History, Empirical Research, and Law Reform: A Short Comment on a large Subject

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We in the law schools (to borrow a phrase from Karl Llewellyn) are living through the "second explosion" of interest and activity in the empirical study of legal institutions and processes. It cannot fairly be said that the philanthropic foundations have supplied the spark for the detonation; but they certainly have provided a lot of the power. Explosions, if uncontrolled, can be destructive. But it is not my purpose, in this brief note, to inquire into the myriad problems that have followed in the wake of recently launched programs of institutional research. Rather, for the purposes at hand, I shall assume that these difficulties can and will be resolved, that project research will be domesticated with reasonable success, and that programs of other-than-doctrinal inquiry will become accepted as a normal and important part of the institutional functions of many law schools. I shall, on these assumptions, confine myself to the consideration of one small, but perhaps neglected, aspect of the broad question: How can the utility of such research be most fully realized?

It would certainly be a gross error to assert that, in the movement toward empirical studies of legal institutions, no significant thought has been given to the role of historical research. If one turns to the Summary of Studies in Legal Education, issued by the Columbia law faculty in 1928 (which occupies a place somewhere near the center of the "first explosion"), he will discover frequent references to historical studies, in connection with both curriculum matters and research. Indeed, one of the objectives, stated in the Summary is "to lay the basis for more serious study of legal history than has hitherto been contemplated in this country." Everyone knows that highly important historical scholarship was inspired by the Columbia studies and by thought of similar orientation elsewhere. And yet, conceding all this, it is probably fair to say that the movement toward empirical research in the law has not been strongly characterized by consistent interest and concern with historical studies. Those attracted to non-doctrinal inquiry have, in general, felt a stronger affinity with the sociologist, the economist, and the psychologist than with the historian and his discipline. The truth is that some of those who, in the last generation, struck the match to the "first explosion" were not only uninterested in, but were inclined to doubt the value of, historical studies, both in and out of the law. And this unconcern, if not hostility, is probably characteristic of a good many who have become active in the contemporary phase of the movement.

It is not hard to advance partial explanations for this situation. One reason undoubtedly lies in the character of much historical research in the closing years of the last century.
History has been written in response to a variety of motivations and to serve a variety of purposes. Certainly, many of "the Historical School" often employed history as an instrument of legal conservatism. It was not entirely without reason that some who pioneered in the application of empirical technique to the study of legal processes recognized in the legal historian their natural enemy. Perhaps more fundamentally, it is a matter of temperament. An urge to disturb the dusts of the past and a desire to apply empirical technique to contemporary issues are not always found in the same person, and rarely in anything approaching the same degree of intensity.

Nevertheless, it seems to me that historical studies have a role to play in the current movement toward empirical inquiry and that this role has not always been adequately appreciated. In making this observation, I am not simply viewing with alarm the current state of scholarship in American legal history. Of course, the field is and has been neglected; but a reasonable amount of very good work is being done, and there are favorable auguries for healthy development in the future. Nor should I be understood as saying that the study of legal history can be justified only by its contributions to empirical inquiry into contemporary problems. No doubt, history, like Emerson's Rhodora, provides its own excuse for being. It may be essential to the "human study of the law," as Boorstin would have it. And it has contributions to make to doctrinal, as well as other sorts of, scholarly endeavor.

But it is the relations between historical and "fact" research that I wish to assay. Presumably, the general object of systematic examination of legal institutions and legal processes is to derive understanding. But most research in the law contemplates that such understanding shall be put to use, and this use may often be the intelligent modification of existing institutions, processes, and doctrine. It is my contention that, in many instances, the attainment of these objectives is assisted and advanced by competent historical inquiry and that, clearly, the construction of any general theory of institutions is not possible without very considerable research into extensive historical sequences, along with much else.

In considering some of the reasons for this, I should perhaps begin with a truism. Most would agree that, ordinarily, intelligent fundamental modification of institutional arrangements requires the grasp of some useful general notions of institutional behavior and a great deal of understanding of what the particular institution is and does. And understanding of what any particular institution is requires knowledge of how it has become what it is. Perhaps most of what I shall say is really summarized in this formulation. But the statement is too general to be either very meaningful or persuasive.

Second, history expands the range of basic data relevant to the understanding and solution of contemporary problems. This is true both with reference to the ordinary functions and functioning of institutions and with reference to the effects of various measures impinging on the operation of institutions and processes. This proliferation of data provides a stimulus to the imagination when making provision for the problems of the future. It may, for example, permit the legislative draftsman to avoid "forgetting something" vital when he undertakes to order or influence the future.

Third, history frequently throws light on what will and what will not work. It frequently suggests something of the price that must be paid in countervailing values. Can it be doubted that a really competent modern account of that great (if noble) experiment, Prohibition, would have much of value to teach as to contemporary problems of social policy and the mechanisms of social control within the legal order? And should one doubt the significance of the element of recurrence in historical development, let him compare the problems of the Thames waterfront in the closing years of the eighteenth century, as described by Radzinowicz in the second volume of his evolving history of English criminal law, with the conditions of the New York waterfront in the 1940's and 1950's.

Fourth, historical research (to state an apparent paradox) is often required to overthrow the dead hand of tradition. To express the thought differently, history needs writing to correct false notions of history and the social consequences of such notions. No matter is of greater importance to the law reformer, or any other apostle of change, than the currently accepted historical image of the institution, arrangement, or process which he seeks to alter. This is true whether he be concerned with the jury, the privilege against self-incrimination, the elected judiciary, or the use of the seal in real estate transactions. As Morris Cohen well said, inertia is the first law of social change, as it is of physics. In the social arena the sanctification of existing institutions is the mechanism of inertia. No claim need be made that the competent writing of history will often, by its own force, guarantee the achievement of that which is needed. But it can and should be asserted that historical research may clear the ground for, and render more nearly possible, the rational and intelligent discussion of what is required to be done. Nor is it the point that historical research is only an instrument of change. For, as Julius Goebel has properly observed, it has lessons to teach as to what are the essentials of the tradition worth preserving as well as to what may sensibly be abandoned or altered.

Fifth, history serves to keep alive insights and proposals of the past which tend to be lost in oblivion. This is a waste of intellectual assets we can ill afford. The lack of continuity relating the work of one investigator to that of another, and the loss thereby occasioned, has frequently been noted in the social disciplines, including non-doctrinal work in the law. I suggest this discontinuity also separates the
The work of the generations. That the problem is real may be illustrated by a trivial example. Who has not experienced the shock of discovering (sometimes by accident) that his “new” idea was being discussed in the law reviews twenty-five or thirty years ago? Again, an interesting contrast is provided by the continuing influence of the Benthamite reformers in England and the comparative absence of influence, in this country, of such innovators as Livingston. I suspect that a complete explanation would take into account the differences in the character and extent of historical endeavor in the two countries.

Sixth, history works economy in another sense. Any major proposal for law reform is likely to involve some preliminary historical investigation, however unsystematic, simply because it is obviously indispensable. Such efforts are usually inadequate because of the labs involved in collecting relevant, but widely scattered, materials. Competent histories which collect and systematize the source materials ease these labs and go far to insure consideration of the proposal on a broader base of information and insight.

The foregoing observations, of course, do not in any sense exhaust the subject. There is an opposite side to this discussion. For the insights and techniques derived from empirical studies of current problems may often be of the greatest utility in historical research. Indeed, in many areas they have contributed wholly new conceptions of what is relevant and meaningful for historical study. Thus, the Kinsey studies, for all their methodological vagaries, and despite the sheer perversity and wrongheadedness that undoubtedly characterize the work, produced insights which are genuine and of continuing value. No subsequent studies of the history and development of American family relations or the regulation of sexual conduct will be able wholly to avoid taking them into account. Two generations of investigation into the relations of economic interest to political theory and thirty years of speculation as to the psychological underpinnings of judicial behavior have eliminated at least the excuse for production of a biography like Beveridge’s magnificent and magnificently naïve Life of Marshall. Moreover, “fact” research in its descriptive aspects often provides a base line from which subsequent change and modification may one day be measured by the future historian with a degree of accuracy never attainable heretofore. Indeed, the state crime surveys of the twenties and thirties and the Wickersham Report of the same period, for all their disappointing limitations, are already, in some measure, serving this function in the area of criminal law administration. One may hope and expect that the function will be served more adequately by the current Survey of the American Bar Foundation.

But allow me to return to my original thesis. The systematic study of things legal from other than a predominantly doctrinal orientation is in its infancy. Maturity is yet to be won. In making this effort, we shall be wise not to overlook the contributions which historical study can supply.