Law and Courts

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A Law & Courts Symposium:

Courts, Law, and the New (Historical) Institutionalism

Introdction

Ronald Kahn, *Oberlin College*

This symposium grows out of the Organized Section on Law and Courts' short course on "Courts, Law, and the New (Historical) Institutionalism," which was held a day prior to the 1998 APSA Meetings in Boston, Massachusetts. For this symposium, the presenters and several participants at the short course were asked to discuss what elements or aspects of the New (Historical) Institutionalism they thought were most and/or least conducive to furthering the study of law and courts. Participants, as you would expect from the diversity of their scholarly interests, chose to respond to the inquiry in quite different ways. The order of the essays here is the same as the order at the short course.

Eileen McDonagh and Rogers Smith lead off the first section with essays exploring the key elements of historical institutionalism. McDonagh contrasts the differences between the "historical" and "rational choice" variants of new institutionalism by examining how they diverge on five key assumptions: rational actor versus interpretive meaning; rationality versus ideas; conceptualizations of time and history; approaches to causality; and assumptions about order, stability and development. Smith explores the unique advantages, problems and promises of historical institutional approaches. He notes that the two primary advantages of such approaches are that they "go beyond models that take preferences as a given by seeking to analyze how actors are constituted in their identities, ideals, and interests," and that they "help us identify and explain probabilistic patterns of historical change." On the other hand, such approaches must confront the problem of "falsifiability," or the subjective nature of adjudicating between competing assumptions about what motivates political actors. A related problem is that in trying to capture too much they "may generate accounts that are no more than complicated narrative that reproduce the world's complexity without illuminating it." Smith concludes, however, by outlining the promise that historical institutional approaches hold for the study of law and courts and encourages further research to realize that promise.

The major themes in the two lead-off essays are explored further in the second set of essays which examine the application of the historical institutional approach to the study of law and courts particularly. Cornell Clayton's essay argues that the distinctive feature of historical institutional approaches for research on law and courts is their ability to focus on particular types of questions or subjects of analysis. Consequently, he emphasizes the continuity between "new" and "old" institutional approaches in public law, the need to recognize that such approaches may employ different methodologies (including positive ones), and the need to recognize when such approaches may be inappropriate. Howard Gillman's essay provides important insights on the promise and limitations of historical and rational choice variants of new institutionalism. He emphasizes the need for methodological diversity in the field but also the recognition of the distinct advantages of interpretive and historical approaches. Drawing upon research in progress, Christine Harrington and Ronald Kahn suggest that historical institutionalism fosters innovative ways to think about courts and law. Harrington demonstrates how the approach can be used to examine "the role of courts in generating the conditions of litigation," rather than focusing on litigation rates as primarily the product of mobilization by litigants. Kahn argues that we must take

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General Information

Law and Courts publishes articles, notes, news items, announcements, commentaries, and features of interest to members of the Law and Courts Section of the APSA. **Law and Courts** is published three times a year in Winter, Spring, and Summer. Deadlines for submission of materials are: November 1 (Winter), March 1 (Spring), and July 1 (Summer). Contributions to **Law and Courts** should be sent to:

Cornell W. Clayton, Editor
Law and Courts
Department of Political Science
PO Box 644880
Pullman, WA 99164-4880
509-335-2544
FAX 509-335-7990
e-mail: cornell@mail.wsu.edu

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We will be glad to consider brief articles and notes concerning matters of interest to readers of **Law and Courts**. Research findings, teaching innovations, or commentary on developments in the field are encouraged.

Footnote and reference style should follow that of the *American Political Science Review.* Please submit two copies of the manuscript; enclose a diskette containing the contents of the submission; provide a description of the disk's format (for example, DOS, MAC) and of the word processing package used (for example, WORD, Wordperfect). For manuscripts submitted via electronic mail, please use ASCII or Rich Text Format (RTF).

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seriously the historical institutionalist premise that the political system is not homeostatic, but rather consists of institutions that are, at times, in conflict because society consists of multiple, conflicting orders. To make better linkages between the Supreme Court and this more complex conception of society, he suggests, we need to study the development of "precedential social constructs" in the Court's decision-making.

Critical assessments of historical institutionalism are offered in the third section of the symposium. Mark Graber and Susan Lawrence lead off with arguments about the futility of the battles over whether scholars should pursue historical or rational choice strands of new institutionalism. They argue for the highest quality scholarship and the appropriate application of each of the strands and other methods of inquiry as well. Michael McCann builds on this theme by arguing for the development of theory and methods that will further permit the integration or reconciliation of the two primary strands of new institutionalism. McCann writes, that "those interested in law's 'constitutive' power could take more seriously its linkages to strategic action and instrumental activity" rather than dismissing those latter concepts as the rightful concern only of rival approaches. Finally, Roy Flemming concludes the symposium with an elegant exploration of the numerous ways in which history is used in law and courts scholarship. He cautions us that scholars who wish to incorporate historical institutionalism into their studies of law and courts should be very careful about how they use history when making causal statements.

KEY ELEMENTS OF HISTORICAL INSTITUTIONALISM

NEW INSTITUTIONALISM: COMPARING RATIONAL CHOICE AND HISTORICAL INSTITUTIONALISM

Eileen L. McDonagh, *Northeastern University*

Of the many types of new institutionalism (Immergut 1998:5; Clayton and Gillman 1999), rational choice and historical institutionalism are of particular interest to the field of law and political science. Although both approaches share common concerns and methods, it is also possible to delineate their distinctive perspectives by reference to five analytic modes: (1) Rational actor versus interpretive meaning, (2) Rationality versus ideas, (3) History matters, but how? Timelessness versus the flow of history, (4) Causality: Decisional sequence versus contextual contingency, and (5) Order: Stability versus development.

I. RATIONAL ACTOR VERSUS INERPRETIVE MEANING

A rational choice perspective tends to focus on the rational actor as a primary component of an analytic context explaining political outcomes; meaning and understanding of a context thus become equated with the dynamics of its rationality. By contrast, historical institutionalists tend to view the analytic context as defining meaning for an interpretive community. Thus, the meaning and understanding of a context are equated to shared cultural and intellectual norms that can range over many concepts, what Ira Katznelson might term a "macroanalytic" perspective (Katznelson 1997).

In the field of law, instrumental models of judicial behavior (Burgess 1993: 445-446) are premised on a rational, power-maximizing actor in which politics is a process for allocating scarce resources and collective behavior is the aggregate of individual-level behavior. Socialization, education, personality, economic, technological contexts, and structural contexts may influence the preferences of the rational actor, but the key element to be explained is the preferences of the individual level actors themselves as a foundation for analyzing the rationality of their decision-behavior. Historical institutionalism, on the other hand, emphasizes interpretive order rather than the rationality of actors. According to this view, judicial processes cannot be understood solely in terms of decision processes that maximize the power of the strong in relation to the weak. Rather, politics is viewed as creating and locating meaning, "which in turn affects the shape of the community and the individual" (Burgess 1993:447). Law and legal rhetoric are "instruments of interpretive order" that express the way communities share an understanding of their "experience, values, and the nature of existence" (Burgess 1993:447). Historical institutionalism in this context looks at the ways "in which symbolic behavior transforms mere instrumental behavior and is transformed by it" (Burgess 1993:448).

According to this view, individual preferences can remain central, politics can be defined solely in terms of decisional outcomes, and the law may remain portrayed as simply "flat decisional rules" (Burgess 1993:448). The difference is that historical institutionalism posits politics and law as transformative, not merely as manipulative or mystifying (Burgess 1993:450).

II. RATIONALITY VERSUS IDEAS

A related distinction is that between rationality and ideas. Rational choice perspectives stress the constraints imposed on decision making processes, so when properly identified,

political phenomena can be modeled in terms of a game defined by a set of decision outcomes. Solving for an equilibrium solution to the game then explains the political outcome of the phenomenon under study in terms of the rationality of the overall process. Historical institutionalists, by contrast, more often explain political phenomena in terms of concepts and ideas that transcend the perspectives of individual actors. The study of institutional processes thus begins with the definition of conceptual frameworks, including "contingent developments [that] stand beyond logic and can only be grasped through historical analysis" (Immergut 1998:19). The resulting understanding of "what happened" goes beyond what any actor at the time might have been able to envision.

To illustrate this distinction we might contrast Barry Weingast's rational choice analysis of the causes of the Civil War with how an historical institutionalist, such as Rogers Smith, approaches that topic. Weingast's interest in American democracy centers on its stability (Weingast 1998:148). The Civil War is important in his analysis as a paramount example of the result of a "dramatic" breakdown of democratic stability in the 1850s (Weingast, 1998:148). He concisely analyses the breakdown in terms of a "spatial model of politics" that delineates a set of decisions of rational actors bent on preserving a balance of power between the North and the South in the Senate over the slavery issue; when the "balance rule" solution broke down, consensus was destroyed, and instability in the form of the Civil War was the result (Weingast 1998:176).

Rogers Smith's analysis of the Civil War, on the other hand, begins with a large conceptual framework for understanding the course of American political history in terms of competing traditions of intellectual and political thought: liberal, republican, and ascriptive. Whatever the motivations and preferences of the actors, the significance of the Civil War lies in the way it increased democratization as part of a trajectory defining American political development. As Smith puts it, "Overall, there have been three great eras of democratizing American civic reforms: the Revolution and Confederation years, the Civil War and Reconstruction epoch, and the civil rights era of the 1950s and 1960s. It is a sign of how strong resistance to realizing liberal democratic ideals has been that during all these periods Americans fought great wars against opponents hostile to such ideals, first the British monarchy, then the Southern slavocracy, then the totalitarian regimes of Hitler and Stalin in World War II and the Cold War years. Only when these circumstances made fuller pursuit of egalitarian liberal republican principles politically advantageous" did reforms actually occur (Smith 1997: 16).

III. History Matters, But How? Timelessness versus the Flow of History:

A rational choice perspective posits the search for principles and maxims often viewed as exhibiting an all but timeless permanency. Such a perspective applied to the study of judicial processes is not oriented toward unraveling the connection between historical processes and the particular configuration of judicial review, but rather to finding in a study of judicial review those principles that verify an underlying model that is ahistorical, and in that sense, timeless. History matters, therefore, in the sense of providing raw material or data that illustrate the enduring and timeless principles under study.

Historical institutionalists, by contrast, are more often committed to uncovering connections between the past and the present on the assumption that the latter can only be understood in terms of the former. Morton Horwitz's analysis of the Warren Court, for example, not only "seeks to capture one of the most eventful and influential periods in the history of the United States Supreme Court—the sixteen years between 1953 and 1969," but also recognizes that "[b]efore we can understand the Warren Court revolution, it is necessary to realize that when we break into the flow of history in order to focus intensively on a particularly period," we must be careful not to underestimate "the importance of events that preceded and followed our own specific period" (Horwitz 1998:3-4).

IV. Causality: Decisional Sequence versus Contextual Contingency

Rational choice perspectives most often use a game as a metaphor expressing the dynamic interactions among a set of actors and the political outcomes of their actions. Obviously, in a game there is sequence, as one decision follows and leads to another; causality thus becomes subsumed under the rubric of sequence. Barry Weingast's analysis of the causes of the Civil War, therefore, takes the form of identifying a series of events whose sequence led to that outcome, where the most important event in the sequence was the demise of the balance rule (Weingast 1998:186).

Historical institutionalists, on the other hand, tend to "view causality as contextual," meaning that it may be impossible to model a causal process using a set of identifiable independent variables (Immergut 1998:19). For example, "because human actors have the capacity to learn from history, it is indeed very unlikely that even the precise levels of unemployment and inflation of, say, the thirties would produce identical fascist movements today" (Immergut 1998:19). Thus, sensitivity to contexts limits generalizability across cases (Immergut 1998:19).

V. Order. Stability versus Development

Rational choice perspectives tend to focus on stability as the overriding systemic characteristic of interest when studying order. In his study of Genoa, for example, Avner Grief is particularly concerned with determining the conditions under which "the rules will change, leading to the rise—or collapse—of the political system," that is, stability versus instability (Grief 1998:23). So, too, as noted above, is Barry Weingast interested in explaining the stability, or lack thereof, of American democracy. Notably absent is the principle of development; more or less stability does not constitute more or less political development. Thus, Weingast does not evaluate in developmental terms political stability combined with slavery in contrast to political instability without slavery.

Historical institutionalists, on the other hand, tend to view institutions as having a "legitimacy beyond the preferences of individual actors" because institutions are "valued in themselves and not simply for their immediate purposes and outputs" (Lowndes 1996:182). As Karen Orren and Stephen Skowronek argue, the relationship between institutions and institutional development presents a picture of interconnecting order; as an institution changes in one direction another may remain static or change in a different direction (Orren and Skowronek 1996). Hence, change is assessed by reference to direction, that is, development, rather than stability. As a result, the same political phenomenon that exemplifies instability from a rational choice perspective, such as the Civil War, can exemplify political development from the perspective of an historical institutionalist. In Karen Orren's analysis of labor and law, for example, change from a common law, feudal order undermining the interests of labor in the United States to a statutory law, liberal order more supportive of those interests constitutes political development, not political instability (Orren 1991).

Though neither rational choice nor historical institutionalist perspectives can or should be boxed into narrow analytic confines, nevertheless, we can discern differentiation between them. Perhaps the most intriguing question—outside the scope of this brief essay—lies not in identification of the systematic differences between rational choice and historical institutionalism, but rather in the sociology-of-knowledge question about why the responses of scholars to those differences can be so varied if not contested.

Historical Institutionalism and Public Law

Rogers. M. Smith, *Yale University*

Apart from my 1989 APSR article, much of what I've written previously on historical institutionalism has discussed it as an approach to studying politics generally, not so much specifically public law. Here I want to explore some of the advantages and problems of historical institutionalism by focusing on what it may be able to contribute to the distinctive concerns of public law scholarship, and on what such scholarship can contribute to realizing the potential of historical institutionalism.

I. Advantages

I see at least two important comparative advantages that may be provided by historical institutionalist approaches. First, much of it tries to go beyond models that take preferences as given by seeking to analyze how actors are constituted in their identities, ideals, and interests. Of course political actors are multiply constituted, with biological and economic needs driving much of their behavior. Yet it seems more than plausible to say that political institutions shape both how they conceive of and pursue those interests and many of their other felt needs and aspirations as well. The American regime's citizenship laws have, for example, bestowed upon some and not others the identities of American citizens, and that status provides opportunities for a range of material pursuits denied to outsiders. It also fosters understandable tendencies to value the well-being of the American regime with which citizens' fates are bound up and to embrace at least some of that regime's traditions and ideals. Political parties are also institutions that help over time to form many of the values of those who identify with them due to early socialization, kinship ties, economic interests, attractions to charismatic personalities, or other reasons. And perhaps more immediately, the American regime has clearly historically shaped the profession of American political science into an institution that, through its training and reward systems, tends to constitute the values of its members in many ways, including skepticism about the legitimacy of professedly undemocratic or religiously-based political regimes. If this constitutive role of politics seems questionable, consider for a moment what it would mean for American citizens, for Republicans and Democrats, for American political scientists, if their regime, their parties, and their profession should suddenly cease to be. Most of us would feel compelled to reconceive our professional and political interests, identities, and ultimately many of our values quite substantially.

A second great promise of historical institutionalist approaches is that they may help us identify and explain probabilistic patterns of historical change, even if we cannot fully predict future political events because of what appears to be the reality of partly self-determining human agency. Though we can rarely know for certain what any individual will do, by examining historical patterns we can judge what most people so situated are likely to do, and that is enough to make probabilistic predictions. I think historical institutionalists have actually done fairly well on this score in recent years. My colleague Stephen Skowronek's analysis of both the various types of challenges facing differently situated presidents and long-term secular political trends led him to suggest when Bill Clinton was elected that he was highly vulnerable to impeachment. I didn't believe it, so I've been impressed. Still, my own work on patterns of racial hierarchy and exclusion led me to say in the late 1980s that we should expect to see new intellectual defenses of racial inequality gaining attention in our own time. Few credited that claim prior to the publication of the *Bell Curve* and *Alien Nation*. Those are rather important developments for political analysts to have anticipated, comparable at least to those identified by other approaches.

II. Problems

Yet any candid assessment of historical institutionalist approaches must acknowledge they present serious problems. The first is falsifiability. It is easy to say that political institutions constitute actors' interests, identities, and ideals, but how do we show that persuasively? A plausible alternative view, embodied in many formal theories, is that actors have fixed but very general preferences ab initio, and that institutions simply provide the contexts that make certain specifications of those preferences more strategic than others. Institutions thus do not constitute preferences; they only affect their strategic expression. Another view is that institutions do provide the resources from which actors forge their preferences, identities, etc., but those resources leave actors with such extensive discretion that knowing the institutions tells us relatively little about who actors will decide they are and what they will do. My worry here is that our best means of adjudicating among these accounts may be ineradicably subjective. When we enter as best we can into the thinking and actions of particular actors, we may conclude that their notions of their basic preferences did change at certain points, or that their economic interests were less important to them than their political ideologies, or we may conclude something else. But often we will be hard pressed to show hard evidence falsifying alternative accounts: the "new" preferences can always be redescribed as new specifications of the same old quite general ones. The actions allegedly taken on the basis of political ideology will rarely be so economically self-destructive that they cannot be credibly described as means to economic ends, short or long-term. Our choices among different accounts may seem to rest simply on subjective intuitions.

The second, related problem of historical-institutional approaches is that in trying to capture the complicated processes of preference formation and transformation as well as the strategic pursuit of preferences, these approaches may generate accounts that are no more than complicated narratives that reproduce the world's complexity without illuminating it--just many damn things after others. Such accounts may or may not be good history, but they are not good social science. They do not offer parsimonious explanations backed by evidence suggesting why some one, two, or at most three independent variables are much more powerful than others.

III. The Study of Law and Courts

Let me now turn for a moment to the political science enterprise of studying law and courts to develop some features of that enterprise pertinent to these problems. Most work on law in political science expresses, in one way or another, two premises. The first is that law and courts are political institutions, distinctive in some ways perhaps but political at bottom, appropriately serving sometimes as independent, sometimes as dependent variables in political analyses. It's because we believe this premise that we believe the study of law and courts belongs in political science. The second premise, however, is that law and courts do display enough distinctiveness or relative autonomy from the features and behaviors of other political actors, institutions, ideologies, movements to merit separate attention. It's because we believe in this distinctiveness or relative autonomy that we have a subfield for such study. These two premises are in some tension with each other, or at least they pose a problem: in what senses are law and courts political institutions, and in what senses are they nonetheless distinctive political institutions? Though I cannot work out the details here, I think our different answers to these questions account for a lot of the variation among scholarly approaches and analyses in the public law subfield.

IV. The Promise of Progress

For me the above points suggest why historical institutionalism and public law studies can be and are being fruitfully united. First, historical institutional work may prove able to show how legal institutions such as the structure of courts, the makeup of bar associations and law schools, the characteristics of legal rules and discourses, all constitute political actors who participate in them in distinctive ways. I think here of Howard Gillman's argument that legal institutions foster notions of special roles and duties that sometimes prompt actors to behave in ways contrary to other elements in their political ideologies and economic interests. If we can show that the duties attached to roles predict judicial

behavior than preexisting ideological attitudes, we will provide substantial support both for the historical institutionalist claim that "institutions constitute" and that legal institutions are relatively autonomous, fostering behavior that cannot be read off from other preexisting political and economic interests. And it is plausible to think we can find evidence that points in these directions. Consider the differing positions on rights of the accused that Earl Warren took as Attorney General of California and as Chief Justice of the United States, for instance, or Lewis Powell's post-judicial tendency to endorse gay rights before liberal audiences when he was unwilling to do so on the Court. It is true, to be sure, that rational choice analysts can answer that both of these examples are evidence of different strategic expression of the same general preference functions in different contexts, not any shift in the actors' real preferences. It is hard to separate this claim from an imputation of insincerity by the actor in one or the other of the settings in which the actor advanced contradictory positions, but as 1998 dramatically underlined, insincerity in political figures is not an unknown phenomenon. Still, if historical institutionalists can offer no incontrovertible evidence that institutions can alter actors' genuine preferences, not merely their expressed preferences, rational choice analysts can also offer no incontrovertible evidence that only shifts in strategic expression are involved. Hence historical institutionalists accounts are not inferior to any others in terms of the "hard" evidence, and if in particular cases close study suggests strongly, if subjectively, that an actors' preferences genuinely were transformed, there is no reason to reject that claim.

Second, merging historical institutionalist and public law concerns may lead us to recognize how legal institutions play a relatively autonomous and significance role in constituting the identities and interests of actors who are not such direct participants in the legal system. I have already noted how citizenship laws, including immigration and naturalization laws, have historically permitted some to become Americans and others not. I think there can be little question that if the many thousands, probably millions of Chinese who would have come to the U.S. between 1882 and 1965 had been allowed to come, the identities, interests and ideals of those persons and their descendants would have been dramatically different than what they were under the Chinese empire, Sun Yat Sen and Chiang Kai Shek, and then Mao's Communists. But Chinese exclusion also reshaped the identities and interests of many Americans, reinforcing beliefs that this was a "white man's nation" in ways that sustained senses of racial superiority for some and justified systems of racial oppression for others. Conversely, the presence of the equal protection clause and other constitutional guarantees have clearly been resources helping Chinese immigrants and other racial minorities not only to advance the claim that disregarding their rights is profoundly illegitimate, but also to believe it themselves. Again, by turning to how legal institutions affect these other sorts of actors, we may be able better to develop both plausible claims that institutions constitute and transform peoples' identities, values, and interests, and that legal institutions often play a relatively distinctive role in these processes.

Finally, the other great promise of historical institutionalism, to discern tendencies in patterns of historical development, also may be advanced by linking historical institutionalist and public law concerns in ways that simultaneously provide support for law's relative autonomy. Again, my study of the history of citizenship laws strongly suggests that we should expect people to adhere to their legally established and recognized identities and privileges. When transformations are compelled, usually by political pressures external to the law such as wars, we can expect periods of political reaction in which the more powerful seek to reestablish their legally supported privileges and status. Hence every period of racial legal reform has been followed by a period of resistance producing stagnation and partial reverses on the road to greater racial equality. This is an argument about predictable patterns in processes of racial development in which the role of law in constituting racial identities is pivotal, supporting claims both that institutions constitute and that legal institutions do so in irreducibly important ways.

Other examples spring to mind. Bruce Ackerman's work on "constitutional moments" shows that concerns to vindicate the legalistic legitimacy of processes of constitutional transformation, even when preexisting legal frameworks were being overturned, has played a significant role in shaping the agendas and strategies of political actors such as the Federalists, the Civil War Republicans, and the New Dealers. Karen Orren's work on "belated feudalism" and her work on "multiple orders" joint with Steve Skowronek also show that legal institutions can carry forward distinctive, politically potent arrangements of economic and office holding rights in ways that limit and condition the political courses available to a wide range of political actors.

Still, it must be said that the promise of historical institutionalism to discern broad tendencies and patterns in political development and to vindicate the claim that institutions constitute and transform actors' preferences is still more promise than realization, and that the contributions of such approaches to public law, and public law to such approaches, have yet to go so far as to answer all concerns about the serious problems I have noted. That is only to say, however, that there is much more work to be done. For the reasons I've reviewed, I think it is work very much worth trying to do.

APPLYING HISTORICAL INSTITUTIONALISM TO LAW AND COURTS

On the "New" Historical Institutionalism

Cornell W. Clayton, *Washington State University*

I wish to suggest what I see to be the distinct advantages of historical institutional approaches for the study of law and courts. Let me begin, however, by agreeing with those who argue that we should be more focused on answering interesting questions in interesting ways than on what we label ourselves (see Graber herein). Still, for better or worse, scholarship takes place within the constraints of a profession where labels often matter a great deal when it comes to the distribution of resources and opportunities. Moreover, identifying what counts as 'good scholarship' sometimes requires us to be self-conscious about the theoretical and methodological norms we ascribe to, particularly if we think that good work might be undervalued without this sort of intradisciplinary discussion. And so I welcome such discussions and I see them as a sign of health and vigor for our field.

I. Why Historical Institutionalism?

I think the renewed enthusiasm for historical institutional scholarship is a very positive development for the law and courts field. I say this not because I believe that historical institutionalism is "better" than other approaches, but because, for reasons I have explained extensively elsewhere (Clayton 1999), I think it has advantages for addressing certain kinds of questions that have been largely ignored by contemporary behavioral and rational choice research. Central among these is how the unique features of courts and other legal institutions may lead to a distinct kind of politics (Gillman 1997). While we all agree that courts and judges are political actors and they are obviously important policymakers, the kinds of politics one finds in courts is quite different than that found in other political arenas such as legislatures or regulatory agencies. Consider, for example, why we don't see "vote swapping" or "log-rolling," a common practice in legislative politics, as part of judicial behavior. Or consider why Justice Powell concluded in Bowers v. Hardwick (1986) that, because neither the parties nor the court below addressed Eighth Amendment concerns, he could not vote to strike Georgia's anti-sodomy statute even though he believed it to be unconstitutional. How do justices come to think of themselves as bound by conventions that require appellate courts to abstain from deciding issues not properly before them?

Behavioral and rational choice approaches are inclined to minimize such differences because they tend to focus on the relationship between the individual policy preferences of legal actors and the policy outcomes of the legal process. By contrast, historical institutional approaches explain these differences by focusing on how historically evolving patterns of value and meaning (institutions) give rise to distinct senses of obligation, commitment or motivation (Burgess 1993, Brigham 1987, Smith 1996b). Rather than assume judges are motivated by the simple pursuit of policy preferences, historical institutionalists make judicial motivation the central subject of analysis. This willingness to treat judicial motivation as a subject of analysis, and not as a topic that is considered exogenous to empirical investigation, seems to be the defining feature and advantage of historical institutionalism relative to other approaches; and it, rather than a particular methodological commitment or a particular conception of institutions and institutional change, explains whether one's work falls within that approach. Now if it is right to define the historical institutional approach in this way, as regarding *focus* and *subjects* of study, then there is room within this approach for many types of research. Historical institutionalism therefore should be thought of as an inclusive, not an exclusive approach. Three related points also follow:

II. "New" and "Old" Institutionalisms

First, the "new" historical institutionalism shares its commitment to a constitutive understanding of legal institutions with a more traditional institutionalism practiced by scholars such as Edward Corwin, Charles Grove Haines, and Robert McCloskey (Clayton 1999). There are important differences between the new and the old. Among these is the acceptance of a less formal conception of institutions, one emphasizing the role of informal norms, myths, habits of thought or background structures such as race, class and gender, in addition to more tangible structures such as judicial doctrines, statutes and constitutions.[1] But these differences are minor when contrasted with the common commitment to understanding how law and the institutional features of courts structure judicial politics, and much will be lost if we obscure the kinship between new and old. This continuity should be a source of pride not embarrassment for institutional scholars, and less attention should be paid to distinguishing a "new" institutionalism and more to the endurance and coherence of institutional research.

III. Methodological Diversity

Second, since historical institutionalism is about subjects of study, it is neither wedded to, nor incompatible with,

particular methods of study. Of course, since those using this approach are interested in understanding the motivational frame of mind of legal actors, they may naturally be drawn to interpretive and narrative methodologies, which provide more direct insight about motivations than positive methodologies, which tend to examine external outcomes or patterns of behavior (Burgess 1993; McCann 1996). Still, it is possible to draw inferences about motives on the basis of external patterns of behavior, so long as one recognizes the limitations of such inferences (something that many positivists are reluctant to do). If the central claim of the historical institutional approach is correct, that legal institutions lead to a distinctive kind of politics, then we should be able to find systematic evidence in the external behavioral patterns of legal actors supporting this claim and, perhaps, build limited models that test key assumptions. Such research will probably always need to build on more interpretive and narrative accounts, if only to make sense out of what are appropriate variables; how else would we know when a particular action or decision should be thought of as "authentic," "strategic" or something else? (Gillman 1997). Nevertheless, positive methodologies provide the benefits of generalizability and empirical rigor that interpretive and narrative accounts alone may lack. Given the concerns raised by Smith (herein) and others (Gates 1991), a combination of methods seems to me to be the most promising route for historical institutional scholarship.

IV. Seeking Answers in the Right Places

Finally, I began this essay by noting that my enthusiasm for historical institutional approaches comes from my belief in its advantages for addressing certain types of questions: Specifically, how legal institutions lead judges and other actors to a distinctive kind of politics. Of course, there are other interesting questions about law and courts, and different approaches may be more or less appropriate to those. Those interested in understanding how judges may sometimes use legal institutions instrumentally in pursuit of their individual policy preferences, for instance, may wish to adopt "strategic" institutional approaches (Epstein and Knight 1998) or more traditional behavioral ones (Segal and Spaeth 1993). My hunch is that several different kinds of political behavior can be found within courts and the legal arena: Why should it surprise us to find that judges and lawyers, like other human beings, operate from a variety of motivations? Proponents of historical institutionalism should avoid the mistake made by some advocates of other approaches, believing that one avenue of research is sufficient for a fully explanatory social science of law and courts. It is highly unlikely that any one approach can ever capture the myriad ways in which politics occurs in courts, or other arenas for that matter (Baum 1998). The "best" scholarship will therefore employ multiple methods and approaches. But, as good scholars, we should be cognizant of why we adopt certain approaches and question whether others may be better suited to the questions for we seek answers.

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1. Elsewhere I have argued that NHI sifts too far in this direction it may rob itself of empirical rigor and the ability to engage in meaningful normative analysis (Clayton 1999).

Some Scattered Observations About Applying Historical Institutionalism to the Study of Law & Courts

Howard Gillman, *University of Southern California*

I want to suggest some reasons why I think the new historical institutionalism has something to offer those who are interested in studying law and courts.

I also want to make it clear at the outset that I have no interest in transforming my preference for historical and interpretive methods into an argument against other approaches to the study of law and courts. It is true that I think that historical and interpretive approaches can nicely compensate for some of the disadvantages of other methods of inquiry, particularly behavioralist and rational choice approaches, but I make this point only as part of a more general belief that all social science methods and concepts should be treated as tools that do some things well but also other things not so well. The test of every approach is whether it helps illuminate the topic under review in ways that we consider instructive or provocative, and this (purposefully elastic) test is foolproof so long as we resist the temptation to be dogmatic by declaring in advance that only work which conforms to our preferred methodology deserves a passing grade. There is widespread and appropriate agreement in our field that behavioralist and rational-choice perspectives have passed this test, and I want to add my voice to those who believe that interpretive and historical approaches do as well.

Rather than repeat a series of sustained arguments that I have elaborated elsewhere (see, for example, my essay in the volume that Cornell Clayton and I just coedited entitled Supreme Court Decision-Making: New Institutionalist Approaches [Chicago, 1999]), it may be best here to show some restraint and just set out some modest and

idiosyncratic observations, none of which have anything to do with clarifying what historical institutionalism "is". To force myself to be as pithy as possible—and, acting strategically, to maximize the chances that this contribution will be read—I want to reduce my points to a series of ten (unwieldy) sentences.

1. Because historical institutionalism can address a wide range of political events and relationships, and because its practitioners tend to conceptualize politics in more constitutive, dialectical (less linear) ways, it may allow students of law and courts to say more interesting and innovative things about judicial politics than has been evident during our decades-long preoccupation with the measurable determinants of appellate court decision making.

2. At the same time, given this decades-long preoccupation, it should be noted that one of the things that historical institutionalism allows one to do is provide more precise or nuanced accounts of actual judicial attitudes and how they are constituted—which is a good thing to be able to do in a subfield that has made the influence of "attitudes" central to its most successful model but which, ironically, spends too little time reconstructing the expressed beliefs of decision makers.

3. Historical-interpretivists deserve credit for moving beyond the complacent assumption that judges merely manipulate the law to pursue exogenous preferences by exploring the question of whether the world view of judges is, at least in part, "constituted" by their professional training or institutional responsibilities; at the same time, we should not make the mistake of assuming that judges in fact have a distinct world view or that they do not act instrumentally or strategically—the actual motivation of a judge in a given case should remain an open empirical question to be scrutinized through historical-interpretive reconstruction.

4. If we are going to explore the question of whether judges and lawyers seem influenced by a distinctive set of institutional structures or norms then it is likely that we will be focusing at least part of our attention on the influence of evolving (and always-contested) conceptions of the judiciary's role in the political system and of the prevailing understanding of the contours of "rule of law" ideology—although it is important to keep in mind, as E. P. Thompson taught us, that legal and political elites have an interest in both maintaining and exploiting "rule of law" ideology.

5. There need be no animosity between historical institutionalists and rational-choice institutionalists, since both are advocating approaches that take seriously the relationship between agency and context (which, from the point of view of scholars who take more "constitutive" approaches to these questions, are not really two different things).

6. On the other hand, the more one takes seriously the importance of agency and context for an understanding of behavior the more indispensable historical-interpretive methods become, since (obviously?) the reconstruction of agency and context is entirely and exclusively a historical-interpretive enterprise; at the same time, it remains to be seen whether our understanding of these relationships will be enriched by efforts to translate the results of historical-interpretive analysis into the stylized conventions of rational choice.

7. We still need to keep in mind (as a reiteration of point 1) that the "situated decision makers" who are the subjects of our analysis (not the objects of our analysis) exist in places other than appellate courts and are influenced by institutional contexts that are not purely judicial ones, such as political regimes, social movements, and economic systems.

8. As we start paying more attention to a wider variety of legal or judicial decision makers who are operating in an array of contexts (from the Justice Department to union halls) it will become increasingly difficult for political scientists who are historical-institutionalists to maintain a distinctive disciplinary identity (since our work will naturally blend with the sort of work being done by our law-and-society counterparts in history, sociology, anthropology, and so on), and this is a strategic challenge for young scholars who are tempted toward historical institutionalism—a challenge that does not burden the counters and modelers who still try to claim that it is their counting and modeling that makes them political scientists.

9. The best way to cope with this challenge is to write as methodological pragmatists and point out that there are certain questions which are central to law-and-courts scholarship that are best explored through the use of these methods; but this strategy only works (for us, anyway) if we can make our colleagues interested in those questions that are best interrogated through these methods—hence, the importance of Mark Graber's overarching advice to be interesting above all else (with "persuasive" running a very close second), even above caring about whether you are properly deploying the methods and concepts associated with The (New) Historical Institutionalism (which is proudly and appropriately distinguished from more positivist approaches by its unwillingness to declare formal methodological criteria for what counts as a proper deployment).

10. There is nothing interesting to be said in the abstract about how to be interesting or persuasive, but historical institutionalism provides as good a rubric as we have had in a long time for opening up new lines of inquiry or approaching old topics in innovative ways (see points 1 and 7); and so

while it is possible to do interesting or persuasive work without invoking the mantra of The New Historical Institutionalism there might be good strategic reasons (having mostly to do with the politics of disciplinary respectability) for like-minded scholars to self-consciously associate themselves with this development at this time.

An Institutional Approach to Litigation

Christine B. Harrington, *New York University*

New (historical) institutionalism is not new to the field of Public Law, though the struggle to define and determine key theoretical assumptions still rages. Professor Susan Burgess writes, "Applications of the new institutionalism to the study of public law are often grounded in rational choice assumptions. This tends to reinforce a view of law and legal rhetoric as merely instrumental tools that foster certain institutional arrangements" rather than "to sustain and to critique the political order (1993 445)." Her analysis of "judicial supremacy" discourse offers a compelling case for understanding this particular rhetoric as an institutional practice (Brigham 1987) of an interpretive order (also see Brisbin 1993; Kessler 1993). At this point in the development of new (non-rational choice) institutionalism the challenge is to articulate the phenomena underlying assumptions of the approach. Specifically, what constitutes an "institutional practice" and how do we interpret its concrete effects?

With the revival of institutional perspectives in the social sciences, March and Olsen argue that the "claim of institutional autonomy is necessary to establish that political institutions are more than simple mirrors of social forces. Empirical observations seem to indicate that processes internal to political institutions, although possibly triggered by external events, affect the flow of history" (1989: 18). We want, however, to avoid resorting to behavioralist concepts of institutions because institutions and actors are not simply interchangeable units of analysis as some behavioralists who employ a "neo-institutional perspective" contend. For example, Epstein, Walker and Dixon argue that "just as individual decision making can be explained and predicted by various social, political, and legal factors, so too institutional behavior can be understood vis-a-vis a stable set of factors over an extended period of time" (1989: 826). They focus on institutional behavior and define it as the sum of micro-level behaviors; courts are treated as institutional actors embodying individual attitudes. Instead of extending assumptions developed at the individual level to institutions, social scientists need to question whether these behavioral assumptions can in fact be appropriately applied to the study of institutions. Can institutions, such as courts be conceptualized as the sum of individual strategic behavior? I think not; indeed it is precisely this instrumental view of courts that new (historical) institutionalists takes issue with and alternatively deploy an interpretive conception to institutional practice (Harrington and Yngvesson 1990). From this perspective, we would want to know whether and how institutionally embedded practices, by which I mean rules, procedures and ideologies, constitute social relations.

In research on U.S. federal appellate civil litigation rates from 1960 to the present, I have been empirically working out an interpretive, historical perspective (see Harrington and Ward 1995). Building on some earlier litigation studies, without replicating behavioralist assumptions about individuals at an institutional level, I argue that we can locate sociolegal phenomena, like litigation, in an institutional context by identifying characteristics or properties of circuit courts which may shape litigation activity.

Most studies of litigation rates over time focus on the mobilization of court services by litigants and the changing nature and volume of demand. They pay little attention, however, to the role of courts in generating the conditions for litigation. We know more about why people turn to litigation than about how courts, as institutions, influence litigation patterns. Courts have their own histories and traditions as well as discretionary powers that might affect the consumption of court services over time. Courts, however, do not cause litigation in some direct way. The process of disputing is complex, as are the institutions engaged in dispute processing. Legal institutions, like other political institutions, signify what may be possible rather than determining outcomes. When courts attract certain kinds of cases they become part of the production of litigation itself. By reinterpreting litigation rates, from demands on courts to attributes of courts, I argue for an institutional analysis of litigation which builds on studies of resource mobilization and conceptualizes courts as political agents that attract litigation.

If the "architecture of the legal system [rules, lawyers and parties] creates and limits the possibility of using the system as a means of redistributive change" (Galanter 1974: 194), then interpretive institutional studies of appellate litigation activity may tell us something about the design of circuits. The conventional approach to analyzing litigation, however, has focused on developing explanatory theories about the use of litigation in an effort to identify what economic,

political, and social factors determine whether people are more likely to litigate certain kinds of disputes. If we interpret patterns of appellate litigation as an effect of circuits, we then turn the question around and ask— what can we learn about the institutional character of appellate courts by controlling for environmental factors, such as political and economic variables, when we compare litigation activity among the circuits? By empirically studying variation in litigation activity we may learn more about processes internal to appellate courts, which, as March and Olsen suggest, may themselves be "triggered by external events," but are often themselves ignored in longitudinal studies of litigation. Specifically, I have been studying two internal properties of circuit courts— reversal rates and method of handling cases (administrative or adjudicative), and one more general characteristic of the institution — the political composition of the bench. I control for environmental factors in order to empirically explore the relative autonomy of circuit courts in shaping litigation patterns over time.

New (Historical) Institutionalism: Relating Supreme Court Decison Making to Social, Political and Economic Change

Ronald Kahn, *Oberlin College*

I want to highlight some new directions for research on Supreme Court decision making fostered by the New (Historical) Institutionalism (NHI). I will emphasize here how NHI might increase our understanding of how justices bring the social, political, and economic world into their decisions. How might "historical institutional approaches" to studying the Supreme Court do more than "generate accounts that are no more than complicated narratives that reproduce the world's complexity without illuminating it" (Smith, herein; also see Flemming herein)?

NHI theory forces us to rethink many of the premises that have informed our analyses of Supreme Court decision-making. First, NHI asks us to reject the idea that institutional change is homeostatic — that legislative, legal, and executive institutions act in equilibrium with each other (See Skowronek 1995: 96). For most, not all NHI scholars, this premise is replaced by the concept of multiple orders in the American political system, with each institution having different standards of legitimate action and different patterns of change (See Orren and Skowronek 1996 and Smith 1997). This central premise of NHI requires scholars not to be arrogant; no longer should they overestimate the role of the Supreme Court as a venue for change (see McCann herein). It also requires scholars to view the role of the Supreme Court in bringing about social change as complex and multidirectional, and interrelated with other institutions. Because the institutional and political contexts of courts differ so much among nations, this premise tells us to think comparatively about courts and elected institutions, both within the American context and in relationship to other nations.

However, a second central premise of NHI, that institutionally grounded standards of legitimate action shape the behavior of political actors, forces scholars to realize that the Supreme Court has a unique place in our political system. This is due to two primary reasons: the power of judicial review gives the Supreme Court far more authority than elected officials to evaluate claims of legitimate action by public institutions, especially in terms of how they treat citizens. At the same time, Supreme Court justices take seriously questions about their own claims of authority, and this concern limits their freedom to act on personal policy and/or strategic grounds. This second premise also tells empirical-interpretive scholars to view the analysis of Supreme Court cases and doctrines as important, because the justices' views of what is legitimate action is affected by their longstanding polity and rights principles as they evolve in precedent.

A third premise, or group of premises of NHI, is that institutions are embedded in a historical context consisting not only of other institutions, but also of broader social, political, and economic structures in society. This is because the "political universe [is] organized and activated by intercurrence [defined as] engagements throughout the polity of the different norms embedded in institutions, the terms of control contested, more or less intensely, in the ongoing push and pull among them" (Orren and Skowronek 1996: 112) and "institutions are not merely influenced by, but are inseparable from, the web of social patterns of cognition and evaluation such as ideology, religion, class, race, and gender that situates all social activity" (Clayton 1999: 33; see also Smith 1997). This has important implications for empirical-interpretive scholars of the Supreme Court. It means we must look both within and without the Supreme Court in order to explain its behavior. We must ask under what conditions do justices take account in their decision making of social, economic, and political structures in the wider society? Can we identify how the Court responds to the outside world, given that justices hold different polity principles as to the relationship of the Supreme Court to political institutions, different rights principles, and different

interpretive philosophies? As political scientists, how do we get beyond the doctrinal criticism favored by most scholars in law schools, while respecting the importance of the internal analyses of cases, because applied polity and rights principles are central to Supreme Court decision making and the place of constitutional law in American political development.

One way to study the Supreme Court as part of American political development is to look at the Supreme Court primarily from the outside, as Gerald Rosenberg does in his excellent book (Rosenberg 1991). However, the best of such studies can only tell us that when the Court acted that other institutions, groups, and individuals had acted before it, and that after the Court acted institutions, groups, and individuals reacted to the Court's decision. While such studies are excellent in warning us not to be arrogant about the Court, because they place its actions within the larger political system, they do not consider on what grounds the Court made its decisions, because they do not take seriously the NHI premise that institutional norms and other standards of legitimacy are central to Court action. These standards may require the Court to reject calls to act or not act by outside institutions, groups, and individuals. At other times, these standards may require the Court to be outward-looking when it makes constitutional choices.

Somehow we need to make better links between Court action and the social, economic, and social structures in the nation, while at the same time respecting the fact that justices feel constrained to make principled, legitimate decisions.

One way to start to do this is to ask whether the Supreme Court develops within its jurisprudence what I will call "precedential social constructs." Precedential social constructs are beliefs about social, political, and economic reality that make their way into constitutional law and, I would argue, become part of precedents, just as are polity and rights principles. One such precedential social construct, for instance, is the Court's construction of "women in society," as it decides equal protection and substantive due process cases. Do members of the Court have a particular conception of women in society? And if so, do they tend to build on this vision, add filigree to it, and thereby bring new social, political, and economic "facts" into constitutional law? Do these "facts," which are really justices' beliefs about how social, political, and economic structures in society impact individuals, groups, and institutions, become part of Court precedents? One only has to look at the far more complex conceptions of women in society in Casey compared to Roe, and in the United States v. Virginia (1996), the Virginia Military Institute case, compared to prior gender discrimination cases, to see that the Supreme Court has an evolving conception (or social construction) of women in society—one that recognizes the changing realities of women in the workplace, home, and social and political venues. Thus, "precedential social constructs" combine abstract principles of "law" (principles that provide the grounds for legitimate action) with beliefs about the social, economic, and political contexts to which the law is to be applied.

By comparing and contrasting the precedential social constructs found in doctrinal areas, we can begin to get a picture of the willingness of justices to define new individual rights (and authority for institutions to act) and do so with a renewed concern for the fact that institutions are embedded in social, political, and economic systems, which affect each other in important ways. We also will need to study the relationship between levels of Court agreement on legal principles and the degree to which it is willing to create precedential social constructs. Under what conditions is the Court more or less likely to incorporate its views of changing social contexts into its decision making? In what doctrinal areas has the Supreme Court refused to recognize (and thereby legitimate) precedential social constructs?

I would argue that in decisions where the Court has refused to recognize precedential social constructs or have defined ones that clearly don't make sense in terms of the social, political, and economic structures of society, we will find that such decisions are more likely to be overturned, as was the Lochner case, or are ripe for overturning in the future, as are Bowers v. Hardwick (1986) and McCleskey v. Kemp (1987). Through the study of the impact of precedential social facts on Supreme Court decision making three additional scholarly objectives can be met. We can gain a better understanding of what it means to have a "living Constitution." We can provide important cues to scholars as to what constitutional theories are needed so we can do a better job in the future of defining the rights of subordinated groups. Finally, we can meet some of the major scholarly objectives of NHI, which seek to place the study of the Supreme Court, and all institutions, within a more complex model of American political development.

CRITICAL ASSESSMENT OF HISTORICAL INSTITUTIONALISM

HISTORICAL INSTITUTIONALISM AS/AND SCHOLARSHIP

Mark A. Graber, *University of Maryland, College Park*

Once upon a time I was a scholar. Then I became a social scientist. Shortly thereafter I became a political scientist, then a political scientist who studied public law. Soon I was a political scientist who studied public law with an emphasis on constitutional law. Now, I am apparently a political scientist who studies public law with an emphasis on constitutional law from an historical institutionalist perspective. If this is not bad enough, recent debates between Rogers Smith (1996b) and Karen Orren (1996) suggest the possible emergence of East Coast and West Coast schools of political science/public law/constitutional law/historical institutionalism. Evolution may produce superior species, but I would rather return to the days when we worried more about our scholarship and less about these conceptual adjectives.

Historical institutionalism generally strikes me as a positive development because practitioners have produced excellent work, particularly when they worry more about explaining the evolution of contemporary political forms and less about whether their work ought to be at the center of such artificial administrative conveniences as political science and public law. The scholarship self-identified historical institutionalists have produced serves as a welcomed reminder that few shortcuts exist in intellectual life. Public law scholarship inevitably requires learning much history, politics and law, not to mention economics, rhetoric and other subjects. Good professional reasons explain why people who do certain kinds of work have to worry about such questions as "is it political science" or "is it public law." The best programs and universities, however, are far more concerned with such questions as "is it good scholarship," and design institutions in ways to ensure that people doing good scholarship are hired and promoted, not banned from the academy by a set of rigidly imposed artificial barriers between subfields, fields, and disciplines.

Historical institutionalists have offered important new tools for explaining political and legal behavior, but the real virtue of their studies may simply be to encourage public law scholars to spend more time investigating actual political and legal affairs. All approaches to public law or politics, after all, must be judged by how well they enable scholars and citizens to explain, predict, manipulate, and assess those legal and political phenomena. Moreover, few models survive historical investigation intact. Inevitably, our theories grow and are enriched by detailed encounters with past legal and political phenomena. Failure to do adequate investigation may unduly limit the conceptual tools we use when analyzing political behavior.

Consider Jeffrey Segal and Harold Spaeth's (1993: 84) claim that in the Dred Scott case "Taney took a page from Marshall's book on devious deviations from proper judicial procedure and declared the Missouri Compromise... unconstitutional," when "[g]iven the ruling that Scott was not a citizen, the majority should simply have dismissed the case for want of jurisdiction." (84). Better historical and legal investigation would have revealed that Taney's analysis of the Missouri Compromise was framed as an alternative ground for denying jurisdiction (Dred Scott: 430; Corwin 1987: 302). If Dred Scott was not a citizen, then he could not sue in a federal court. No rule requires that justices include in their opinions every ground for reaching their conclusions. Thus, attitudinal models may be quite helpful explaining why Taney choose to provide a second ground for denying jurisdiction when one was quite sufficient. Still, Taney did not include in Dred Scott his thoughts on other matters, such as proposals to acquire Cuba or the best recipe for roast chicken. The Dred Scott opinion may best be understood as expressing all of Taney's attitudes on race and slavery that he believed existing legal conventions permitted him to express. More generally, this brief history suggests that we may better understanding judicial decision making by focusing on the interaction between strategic, attitudinal, and legal "variables" then by engaging in competitions aimed at proving which variables, standing alone, explain the most behavior.

Historical institutionalism provides a particularly valuable tool for a liberal arts curriculum. Education, from Plato's Republic to Bloom's The Closing of the American Mind, has always been understood as a means for critically evaluating preferences about the good life and good society, and not simply as providing the skills necessary to obtain various material goods. Prominent explanations of judicial behavior do not encourage such reflection. One school of thought understands justices as attempting to realize preexisting policy preferences. A second looks more closely at the strategies justices use to realize those preexisting preferences. Historical institutionalists, by comparison, are developing tools that teach students that what is in a person's interest is contestable, that different understandings of interest have competed over time, and that through education and experience people may change their understandings of their best interests. Such reflections, I believe, will not only produce better understandings of political phenomenon, but, more important, better citizens.

The New Historical Institutionalism: Taking the Promise Seriously

Micheal McCann, *University of Washington*

I have argued on a number of occasions that the New Historical Institutionalism often has represented little more than attractive new packaging for legal scholarship that is quite traditional in character and focus. This strikes me as especially true to the extent that the New Historical Institutionalism (NHI) has only further deepened our section's historical obsession with how U.S. federal appellate court judges, and especially the Supremes, act and choose in their lawmaking activities. However, I want to explore the other, more positive side of this contention in my brief comments here. In short, this essay recognizes four types of intriguing promises that the NHI movement offers for the development of innovative, important scholarship regarding law and courts.

First, and most generally, the NHI movement raises the possibility of facilitating meaningful exchanges with scholars doing similar work in other subfields of our discipline, other disciplines, and interdisciplinary traditions such as "law and society" scholarship. At a time when scholars with diverse interests are rediscovering the significance of law in political affairs around the world, our ability to communicate our developed insights about legal institutions, norms, and practices in widely recognized and respected intellectual terms – such as those identified with NHI — could be quite exciting. Given our subfield's longstanding Rodney ("I can't get no respect") Dangerfield complex, this prospect for the expansion of our scholarly audiences and influence is a welcome development.[1]

Second, NHI could be important to the extent that it pushes us to study law in new venues and relational contexts. As I read the record, NHI scholarship generally has emphasized the dynamic, conflictual, unstable, unsynchronized relationships among multiple institutional orders within polities. For public law scholars, it thus stands to reason that the NHI movement ought to encourage new studies regarding the tenuous relationships among different types of courts as well as between courts at all levels and other state and non-state institutions. The former might include attention to the broader linkages between judicial institutions and other official institutions of government in the formulation and implementation of discrete public policies as well as in both sustaining and contesting broader structures of hierarchy and domination. The latter might include greater attention to how official legal institutions shape and are shaped by the everyday politics of communities, neighborhoods, workplaces, schools, health care organizations, religious institutions, the professions, business enterprises, and the like.

Third, if we follow developments among new institutionalist scholarship in other subfields and disciplines, we might also expect greater emphasis on comparative analysis in the study of law and courts. This could involve self-conscious comparisons among legal institutions within specific polities, among legal institutions in different polities, and among institutional arenas at transnational and international levels.

Fourth, and finally, one might expect that the NHI characterization of institutional orders as inherently dynamic, indeterminate, interactive aggregations of conventional knowledges and ideas would provide a catalyst to more sophisticated, innovative theorizing about critical conceptual issues related to socio-legal study. On the one hand, this could involve greater attention to specifying more carefully the types of overall analytical frameworks that we generate. For example, one very welcome implication of the NHI is its affirmation that legal norms and conventions matter rather more than realists and behavioralists traditionally have contended. But how do we express this position theoretically? Should we make our claims in conventional terms of "causality"? By contrast, should we reject that concept, redefine its meaning, or replace it with different ways of thinking about cognate terms like "influence," "effects," "power', etc.? How do we conceptualize the position that law is at once consequential and indeterminate, contingent, and dialectically interrelated with extralegal forces? Likewise, many NHI public law scholars claim to be "interpretivists." But what does this mean? What are we achieving when we try to "understand how" rather than "explain why"? Is this really what we are doing? Can we be more clear and consistent in framing what we are trying to do? Finally, as Roy Flemming's excellent note in this same forum posits, what are the promises and limits of "thick," narrative, case-oriented historical methodologies for the types of understandings that NHI scholarship tries to convey?

On the other hand, one might hope that we would find in the NHI movement motivations to develop specific new analytical concepts and to expand our repertoire of interpretive constructs. I propose here just one of many such projects. In short, those interested in law's "constitutive" power could take more seriously its linkages to strategic action and instrumental activity. Rather than dismissing those latter concepts as the rightful concern only of rival approaches, NHI emphasis on institutional "practice" provides an opportunity to redefine or reexamine "strategic" action in ways that make sense within the constitutive framework for analyzing legal institutions. In any case, the range of basic

analytical concepts highlighted by NHI approaches and deserving of similar development is boundless.

These, then, are the promises that I identify with the NHI. In this view, NHI will matter to the extent that it shakes things up, pushes us in new directions, redefines our research agendas, and connects our work to broader audiences. And there is some evidence that this is happening in the Law and Courts subfield. Some recent scholarly works by prominent NHI scholars – including Rogers Smith's brilliant Civil Ideals and various writings by Karen Orren — have dramatically expanded our views about the interrelationship among legal and other institutional arenas of power in broad historical perspective. Other scholars are linking NHI approaches to studies of legal mobilization politics and resistant action in various institutional arenas of social interaction and struggle. Yet other analysts are building on NHI perspectives in constructing cross-national and transnational socio-legal research projects. And still other scholars are engaging in terrific research that is looking at our subfield's traditional obsession with federal appellate courts, and their relation to other institutions, social groups, and the mass public, in fascinating and provocative new ways.[2]

This is all very exciting. To the extent that the NHI becomes a catalyst, guiding light, and rallying cry for these bold, innovative research developments, I am an enthusiastic advocacy. To the extent that NHI provides mostly a new justification for duplicating and defending the same old research agendas, I find rather less reason for excitement.

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1. I must acknowledge that the "other" wing of new institutionalist study – that relying on rational choice and game theory – holds considerable promise in much the same way.
2. The work of John Brigham most prominently comes to mind here.

Of Method and Mission

Susan E. Lawrence, *Rutgers University, New Brunswick*

We, as political scientists, spend an inordinate amount of time debating our methods, and, in so doing, often lose sight of our mission. We, once again, seek to evaluate competing methodologies in terms of which has a greater claim to the label "science," while neglecting the question of which methodology is more likely to tell us something important about "politics" or "the political."

This might not be such a problem, if we stuck to a reasonable definition of "science" — something akin to what can be found in the OED or Webster's.[1] But, what we in the discipline mean when we get involved in these debates about which method is more "scientific," is which has the most attributes we associate with the methods of the natural or physical sciences — rigor, replicability, falsifiability, objectivity. This debate is a distraction. We simply can not use the same standards as the physical sciences because we are studying "politics" — how real human beings live together — and our moral sense, the regulations of the NSF, the dynamic complexities of human interaction, and the uniqueness of each individual and his/her life experiences simply will not allow us to conduct controlled experiments on much of what is truly interesting about human politics. This is not to say that we should abandon disciplinary requirements that our work be systematic, explanatory, theoretical, cumulative, and self-consciously forthright about the limits and biases of our various methods. It is to argue that we would be better served by evaluating our methods in terms of our mission — to study, understand, explain, and theorize "the political" — than in terms of approximations of models developed for the study of very different phenomena.

What if we go back to "political" and ask, not which method is more "scientific," in some reified abstract sense borrowed from other disciplines, but which method is more likely to answer, in systematic, theoretically formative and informed ways, important questions about "politics" and "the political?" This is the invitation I issue to both the friends and foes of the New (Historical) Institutionalism. What important questions of "politics" does the New (Historical) Institutionalism allow us to systematically address that other competing methods do not? Does the New (Historical) Institutionalism allow us to develop explanations of aspects of "politics" and "the political" that other methods have abandoned? What does the New (Historical) Institutionalism add to our understanding of — knowledge of — "politics?" How does the New (Historical) Institutionalism advance our mission — to study, understand, explain, and theorize "the political?"

Take one illustrative example. One of the fundamental questions of "politics" is "how is it that somebody else gets to tell me what to do?" or, more eloquently, "what gives rulers legitimacy?" The American experiment is an attempt to legitimate government power by both resting sovereignty in the subjects and holding the government to the rule of law and standards of due process in the exercise of its power. To the extent that decision making by unelected judges is unaffected by the law, both self-government and the rule of law are compromised as judges displace the decisions of the people's representatives, and the announced rules by which government must exercise its power, with the judges' own

policy preferences. To do so is to return to the arbitrary rule of a select few and to abandon the very grounds upon which we rely to answer the legitimacy question of why we should obey.

Behavioralism has indeed confronted us with this problematic. The attitudinal study of judicial decision making has concluded that judicial decision making is determined by judicial policy preferences, not the law. But, I wonder whether that conclusion is not dictated by the methodology. It is not necessary to rehearse the many critiques of the attitudinal model of judicial decision making here. Let me simply remind readers that the attitudinal model, at its best, primarily addresses questions about why different judges decide differently. It does not attempt to tell us anything about the context within which those decisions are made or the constraints on what decisions are even possible. It does not attempt to tell us anything about how judges think about the conflicts that come before them and how their perceptions of their institutional role in the American system shapes and filters those policy preferences that the behavioralists find reflected in judicial voting. It does not attempt to tell us anything about the role of legal socialization, and the substance of the law, in restraining arbitrariness in judicial decision making. And, while strategic accounts filter judicial policy preferences through the procedural norms of the specific judicial institution (for example, the rule of four in the Supreme Court), they, too, leave these questions begging.

The New (Historical) Institutionalism, does, I think, have something to say about how the law, legal norms, and legal socialization, shape the way judges think about the policy problems that come before them, the range of options that can even be considered in a particular historical context, and the reasoned responses judges give to them in judicial opinions, i.e., the role of the rule of law. For example, behavioralism and strategic choice theory have important things to tell us as we seek to explain how it was that the Supreme Court came to a unanimous decision in Brown v. Board. of Education (1954), and how it came to adopt the "all deliberate speed" formula. But, those approaches do not explain why the Court could seem to buck public opinion and lead the way on desegregation, or why (or if) the Court could legitimately abandon Plessy v. Ferguson (1896). In terms of the Court's attempt to legitimate its decision by crafting a justification for it, why the did the Court not simply find "race" as a category to be void for vagueness; or, treat the whole case as one about education rather than race;[2] or, employ appeals to moral reason to legitimate its decision? Nor, do the behavioral or strategic choice approaches tell us much about how the specific language of the Brown opinion shaped future litigation and doctrinal development, making certain types of social change possible both through the courts and through the legislative processes, and making other types impossible — or at least highly improbable. In short, they cannot tell us much about whether, and how, the Court's decision violated (or interacted with) either of our legitimating norms: popular sovereignty or rule of law. These, of course, are different questions than those that the behavioralists and strategic choice theorists have set out to investigate, although I think that for our predecessors in the Legal Realist movement they were very much related. Nonetheless, they are important questions for our understanding and explanation of "politics" and "the political;" and, it seems to me that the New (Historical) Institutionalism allows us to again investigate such fundamental questions in a way that many of our other approaches miss.

I doubt that the New (Historical) Institutionalism and its methods hold the key to explaining all the important questions of "politics" and "the political," nor am I persuaded that any of our other methods hold the potential for full explication and explanation. A theoretically satisfying account of politics is unlikely to follow from dogmatic adherence to any one, single, methodology, no matter how much it comports with whatever it is we mean when we label our work "science." If we must continue to debate the relative merits of our various methodologies, lets at least do so in terms of our mission — to study, understand, explain, and theorize politics and "the political."

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1. From The Compact OED: "1.a. The state or fact of knowing... 4.a. In a more restricted sense, a branch of study which is concerned either with a connected body of demonstrated truths or with observed facts systematically classified and more or less colligated by being brought under general laws, and which include trustworthy methods for the discovery of new truth within its own domain..."
2. Compare San Antonio Independent School Dist. v. Rodriguez (1973). And, see the post-modern arguments that "race" is a social construct.

Speaking prose? Writing Stories?
Causal Narratives and the New Historical Institutionalist Persuasion

Roy B. Flemming, *Texas A&M University*

My first social science stories were about policy change in the criminal courts of Baltimore and Detroit (Flemming 1982). I chose the narrative approach because regression analyses of the decision processes couldn't answer important questions that emerged during my interviews with officials and judges, field observations, and archival research. To be specific, if I had not taken this narrative turn, I would have lost sight of the contexts and contingencies that influenced when and how change occurred in the courts. As Griffin (1992, 405) explains, narratives portray social phenomena as "temporarily ordered, sequential, unfolding, and open-ended 'stories' fraught with conjunctures and contingency." Still, to my eyes, the stories standing alone were not satisfying theoretical narratives. So I developed "process models" encapsulating what I thought were the logics behind the policy changes.

What I had stumbled on was an important difference in thinking about theory and causality. Lawrence B. Mohr (1982) distinguishes between "variance theory" and "process theory." Variance theory, as exemplified by a multiple regression model, deals with variables where an independent variable is a necessary and sufficient condition for the outcome (Y) and with efficient causes where the temporal ordering of the independent variables has no bearing on Y. In contrast, process theory deals with discrete states and events that are necessary but not necessarily sufficient conditions for an outcome to occur, and with final causes where temporal ordering is critical to the outcome. The premier example of temporal causality is the garbage can model of decision making (Cohen, March, and Olsen 1972).

Process theory typically takes a narrative form even when presented as a simulation whereas variance theory assumes algebraic and mathematical expressions. Narratives are analytical constructs tying together actions and happenings through an inherent sequential logic. Chronological order, however, does not necessarily suggest causal significance; otherwise, simply ordering "one damn thing after another" would constitute a causal explanation. Instead the narrative must be unpacked and reconstituted in order to construct a replicable, causal interpretation (Griffin 1993).

I mention these matters because I feel the "New Historical Institutionalist persuasion" (NHIp) in one way or another will confront process theory notions of causal narratives if it genuinely takes history seriously using case-oriented methodologies. I see this as a more fundamental divide (cf. Abbott 1990a) than the question of whether NHIp can live with rational choice or game theory, for instance (Aldrich 1994; Robertson 1994; Smith 1996). The issue of causal narratives only arises, however, when history is used in particular ways, but as it happens this is also when NHIp's promise for new understandings faces its greatest challenges.

I would like to raise some cautions about the use of history, which I feel should be taken into account in the practice of the New Historical Institutionalism. Social scientists use history in many different ways. For the purpose at hand, I will follow Aldrich (1994) who identified three basic usages of history. The first and most common is history as a source of data. History adds more observations for variable-oriented methodologies. Spaeth's Judicial Data Base, for example, created new research opportunities for law and courts scholars. As other data bases for the federal appellate courts and state supreme courts become available, this use of history will accelerate. When time matters in the analysis it takes such forms as time-series, Markov chains, or event histories (e.g. Smith 1984; Box-Steffensmeier and Jones 1997). Statistical tools for these variable-oriented analyses are well-developed. These analyses, however, are not intrinsically narratives, case-oriented, or process theories. Time is the essence of these analyses, not history. At best they may provide "thin narratives."

Social scientists also use history for comparisons (e.g., Skocpol and Somers 1980). Time becomes the context of the phenomenon being studied. Temporal contexts generally function as indicators of institutional or structural arrangements. These contexts are then used to identify regularities in causal processes that can be generalized across the cases being compared. A recent example of this strategy is Ackerman's (1998) analyses of three "constitutional moments" in American history. Sewell (1996) calls this usage "experimental temporality" because the periods or contexts being compared are analogous to trials in experimental research, and therein lie the strategy's difficulties.

Historical comparisons are rarely independent or equivalent trials, which compounds the related drawback of too few cases and too many variables that typically troubles comparative history. A further aggravation is that historical comparisons hinge on periodization which is not a matter of measuring occurrences in time but of defining the phenomenon being studied over time (Abbott 1984). As Griffin (1992, 407) notes, "The problems of periodization and the detection of 'transitory causal regularities' are closely related." If the periods making up the "sample" of time are ill-defined, any perceived regularities will be questionable. While the narratives can be compelling, even sophisticated users of history for comparative purposes run into fundamental logical and empirical problems (Buroway 1989).

The third use of history, "history as process," or what Sewell (1996) refers to as "eventful temporality" recognizes the role of events in history. It assumes these events are "path dependent" rather than independent of each other so that earlier events or decisions affect the outcomes of a sequence of events or decisions. It also assumes causality is temporally heterogeneous and not uniform over samples of time as in comparative history approaches. Abell (1987) and Abbott (1990b; 1992; 1995) have written extensively on the theoretical questions raised by narratives. While both of these sociologists reject the variable-oriented model, they define "events" differently and they approach from different angles the complex issues raised by "multiple plot structures" in which events lie in many narratives at once and processes involving multiple contingent structures of events.

The nub of the problem, of course, is determining whether and how causality exists in a sequence of events using case-oriented, "thick" narrative methodologies. In operational terms, a particular sequence of events in a narrative can be modeled using an "event structure" that typically takes the form of a "tree" of empirically linked events. Heise (1988; 1989) has developed a computer-assisted program that employs qualitative data to generate these graphs or trees of events. These graphs facilitate the study of the event structure. According to Griffin (1993) event structures reveal the most significant events in sequences, indicate the influence of human agency, contingency, and path dependence, and show how causality is embedded in the temporality of the events.

Narratives are more than stories, and narratives are only the first step toward uncovering temporal causality. Taking history seriously raises fundamental questions about conceptions of time, history, and causality in the New Historical Institutionalism. In this short essay I emphasized how these conceptions are tied to how history is used. This emphasis clearly ignores for the moment the other half of NHIp, namely its institutional focus. It is well to conclude then by recalling Orren and Skowronek's (1994) contention that asymmetrical, desynchronized interactions among institutions create political time and make history. This view of time as a function of plural institutional orders adds further complexities to the New Historical Institutionalist persuasion's evolving theoretical grammar.

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State Supreme Court Data Project

Paul Brace, *Rice University*
Melinda Gann Hall, *Michigan State University*

We are very pleased to report that the State Supreme Court Data Project, currently being conducted jointly at Rice University and Michigan State University, with support from the National Science Foundation, is off to a terrific start! As we reported last year, we are in the process of assembling data on the courts of last resort in all fifty states for several recent years. By May 2000, when our current project ends, we will have gathered at least two complete years of data (1995 and 1996) for approximately 14,000 decisions reached by over 400 justices.

In the following paragraphs, we describe in detail our progress on the project. For additional information and to preview the coding template and other reports described below, we also invite you to peruse the Data Project web page (http://www.ruf.rice.edu/~pbrace/statecourt/). But first we reiterate the fundamental purpose of the project.

Scientific Value of the State Supreme Court Data Project

The State Supreme Court Data Project rests on the assumption that the lack of a single reliable and systematic data base has impeded the development of comparative state judicial scholarship. Students of state courts have been forced to construct unique data sets to explore their scholarly interests. Evaluation of even seemingly simple and straightforward hypotheses about single courts requires extensive hours of case collection and reading. This tedious and time-consuming activity may have some rewards but it also detracts from the time available for thinking, analysis, and writing. Even more troubling, this approach will not provide the kind of readily accessible standardized data that have elevated the study of voting in Congress and the United States Supreme Court. Without a systematic, fifty-state data base, many findings will remain anecdotal and descriptive, and more general theories will continue to be elusive. This is particularly unsettling because, as we have argued, American state courts stand as diverse comparative laboratories that offer analytical leverage for addressing many important contextual and institutional hypotheses. Systematic analysis of multiple state courts not only can enhance our understanding of state adjudication but will promote development of theories capable of unifying the contextual and individual forces affecting judicial decisions.

Current Status of the State Supreme Court Data Project

As we noted in our original proposal, the pivotal concern with attempting to construct a fifty-state data base was whether it could be collected in a practical amount of time, with the expenditure of a reasonable amount of resources. We have now demonstrated that it can. Specifically, in addition to obvious needs like setting up offices and training the graduate students participating in the project, we have accomplished the following.

A. We have developed a coding scheme using Microsoft ACCESS that provides comprehensive information entered directly into laptop computers with a remarkable degree of reliability. These tasks, because of their critical importance and complexity, occupied much of the first year of the project. In our original proposal, we presented a draft of a coding scheme, built from a number of sources. First, we made a concerted effort to duplicate the coding schemes for the United States Supreme Court and Courts of Appeals data projects. However, given the extraordinary differences in issues litigated in federal and state courts, these schemes had to be modified substantially for state supreme courts. To begin this process, we collected data on all cases decided in 1993 by a sample of seven state supreme courts (California, Illinois, Kentucky, Louisiana, New Jersey, Ohio, and Texas) and assembled various statistical reports by the National Center for State Courts. Our first attempt to reconcile these various sources, including our own experience in coding state court data, was reported as the draft just mentioned. However, we clearly recognized that this scheme was necessarily tentative, given the absence of any fifty-state studies on which we could rely and the extraordinary complexity and variety of state litigation.

To refine and "bench test" the coding scheme systematically, we began in Summer 1997 to draw samples of cases from all states and then code. Simultaneously, we created a template using Microsoft ACCESS software to systematize the coding process by providing discrete choices on most of the variables contained the template and by creating protocols that exclude later choices based on previous selections. In other words, we completely automated the case coding process, eliminating the need for messy paper files and subsequent data entry while simultaneously guaranteeing a significant level of validity and reliability.

After numerous rounds of revision using sampling from the states, formal reliability tests were conducted at Rice University in Fall 1997. Overall, the results were impressive. Of 323 items of information contained in the template, only 30 (9.3%) produced inter-coder reliability rates less than 90%, while 293 items resulted in reliability scores of 90% or greater. In fact, 255 questions, or 78.9%, produced perfect reliability.

Though clearly acceptable by current scientific standards, we wanted to do better. Therefore, we asked numerous scholars for their comments on the template and also

continued the sampling procedure. Our fully revised, final template was formally tested at Michigan State University in Fall 1998. As we had hoped, we had improved our results substantially. Of 423 items in the final template, only 5 (1.2%) produced less than 90% reliability. A close examination of the suspect items revealed that incongruities could be resolved by coder education rather than template revision. Also, to address any future issues along these lines, we established the practice of consulting before coding any ambiguous information and of keeping a "lab book" of coding rules to govern these potentially confusing situations.

As mentioned, all of this information (i.e., the template, reliability tests, coding rules) is available on our State Supreme Court Data Project web page (http://www.ruf.rice.edu/~pbrace/statecourt/). In sum, we are certain that we are providing valid, reliable data that is highly comprehensive (as mentioned, we include 423 items on each court decision). Of course, we will continue to monitor reliability regularly during the project, as we experience personnel changes and as we code new states that may use different reporting techniques or engage in litigation not common across states. Regular and systematic inter-coder reliability testing will quickly alert us to potential problems that require any corrective measures.

B. We have identified the universe of cases decided in each state court of last resort in 1995 and 1996. One of the most formidable features of a fifty-state project is the number of decisions that must be read and coded. Based on data we collected for 1994, we estimated that each state court of last resort issues an average of 228 decisions per year, while some issue as many as 1500 (note: there are fifty-two state courts of last resort; Texas and Oklahoma each have two). As a practical solution to balancing time and cost with information, our Board of Overseers suggested that we code all cases in states issuing 200 formal decisions (with signed or per curiam opinion) or less per year, and draw a random sample of 200 cases from states deciding more than 200. To accomplish this task, it was essential to identify the actual number of decisions issued with opinion per year for each state supreme court. We utilized the following procedure, which certainly is cumbersome but is the only practical approach given the lack of alternative sources.

First, we identified all volumes of the West Regional Reporter series containing cases decided by state supreme courts in 1995 and 1996. Next, we xeroxed all pages in each volume's Table of Contents and then eliminated cases that did not meet the following criteria: decided by a state court of last resort, decided in 1995 or 1996, decided with signed opinion or per curiam opinion exceeding five paragraphs in length. We then tallied the number of cases for each court. Of 52 state courts of last resort, the total number of cases decided in 1995 and 1996 with opinion is 13,938. We are required to sample in eighteen of the states.

C. We have identified all natural courts in state supreme courts in 1995 and 1996. Unlike the federal courts, numerous state supreme courts report cases to the various reporters without specifically identifying the justices participating in each decision. While the writer of the opinion of the court is identified, along with any justices who concur or dissent, justices who join the majority but do not write often are not listed. Therefore, it becomes critical to establish exactly who is on each court on any given date in order to know who actually voted in the case. Membership charts at the beginning of each volume of the West reporters are useful but not always accurate. Charts are changed well in advance, or well after, the cases are reported in the volume.

To ascertain each natural court for all state supreme courts, we searched all published sources, including The American Bench, Martindale-Hubbell, and state "blue books." Additionally, we searched each state's web page. As a final measure, we made numerous telephone calls to Clerks of Court. As a consequence, we now know exactly who was sitting and their precise dates of service for all fifty-two state supreme courts. Overall, there were 196 natural courts in the fifty-two courts of last resort in 1995 and 1996.

These data, while intrinsically interesting themselves and are reported on our web page, will be used to enter information about each justice's vote for each case into the template. These data can be used to explore many intriguing and theoretically important hypotheses, including those concerning membership change in state high courts.

D. We have completed the assembly of biographical data from published sources for all justices sitting in 1995 and 1996. After identifying the 429 justices sitting on state high courts in 1995 and 1996 and their exact dates of service, we began to assemble biographical data about each of the justices from published sources, including The American Bench, Martindale-Hubbell, and state "blue books." Additionally, we searched each state's web page. When combined with the data contained in the template about each case, these data will allow analyses of the relationships between justices' attributes (e.g., age, tenure on the court, partisan affiliation, religious affiliation, education, gender, race, ethnicity, and past political and judicial positions) and their decisional behavior. Standing alone, these data document the characteristics of justices serving in state courts of last resort, including their career paths to the bench.

We now have assembled data from all readily accessible sources. Of course, the data are incomplete, since many justices do not report such important characteristics as partisan affiliation or religion. To make the biographical data

as comprehensive as possible, we will be sending a questionnaire to each justice still serving, and to each state court administrator and clerk of court in Summer 1999. By using multiple sources, these biographical data will be a comprehensive single source on state supreme court justices. We will include these data in the data base itself and also will present them as an independent document on our web page.

E. We are now fully engaged in the process of coding cases. Based on our past experiences, we estimated that coding each case would require about fifteen minutes on average. We were right on the money.

After developing the template and all of the additional projects listed above, we have now completed the coding for 24 of the states, for a total of over 3,500 cases for 1996. We estimate that we will deliver, minimally, two full years of data (1995 and 1996), with the possibility of more if the coders gain efficiency over time. Needless to say, this effort will dominate our schedule for the remainder of the project.

F. We are preparing to add basic institutional features and selected contextual variables to the data base. In Summer 1999, we will assemble all published information on the basic structural characteristics and operating rules governing state supreme courts. We will begin by consulting the many excellent published sources, including those produced by the National Center for State Courts and the Council of State Governments. These sources provide detailed information on such features as selection procedures, terms of office, mandatory retirement provisions (if any), and selection procedures for the chief justice. Other information, such as rules governing opinion assignment or other operating procedures, may be more difficult to obtain. To gather this information, we will prepare and mail questionnaires to state court administrators and also make phone calls to clerks of court.

For contextual variables, we will include well-known measures of inter-party competition (Holbrook and Van Dunk, 1993), elite and citizen ideology (Berry et al., 1998; Erikson, Wright and McIver, 1993), and other state-level contextual variables. In addition, we will supply longitudinal measures of income and economic performance (Survey of Current Business), population and population change (Statistical Abstract), and the partisan composition of legislatures and governors (Book of the States). While including state-level contextual measures in the data base is a very simple matter, this linkage substantially expands the size of the user community to scholars whose interest in the judiciary is a peripheral one. Moreover, any contextual measures not included in the data base are added quite easily by individual users.

What Next?

As stated earlier, the bulk of our effort in the upcoming months will involve case coding. Additionally, we will be constructing data sets on the biographical characteristics of the justices, institutional features of state supreme courts, and state-level contextual variables. Additionally, we have plans to develop an electronic dissemination program that will allow users to obtain the precise data they need instantaneously over the internet (more on that in our next report!). By May 2000, the State Supreme Court Data Base will be fully operational and easily accessible to the user community over the internet and through the ICPSR.

With the release of the data, we hope to stimulate interest in the politics of state courts and to promote efforts to move our theories forward by providing a laboratory through which the extraordinarily complex relationships that affect the politics of the judiciary can be unraveled. We expect the data base to be attractive not only to scholars just entering the field but also to more established scholars who wish to expand their scholarly interests. We are convinced that studies of the states, while intrinsically important in their own right, offer outstanding opportunities for transcending current theoretical bounds, and we hope to see many other scholars engaging in these pursuits.

Acknowledgments

In accomplishing the above tasks, we have benefited tremendously from the encouragement and advice of others. We thank our Board of Overseers, composed of Robert Carp of the University of Houston, Eugene Flango of the National Center for State Courts, Lawrence Friedman of Stanford Law School, Harold Spaeth of Michigan State University and Donald Songer of the University of South Carolina. We also thank our research assistants, who have worked so hard on the project: Sara Benesh, Chris Bonneau, and Wendy Martinek at Michigan State University; and Kevin Arcenaux, Kellie N. Sims-Butler, Martin Johnson at Rice University. Also at Rice University, Richard Engstrom provided excellent assistance with the web site, supported by funds from the Baker Institute for Public Policy. Alan Thornhill and Krist Bender of DACNET have offered expert advice on the development of search and retrieval software. Finally, we are especially grateful to Laura Langer (Washington State University), whose contributions to the project, including the development of the template and the reliability tests, have been invaluable.

Books to Watch For

Sue Davis, *University of Delaware*

Send information about your forthcoming work to Sue Davis at: suedavis@udel.edu

Keith E. Whittington's (Princeton University) Constitutional Construction: Divided Powers and Constitutional Meaning (Harvard University Press) will be available in June. **Whittington** argues that the Constitution has a dual nature. The first aspect, on which legal scholars have focsed, is the degree to which the Constitution acts as a binding set of rules that can be neutrally interpreted and externally enforced by the courts against government actors. But the Constitution also permeates politics itself, to guide and constrain political actors in the very process of making public policy. In doing so, it is dependent on political actors, both to formulate authoritative constitutional requirements and to enforce those fundamental settlements in the future. **Whittington** characterizes this process, by which constitutional meaning is shaped within politics at the same time politics is shaped by the Constitution, as one of construction. He argues that the construction of constitutional meaning is a necessary part of the political process and a regular part of our nation's history, how a democracy lives with a written constitution. He develops this argument through intensive analysis of four important cases: the impeachment of Justice Samuel Chase and President Andrew Johnson, the nullification crisis, and reforms of presidential-congressional relations during the Nixon presidency.

Donald Grier Stephenson Jr.'s (Franklin and Marshall College) Campaigns and the Court: The U.S. Supreme Court in Presidential Elections (Columbia University Press) will be published in May. **Stephenson** analyzes of the relationship between electoral politics and the Supreme Court in American history, demonstrating how the Court has played a major role in presidential campaigns. Voters' perception that the dominance of one party at the polls may translate into that party's dominance on the nation's highest court has often made the Court a significant issue in presidential elections. Drawing from four areas of political history--party evolution, presidential campaigns, and judicial and constitutional development--**Stephenson** examines periods when the Court has been an issue in elections as well as when it has not, and explains how the Court's shifting position within electoral campaigns is a crucial aspect of the American political process.

Race and Representation Affirmative Action edited by **Robert Post** (Boalt Hall School of Law, University of California, Berkeley) and **Michael Rogin** (Political Science, University of California, Berkeley) addresses the following questions. Why has affirmative action has become the lightning rod for conflicts over racial inequality in the United States? Have color-blind legal and political doctrines intensified or ameliorated America's racial divisions? Focusing on the politically driven decision of California's governor and the Board of Regents of the University of California to end affirmative action at the university, the subsequent enactment of an amendment to the California Constitution prohibiting the state from engaging in affirmative action, and judicial decisions in Texas that used the federal Constitution to prohibit affirmative action at state universities, the contributors to this volume assess the current state of the tumultuous controversy over affirmative action. For more information please visit http://mitpress.mit.edu/promotions/books/POSAPF98

In First Princples: The Jurisprudence of Clarence Thomas (New York: New York University Press, 1999) **Scott Douglas Gerber** examines Clarence Thomas' judicial opinions and votes, his scholarly writings, and his public speeches. The publisher's description tells us that "what Gerber finds is likely to surprise Justice Thomas's critics and supporters alike." On January 30 C-Span devoted a segment of "America and the Courts" to Gerber's book. "America and the Courts" airs on Saturdays at 7pm ET on C-SPAN.Visit the website at www.c-span.org/guide/courts/amcrt

Kanishka Jayasuriya (Asia Research Centre Murdoch University Western Australia) is the editor of LAW, POWER AND Capitalism in Asia: The Rule of Law and Legal Institutions (Routledge 1999). The result of an interdisciplinary collaboration between political scientists and legal scholars, this volume includes a range of material on law and economic development, governance and rule of law issues, constitutional and administrative law, courts and politics, judicial organisation and judicial independence. It addresses judicial politics and independence in the AsiaPacific and has a very specific focus on the impact of state institutions and ideologies on conceptions of the rule of law and judicial independence.

In When the Nazis Came to Skokie: Freedom for Speech We Hate (University Press of Kansas, April), **Philippa Strum** (City University of New York-Brooklyn College) offers a dramatic retelling of the controversial case that began when a neo-Nazi group announced its intention to parade before the Jewish residents of a Chicago suburb in 1977. The American Civil Liberties Union took the case and successfully defended the Nazis' right to free speech.

The Pullman Case: The Clash of Labor and Capital in Industrial America (University Press of Kansas, May) by **David Ray Papke** (Indiana University School of Law-Indianapolis). Set against the passions and inequities of industrial America, this is the story of the 1894 American Railway Union strike against the Pullman Palace Car Company and the subsequent Supreme Court decision, In re Debs, that defined the terms of federal labor law for decades to come.

Leaving the Bench: Supreme Court Justices at the End (University Press of Kansas, June) by **David N. Atkinson** (University of Missouri-Kansas City. This first comprehensive historical treatment of Supreme Court justices' deaths, resignations, and retirements explains when and why justices step down. Combining legal and medical history and making liberal use of anecdotes, the author tracks the impact of illness, disability, and aging on the Court.

Upcoming Conferences

1999		
Law & Society	May 27-30	Chicago Illinois
APSA	September 2-5	Atlanta Georgia
SPSA	November 3-7	Savannah Georgia
NEPSA	November 11-13	Philadelphia Pennsylvania
2000		
SWPSA	March 15-18	Galveson Texas
WPSA	March 24-26	San Jose California
MWPSA	April 27-30	Chicago Illinois

Conferrences, Events and Calls for Papers

The Law & Society Association Workshop and Annual Meeting

The Law and Society Association is sponsoring a "Didactic Workshop" on social science research methods in conjunction with its 1999 annual conference to be held in Chicago. The workshop is designed for law faculty who are interested in expanding their scholarly capabilities by learning how to gather and analyze data.

The workshop will be led by two experienced and well-regarded scholars: Prof. Joseph Sanders, Visiting Professor of Law at the University of Texas at Austin, and Prof. Carroll Seron, Baruch College in New York.

The workshop will take place on May 26, 1999 at the conference hotel in Chicago from 9:30-4:00. It will convene with an overview session on social science methods for law teachers. The group of participants will then be divided into two working sessions. Lunch will be provided. At the conclusion of the workshop, participants will reconvene together for a plenary session.

To register, send a letter describing your general research interests, a one-page description of a potential empirical research project, and a $60 registration fee to: Lissa Ganter, Law and Society Association, Hampshire House, Box 33615, University of Massachusetts, Amherst, MA 01003-3615(phone 413-545-4617; fax 413-545-1640). Participants' research descriptions will be reviewed by the workshop leaders and utilized as part of the pedagogy.

The deadline for registration is April 15, 1999. Early registration is recommended as enrollment will be limited to 15 participants. Participants in the Didactics Workshop will also be required to register for the Law & Society Association's annual meeting which will be held May 27-30, 1999 at the Renaissance Hotel in Chicago. Details about the annual meeting can be found on the Association's website: http://128.119.199.27/lsa/ann_mtg/am99/meeting.htm

Spectres of Law: Legal Theory at the fin de siecle

"Spectres of Law: Legal Theory at the fin de siecle," Critical Legal Conference 1999. Dates: September 17-19, 1999. Location: Birkbeck College, University of London. Call for Papers: Law is haunted by many spectres; excluded memories which retain a ghostly influence on the present. How can these memories be revive? Premonitions of what may be to come also disturb us: the possible futures of law yet to be determined. How can legal thought face its destiny? Thinking these issues through opens another problematic. Although critical legal studies continues to reinvent itself, we need to ask difficult questions. Is the noisy critical spirit an intellectual poltergeist frustrated by its inability to achieve form? The eve of the new millenium is now the moment to assess the past and to project ahead.

The Bloomsbury District of London, with its occult energies and dark shadows, is the locus for this celebration of restless and creative legal thought. Confirmed plenary speakers include: Costas Douzinas (Birkbeck College), Peter Fitzpatrick (Queen Mary and Westfield College), Pierre Schlag (University of Colorado), and Mariana Valverde (University of Toronto). Papers are sought in the following sections: Critical Moments in Law's Histories; Critical Race Theory; Law and Memory; Law and Literature; Radical Legal Practice — Shattered Dreams, Primordial Apparitions; Crossing the Borders — The Evolution of Critical Legal Studies by Race, Class, Gender, Theology and Geography; Critical Legal Studies 2000 — Retro and Prospect; Utopias — Law, Space and Time; Law and the Sacred; Gender and/in Justice; and CLS Beyond Critique? Possible Goals, Methods, Strategies and Limits of Critique in Legal Scholarship. Paper proposals are due by June 30, 1999. For more information please contact our website at www.bbk.ac.uk/Departments/Law/clc99.htm

Samuel Krislov, Recipient of the 1998 Lifetime Achievement Award

This award was created in 1995 for the purpose of honoring a distinguished career of achievement and service in the field of law and courts, broadly defined. It is given annually to a scholar with at least twenty-five years of service or who has reached the age of 65.

This year's committee unanimously selected Professor Samuel Krislov to be the 4th recipient of the "Lifetime Achievement Award." Professor Krislov received his M.A. from NYU in 1952 and completed his Ph.D. at Princeton, under Alphas Mason, in 1955. Since 1964, Professor Krislov has worked at the Department of Political Science, University of Minnesota. Prior to Minnesota, he was an Instructor at the University of Vermont and at Hunter College. In 1956, he began as an Assistant Professor at the University of Oklahoma, and he then went on to become an Associate Professor at Michigan State University.

The breadth of Professor Krislov's scholarship is truly remarkable His substantive areas of research constitute the field as we know it today: constitutional, administrative and criminal law; judicial behavior, judicial process, law and society; and global and comparative perspectives on law and legal institutions.

As a scholar, Professor Krislov established a distinguish career not, as a former student put it, "because he has a single-minded commitment to one paradigm," but because he is "eclectic and creative." Indeed, a review of the methodological and theoretical analyses Professor Krislov applies, makes a strong case for diversity in scientific inquiry.

During the decade of the 1960s, Professor Krislov was among the first to apply rational choice analysis to judicial behavior. His articles on game theoretical analysis of coalition formation on the Supreme Court, the effects of amicus curiae briefs and group theory appeared in Political Science, Public Administration, and Sociology journals, as well as distinguished law reviews. It was also in this decade that Professor Krislov's now classic books on the U.S. Supreme Court were published. The Supreme Court in the Political Process (1965) and The Supreme Court and Political Freedom (1968) anchored the post-war study of Law in Political Science. He added weight to earlier arguments, that doctrine and decision making were influenced by partisanship, and his work on legislative and judicial policy gave intellectual direction to subsequent scholars who today connect doctrine, behavior and process to institutions. Portions of these two books have been re-printed widely in American Politics texts.

His work on judicial impact opened new empirical challenges for the study of legal and social policy making, which again led future generations of scholars to rethink their tools of analysis. We now have expanded concepts for studying the political effects of rights movements on legal policy. Professor Krislov's policy work, spanning race, religion, welfare and crime, broadened the field, yet he has always maintained a close working relationship with most parts of the field.

Though his editorial work, Professor Krislov created new series on the Supreme Court at Free Press and Little Brown. As editor of the Law and Society Review from 1968-1973, he pioneered the creation of an internationally distinguished journal in our field. Professor Krislov has also been an editorial pioneer for the American Political Science Association, where he served as the founding editor of Teaching Political Science from 1972-1978.

These achievements have been recognized, nationally and internationally, in the many grants, honors and fellowships awarded to Professor Krislov over the course of his distinguished career. He received grants from: the National Science Foundation; the National Institute on Mental Health; the Social Science Research Council; the Russell Sage Foundation; to name a few. He has also been a Guggenheim Fellow, a Ford Foundation Fellow, a Fulbright Fellow, a Fellow at the Interuniversity Institute for International Law at the Hague, a Fellow at the European Studies Institute in London and Durham, and a Bush Foundation Fellow.

In addition to Professor Krislov's scholarship, his career is uniquely distinguished by an outstanding record of institution

building. He has produced a phenomenal record of service to the discipline and field. Along with mentoring a stellar group of students, Professor Krislov's impressive record of service has built institutional and professional places where we do our work.

He has been the chair and member of the National Research Council; consultant to the Office of Improvement of Justice, Department of Justice, the Carnegie Foundation Panel on Scientific Evidence in the Courts and the U.S. Office of Education. He has served on the American Political Science Association Council, the Program Committee, and several other APSA committees. In addition, Professor Krislov has served on American Association of University Professors committees on Academic Freedom and Rules. He is a founding member of the Law & Society Association and member of their Board of Trustees as well as President of the association. Professor Krislov was also the President of the Midwest Political Science Association Member, and a member of the Minnesota State Board of Judicial Standards. And, of course, he has been our president— President of the Law and Courts Section of the APSA.

In short, Professor Krislov has had the kind of distinguished professional career that the Lifetime Achievement Award was designed to recognize. His has produced a record of scholarship and service to the profession to which our section can point with pride. We are delighted to be able to pay tribute to that career and to him here today.

1998 APSA Section on Law and Courts "Lifetime Achievement Award" Committee Members:

Professor Christine B. Harrington
Department of Politics and Institute for Law and Society
New York University

Professor Barbara Hayler
Criminal Justice Department
University of Illinois at Springfield

Professor Kevin McGuire
Department of Political Science
University of North Carolina

SPAETH'S DATABASES NOW AVAILABLE ON THE WEB

Harold Spaeth's NSF-supported U.S. Supreme Court Judicial Databases are now accessible on a website of the newly established Program for Law and Judicial Politics at Michigan State University: www.ssc.msu.edu/~pls/pljp. Both the original database extending from the start of the Warren Court in 1953 through the end of the Court's 1997 term and the expanded database which includes the conference votes of the Vinson and Warren Courts are downloadable. The documentation for each of these databases may also be downloaded as a WordPerfect file.

For those associated with member institutions of higher education, copies of the databases and their documentation will remain available from the Inter-University Consortium for Political and Social Research at Ann Arbor as ICPSR 9422 and ICPSR 6557, respectively.

The databases at the Michigan State website are downloadable only as SPSS files at the time of this writing. They should also be available in SAS and ASCII by the time you read this.

Subscriptions to **Law and Courts** are free to members of the APSA's Law and Courts Section. Please contact the APSA to join the Section.

The deadline for submissions for the next issue of **Law and Courts** is July 1, 1999.

Law and Courts

Cornell Clayton, Editor
Department of Political Science
PO Box 644880
Washington State University
Pullman WA 99164-4880

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