not affirmed, some of the methodological aspects of the social science tradition. But the depth of his attachment to that tradition was another matter, as can be seen most vividly from his later activities in the campaign for bankruptcy reform.

Comprehensive bankruptcy reform was not to be had in the early 1930's, although eventually the provisions for wage earner amortization of debts that Douglas had urged were adopted. Yet when such amortizations were approved, Douglas was op-

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425. This was exactly the same attitude Clark faced in dealing with his Harvard critics who were part of the same progressive reform tradition.

426. Two of the most enchanting days I have ever spent were spent interviewing Dorothy Swaine Thomas and Emma Corstvet Llewellyn. The grace, charm, and wit of each plainly captivated me and has surely influenced my view of events at Yale in a way that the generally, though by no means universally, less interesting lawyer interviewees were simply unable to do. Professor Robert Gordon has suggested, quite accurately, I think, that one of these influences is an observable preference in this section for the rather "hard-edged" method that both women were working with in the twenties and thirties. But whatever the source, the preference is defensible. Lawyers are quite used to, and comfortable with, "soft" methods; it is very easy to drift from soft methods into the kind of arm-chair theorizing, really the reifying of one's biases, that is much of legal policy analysis today. While hard methods have their own problems, most notably the rigid constriction of the sphere of the knowable to that of the countable, given the acknowledged methodological biases of lawyers these problems are preferable to the risk that soft science will turn into no science at all.

427. See text accompanying notes 195 & 234 supra.

posed to them on the basis of "the facts" about bankruptcy, largely the same facts on which he had earlier supported amortization. Of course, in the intervening years Douglas had learned something new; he had learned the opinion of his colleague Wesley Sturges, as thoroughgoing a skeptic as the Realists ever produced, that the basic problem for wage earners was "credit management"—an inability to intelligently limit the use of credit to a reasonable estimate of the amount of future income—so that amortization of consumer debts was simply a sentence to future abject poverty. Douglas' "facts" could support that argument just as well as his earlier one in support of wage earner amortizations, for ultimately Douglas believed that his study had uncovered the causes of bankruptcy. Although he admitted that the determination of causation was "extremely hazardous," since cause and effect "curiously intermingle" and "appear differently to different observers," he was convinced that a "dominant characteristic" could be determined and, by implication, that from this "dominant characteristic" causal relationships could be inferred as a matter of something close to informed common sense. On the basis of these "dominant characteristics" and a comparison of the figures on the incomes of his bankrupts and some early attempts to define standard budgets for wage earners, Douglas concluded that amortization was impossible in most cases.
One can imagine Dorothy Thomas tearing her hair after learning of Douglas' argument. Whether his estimate of causality was right or wrong, as far as she was concerned Douglas went about his evaluation in the wrong way. Indeed, not only did he not follow the appropriate methodological rules, he reverted to the kind of arm-chair theorizing that methodological rules were designed to foreclose. Here the partial commitment of Douglas, a personal friend, to the social scientific tradition was in some ways more disturbing to deal with than the noncommitment of lawyers like those on Ballantine's committee. For the flip-flop from one tradition to another only emphasized the ways in which careful, statistical research was apart from the "prevailing dominant attitudes in the law school."

Finding the appropriate response to the less than total commitment of lawyers like Douglas to the social scientific tradition was difficult for young social scientists like Thomas and Corstvet who were active missionaries for their young discipline. But, in general, it was to gravitate toward the lawyers more committed to the social science tradition than most. Thus, three years later Corstvet tried to do research with Clark, who had so fervently defended the "value" of the "detailed work" she had done for the auto compensation study, and both she and Thomas would try to work with Underhill Moore who was more singlemindedly devoted to scientific method and who more fully accepted the lengthened reform horizon of academic social science than any of his colleagues. Yet, in the end, other things seemed more interesting and so, affirming the tradition of which they were solely a part, Thomas and Corstvet drifted away to investigate those other things.

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435. Interview with Dorothy Swaine Thomas, June 3, 1975.
436. See text accompanying notes 522-81 infra.
438. I will endeavor to prove this in "American Legal Realism and Empirical Social Science: The Singular Case of Underhill Moore" (forthcoming).
Whether this attempt at joint empirical legal research might have had any other conclusion is highly doubtful. Had the social scientists been regular full-time teaching faculty, perhaps their views would have carried more weight. Had the social science of the time been in a less prickly phase methodologically, the task of meeting the social scientists' norms might have been easier. But, even under the best of conditions the tension between the two traditions was probably too great for the lawyer reformers and the social scientists to have engaged in much joint research without each first substantially compromising its own tradition, as an incident that Dorothy Thomas related aptly illustrates. While at Yale she taught a seminar in empirical legal research. One semester, Thurman Arnold resolved to take it. He attended regularly until it came time for the students to present their research projects, at which point he objected to the student presentations on the ground that he had come to hear her, not them, and thereafter he stopped coming. Such insensitivity to the importance of student research for recruiting membership in the social scientific tradition may be understandable in someone as removed from that tradition as Arnold was, but however understandable, such actions by the lawyers that casually undercut efforts to sustain and expand that community and its traditions did not make it easier for individuals within that tradition to continue to work with persons outside it. With a whole world to investigate, it is hardly surprising that the social scientists chose to follow a path that made their work easier.

Thus the social scientists found unacceptable the unwillingness of the lawyers interested in empirical legal research to act in support of the methodological imperatives of the nascent social scientific discipline and would not provide continuing support for that research. Similarly, the sympathetic legal community, locked in the progressive reform tradition, found empirical legal research that was unrelated to its current reform interests irrelevant and thus, would not provide continuing support of such research. Perhaps lawyers like Clark and Douglas, still interested in empirical legal research but caught between these two traditions, might

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439. And surely the fact that Walton H. Hamilton, the one regular full-time faculty member whom everyone at Yale identified as a social scientist, was actively opposed to empirical legal research, see note 165 supra, meant that such weight as the views of Thomas and Corstvet would otherwise have been given was seriously diminished.

440. Professor Robert Gordon has supplied the unusually apt adjective.

441. Interview with Dorothy Swaine Thomas, June 3, 1975.

442. Clark's interest brought forth the bar survey. See text accompanying notes 522-81 infra. Douglas too would lead one more "empirical" research project before moving to
have been able to keep their enterprise going themselves. To do so it would have been essential to secure funds, a task made difficult by the Depression. The foundations had not seemed particularly eager to support empirical legal research, and at Yale general university funds were not to be had. But the Institute of Human Relations might have been tapped for funds. Why that did not happen is less a problem of relationships between lawyers and social scientists than of the peculiar nature of the law school’s relation to that unusual institution.

C. The Institute of Human Relations

For the short while that he remained at Yale after he lost his fight to locate the law school away from the college and near the Institute of Human Relations, Robert Hutchins began to turn his newly created opportunity for financing social science research into a way to draw the law school toward academic pursuits similar to those he thought would have followed from the change in location. He went about this task with his usual dispatch. Although he had been directed to plan the Institute’s program in the social sciences with Professor Edgar S. Furniss of the department of economics, sociology and government, and although a central topic for that program—the family—was adopted, it is hard to see any plan in what happened other than that of the law


443. Dorothy Thomas said that foundation funds were not particularly difficult to obtain during the Depression. Interview with Dorothy Swaine Thomas, June 3, 1975. Although she ought to have known, I can find no one who agrees with her. And even if she were correct, the law school thought funds were hard to obtain. See Report of Dean Clark, 1931-32, at 20.

444. Yale was very badly hit by the Depression. Charles E. Clark to Robert M. Hutchins, Mar. 15, 1933, Hutchins papers (“rumor” that Yale is in worse shape than elsewhere in country).

445. See James R. Angell to Edgar S. Furniss, Feb. 12, 1929, IHR Files.

446. Yale Minutes, Feb. 21, 1929. The reason behind the choice of topic eludes me. Donald Slesinger said it seemed “obvious at the time” (Interview with Donald Slesinger, July 8, 1975), but it is not obvious now, especially given the notion of exploring law as a mechanism of “social control,” see text accompanying note 161 supra, that animated at least some Realist thought.
school to collect for itself as much of the available money as possible. Hutchins’ collaborator, Donald Slesinger, was named executive secretary of the Institute. Slesinger found two social scientists who were studying juvenile delinquency in Boston and offered them money to extend their study to New Haven; he then hired Dorothy Thomas and Emma Corstvet. Simultaneously, Hutchins recruited Underhill Moore, whose interest in Yale was said to be “largely due to” the existence of the Institute, and whose empirical research was later financed by it. Hutchins also tried to get his entire faculty directly involved in planning the Institute’s activities. Together the two men were successful enough, or at least thought themselves successful enough, for Hutchins to predict that “sooner or later” the Institute would “supplement” the work of the law school “at every point.”

When Clark became dean, he continued Hutchins’ program. He began the school year with a series of discussions of Institute projects and at the same time secured funding for Douglas’ bankruptcy study. Thus, on paper at least, Hutchins and Clark were eminently successful both in welding the law school to the Institute and in securing Institute funds for research. The total funding of people and projects associated with the law school grew from over one-third of the social science budget in the first year of operations to over two-thirds just two years later, and at the same time the number of persons formally associated with

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447. See Yale Minutes, Mar. 21, 1929. The two were William Healy and Augusta F. Bronner. Their work eventually resulted in the publication of W. Healy & A. Bronner, New Light on Delinquency and Its Treatment (1937). They were chosen rather than Sheldon and Eleanor Glueck, who had by then begun their work at Harvard, because Slesinger found the Gluecks’ work “competent, but rather superficial and unimaginative.” Donald Slesinger to James R. Angell, May 30, 1929, IHR files.

448. See notes 297 & 303-04 supra.

449. See Underhill Moore to Robert M. Hutchins, Jan. 28, 1929; Robert M. Hutchins to Underhill Moore, Jan. 30, 1929, Moore papers; Yale Minutes, Mar. 29, 1929.

450. See Yale Minutes, Feb. 21, 1929; Feb. 28, 1929; Mar. 21, 1929.


452. See Yale Minutes, Oct. 3, 1929; Oct. 10, 1929; Nov. 11, 1929; Dec. 12, 1929.

453. See text accompanying note 332 supra.

454. Compare Institute of Human Relations, Budget, 1929-30, with Institute of Human Relations, Budget, 1931-32, Angell papers. There were limits, however; Hutchins reported that the Institute would not fund work in trade associations although it would agree to work on the subject if independent funding could be acquired. Yale Minutes, Feb. 28, 1929. Unintended side effects soon appeared, too; within a year of the Institute’s founding, Clark reported that Institute money for social science research would be hard to come by since it was almost all allocated. He did not mention that the allocation was largely to law school affiliated projects. Yale Minutes, Dec. 12, 1929.
both the law school and the Institute tripled—from four to twelve.\footnote{455}

From such events one might expect that when Clark needed funds to support his faculty's empirical legal research the Institute would have proven a fairly dependable source. But it turned out not to be; indeed, it soon was virtually no source of funds at all. How that happened, how Hutchins' goose virtually stopped laying any eggs at all, further illuminates the problems the Realists faced in their attempt to do empirical legal research at Yale.

Although the Rockefeller Foundation envisioned a program of cooperative research\footnote{456} into the medical, psychological, and social aspects of human behavior,\footnote{457} the terms of its grant did little to foster, and much to hinder, such a program. Stipulations in the grant specified that over three-quarters of the operating funds were to be devoted to the two existing foundation-supported projects—the Institute of Psychology and the Institute of Child Development—and one new one—the psychiatry department at the medical school.\footnote{458} Thus, any cooperative research at the Institute would require the coordination of financially independent research programs or the use of the less than one-quarter of the operating funds—$65,000 per year—left over for the "social sciences."\footnote{459}

Given the realities of the funding, concern was expressed almost immediately about the Institute's division into pieces.\footnote{460} Nevertheless, the Yale Corporation confirmed a compromise plan,
drafted in the fall, before the terms of the Rockefeller grant were known, for an executive committee to run the Institute. That plan only emphasized the separateness of the individual programs by seeing that each little interest group had its own representative on the committee. Thereafter the potential for actual cooperative research was further diminished by the de facto adoption of a budgetary process that gave the executive committee the task of coordinating research, but left it without any but hortatory tools to accomplish that task.

Several people tried to counteract these centrifugal forces in the Institute, at least as they affected the social sciences. Hutchins intended to plan a unified program for the social sciences. But that effort was never undertaken, and Clark never picked up on the idea. Angell tried to help by making it easier for members of social science departments to meet regularly on an informal but official basis, but even that attempt was unsuccessful.

Slesinger, who took a special interest in the social sciences at

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461. Minutes of Yale Corporation, Apr. 13, 1929. I infer that the arrangement was a compromise from Charles Seymour to James R. Angell, Oct. 30, 1928, Angell papers (reporting terms), and Donald Slesinger to Robert M. Hutchins, Nov. 29, 1929, Hutchins papers (referring to "a scrap similar to [the one over] the chairmanship last year").

462. May, supra note 458, at 148, reports that the membership consisted of the president (Angell); the provost (Seymour); the deans of the medical school (Winternitz), law school (Clark), and graduate school (Wilbur Cross, who was replaced in 1930 by Edgar Furniss); representatives of the department of economics, sociology and government (Furniss) and the Institute of Psychology (Angier); and the executive secretary (Slesinger, replaced in 1930 by Mark A. May). The group had been functioning even before the Corporation got around to giving its approval. See James R. Angell to Edgar S. Furniss, Feb. 13, 1929, IHR files.

463. Interview with Mark A. May, June 9, 1975. See Institute of Human Relations, Executive Committee, Minutes, Oct. 28, 1930, IHR files, (not the function of executive committee to "discuss or pass upon" items in individual research group budgets).

464. Robert M. Hutchins to Underhill Moore, Jan. 30, 1929, Moore papers. Hutchins intended to use Charles E. Merriam, a political scientist at Chicago; Wesley Mitchell, an economist at Columbia; Charles A. Beard, the historian; W. I. Thomas, the sociologist; and Lawrence K. Frank, an official of the Rockefeller Foundation, as his consultants. Moore suggested adding to the list Floyd Allport, a psychologist at Syracuse; Franz Boas, the anthropologist; and W.F. Ogburn, a sociologist at Chicago (Underhill Moore to Robert M. Hutchins, Jan. 31, 1929, Moore papers) and John Dewey (Underhill Moore to Robert M. Hutchins, Feb. 1, 1929, Moore papers).

465. Underhill Moore to Charles E. Clark, Apr. 29, 1929, Moore papers (reminding Clark of the planning proposal). Both Hutchins and Clark may have felt that the decision made in the interim to study "the family" obviated the need for the kind of planning originally thought appropriate.

466. See James R. Angell to Robert M. Hutchins, Feb. 18, 1929, IHR files (law school and department of economics, sociology and government to swap members of their Board of Permanent Officers). There is no record of any member of that department ever attending either a faculty or board of permanent officers meeting.
Yale and wanted to use the Institute to make them "unbeatable,"467 worked as hard as anyone to generate cooperative investigations,468 but after nearly a year of such work he became convinced that it would be impossible "to force mature workers into a cooperative program."469 Others expressed fears that the Institute would break into pieces,470 and at the turn of the year Slesinger declared that the Institute had already broken into three pieces.471 President Angell "vigorously" denied the assertion, while tacitly admitting its truth by predicting that an eventual "consolidation of the general mass" of research would take place.472 The university community added its opinion on the matter by dubbing the Institute's new building "the place with the kids, the kooks, and the apes,"473 thus recognizing the lack of relation among the Institute's dominant entities.

The potential of the naturally insular law school for involvement in the activities of the thus fragmented Institute was diminished by the fact that the quarrelsome Winternitz,474 and Clark, not only quarrelsome, but also incapable of ever forgetting a lost point,475 could not get along. As soon as Hutchins announced his

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467. Robert M. Hutchins to Donald Slesinger, Jan. 1, 1930, Hutchins papers.
468. See, e.g., Donald Slesinger to James R. Angell, May 31, 1929, IHR files.
469. Donald Slesinger to James R. Angell, Nov. 5, 1929, IHR files.
471. Donald Slesinger to James R. Angell, Jan. 16, 1930, IHR files. (Slesinger's letter of resignation).
473. Interview with Emma Corstvet Llewellyn, Aug. 19, 1975. The references are to Gesell's child development clinic, Winternitz's psychiatry facility, and the studies of anthropoid behavior by R.M. Yerkes of the Institute of Psychology. It is notable that the community never even noticed the social sciences.
474. Angier called Wintemitz "the stormy petrel," an oblique reference to his diminutive stature. Roswell P. Angier to James R. Angell, Oct. 29, 1929, Angell papers. Slesinger recounts his problems in dealing with Winternitz in Confidential Memorandum for Mr. Angell, Oct. 22, 1929, IHR files; and Donald Slesinger to Robert M. Hutchins, Nov. 29, 1929, Hutchins papers. Comments about Winternitz's quarrelsome were also made by Dorothy Swaine Thomas, interview, June 3, 1975. Mark A. May, interview, June 9, 1975, noted that Winternitz was given to the delightful practice of calling Protestants by Jewish names as well as the reverse.
475. Dorothy Thomas, interview, June 13, 1975, called Clark quarrelsome. The Angell papers are full of support for both this assertion and the notion that Clark even had trouble understanding when he had lost. See, e.g., Charles E. Clark to James R. Angell, Dec. 14, 1931 (cavil after losing fight over organization of the Institute); Thomas W. Farnam to James R. Angell, May 19, 1936, May 20, 1936, Angell papers (reporting a dispute between Clark and Moore). To add to these problems Clark was remarkably insecure and needed repeated reaffirmations of his value to the university. See, e.g., Charles E. Clark to James R. Angell, Feb. 4, 1932 (reporting offer to teach at Chicago); James R. Angell to Charles E. Clark, Feb. 5, 1932; Charles E. Clark to James R. Angell, Aug. 30, 1935 (reporting second offer to teach at Chicago); Charles E. Clark to James R. Angell, Nov. 6, 1935 (again); James R. Angell to Charles E. Clark, Nov. 8, 1935; Charles E. Clark
departure for Chicago, if not before, Winternitz began to act as if he ran the Institute. Clark, suspicious of Winternitz's motives, feared that he intended "to swallow us all." Hutchins advised Clark that Winternitz's style was pure bluster—"fight and he will go away"—and even offered his help in dealing with Winternitz. Yet, Clark so let the matter get out of hand that Winternitz felt free to attempt to control law school research projects and even to dispute the right of the law school to seek foundation funds for its research while the medical school was seeking to tap the same sources in a drive to increase its own endowment.

The result was predictable. In less than a year Clark, who had no personal tie to the Institute, began to lose interest in its affairs. Although he continued to attend executive committee meetings, he soon began to see the Institute as uninterested in "law projects" and to view its ostensible topic for joint investigation in the social sciences—the family—as "some flumdoodle" to be overcome when seeking funds from the Institute's coffers. About the same time, a potential alternative source of leadership in cementing the law school to the Institute was lost with Slesinger's departure and his replacement as executive director of the Institute by a member of the psychology department. And so, discussion of Institute affairs soon ceased at faculty meetings, and the Institute instead became the private concern of those who were associated with it.

In these circumstances leadership in establishing the law school's relationship to the Institute would have had to come from within the faculty generally. Since within a year or so after the Institute was founded twelve persons associated with the law

to James R. Angell, Jan. 1, 1936 (reporting offer to teach at Harvard); James R. Angell to Charles E. Clark, Jan. 16, 1936 (counseling the avoidance of "those who dwell in Egyptian darkness"), Angell papers.


Interview with Robert M. Hutchins, June 20, 1975.

477. Robert M. Hutchins to Charles E. Clark, Nov. 4, 1929, Hutchins papers.

478. Id. Winternitz was trying to control the juvenile delinquency project which at the time was admittedly being financed in part by non-social science funds. Donald Slesinger to Robert M. Hutchins, Nov. 29, 1929, Hutchins papers. Winternitz also tried to control space in the Institute's building. See James R. Angell to Milton C. Winternitz, June 5, 1930, IHR files.

479. Charles E. Clark to Underhill Moore, July 24, 1930, Moore papers.

480. Donald Slesinger to James R. Angell, Jan. 16, 1930, IHR files (resignation). The replacement was Mark A. May who was an educational psychologist.

481. There is no mention of the Institute in the faculty's minutes from January 1930 until November 1931:
school also had become associated with the Institute, such an alternative might have seemed plausible; however, a close look at those twelve persons suggests otherwise. Only six of the twelve were regular faculty members. Of these six, only Douglas and Moore were engaged in research funded by the Institute, but Douglas had virtually nothing to do with the Institute and Moore had little to do with the law school. Of the other six, three were research assistants who as such had limited entree into faculty life generally. Of the remaining three, the two social scientists whose study of delinquency absorbed one-third of the law school's part of the Institute's budget never were a part of the life of the school; indeed one of them was not even working in New Haven. Dorothy Thomas, who took up another third, advised Douglas and Moore, but most of the time she pursued her own program of research into techniques of observing human behavior, although she did teach one seminar at the law school and occasionally showed up at the weekly faculty meetings. There was little cement to these twelve individuals and thus the institution that was to supplement the work of the law school at every point was allowed to become something else, if not something less.

What that something else was, at least in the eyes of most of the law school faculty with an interest in social science research, seems to have been a source of funds and a reservoir for technical assistance, but little else. Here Douglas' actions with respect to the business failures project are illustrative. When the exigencies of the program for reforming bankruptcy law and practice made further research unnecessary, Douglas stopped doing bankruptcy research and effectively terminated his already limited association with the Institute and its activities. The Institute was for him a source of funds and not an entity for continuing cooperative

482. See lists in note 455 supra.
483. Borchard, Clark, Dodd, Douglas, Moore and Smith. Dodd's leave of absence to work on his study of Workmen's Compensation began in fall 1930, so he could not be of much help.
484. Corstvet, Samenow, and Sussman.
485. Slesinger, Confidential Memorandum for Mr. Angell, Oct. 22, 1929, IHR files (Healey never there).
486. See D. THOMAS, THE OBSERVABILITY OF SOCIAL PHENOMENA WITH RESPECT TO STATISTICAL ANALYSIS (1931). Why this project never resulted in experiments on the psychology of evidence escapes me, especially since Thomas explicitly noted the potential crossover in a memo circulated to the entire law faculty. Arnold, Smith & Thomas, Memo, Nov. 27, 1931, Moore papers.
487. See text accompanying note 414 supra.
488. See text accompanying notes 367-70, 428-34 supra.
research. Still other projects similarly illustrate the faculty's actual understanding of the Institute's limited role in the law school's research program; for example, the work of George Dession.\textsuperscript{489} Although many law school faculty members who were not then participating in a sponsored research project were invited to participate in the Institute's activities, only Dession used the Institute either for the stimulation of new associations or for help designing research. Dession, who was searching for an understanding of the psychological basis of criminal responsibility, invited himself to the Institute to learn what it had to offer.\textsuperscript{490} Yet his actions serve only to underline the law school's attitude toward the Institute. Dession went to the trouble to take courses in the medical school in order to learn about psychiatry.\textsuperscript{491} When choosing a collaborator he started with a locally well-known practicing psychiatrist.\textsuperscript{492} Then he became unhappy with the "objectivity" of this first expert,\textsuperscript{493} and so he found another, a social psychologist.\textsuperscript{494} That collaboration worked no better, and though Dession "returned" to teaching with his first collaborator,\textsuperscript{495} he gave up virtually all collaborative work, for what he wanted was not cooperative research but the "facts" about mental illness.\textsuperscript{496} He wished to make such use of the facts as he wanted, not to deal with an expert's idea of what those facts might mean.\textsuperscript{497}

\textsuperscript{489}. Dession (b. 1905, A.B. 1926, M.A. 1927, Cornell) was a 1930 graduate of the law school who stayed on as a teaching fellow.
\textsuperscript{490}. Dession had spent the last semester of his third year working as an assistant district attorney and during his first year he offered criminal law and a seminar called "The Legal, Psychological and Psychiatric Aspects of Crime" taught with Thurman Arnold and Eugene Kahn, the psychiatrist Winternitz found to head his psychiatry department. Although his course list indicates that he began work in criminal law and psychology almost immediately, and although Dorothy Thomas (interview, June 3, 1975) distinctly remembers his interest in empirical research in general and the Institute in particular, his name appears on no list of faculty participants in the Institute that I have been able to find.
\textsuperscript{491}. Yale Minutes, May 29, 1930.
\textsuperscript{492}. Dr. Eugene Kahn.
\textsuperscript{493}. See Charles E. Clark to James R. Angell, Nov. 21, 1931, Angell papers. Exactly what the problem was with Kahn I cannot learn. Hutchins described Kahn as a "total loss" whose acquisition was one of the two reasons for the Institute's failure, but did not explain why he thought this was so. Interview with Robert M. Hutchins, June 20, 1975.
\textsuperscript{494}. Edward S. Robinson, later Arnold's collaborator, see note 263 supra.
\textsuperscript{495}. I use "returned" advisedly. Dession never gave up teaching his seminars with Kahn but for a while tried working with Robinson in another seminar. Robinson seems to have been largely uninterested in criminal law. See generally E. ROBINSON, LAW AND THE LAWYERS (1985).
\textsuperscript{496}. This theme is recurrent in the memorials written about him after his death. See, e.g., Douglas, George Dession, 5 BUFFALO L. REV. 3 (1955); Fortas, George Hathaway Dession, 5 BUFFALO L. REV. 5 (1955).
\textsuperscript{497}. All of which is not meant to slight Dession's scholarship and impact on legal education. He was the virtual fountainhead of the law and psychiatry movement in the
Given the limited relationship to the Institute that the law school had defined for itself over time, the decline of the school's participation in the Institute's affairs might be seen as but a rerun of the attempts of Thomas and Corstvet to collaborate with the lawyers on research. Yet as such it was a rerun with a difference; Thomas and Corstvet left whatever joint research there had been only to continue their scientific work elsewhere. Clark, and to a lesser extent Douglas, did likewise. But when the lawyers finally realized they had given up any real connection to the Institute, they alone were left with the feeling that something had been lost; the other participants in the Institute continued as before. Clark described this loss as one of access to an important source of funding for empirical research and ascribed it to a reorganization of the Institute's executive committee in fall 1931. But an examination of that event and its consequences suggests a different source for the sense of loss.

When the proposal to reorganize the executive committee with an eye to better coordinating the Institute's researches, especially in the social sciences, was first aired, Clark, who had not even been included in the initial discussions, not only did not complain about the slight, but even suggested that termination of the law school's "tenuous" connection with the Institute might be an easy way to solve the problem of coordination of social science research. Yet, as soon as he and the faculty heard that their

law schools. Through his work with Richard C. Donnelly, he is one of the men who made R. DONNELLY, J. GOLDSTEIN, & R. SCHWARTZ, CRIMINAL LAW (1962) and J. KATZ, J. GOLDSTEIN, & A. DERSHOWITZ, PSYCHOANALYSIS, PSYCHIATRY & LAW (1967), possible. His articles still have an unusually contemporary ring. See Dession, Psychiatry and the Condition of Criminal Justice, 47 YALE L.J. 319 (1938); Dession, The Technique of Public Order: Evolving Concepts of Criminal Law, 5 BUFFALO L. REv. 22 (1955). Nevertheless he was not interested in cooperative research, but rather in reform of the criminal law. And for reform he wanted facts. The major impact on Dession was not the field of psychiatry, but rather the person of Harold Lasswell, who provided him with an understanding of the legal system that supported the values Dession felt were lacking in the criminal law, as can be seen from a comparison of the two articles cited above.

499. Charles E. Clark to James R. Angell, Nov. 10, 1931, Angell papers. For three reasons I infer that the discussion was begun by President Angell. First, it was he who had first articulated a coherent notion of what the Institute as an entity might be, and when he did so, in his final proposal to the Rockefeller Foundation, his notion centered on cooperative research into human behavior. See text accompanying note 136 supra. Second, Angell hardly was known for the forceful imposition of his ideas on other people. It was characteristic of him not to have insisted on implementation of his preference for such research by the Institute at its outset, but rather to hope that coordinated research would come about naturally and, if not, to move cautiously to bring it about. See KELLY, supra note 28, at 971; Interview with Robert M. Hutchins, June 20, 1975. Third, the plan of reorganization that was developed advanced his ideas by creating a single executive
commitment to, and capacity for, empirical research had been challenged, and learned that, as a consequence of the reorganization, Clark would lose his place on the committee and a member of a competing department would assume control over research money for a new single topic of research in the social sciences, and they began to fight. First came the charge that the law school was being pushed out of the Institute and then a virtual demand that, because the law school's collection of research projects was "of such importance and value," it should be made "the nucleus for the future research" in the social sciences. Both pro-

in the social sciences who could foster the desired cooperation, yet was not an overly forceful change in the way business was done at the Institute. See note 501 infra.

Why anyone thought that the lack of cooperative research was a "problem" escapes me. There seems to have been no pressure applied by the Rockefeller Foundation. Conceivably, Dean Furniss used Angell's interest in such research to advance the interests of his department, which had been largely cut out of the Institute, primarily because its chairman lacked sympathy for the newer quantitative social science methods. See note 298 supra & accompanying text. Furniss, one of the younger men in the department, evidently was more interested in such methods and in the modernization of his own branch of the department—economics. The Institute was plainly a potential vehicle for advancing both interests, just as the Institute of Psychology had advanced similar interests of President Angell and others when it was established eight years earlier. See note 119 supra. The difficulty with this speculation is that Furniss' impact on the form of the reorganization and the Institute generally seems to have been marginal. The only other possible source of the notion that the lack of cooperative research was a problem, other than Angell, is Mark A. May. On the basis of my interview with him, I seriously doubt that May could have moved Angell on the matter if Angell had not wanted to be moved.

500. Yale Minutes, Nov. 12, 1931 (reporting remarks by Dean Winternitz at a public, nonuniversity gathering to the effect that it had been a mistake to include the law school in the plan of the Institute and that the law school's faculty lacked the ability to direct or even engage in research projects). These deliberately provocative remarks had their intended effect; the faculty became furious at the public affront and concerned about the truth of the matter stated. Id. Although Winternitz later denied making the remarks, see Yale Minutes, Nov. 19, 1931, throughout the discussion about reorganizing the Institute there is a recurring sense of hurt pride on the part of Clark and his faculty that made rational discussion difficult. See, e.g., Charles E. Clark to James R. Angell, Nov. 21, 1931, Angell papers. I suspect that in the end this sense of hurt pride made it more difficult for the faculty to participate in the research at the reorganized Institute.

501. The plan called for Furniss, by then Dean of the Graduate School, to direct activities in the social sciences, including psychology, and for Dean Winternitz to direct all of the psychiatric research including child development. They, together with Angell and May, would form the executive committee. See May, supra note 458, at 148; Interview with Mark A. May, June 9, 1975.

502. See Charles E. Clark to James R. Angell, Nov. 18, 1931, Angell papers. Yale Minutes, Nov. 12, 1931; Nov. 19, 1931 (reporting appointment of committee on research consisting of Douglas, Arnold, Moore, Smith and Thomas).

503. Charles E. Clark to James R. Angell, Nov. 18, 1931, Angell papers.

504. Charles E. Clark to James R. Angell, Nov. 30, 1931, Angell papers. The letter was based on a report submitted by an ad hoc committee on research that was apparently largely written by Dorothy Thomas. The report reviewed the law school's research activities, suggested several extensions of those activities, noted that while the Institute provided "the most direct avenue" for developing the law school's research, such work could also be done outside the Institute, and suggested that the choice to work inside or outside the Institute turned on whether the law school program could "become a
tests and demands were, however, quite deftly dismissed by President Angell, who allowed the reorganization to proceed as planned.

The law school's response to the threat of administrative reorganization, a response complete with an ad hoc emergency committee on research and numerous, interminable decanal letters, was definitely exaggerated in view of the limited use the law school had made of the Institute's resources. While that exaggeration might possibly be attributed to a real threat of a loss of funds for empirical research, the later history of the law school's relation to the Institute shows that this was not the case. It took a year for the reorganized executive committee to select a new subject for joint study. By that time the juvenile delinquency study was

center for the development of the Social Sciences at the Institute." It proposed a research program for the entire social science group focused on "the interrelation between governmental policy . . . with the habit formation and behavior reactions of the people." Arnold, Smith & Thomas, Memo, Nov. 27, 1931, Moore papers.

505. Angell deflected the first charge in James R. Angell to Charles E. Clark, Nov. 19, 1931, Angell papers (expressing "puzzled surprise" at Clark's assertions). Clark responded by inviting Angell to speak at the next law school faculty meeting in a letter that contained several very truculent swipes at the medical school and revealed exactly how deeply Winternitz's remarks had wounded the faculty's pride, when it noted that the faculty was "properly nervous" that its "capacity for original research" was being "doubted." Charles E. Clark to James R. Angell, Nov. 21, 1931, Angell papers. Angell reacted with an unusual expression of irritation and suggested that Clark rid his mind of the "hobgoblins which seem to have their home in the Medical School." James R. Angell to Charles E. Clark, Nov. 24, 1931, Angell papers. Clark, unable ever to let anyone get the last word, responded emphasizing exactly how insecure the law school was by first, dragging out assertions from alumni that the stress on social science research was causing Yale to allow students to be graduated who could not pass the New York bar and then, seeking assurance that the research activities "most dear and satisfactory to us" were viewed with "official approval." Charles E. Clark to James R. Angell, Nov. 24, 1931, Angell papers.

Angell deflected the second at a regular law school faculty meeting. Yale Minutes, Dec. 4, 1931. In the interim, Clark's new proposal had been effectively undercut by Mark May, who suggested that it was nothing less than an attempt to take over the social science program of the Institute and thus would both alienate other potential participants and undermine the notion of cooperative research. Mark A. May to James R. Angell, Dec. 2, 1931, Angell papers. His bluff called, Clark tried an ingenious strategic retreat, but the effect was unconvincing, when he suggested that in fact the law school's plan was the one May desired but was unable to recognize. Charles E. Clark to James R. Angell, Dec. 2, 1931, Angell papers.


507. See note 504 supra.

508. See, e.g., Charles E. Clark to James R. Angell, Nov. 18, 1931; Nov. 21, 1931; Nov. 24, 1931; Nov. 30, 1931, Angell papers. Clark generally managed to produce two letters to every one from Angell; the timing of them suggests that he usually composed his two page, single spaced replies immediately upon receipt of Angell's letters.

509. Institute of Human Relations, Executive Committee, Minutes, Nov. 7, 1932, IHR files. The subject chosen was the City of New Haven. Mark May had suggested the program at the time of the reorganization dispute. Mark A. May to James R. Angell, Nov. 9, 1931, Angell papers. At that time Angell suggested the need to "stress . . . not
Douglas had lost interest in bankruptcy, and with it his need for the Institute's funding; and Dorothy Thomas had lost interest in the law school. In the interim no faculty member had sought funds for a new project. And in the succeeding years only Underhill Moore chose to seek such funds; he had little difficulty redirecting his research to fit in with the new order and was able to participate generally in the affairs of the Institute.

Despite protestations to the contrary, therefore, the reorganization made no financial difference to the law school's empirical research activities; the source of the very real sense of loss must be found elsewhere. In searching for that elsewhere a distinction between source and occasion is helpful. The occasion for first recognizing the sense of loss was without a doubt the reorganization. After that event the Institute was no longer felt to be a ready, if recalcitrant, source of funds. Thus the lawyers thought that they had lost something in the reorganization. But the real source of the sense of loss was their choice, made back in 1929–30, to treat the Institute as a repository of facts and funds and little else, and thus to affirm generally their relation to the progressive reform tradition. They effectively cut the school off from the scientific enterprise that for the rest of its participants, all part of one or another of the scientific traditions, was at the core of the notion of the Institute. The exaggerated response of the law school to the reorganization plan therefore masked the embarrassment of having the actuality of the school's relationship to the Institute pub-
licly disclosed by being publicly acted upon.\footnote{514} Such public disclosure undercut the school's pretensions, dating back at least to Hutchins' selection as dean, to a "scientific" tradition, and did so in front of, and with the tacit approval of, the very man—President Angell—whom that scientific tradition was intended to impress.\footnote{515} The sense of loss was thus not that of a loss of funds but that of a loss of part of the school's claim to science, in turn a part of its claims to being a distinctive part of the law school world generally, and to being an integral part of the university's emerging scientific community.\footnote{516}

To identify the law school's real loss as a result of its limited relationship to the Institute is not to deny the reality of its perception of the source of that loss. If anything, identification of the real loss heightens the force of the perceived one as an explanation of the difference between the attempt to collaborate with Thomas and Corstvet and the rerun of that attempt. The feeling that the law school had lost a source of funds came at the height of the Depression when funds seemed scarce generally,\footnote{517} and were in fact scarce for the kind of projects Clark and Douglas had engaged in. Such a feeling was debilitating when it came to thinking of starting any of the new projects that had been floated among the faculty,\footnote{518} and doubly so when that feeling was inextricably bound up with a sense that the pretentions of being actively engaged in the doing of science had been undercut. Now that the enormous real costs and effort associated with even small empirical research projects were known, the addition of other debilitating features to them did not encourage new beginnings. How much more empirical research would have been done at the law school had the faculty become a part of the Institute instead of drifting away from it is admittedly hard to quantify.\footnote{519}

\footnote{514} See Charles E. Clark to James R. Angell, Nov. 18, 1931; Nov. 21, 1931, Angell papers.
\footnote{515} See text following note 89 \textit{supra}.
\footnote{516} See Charles E. Clark to James R. Angell, Nov. 24, 1931, Angell papers.
\footnote{517} See, \textit{e.g.}, Report of Dean Clark, 1930-31, at 30; Report of Dean Clark, 1931-32, at 6, 19, 20. Dorothy Thomas doubted the truth of Clark's assertions (Interview, June 3, 1975), but the important matter is that Clark thought it was true.
\footnote{518} See Arnold, Smith & Thomas, \textit{supra} note 504. The suggestions included evidence studies, studies of the relationship between taxation and saving, and studies of state Blue Sky laws.
\footnote{519} This is especially true in view of the later history of the Institute. The social scientists did not exactly head straight for cooperative research among themselves. The decision to study New Haven was not a success, and in 1936 a further reorganization made Mark May director and created an executive committee of university officers and Dean Winternitz. Two years later membership was again changed, this time to include
The recognition that the law school was not part of the Institute by no means killed the lawyers' interest in their understanding of cooperative research or their interest in empirical research. The most fruitful of all the cooperative research between lawyers and social scientists was barely begun, and Clark would begin yet another bit of empirical research. But the reorganization had changed the way in which both enterprises were to be carried out. Cooperative research would continue to be undertaken on the law school's terms and would thus reflect both the progressive reform tradition and the social science tradition in proportions that the lawyers would determine. Empirical research would be harder to begin, even for Clark who was so obviously dedicated to it and relatively swift to adopt its principles. Now, cut off from the support of both the social scientists and the old-fashioned progressive reformers, and from the money of the Institute, Clark turned to that now great, but then new, source of funds—the federal government.

D. The Bar Survey

During the debate over the reorganization of the Institute of Human Relations, Charles Clark, in closing one of his numerous letters to President Angell, vowed "to endeavor so far as lies in my power to secure means for the continuance of our work . . . , if not from the Institute, then in whatever manner may be possible." At the time he had some reason for his note of dogged determination combined with the feeling that the task would not be easy: within the past month he had finally succeeded in getting the Rockefeller Foundation to release enough funds to allow completion of a truncated version of the federal courts study. But it was two full years before Clark's determination was at all tested.

mostly Institute staff members. The 1936 reorganization was accompanied by a decision to create a centrally administered research fund to be used to develop inter-disciplinary research. The 1938 reorganization reflected the emergence of the desired program of research from common interests of a group of young psychologists working with Clark, Hull and a group of similarly minded young anthropologists, May, supra note 458, at 148-50. Thus ultimately Slesinger was right when he suggested that cooperative research would not come from older workers. See Donald Slesinger to James R. Angell, Jan. 16, 1930, IHR files.

520. Arnold had just met Edward S. Robinson. See Charles E. Clark to James R. Angell, Nov. 21, 1931, Angell papers. Their joint seminar, The Judicial Process from the Point of View of Social Psychology, was first given in fall 1932. For the results of this collaboration see note 263 supra.

521. See text accompanying notes 522-87 infra.


523. See text accompanying note 233 supra.
In the interim, Clark struggled to get the Federal Courts project finished and to keep his faculty happy as the Depression cut into Yale's resources and the lure of jobs in the fledgling Roosevelt administration made that faculty somewhat footloose.\footnote{524.} One of the rewards for these efforts at Yale was his election as president of the Association of American Law Schools for the year 1933, an honor, at least at that time, passed from hand-to-hand among the deans of the major law schools.\footnote{525.} The job apparently amounted to little more than the opportunity to see that the program of the December annual meeting of the Association was to one's liking. Clark did just that; he produced a program, heavy with comment by his friends,\footnote{526.} that reflected current controversies in jurisprudence\footnote{527.} and a current concern about the legal profession's societal obligations growing out of claims that the economic distress of the bar caused by the Depression had brought forth shoddy practices among practitioners.\footnote{528.} Clark's rather rambling, nondescript presidential address emphasized this second theme and suggested that the A.A.L.S. sponsor a "careful study of the bar's functioning" in order to learn whether the bar was in fact meeting its obligations.\footnote{529.} With the help of his friend

\footnote{524. As a preview of what was to come, during the summer of 1933 Arnold and Sturges worked for Jerome Frank at the Department of Agriculture. Abe Fortas, who was to start teaching in fall 1933, went with Arnold and Sturges and got so involved in his work that he stayed in Washington for fall semester, too. And J. Howard Marshall, the assistant dean, who had made himself an expert on petroleum production, went to Washington to serve as a member of the Petroleum Administration Board and stayed for the entire year. Report of Dean Clark, 1933-34, at 10, 11, 12.}

\footnote{525. Leon Green, interview, June 19, 1975 (referring to the practice as "the deans' conspiracy").}

\footnote{526. Robert Hutchins, Leon Green, and Karl Llewellyn all spoke. 1933 A.A.L.S. HANDBOOK 135, 136.}

\footnote{527. Roscoe Pound, Jerome Frank, and Judge Joseph C. Hutcheson spoke at a symposium, "Modern Trends in Jurisprudence." Id.}

\footnote{528. The source of the notion that the Depression had brought forth shoddy professional practices, one of the numerous ways of saying that there were too many lawyers and too little legal business, is a mystery. Clark first asserted it in spring 1932. Report of Dean Clark, 1931-32, at 5. Of course, in one sense the source of this idea is as unimportant as is the precise source of the ideas in the Hutchins-Clark procedure proposal, for the notion that the bar is overcrowded and that overcrowding is leading to shoddy professional practices is as ubiquitous as the notion that procedural reform is the appropriate cure for popular dissatisfaction with the law. J. AUERBACH, UNEQUAL JUSTICE: LAWYERS AND SOCIAL CHANGE IN MODERN AMERICA (1976) chronicles the persistent complaints about overcrowding over the last hundred years and locates their cause in attempts by elite lawyers to preserve their status in the profession. While this explanation is partial in that it ignores the similarly virulent complaints by low-status lawyers that cannot be accounted for as manipulations of opinion by elite lawyers, it is plainly correct.}

\footnote{529. Clark, Law Professor, What Now?, in 1933 A.A.L.S. HANDBOOK 14, 21. Curiously, despite the fact that contemporary jurisprudence was on the program, not a soul mentioned the relationship of Clark's proposal to the then already well-worn controversies over realism.}
Karl Llewellyn, Clark had the project referred to the Association’s Committee on Cooperation with the Bench and Bar, a group that at the time was a resting place for former presidents of the Association, and which Clark had managed to get himself selected as the chairman of for 1934.  

In suggesting a survey of the actual functioning of the bar, Clark was working in largely uncharted territory, although he had a model—a recently completed survey of the medical profession.  

The idea for such a survey was hardly original with Clark; it had possibly been suggested to him by Karl Llewellyn, who knew that in spring 1932 a committee of the New York County Lawyer’s Association, largely in the person of the indefatigable, if slightly crank-like, Isidor Lazarus, had proposed a questionnaire study of the income from, and nature of, the practice of the legal profession in that city. Other people had the idea, too. In late fall 1932, the California Bar Association surveyed recently admitted attorneys. A year later, the University of Wisconsin Law School was given about thirty recent graduates to do research as part of the Civil Works Administration project there. Professor Lloyd Garrison managed to capture seven of

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530. 1933 A.A.L.S. HANDBOOK 124-25. The Committee on Cooperation was a perfect vehicle for Clark’s proposal. It was a ten year old enterprise of Professor Joseph Beale of Harvard. Beale, acting as the Association’s floor leader, had urged its creation as a nearly euphoric response to the presidential address of William Draper Lewis, director of the then newly formed American Law Institute, who had suggested that the law teachers were now a coequal branch of the profession with the duty of cooperation with the other two branches—the bench and bar—in the improvement of the law. 1924 A.A.L.S. HANDBOOK 87-89, 97. The committee had never really done anything, despite Beale’s continuing efforts. First it had tried to get up a juristic center; then it had branched out into surveying developments in legal education, only to be chastised for stepping on the toes of the Committee on the Curriculum. 1933 A.A.L.S. HANDBOOK 138-39 (recounting history). Thus in 1933 it needed something to do.


532. Cf. Karl N. Llewellyn to Charles E. Clark, May 3, 1934, Clark papers, Beinecke; Llewellyn, Remarks in 1934 A.A.L.S. HANDBOOK 83-88. There are many Lazarus letters in the Clark papers, Beinecke. For a slightly more accessible example of the temper of the man, see Lazarus, Separate Concurring Statement to American Bar Association, Special Committee on the Economic Condition of the Bar, Report 6-7, bound with AMERICAN BAR ASSOCIATION, SPECIAL COMMITTEE ON THE ECONOMIC CONDITION OF THE BAR, THE ECONOMICS OF THE LEGAL PROFESSION (1938) [hereinafter cited as THE ECONOMICS OF THE LEGAL PROFESSION]. Lazarus describes his survey, which in fact was not begun until fall 1934, id. at 155-77.

533. THE ECONOMICS OF THE LEGAL PROFESSION, supra note 532, at 190-93.


535. The same Garrison who participated in Donovan’s, and ran Thatcher’s, bankruptcy investigations.
these graduates and set them to work gathering data, largely from
the Wisconsin state income tax records, then open to public in-
spection, on the history of the Wisconsin bar and the income of its
members from 1927 to 1932. Using these data he attempted to
compare the growth of the bar with the economic growth of the
state and hoped to determine whether the bar was "overcrowded" as
had been alleged in some quarters.

Clark knew he wanted to follow neither Garrison's study nor
the California or New York questionnaire surveys. He had already
done two studies based on accumulated records and knew the
limits to such an effort first hand; he had learned from Dorothy
Thomas the limits to the reliability of data drawn from mailed
questionnaires. Given what he wanted to know, he would have to
do large personal interviews. But the costs of a national survey by
personal interview, even of a carefully chosen sample of the bar,
much less of the general public, was well beyond the ability of
the A.A.L.S. to finance. So Clark set off to find the necessary funds.

Yale plainly did not have the money, and even a cursory
inquiry disclosed that none of the major foundations was inter-
rested in the project either; so Clark, perhaps influenced by
Garrison's windfall, turned to the federal government, specifi-
cally the Federal Employment Relief Administration (FERA). Armed
with an appropriate letter of introduction, Clark traveled
off to Washington. There he presented the rather simple argu-
ment that getting the facts about the economic condition of the
bar would lead to the improvement of its condition in the short
run, through employment in the survey, and in the long run as
well. But although his project was met with "the greatest of in-

536. Garrison, supra note 534, at 59, 60.
537. Id. at 59. Garrison also planned to study the correlation between success in
law school and monetary success in practice. Id. His results were published as Garrison,
A Survey of the Wisconsin Bar, 1935 Wis. L. Rev. 131. They showed first, that during
the early years of the depression, although the increase in the number of lawyers was
proportionally greater than for other professions, income levels remained higher than
that of doctors who were general practitioners; second, that during the period 1880 to
1930 the increase in the number of lawyers was proportionately less than almost any
index of either legal or economic activity; and third, that students who obtained better
grades in law school tended to have higher incomes in practice.
538. See note 444 supra.
539. See, e.g., Charles E. Clark to Frederick P. Keppel (Carnegie), June 22, 1934,
Clark papers, Beinecke.
541. See Charles E. Clark to Jacob E. Baker (Asst. Adm., FERA), May 15, 1934,
Clark papers, Beinecke (summary of meeting). Clark planned to use A.A.L.S. member
schools to do the local administration of the survey together with a central staff to do
the tabulation and to write the report. He envisioned interviewing lawyers only aiming
to learn the number of lawyers, type of practice, and the kinds of services performed.
Clark came home with no money, for he was informed, first, that there was no money available at the time; second, that even if money were available it was not available directly to individuals, or even universities, but only through state and local employment relief agencies; and third, that even such state and local agencies could fund only projects for which there was a “working procedure” approved by FERA. Undaunted by what must then have seemed a particularly onerous obstacle course because of the assurance that money would eventually be available, Clark set out to get approval for the project through the issuance of an acceptable working procedure. Another trip to Washington, many letters, and six months later FERA produced a one and one-half page working procedure. After one year’s worth of work, Clark still had no money.

In the process of negotiating with FERA, Clark had been forced to define exactly what he intended to do. That process involved a considerable advance on Garrison’s study and on the California and New York County surveys. Clark’s field workers were to interview a sample of lawyers with an instrument directed largely at the nature of their practice and their income from it and at the same time interview a sample of the general public with an instrument directed at learning the amount of potential legal business, particularly of a preventive nature, in the community. And, by borrowing Emma Corstvet from her work at the Institute on Underhill Moore’s parking studies, he had managed to get a good preliminary draft of the questionnaire for lawyers.

542. Clark, supra note 540, at 89. The quote is Clark’s own words. He did recognize that that interest might well have been momentary, as it turned out to be. Five months later Baker had forgotten about the project, although when his recollection was refreshed he was again “quite enthusiastic.” Charles E. Clark to Edson R. Sunderland (prof. Michigan, Committee member), Oct. 28, 1934, Clark papers, Beinecke.

543. Clark, supra note 540, at 89.

544. See Charles E. Clark to Jacob Baker, Sept. 4, 1934, Clark papers, Beinecke.

545. See Charles E. Clark to Karl N. Llewellyn, Sept. 24, 1934, id. (can you come to meeting Oct. 5, 1934?).

546. The working procedure is reproduced in Committee on Cooperation with the Bench and Bar, Report, in 1934 A.A.L.S. HANDBOOK 183-84. Baker actually worked faster than the gross elapsed time would suggest. Once he agreed to sponsor the project, he prepared a draft work procedure in two weeks and then held up issuing the approved procedure for over a month and a half to let Clark have a chance to seek foundation funds for a central office staff to direct the study. See Jacob Baker to Charles E. Clark, Oct. 18, 1934; Oct. 31, 1934; Nov. 13, 1934, Clark papers, Beinecke. See note 551 infra & accompanying text.

547. Clark, supra note 540, at 91. See also Karl N. Llewellyn to Jacob Baker, Oct. 19, 1934, copy in possession of author (includes sketch of entire project). The idea to expand the study to include interviews with potential clients was first made in Karl N. Llewellyn to Jacob Baker, Oct. 19, 1934, Clark papers, Beinecke.

Unfortunately, Clark's careful planning also disclosed a new problem. From the beginning the studies were to have been conducted locally.\textsuperscript{549} This fact meant that Clark would not only have to convince friends at other schools to carry out the research, but he would also have to establish an office to coordinate the several local studies and do the composite analysis of the results. Clark knew from his experience in the federal courts project that once a successful pilot project was completed, it was easy to generate interest from other academics, so he did not expect any difficulty securing law school cooperation. Based on the same experience he did not expect any problem securing federal funding for the central office either, but he soon learned that such funding was extremely doubtful.\textsuperscript{550} So again Clark turned to the foundations\textsuperscript{551} and then followed up on the suggestion that the Connecticut State Employment Relief Agency might use its federal funds to support the central office.\textsuperscript{552} But the best Clark could do anywhere was to determine that successful completion of a pilot project would likely make it easier to obtain funding for any central planning and evaluation office to coordinate the project.\textsuperscript{553}

No matter which side of his problem Clark chose, it seemed as if Yale would have to run a pilot study to generate any movement on the project. So, after a short stop at the 1934 A.A.L.S. convention, where bar surveys were one of the two major topics on the program, Clark filed his application for his field workers and their supervisor with the Connecticut FERA office, under the working procedure he had largely written.\textsuperscript{554} Indeed, through

\textsuperscript{549} See note 541 supra. Originally this organization was adopted in order to increase convenience and maximize the involvement of A.A.L.S. member schools. In fact it was necessary given the structure of the FERA program, which had been decentralized in order to build local support, although Clark did not learn this fact until his second trip to Washington. See Jacob Baker to Charles E. Clark, Sept. 20, 1934; Charles E. Clark to Members of Committee on Cooperation with the Bench and Bar, Oct. 17, 1934, Clark papers, Beinecke.

\textsuperscript{550} Jacob Baker to Charles E. Clark, Oct. 18, 1934, Clark papers, Beinecke.

\textsuperscript{551} Charles E. Clark to Stacy May (Rockefeller Foundation), Oct. 24, 1934; Stacy May to Charles E. Clark, Dec. 28, 1934; Charles E. Clark to Frederick P. Keppel, Nov. 24, 1934; Charles E. Clark to Evans Clark (Twentieth Century Fund), Nov. 27, 1934; Evans Clark to Charles E. Clark, Dec. 4, 1934, Clark papers, Beinecke. The Rockefeller Foundation apparently refused the grant because one of its lawyer board members, Raymond Fosdick, soon to be Foundation president, thought the project useless. See Charles E. Clark to Phillip W. Wickser, Nov. 8, 1934, Clark papers, Beinecke.

\textsuperscript{552} See Charles E. Clark to Justin Miller, Jan. 7, 1935; Charles E. Clark to Corrington Gill (Baker's replacement as Asst. Adm. FERA), Jan. 7, 1935, Clark papers, Beinecke; Clark, supra note 540, at 90.

\textsuperscript{553} Emma Cortvett Llewellyn to Charles E. Clark, Jan. 15, 1935; Charles E. Clark to Karl N. Llewellyn, Feb. 9, 1935, Clark papers, Beinecke. Clark, supra note 540, at 90.

\textsuperscript{554} See Charles E. Clark to Elanor E. Little (Conn. FERA Adm.), Jan. 19, 1935; Elanor E. Little to Charles E. Clark, Jan. 29, 1935; Charles E. Clark to Elanor E. Little, Jan. 30, 1929, Clark papers, Beinecke.
Emma Corstvet, now at work on the other survey instrument, Clark had even identified an acceptable supervisor; but the regional FERA office quite politely declined to provide him with any workers on the ground that FERA money was available only to public educational institutions—and Yale was a private university. After a flurry of correspondence with Washington, it was perfectly plain that Clark had indeed worked for fifteen months without having once read the statute. Somewhat embarrassed, he began the search for alternative financing.

With a little bit of finagling, Clark was able to get the regional FERA office to provide him with a supervisor out of its budget. But finding field workers was more difficult. The local FERA committee was the only potential sponsor, and although Washington said that the committee would help, in fact it refused to sponsor anything legal without the approval of the New Haven County Bar Association. That bar association, whose relationship with the Yale Law School faculty varied from mutually studied scorn to downright open warfare, was unwilling to endorse the project at all. Faced with the likelihood of having a supervisor with no one to supervise, Clark turned to the A.A.L.S. for money to support “its” project. With some help from two friends on the Association’s executive committee, the Association agreed to the expenditure, but only on the understanding that no further

555. Charles E. Clark to Corrington Gill, Feb. 12, 1935 (what do I do now?); Corrington Gill to Charles E. Clark, Feb. 14, 1935 (“a number of annoying minor difficulties remain to be solved”), Clark papers, Beinecke. The problem was doubly critical because nearly half of the law schools targeted as participants in the national study were also private schools. See Karl N. Llewellyn to Charles E. Clark, Nov. 7, 1934, Clark papers, Beinecke (listing targets).

556. See Act of Feb. 12, 1934, Pub. L. No. 73-93, 48 Stat. 351 (widening program from grants to “States” by including grants to “public agencies”).

557. W.R.F. Stier to Charles E. Clark, Feb. 20, 1935 (use New Haven FERA office as sponsor); Charles E. Clark to W.R.F. Stier, Feb. 21, 1935 (they will not do it); W.R.F. Stier to Charles E. Clark, Feb. 27, 1935 (I will pay two months salary), Clark papers, Beinecke.


559. Mrs. Samuel C. Harvey to Charles E. Clark, Mar. 15, 1935; Charles E. Clark to Members of Committee on Cooperation with the Bench and Bar, Mar. 19, 1935, Clark papers, Beinecke.


561. Charles E. Clark to Rufus C. Harris (dean at Tulane and A.A.L.S. President), Apr. 29, 1935, Clark papers, Beinecke.
requests for assistance would be forthcoming. In early spring the first of the field workers headed off.

Data collection proceeded in New Haven and Hartford until August 1935. Over 500 people were interviewed, and eventually the survey of potential legal business was split into two pieces, one for businesses and one for everyone else. The resulting study was, with a carefully chosen sample complete with explicit rules for substitutions and an instrument with explicitly formulated coding rules, the most sophisticated methodologically of all the studies done by Clark or Douglas. Preliminary analysis of the data showed that there was much legal business to be done in low- and middle-class communities, if only the lawyers and clients could be brought together at an appropriate price. Clark was pleased, though hardly surprised, with the results. But there was no one to write up these findings, for during the summer Clark had become the reporter to the committee charged with drafting the new Federal Rules of Civil Procedure, and Emma Corstvet had returned to her work for Underhill Moore.

Since Clark was unwilling to let the data just sit there, he sold the project of analyzing the data and writing a “Bar Survey Manual” based on the analysis to a “quite brilliant” new S.J.D. candidate named Edward H. Levi, who set to work, or so Clark thought. But by summer 1936, when Levi returned to the University of Chicago Law School, he still had not finished the work, and even some push from his new employer was of no immediate help in getting it completed. Meanwhile, Clark completed his obligations to his grantors by filing with FERA and A.A.L.S. a crude preliminary report of the survey done the previous fall by Emma Corstvet and the field work supervisor.

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562. Rufus C. Harris to Charles E. Clark, May 17, 1935, Clark papers, Beinecke. The two friends were Harris and Herschel W. Arant, the Association’s secretary-treasurer.
563. Committee on Cooperation with the Bench and Bar, supra note 560, at 193.
565. See Clark & Corstvet, supra note 564, at 1276 (methodological discussion).
566. Committee on the Cooperation with the Bench and Bar, supra note 560, at 194.
567. Id. (the results “sustain our hypothesis as to the maladjustment of legal business”). See also Charles E. Clark to Emma Corstvet & Bernice Smith (supervisor of field workers), Oct. 26, 1935, Clark papers, Beinecke.
569. See George T. Bogert to Charles E. Clark, Oct. 13, 1936; Oct. 21, 1936, Clark papers, Beinecke.
570. See Charles E. Clark to Emma Corstvet & Bernice Smith, Oct. 26, 1935, Clark papers, Beinecke. The findings in Corstvet’s report are set forth in Committee on Coopera-
At the same time the writing up of the Connecticut survey results was proceeding at a snail's pace, progress on generating a national survey ground to a halt. Clark, having concluded that FERA would not be able to sponsor such a survey, had set for himself and his committee the task of finding another source of financial support, but he was unable to find the time even to look. And an attempt to use Corstvet's work as the basis for generating an A.A.L.S. pamphlet on the results of the already completed surveys foundered because two of the moving parties, Clark, who was still at work on the Federal Rules, and Llewellyn, who was at work on his magnificent group of sales articles, could not find the time to do the work. And so, after three years' work, Clark and his committee had generated a working procedure, a short summer's worth of field work, and a preliminary report.

At this point the chairmanship of the Committee on Cooperation passed to Wiley B. Rutledge, who, as dean of the University of Missouri Law School, had been somewhat skeptical of the survey of the Missouri Bar. That spring he saw to it that the entire project was shifted to a special American Bar Association committee, chaired by Garrison, who was then dean at Wisconsin.
Garrison had the pamphlet written summarizing the studies done so far, but his committee killed the project with the dubious conclusion that “experimentation in planning and method” had not proceeded “far enough” for a successful national survey.577

In late summer 1937 Levi finished his report. Clark was disgruntled both at the delay and at what Levi had been doing instead of working on the bar survey materials.578 Corstvet was simply unhappy with Levi’s work and decided to do an article of her own because she felt the data to be richer than Levi had reported them to be.579 Her article, published jointly with Clark in late spring 1938, almost four and one-half years after the project was begun, was unpretentious but informative.580 It told a now familiar story of clients who had a significant need for legal advice but who never got any, in part for lack of a way to find a reputable lawyer, and of lawyers, hardly wealthy overall, doing a very varied practice. But at the time no one cared about such mundane findings—no one, that is, but the New Haven Bar Association. It got upset enough about the suggestion that it might not be serving the public adequately that it appointed a committee to study the article.581 Charles E. Clark, one year away from an appointment to Learned Hand’s court, never tried empirical research again. Emma Corstvet, by then on the Sarah Lawrence faculty, never tried such narrowly legal research again, either.

Why Clark’s national bar survey never got started, much less

the difference between the Connecticut study, a true survey by sample, and the Wisconsin study, a census, and the merits of each, because Garrison had confused the two. At the time the doing of empirical research in no way implied any social science sophistication, much less quantitative understanding.

577. The Economics of the Legal Profession 8, supra note 532. The committee’s findings are generally not interesting except for their perversity. They simply repeat assertions about low salaries, lack of jobs, sub-standard proprietary law schools, unauthorized practice and general popular dissatisfaction. To these ancient assertions they add the existence of much legal business in low income groups, a finding from Clark’s data, and the large overhead in law practice, a finding in Wisconsin and New York. Id. at 11. Of course, Garrison’s own study provided evidence that salaries were not low and that legal business was probably more plentiful than fifty years earlier. See note 537 supra.

578. Charles E. Clark to Emma Corstvet, June 17, 1937 (“Levi is . . . now engaged in reorganizing the teaching of law along Mortimer Adler lines . . . .”); Charles E. Clark to Emma Corstvet, Oct. 16, 1937, Clark papers, Beinecke.

579. Emma Corstvet to Charles E. Clark, Sept. ?, 1937; Charles E. Clark to Emma Corstvet, Nov. 22, 1937; Emma Corstvet to Charles E. Clark, Jan. 24, 1938; Feb. ?, 1938, Clark papers, Beinecke.


581. See William A. Bree (N.H. Bar Assn.) to Charles E. Clark, June 14, 1938; Charles E. Clark to Paul Shipman Andrews (dean at Syracuse), June 15, 1938, Clark papers, Beinecke.
finished, is a complex question, but much of the answer can be found in the difficulties he faced in getting even his little study completed. Clark began work exactly as he had begun in the Connecticut courts and auto accidents studies and as Douglas had done in the business failures project. Faced with what he perceived to be a problem, he drew on his participation in the progressive reform tradition and set out to get the facts. In this case the problem was the impact of the Depression on lawyers, although the vernacular expressions were catch phrases like “overcrowding,” “unauthorized practice,” and “quota admissions,” and, if one may intuit from a research proposal, the facts were a poor distribution of legal services. But finding the facts was no longer as simple as it had been in 1927 when Clark threw four law students into a court clerk’s office in an effort to find the facts about law administration in Connecticut. By 1934 Clark was enough a part of the social science tradition that fact gathering meant research design, questionnaire development, field worker supervision and the rest of the accoutrements of modern social science. That kind of an enterprise could not be carried off on a shoestring. So Clark set out to find someone who would support his research. But by 1934 Clark was left with only the federal government. That grantor, as Clark had learned once before, proved to be no more tractable than any other, no more interested in empirical legal research than in a myriad of other things, and perhaps even less. Doing even a little bit of research had become a seemingly enormous chore, more than even Clark’s commitment to the social science tradition could support. And so, when the opportunity presented itself, as in drafting the new Federal Rules of Civil Procedure and in reconstituting the American Bar Association, Clark slid back into the more direct pursuit of reform, as Douglas had done, by following things of “special in-

582. Clark advocated quota admissions to law school as a more humane way of limiting entry into the profession than by manipulating the pass rate on the bar examination. See Clark, The Selective Process of Choosing Law Students and Lawyers, 7 AM. L. SCHOOL REV. 913 (1933); Clark, Making Selective Admission to the Bar Practicable, 8 AM. L. SCHOOL REV. 13 (1934).


584. Curiously Douglas was left in the same position. His last study—protective committees in corporate reorganizations—was also federally financed.

585. There is a quite fascinating letter box in the Clark papers, Beinecke, detailing Clark’s efforts in 1935 to reorganize the American Bar Association in order to neutralize its voice in national politics by turning it into a lawyer service organization. The reorganization attempt was ultimately successful, although the results were not exactly what he had expected, as Clark soon learned in the “Court-packing” controversy.
terest, just as had Dorothy Thomas and Emma Corstvet.

The tension between progressive reform and social science can of course be overemphasized. The lack of financial and other support for empirical legal research, aptly captured in Clark's discouragement when after a year's worth of work he was told that Yale as a private university was ineligible for funds under his project, alone played a significant part in the decline of such research at Yale. After Clark learned of the federal government's indifference there was no place left to turn. The Harvard law reformers, the social scientists, and the Institute had all turned out to be rather unconcerned about the kind of empirical legal research Clark and Douglas wished to do; each group had other interests or objectives. Had there been a supportive National Foundation for Scientific Legal Research the result might have been different. But there was no such entity; there was only FERA, convinced that the way to build support for its programs was by co-opting local forces. With such weak support, empirical legal research died at Yale, swallowed up in the other activities that made it an exciting place and the hot-bed of American Legal Realism.

III. Why the Law School Stopped Moving

A good story, it is said, should have a beginning, a middle and an end. By this canon, the story of the Yale Realists' attempts at empirical research in law has not been a good one, for if there has been any form to it at all, it has been that of a slow descending curve from enthusiastic high hopes and almost frenetic activity to doubt and virtual inactivity, but without any denouement. Of course to say that the story has been one of decline is not to say that an entire institution—the Yale Law School or the Realist movement—slipped slowly down the figurative drain. Indeed, just the opposite is true: as "scientific" research in law declined, the philosophical side of Realism, which gave the movement its name and fame and tied it inextricably to Yale, flowered. And, more
important, the political reformist side of Realism,⁶⁸⁹ which when combined with its "scientific" side defines or isolates this movement in legal scholarship far more readily than the philosophical debates of the thirties, continued in full force and even gained in strength. But on the "scientific" side, on the side of empirical social science as it was understood then, the story can only be characterized as one of decline.⁶⁹⁰

The reasons for the decline of the Yale Realists' efforts at empirical legal research are numerous and in one sense accidental. They are accidental because the background against which the story must be seen was surely as favorable to an attempt to do, and institutionalize the doing of, such research as anyone had a right to expect. Socially and politically it was a time when scientific research was a good thing. Herbert Hoover, the great engineer, was President. He, who would bring his practical, objective science to bear on American government, who in the Department of Commerce had fostered the collection of incredible quantities of "facts,"⁶⁹¹ and who continued to foster detached inquiry into social problems,⁶⁹² at least gave social scientific enterprises a setting in which they could try to flourish. And other elements of the social scene helped, too. Taylorite scientific management was a significant force in business;⁶⁹³ the progressive tradition of commissions of detached inquiry was still prevalent; and even the general public was, it appears, fascinated by statistics.⁶⁹⁴

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⁶⁸⁹. See, e.g., Douglas' work in securities law, Clark's work on procedural reform that culminated in the adoption of the Federal Rules of Civil Procedure, Llewellyn's work on reforming sales law that ultimately produced the Uniform Commercial Code, Hamilton's work on destroying economic due process, Arnold's work at reviving the antitrust laws, and Borchard's tireless activities on behalf of the Federal Tort Claims Act and the declaratory judgment. What holds these diverse activities together is that at the time they were seen as liberal, reformist projects. One of the characteristics of Realism as a movement was its slightly left of center politics.

⁶⁹⁰. Even more obviously so when the experience of the short-lived Johns Hopkins Institute of Law is taken into account, which I hope to do presently.

⁶⁹¹. For a revealing account of Hoover's policies as Secretary of Commerce, see Rothbard, Essay, in Herbert Hoover and the Crisis of American Capitalism 35 (J. Huthmacher & W. Susman eds. 1973).

⁶⁹². See, e.g., President's Committee on Social Trends, Recent Social Trends (1933), discussed extensively in Karl, Presidential Planning and Social Science Research: Mr. Hoover's Experts, 3 Persp. Am. Hist. 347 (1959).


Academically the background was equally favorable. The social science disciplines had just finished their fragmentation along methodological lines from a unified science of political economy into their now invariable universe, but the concomitant fragmentation of academic departments was still underway. This was the time of the early reception of quantification into social science, when counting seemed enough and statistics as it is known today was in its infancy. Thus, method in the social sciences was only beginning to adopt the now familiar norms, and indeed major methodological works were still being written canonizing all then generally accepted methods within given disciplines. Graduate education was expanding rapidly, in part to supply the needed Ph.D.'s to staff the new social science departments in every educational nook and cranny in the country. Professional education, especially in medicine, but also in law, was still in the process of reform. Foundation interest in and support for education, especially professional education, was at a high point. On a more narrowly institutional level, Columbia, it appears, was still suffering from growing pains; Yale, just becoming a university; and Johns Hopkins, still trying to duplicate its medical successes. Thus, there was great potential for new educational openings in general and openings toward an active, diversified social science community as well.

Professionally, the background was admittedly nowhere near as favorable. The enormous American Law Institute scholarship engine had already been set in motion, its wheels well-greased with money that might have been captured for scientific research


596. Yale's Department of Economics, Sociology and Government is a good example of the halfway stage in the fragmentation of the social sciences. Psychology did not split off from philosophy at Yale until 1927.

597. See notes 175, 302 supra.

598. Interview with Dorothy Swaine Thomas, June 3, 1975.

599. SOCIAL SCIENCE RESEARCH COUNCIL, A CASEBOOx ON SCIENTIFIC METHOD (S. Rice ed. 1931) is a good example of such a work.


in law, but that instead lined the pockets of more traditional legal scholars.\textsuperscript{602} That organization provided now tax deductible opportunities for slightly left of center, upper caste lawyers to socialize in an atmosphere that reinforced the notion that theirs was a learned profession and thus further separated them from the stench of the \textit{untermenschen} of the profession. Even more debilitating was the notion fueled by the Institute's mere existence that library, not field, research was \textit{the} method of legal research among the group in the profession that was the most likely to support empirical research in law. And the profession as a whole, or at least that upper portion about which something is known, was surely not interested in social science intrusions into the "practical" training for the practice of law, although the phenomenal use of the "bar survey" suggests that at least parts of the profession were not adverse to using "scientific" methods when such methods seemed to advance the profession's interests. But the professional background could hardly be expected to be favorable at any time; union spokesmen can be expected to oppose innovations in the craft that smell of automation or, in that marvelous English word, of redundancy, and this is as true of unions of persons whose craft skills are mental as of those whose skills are manual.

If the social, political, and academic, if not professional, background was largely supportive, what then were the reasons for the decline of empirical research in law and thus of the scientific side of Realism? Speaking of Yale alone, these were the usual accidents of time and of person—the Depression and Deans Hutchins and Clark—and the nonaccident that was the nature of the research enterprise itself. The first two have not yet been extensively discussed but their importance should not be underestimated.

In some sense the Depression did the dirty work. If business had continued its short-lived postwar boom there might well have been enough foundation money for starting much empirical research in law. But the Depression came, and instead of watching the Rockefeller's largess almost fall out of the trees, Charles Clark saw the money tree wither. Virtually untrained in the then developing art of grantsmanship, he looked for money to support his predetermined research objectives rather than attempting to orient his research toward grantors' interests. Thus, he was reduced first

\textsuperscript{602} On the history of the ALI, see H. Goodrich & P. Wolkin, The Story of the American Law Institute (1961); Goodrich would, however, rather strongly disagree with my characterization of the Institute and its program. E. Brown, Lawyers, Law Schools and the Public Service 243 n.1 (1948) is some support for my position.
to asking George Wickersham if he could find some money and then to scrounging for free assistants from the government. Other more single-mindedly dedicated scholars like Underhill Moore were left to get what little support they could from the increasingly frugal Institute of Human Relations and then, when that money was gone, to finance research out of their own funds. Thus, the Depression brought home the realities of modern social science research and belied the Realists’ initial feeling that the diversity of social science presented numerous opportunities.

At Yale the Depression worked against establishing the Realists’ empirical research activities in another way, too. The decline in available funds led to cutbacks in Yale’s educational programs, as would be expected. The result of such cutbacks appears to have been that the more recent claimants on the available funds fared worst and fringe benefits, such as having research assistants, were generally eliminated. Since the empirical research activities of the Realists at Yale were not only new but also expensive ones that Yale had almost never funded except with soft money, any attempt to turn to Yale to support the expense of such research was squarely stymied, while at the same time the few research assistants that were available were slowly eliminated. Had empirical legal research been begun even five years earlier and the cost of the research establishment transferred, at least in part, to the University’s budget, the effect of the Depression on such research might have been far different.

Despite the obvious importance and impact of the “accident” of the onset of the Depression, the “accident” of men, particularly their styles of leadership, was probably more important. Empirical research began at Yale because Robert Hutchins was there. Both the state and federal procedural studies were begun under Hutchins’ impetus. Douglas’ field research was not begun until Hutchins

603. Interview with Jane Moore, May 19, 1976. The records of the Institute bear this out. See Underhill Moore to Mark A. May, May 10, 1937, IHR files (over $3,000 spent to date). See also Underhill Moore to James R. Angell, May 18, 1936, Angell papers.

604. See, e.g., Thomas W. Farnam to Underhill Moore, Apr. 27, 1936; Memorandum, Law School Budgets 1925-34 (n.d.), Angell papers; Yale Minutes, Nov. 3, 1932; Feb. 16, 1933; Apr. 26, 1934. After research assistants came salary increases and accelerated tenure consideration for junior faculty members. See Board of Permanent Officers, To the President and Fellows of Yale University, June 1, 1935, Angell papers (remonstrance drafted by Arthur L. Corbin).

605. Only the four research assistants who began the Connecticut Courts study, Douglas’ assistants on the Philadelphia grocers study, and a single assistant for Moore, who often did no empirical work, were paid for out of regular university funds.

brought him to Yale and conceivably was not even formulated before he was hired. The law school's participation in the Institute was due solely to Hutchins' interest in making that connection. All these activities are reflections of Hutchins' personality or style. His protestations to the contrary notwithstanding, he was passionately interested in educational reform from the start of his career, even if, perversely but revealingly, he had never systematically thought about the subject until after he had left Yale. His style in pursuit of that interest was not to think out educational plans carefully or even to build institutions patiently, but instinctively, but rather to shake things up constantly while following any and every promising lead. It was a lively style played out not just in encouraging empirical research, but also in appointments, curriculum reform, and general educational policy. As styles of administration go, it was probably just what Yale needed to turn its law school from a promising place into an exciting one. But it was a style that had its limitations and, as it turned out, critical ones at that.

As a summary of those limitations it might be said of Hutchins, as Carlyle said of Matthew Arnold, "He led them into the Wilderness and left them there." Bright ideas about changing institutions seldom go anywhere unless institutionalized, preferably all by themselves. Hutchins never institutionalized any of his educational innovations at Yale. Had he stayed longer perhaps he might have done so, although his record at Chicago appears to suggest the contrary, but no matter what might have been, what was, was that all of the Hutchins-inspired enterprises were started and then left to sink or swim. The Institute was begun with a firm, though only tangentially relevant, idea of why the law school was part of its program, but no idea what its program would be to which the law school's nonexistent program of research would relate and, although it was not Hutchins' fault, no substantial

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608. M. Adler, Philosopher at Large 128-29 (1977). The only hint of systematic thought about education that appeared in print before Hutchins left Yale is in the last paragraph of Hutchins, Modern Movement in Legal Education, 1928 A.A.L.S. Handbook 30, 33. The thought there expressed about the ability of the University to "afford" educational experimentation because it possesses "a life that is nearly immortal" is hardly unusual.
609. As quoted in D. Hollinger, Morris Cohen and the Scientific Ideal (1975) and in Stevens, supra note 17, at 481 (applied to Realism generally and, I think, somewhat erroneously). The epigram fits Hutchins better than Arnold.
funds to work miracles with. Clark's and Douglas' research was begun with no particular idea where it would lead or how it would be financed. Curriculum reform was started in three different directions in less than six months. The honors program never really had a purpose, except to get students out of the rut of classes. A pattern emerges rather clearly: start something and, if necessary, see what it might be turned into.

The consequence of Hutchins' brand of educational leadership was that whoever inherited a project—something not always, or even often, a known quantity at the outset—defined what that project would be. When no heir appeared, as in the case of systematic curriculum reform or the honors program that lived to plague the faculty at its weekly luncheon meetings for years, the project went nowhere. But most often the task of definition, the responsibility of heirship, fell to Charles E. Clark. That fact was not without its consequences either.

The two friends—Hutchins and Clark—could not have been more different had one been a Laplander and the other a Hottentot. Although it could not responsibly be said that empirical research died at Yale because Charles Clark was there, it is notable that although he passionately believed in "fact research," as he called it, and loudly lamented its decline, Clark was able to do little to keep any going. Not only was nothing new started, nothing was started while there were plenty of ideas for research projects just lying around. True, both the two procedure studies and the auto compensation study were eventually completed. But only the legal needs study had really been Clark's idea, and it went nowhere. Moreover, Clark was unable to capitalize on either Douglas or Moore, his resident engines of social science research, was unable to make anything out of the law school's connection with the Institute, and was even unable to generate any real educational innovation during his deanship, a period four times longer than Hutchins'. Yet Clark's tenure was not without substantial achievements. He held the Yale Law School together through years of declining resources while federal agencies made extraordi-

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611. See, e.g., Charles E. Clark to Eugene V. Rostow, May 31, 1946, Clark papers, Law, supra note 164 (commenting on a faculty report again attempting to make sense out of honors work).


613. See note 518 supra.
nary demands on everyone's time. At the same time he managed to recruit a faculty of unusual talent and, ultimately, achievement. And he saw to the development of an effective committee system, necessary to run the law school, which had doubled its faculty in the ten years following Hutchins' selection as acting dean.

Clark's achievements and failures trace a pattern of activity as telltale as that of Hutchins'. The procedure studies were completed in large part because Clark saw to it that they were completed; the thought that something once begun might not be completed was inadmissible to him. And the auto compensation study was so finely wrought that it is a monumental example of a concern for careful work overwhelming the rather mundane purpose for which it was done. Such changes in the educational program of the law school as were undertaken were all considered, reported, and justified almost to death by a committee structure Clark had created. And although his faculty was unusually talented, it was not audacious any more than Clark was an audacious dean. Taken together these events reveal a man who in his approach to problems and in his style, if style is the right word, was always careful, measured though not plodding, and above all thorough. That was Clark's way—lawyerly, in a word. It was a style appropriate to times of consolidation or decline, when order and the husbanding of resources are appropriate. Thus it is not surprising that Clark's achievements as he guided the law school through the Depression were largely administrative. But with respect to new activities and partial programs it was a style with limitations, just as Hutchins' had been.

The limitations of Clark's style were two. The first can be

614. The participants in the law school's periodic exodus to Washington included Arnold, Clark, Dession, Douglas, Fortas, Hamilton, Marshall, Harry Schulman and Sturges. Some, like Fortas and Marshall, never returned to Yale; others worked anywhere from just a summer to a year or more.

615. The growth of the committee system can be seen only by patiently reviewing the minutes of the law school's weekly faculty meetings for the period of Clark's deanship, a job that reminds one how trivial one's own faculty politics must be and how ultimately boring C.P. Snow's "The Masters" is.

616. See, e.g., Charles E. Clark to George W. Wickersham, Oct. 2, 1931; Charles E. Clark to Sam Bass Warner, Jan. 25, 1933, Clark papers, Beinecke. For examples of similar efforts with respect to the bar survey, see Charles E. Clark to Karl N. Llewellyn, Mar. 4, 1936; Charles E. Clark to Emma Corstvet, June 8, 1937, Clark papers, Beinecke.

seen in several incidents. Despite Clark's worries that Douglas would leave for Chicago, when Douglas' bankruptcy studies were finished his interest was allowed to drift, drift probably induced in part because of his rather stern introduction to real social science method administered at the hands of Dorothy Thomas. Moore too was left to move along his own track, although the cantankerousness of these two neighbors probably made it impossible for Clark to have moved Moore in any particular direction, even if he had wanted to move him. Similarly, Clark was unable to make anything out of the law school's connection with the Institute. Granted, he was left with at best a half-baked idea; yet that fact was less a liability than a potential asset, one in which President Angell took relatively personal interest. But realizing on potential assets was not Clark's forte. Given a sensible project, he could execute it; create one out of scraps and pieces, he could not. All the care and thoroughness he could muster was of no help in such a task. Ultimately, it was just not his style.

The second limitation was related to the first. Just as Clark could not create out of scraps, he could not lead out of diversity, either. His idea of leadership was leadership by demonstration. He could, and did, show what kind of activities he thought were appropriate for legal scholars—a rather promising combination of empirical study and policy analysis that might have led away from the kind of data-free social science that is the dominant mode of law school legal analysis today. He made his silent point over and over again, especially in the procedure studies and the bar survey, but if no one wanted to learn from the demonstration, Clark was not the dean to think up new and exciting things.

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618. Moore and Clark fought repeatedly. See, e.g., Thomas W. Farnam to James R. Angell, Apr. 20, 1936, Angell papers (detailing attempts to mediate a fight between the two over research assistants; "This will be my last effort to get these two contrary characters together."). Moore believed that Clark's "temperament" resulted in "leadership and administration" that was "morale-destroying." Underhill Moore to Arthur L. Corbin, Apr. 28, 1939, Moore papers. Clark, on the other hand, was more charitable, at least in public. See Clark, Underhill Moore, 59 Yale L.J. 189 (1950). I doubt that the two men could ever have gotten along, but their relationship was not helped by the fact that Moore's salary was greater than Clark's (see, Charles E. Clark to Charles Seymour (provost), Feb. 10, 1934, Angell papers) and that Moore bought the house next door which was larger and up the hill.

619. Clark recognized that he had been left with only a glint of an idea, Charles E. Clark to James R. Angell, Dec. 29, 1936; Jan. 5, 1937, Angell papers, although as these letters show he never understood that this fact was a potential asset.

The estimate of the extent of Angell's interest in the Institute was noted by Mark May who recalled that Angell called at least once a week to talk about affairs at the Institute. Interview with Mark A. May, June 9, 1975.

620. I would like to thank Professor Jacob Hyman for contributing this felicitous description of this aspect of Clark's style of leadership.
to do that might tempt others to follow his lead despite themselves. He was too thorough for that kind of serendipity. His work was self-describedly "practical."^621 It was sophisticated in a technical way, but as such it was hardly the sort of thing to galvanize others into action.

The consequence of Clark's brand of educational leadership was as striking as in Hutchins' case. The faculty Clark inherited was alive with energy and activity;^622 it plainly knew that it was special. It did not lack for ideas; if anything, it had too many. What it lacked was direction.^623 Hutchins left behind scraps and pieces of that needed direction, largely pointing toward empirical legal research. But Clark could not work with scraps and pieces, and regrettably his faculty was not patient (one almost wishes to say humble) enough to learn from careful, but not flashy, examples. And so the momentum that Hutchins brought to the faculty was allowed to spend its force, as well as to grow, in pursuits other than empirical research, as the Depression and the nature of the research enterprise made that kind of research less attractive.

Hutchins never recognized the part that his personal style played in the demise of what he started at Yale. The best he could do was to suggest that the group as a whole may have been operating with "too narrow a theoretical base."^624 Clark, on the other hand, did, at least in part. He expressed that understanding ten years after the research was over as he spiritedly defended his procedure enterprise from criticism like that of Frankfurter and Hart, criticism that had by then grown in volume:

... I resent the repeated assertions that these studies were without plan or purpose beyond "piling up the data." On the contrary, they were aimed at very definite things, with clear hypotheses worked out as a result of law practice, plus teaching of procedure.

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621. Charles E. Clark to Filmer S.C. Northrup, Jan. 10, 1948, Clark papers, Law. See text accompanying note 625 infra for extended quotation from this letter.
622. See Leon Green to John Henry Schlegel, June 4, 1975, not that the observation really needs any support.
623. Put another way, Clark was not faced with a problem of "wet tinder" such that his job as dean might have been described as lighting fires. Clark's faculty was more like late summer chaparral. The danger of brush fires was always present and the major problem was that like most brush fires the faculty burned very hot, but not for very long—other than Corbin and Moore, few men on Clark's faculty were known for their sustained scholarship. But cf. James R. Angell to Charles E. Clark, Nov. 17, 1931, Angell papers ("The moment you appear to falter, or lose heart, your colleagues will instantly be unfavorably affected.").
624. Interview with Robert M. Hutchins, June 20, 1975. To some extent I agree, but the significant point is that in explaining the decline of the Realists' attempts at empirical legal research Hutchins completely neglected his role in starting that research.
That was a time of emphasis on "law in action" in place of "law in books," and perhaps our arguments and money-raising pleas stressed this then popular approach more than we would now do. But looking at the studies in perspective I do not believe either our purposes or our plans could be misconstrued. I expect some of this is really a reaction that our objectives were too lowly, too practical to be considered on any philosophical basis. That is a sounder approach to criticism, for our purpose was quite immediate and practical. For better or worse that was my turn of mind; and I am quite prepared to accept responsibility for that, for doing something within our grasp, rather than trying to harness the stars.625

Perhaps "practical" is not the best word to describe Clark's limitations, but Clark quite obviously tried to do things within his grasp and not to harness the stars as Hutchins might have done. Unfortunately, all that was within Clark's grasp, all that could be done with some assurance that what was begun would be completed, was to preside patiently, lovingly even, over the slow winding down of empirical legal research at Yale. And so that was what Clark, the good lawyer, did.

Clark's understanding of his own limits is at the same time a good place to begin to explore the third factor in the decline of empirical research at Yale: the by no means accidental limits that the nature of the research enterprise placed on its own capacity for survival. Empirical legal research began as a practical, reform-minded enterprise for all those engaged in it, as this recounting of their research attempts to show. It lacked any grand theoretic justificatory structure. Rather, it proceeded on the basis of intuitions about the legal world gathered, as Clark noted, in the course of teaching and practice.626 Yet it is possible to intuit in the actions of Hutchins, Clark, and Douglas, if not a grand theoretical structure, at least a lower level one. These three scholars wished somehow to combine law reform and social science in order to improve law and legal education, or in more abstract terms, to cross the progressive reform tradition with the emerging social science tradition to form a hybrid that might invigorate what Leon Green aptly described as a legal world gone "stale."627

625. Charles E. Clark to Filmer S.C. Northrup, Jan. 10, 1948, Clark papers, Law. See also Charles E. Clark to Ruth Field, n.d., Clark papers, Law, [written in response to Simpson & Field, Law and the Social Sciences, 32 VA. L. REV. 855 (1946)] which is, however, not as pericpicacious about the law school's role in the Institute.


627. Interview with Leon Green, June 19, 1975; Leon Green to John Henry Schlegel, June 4, 1975.
In two ways the idea was a plausible one. First, each of these three men looked quite naturally to the progressive reform tradition. Each was a part of the liberal wing of the legal profession, as well as of the legal teaching profession, and the progressive reform tradition had always been a home for liberal politics, for Reform. At the same time, all three were university academics who looked quite naturally toward the university community of which they were, or at least hoped to be, a part. Since gathering facts had always been part of progressive reform, and since the university had its own store of social fact gatherers, it was quite natural to seek the facts from the people whose business it was to collect the facts, especially since the social sciences were in origin part of the progressive reform tradition.

Second, in less personal terms the combination also made sense. The process by which each of the social sciences split off from an undifferentiated social science, and thus from activist reform, had left behind law, medicine, and divinity as the rather hollow core of social science, and thus as the major heirs of progressive reform. Divinity, always a respectable academic discipline, was rapidly receding in importance in the secular academic community. Daniel Coit Gillman, Abraham Flexner, the American Medical Association, and the Carnegie Foundation had seen to it that medicine became an academic discipline too, and in so doing used the same general tactics that the social sciences had used: isolate the amateurs and adopt the language and manners of natural science. Law, in contrast, had taken a significantly different route when it became an academic discipline. While the new law teachers moved quite vigorously to isolate the amateurs

628. As evidence for this proposition, if any be needed, one should note the support of Hutchins and Clark for Sacco and Vanzetti. See Charles E. Clark to Leon Green, Mar. 11, 1959, Clark papers, Law (it was Hutchins "who got me into trouble with the Sacco-Vanzetti case"); text accompanying note 100 supra. W. DOUGLAS, GO EAST, YOUNG MAN (1974) is littered with evidence.

I believe that the Realists were generally the political left wing of the teaching profession, although surely no more than mildly left of center on a national spectrum, and that this fact is one of the items that distinguishes the movement. Proof of this assertion will be presented in forthcoming articles.


630. I infer this from T. HASKELL, supra note 629; it is he who emphasizes a generalized "Social Science" as the root of all the social sciences and more importantly emphasizes the relationship between the rise of the university and the differentiation of the social sciences as academic disciplines.

by insisting on the importance of “full-time” teachers, they grabbed on to science in a wholly different sense than did all of the rest of the new academic disciplines. The reasons for this choice, however interesting, are for present purposes irrelevant. What is relevant is that, as developed by the generation of scholars that came to intellectual maturity before the First World War, legal science was scientific by the most imperfect of analogies. Only by assuming the existence of an immanent law could the search for that law in the reported instances of its application be considered a scientific enterprise. Moreover, making such an assumption, the new legal academics were ignoring the tendency of all of the other offspring of social science to look for the social or biological origins of human artifacts. By thus choosing to follow a more ancient conception of “science” as “rational” rather than “empiric” activity, these men allowed legal science to be left behind in the rush to create academic disciplines to populate the halls of the new university, the surviving rationalistic, historical, deductive intellectual dinosaur in the community of scholars. Were one embarrassed or puzzled by being part of such a dinosaur, or only intrigued by the rest of the university as it passed by, as surely Hutchins and Clark, if not Douglas, were, reuniting academic law with its successful former cohorts might have been a way to overcome embarrassment or, more simply, to join the parade.

Although the idea of crossing the progressive law reform tradition with the social science tradition may have seemed plausible at the time, in fact it was not. Law reform and the social sciences had shared a common root in the social science of the late 19th century, but their paths in the 20th century had been very different. While the agenda and methods of the legal remnant of the progressive reform tradition remained roughly the same as they had been in the late 19th century, the social sciences changed radically. Causality became less of a surface phenomenon; method and peer acceptance became more important as a test of truth; reform receded as an objective in the near term; departmental politics replaced national politics. In all likelihood these changes were a social precondition to acceptance of the social sciences into the

632. Auerbach, Enmity & Amity: Law Teachers and Practitioners 1900-1922 in 5 Persp. Am. Hist. 551, 551-53 (1971) emphasizes this fact as he describes the process by which law teaching became an academic discipline.

university. The divorce of research from an immediate political program provided political insulation for the university and assured the political acceptability of its activities to the community, both of which were necessary before the new university could be allowed to exercise a substantial degree of autonomy when performing the critical social function of educating the children of the middle class. But even if seen more narrowly as elements necessary to the assertion that the social sciences had adopted the scientific ideal, these changes made reform just about the least important aspect of social science. However, for the legal heirs of the progressive reform tradition, reform was the point of any enterprise. Thus at loggerheads, it soon became apparent that neither group would compromise and that anyone who wished to cross the two traditions would have to pick a way between them.

As Hutchins, Clark, and Douglas tried to pick that way, they soon learned that all by itself social science would be a difficult horse to ride, at least if law reform was to be its partner in bringing academic law out of the age of dinosaurs. As Douglas remarked a year or so after he gave up the business failures project, “All the facts which we worked so hard to get don’t seem to help a hell of a lot.” If what was wanted was the facts on which to base the argument for reform, quantitative empirical research either produced too many, as in the courts studies, or worse, a very few at an enormous cost, as in the business failures or auto accidents projects. And then there was the matter of time. Of the completed studies the shortest, auto compensation, took three years, and the median time was nearly five years. It did not help the cause of empirical research to learn so immediately how time-consuming and also costly the careful collection and preparation of field data in fact was. Nor was it helpful to walk squarely into the technical revolution in social science methodology; it surely was disheartening for law professors to learn at the outset that they needed technical skills they had had no reason to acquire, in order to do work that, when first thought of, had seemed little more difficult or complicated than calling up a friendly banker to learn a little something about commercial practice. In short, the match was wrong.

Hindsight suggests that such a mismatch was not wholly unpredictable. While in some respects law and the social sciences developed very differently as academic disciplines, in other ways

their development was quite similar. While law remained a part, possibly the only part, of the progressive reform tradition, the academic lawyers by and large fell away from that tradition.\textsuperscript{635} True, they talked reform and some, like Pound and Frankfurter, seem even to have participated in some, but the average legal academic of the early 20th century was busy ordering legal doctrine and assembling casebooks. Like their social science brethren they too had grown away from directly tinkering with the body politic. While this development cannot possibly be attributed to the imperatives of the scientific ideal, it was probably as much a precondition of university citizenship for the new legal academics as for the social scientists, and for like reasons. As a result, control of law reform in the progressive tradition remained largely within the profession, in the hands of the leaders of the bar like Wickersham and Ballantine.

While "reform" might have been a broad umbrella, in professional hands it was not, but rather was quite obviously directed toward "safe" channels, whether from a limited social vision, from solid political preference, or from a Kolkoesque desire to avoid worse alternatives. Thus, reform remained a surface notion in which all, or most, of the defects in the law were procedural or remedial: the courts, congested; and the rules, uncertain; and in which higher standards for admission to the bar were the cure to both overcrowding and unethical practices by lawyers. Quite obviously, this view aided the social and economic position of both these lawyers and their clients. More importantly, because these professionals were the heirs of the progressive reformers, in most quarters theirs was \textit{the} view of law reform. They thus defined what research was sensible to pursue and what results were intelligible, and through informal relationships with major funding sources they could help to make their opinions about sense a reality.

Clark and Douglas felt the limits that the profession's definition of reform placed on research quite directly when trying to explain the absence of court congestion, when trying to outline the contemporary significance of the diversity jurisdiction, when justifying all the work done on the auto compensation study, and while seeking funds for the bar survey. Douglas reflected these limits when giving up the business failures project. If one consid-

\textsuperscript{635} Here, I disagree with Auerbach, supra note 632, at 553-61, who emphasizes a commitment to reform as an integral part of the process by which law teaching became an academic discipline. For me Auerbach's analysis places entirely too much weight on professional rhetoric and too little on the day to day activities of law teachers.
ers the attempts of both men to do empirical research as part of a more general attempt to institutionalize the doing of such research, still other aspects of the limits of the profession's definition of reform appear. And taken together the examples suggest why the mismatch of reform and social science was not unpredictable. If one's object was to invigorate a legal world gone stale, the social science tradition was surely impotent, for although it was a potential source for an accurate perception of social conditions, its prospect was almost wholly passive. At the same time, the progressive reform tradition, at least in the hands of its legal heirs, was a mirror image of the impotency of the social science tradition, for although determinedly activist, its view of reform was largely divorced from any even marginally accurate perception of social conditions. Thus, both the defectors from the progressive reform tradition and its legal heirs suffered the same fate; and legal academics, caught between the two traditions in such a way as to have the worst of both worlds—a passive prospect and a poor perception of social conditions—might well have wondered whether a social and political structure that had thus neutralized both traditions was likely to be supportive of an attempt at hybridization that intended to unite the activist aspect of the legal remnant of the progressive reform tradition with a potentially accurate view of the social conditions of legal institutions.

636. The profession's definition of reform pointed toward a mode of research consisting of individual studies either brought to a conclusion or not, depending on the patience of the researcher and the exigencies of the campaign for reform. But it is difficult to see how such an "occasional" enterprise might have been perpetuated, for to do so one would have had to institutionalize not a tradition of doing a particular kind of work, but a tradition of doing any work that seemed appropriate to a given reform objective—hardly the kind of thing out of which departments or even Institutes are made.

637. Support for this understanding of how the mismatch between the progressive reform tradition and the social science tradition was a primary factor in creating the inherent limits on the survival of the Realists' research enterprise can be found in the vaguely contemporary ring to this recounting of the research, as shown by one of the subsidiary themes isolated at the outset. Viewed as a whole the activities of Clark and Douglas made up a massive interdisciplinary research project. In it they met the usual problems of research design, data interpretation, temporal constraints, and monetary limitations. While faced with all these technical problems, the two men also had to face the desire of each intellectual unit in the supposedly joint activity to impose its view of the nature and meaning of the research. Each group, first, the progressive law reformers, then the social scientists, thereafter, in the Institute episode, both the lawyers and the social scientists, and finally, the funding sources, had its own vision of the world and insisted on enforcing that vision, largely because in the modern balkanized intellectual world only that vision was intelligible to those holding it. In such a world working together really means coordinating disparate visions as an exercise in the art of the possible—essentially the role that Clark learned to play and played most successfully in the bar survey.
While the three factors of time, person, and nature of the research enterprise had the greatest impact on the decline of the Realists' efforts at empirical research, one should not ignore the fact that these were wholly fortuitously combined with a rise in the heat of political battles after the onset of the Depression and a concomitant increase in the opportunities to participate in those battles after Roosevelt's election. This development only accentuated the difficulties with the nature of the empirical research enterprise. As Clark and Douglas fell away from empirical research projects they, and others similarly inclined, could and did fall into advocating or participating in reform—often the same reforms that had animated their earlier research. Thus, these scholars could feel as if they were not changing their concerns or abandoning their beliefs and, at the same time, smoothly change their actual activities to accommodate new circumstances. That fact was ultimately the most unfortunate, at least for empirical research. The new "political" activities both required less elaborate fact-finding and proved compelling enough that, except for Moore who was always the "scientist" and only in late desperation tempted by Washington's panjandrums, none of the Realists really missed the old enterprise. Consequently, there was little if any interest among these men to revive empirical studies when economic and political conditions had become more favorable to such an enterprise. What was left behind was a pile of dusty studies and the notion far too prevalent today that social science is all very interesting and ultimately very relevant to legal problems, but that until the social scientists spend some time examining legal problems, the study of law can go on as it has, data free and value laden.

To suggest that the decline of the Realists' attempts at empirical research in law was the result of accidents of time and place and the not so accidental nature of the research enterprise itself is simultaneously to suggest, if only faintly, that the story of those attempts, Fowles-like, might have had a different ending. And I mean to suggest just that, for although it has been suggested, and not without some justification, that the Realist movement began its fragmentation and decline with the deanship crisis at Columbia in May 1928, the significant event was surely a year later.

Whether that event be seen as Hutchins' resignation and Clark’s selection as his successor in April 1929, or the great stock market crash several months later is unimportant, but up until that event, given the relevant social and academic background, there was at least a chance that empirical research in law would have been a major part of American legal scholarship and thus of American legal education. Given Hutchins' wish to put the law school in a center of graduate and professional education, and his friendship with Winternitz, the "might have been" law school probably would have resembled medical education more than Cook's Institute at Johns Hopkins. Empirical research would have been a part of the work of the faculty and graduate students, but not of the students who would benefit from the increased knowledge and then head for professional careers. But the matter turned out otherwise. Whether legal education is better or worse for that fact is an open question, but if what "might have been" had been, Realism would have become a very different force in legal thought, and legal thinkers would know a lot more about the world of their thought than they do.