On Constructing a Science of Comparative Judicial Politics: Tate & Haynie's "Authoritarianism and the Functions of Courts"

Howard Gillman

Tate and Haynie's (1993) recent essay in these pages on the functions of courts in authoritarian regimes is evidence of an increasing interest among American political scientists in the comparative study of judicial politics. There have always been a few among us who have argued for the benefits of broadening our perspectives beyond the American judiciary. As the story goes, the effort began 30 years ago with just a handful of pioneers meeting in a Dallas hotel room to discuss ways to make comparative judicial studies a more conspicuous feature of public law scholarship among political scientists (see Abraham 1987). Over the years a fairly diverse and distinguished group of social scientists have contributed to this effort (see Tate 1987). By 1981 there was sufficient interest among scholars affiliated with the International Political Science Association to convene the Mansfield College Conference on Comparative Judicial Studies. That same year Martin Shapiro gave a boost to these effort with the publication of his influential and widely read *Courts: A Comparative and Political Analysis*. Still, it was not until 1987 that the first collection of essays was published on "comparative judicial systems," and even then the papers in that volume were characterized in the subtitle as representing "challenging frontiers in conceptual and empirical analysis" (Schmidhauser 1987). Over the course of 30 years a few more pioneers were recruited, but apparently the terrain was still a frontier.

It is heartening to see that this terrain is increasingly becoming well-developed territory, a welcome development even for those of us who choose to spend most of our time thinking about American law and courts. Among other things, a comparative perspective helps us think about courts in ways other than with reference to the functions assigned to courts in liberal theory, such as offering protection for individual rights or a defense against the exercise of lawless authority. Courts can be seen more clearly as an extension of a regime's power and not just a shield against it. Moreover, a comparative perspective may help us see

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that there are more important things one can say about judicial politics than that a judge's decision may be influenced by her or his ideology. For these reasons and others, Tate and Haynie are correct to point out that the "place and function of courts in authoritarian regimes is too little discussed" (p. 710).[1]

However, what concerns me is that most of the advantages to be derived from an increased interest in comparative judicial politics will be lost if scholars take seriously Tate and Haynie's suggestion that the time has come to move beyond narrative accounts of particular courts and begin the process of developing an "empirically based theory" of the institutional performance of courts in general during transitions to authoritarianism. The authors make no formal effort to identify the characteristics of this sort of theory building, but it is safe to assume that they believe that future research should (*a*) be based on "reliable data—that is, on reproducible quantitative measures—that (*b*) allow for the verification or falsification of general hypotheses or law-like statements like "courts that encounter a transition to authoritarianism will experience a reduction in their involvement in social control." Apparently, the belief is that once we verify a sufficient number of hypotheses, we will be in a position to construct a "full-fledged" scientific theory (p. 707) of what sorts of things happen to courts when faced with the imposition of authoritarian rule.

Many scholars who have engaged in comparative judicial research have expressed similar yearnings for the construction of general models of judicial behavior—that is, models that go beyond descriptions of the behaviors of particular courts to the identification of relationships that ostensibly apply universally to all courts (Schmidhauser 1987:240; see in general Gibson 1986). In fact, it is often claimed that the distinctive virtue of a comparative approach is that it helps us examine the properties of courts in general rather than the behavior of particular courts (Tate & Handberg 1991:461–62, following Przeworski & Teune 1970).[2] Ironically, though, at the same time that the enthusiasm for a science of comparative judicial politics has been building, there has also been a resurgence among other sorts of political scientists in historical, interpretive, and ethnographic studies of law

[1] In this essay page numbers unaccompanied by a more complete reference refer to Tate & Haynie 1993.

[2] Gibson (1986:151-53) addresses the issue of the "generalizability of theory" when he reviews Przeworski and Teune's (1970:7) distinction between "idiographic approaches," which holds that "the interactions of various characteristics within each [institution] creates unique, or at least varying, patterns of determination relative to each [institution]," and "nomothetic theory," which assumes that "if all relevant factors were known then the same multivariate statement would yield a deterministic explanation regardless of time and space." For examples of objections to this program see MacIntyre 1978 and Almond & Greco 1990.

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and courts.[3] These scholars tend to be more interested in constructing narratives than in moving beyond narratives to the identification and verification of a set of discrete hypotheses about institutional or individual behavior.[4] In contrast to those who consider these sorts of narratives a second-rate or unreliable form of inquiry, many who are associated with this "interpretive turn" in the social sciences (Hiley et al. 1991) believe that the search for general hypotheses is premised on the mistaken assumption—inappropriately borrowed from the natural sciences—that institutions and individuals are naturally disposed to behave in certain ways under certain conditions (see Almond & Greco 1990), an assumption that, for example, inspires some social scientists to ask questions about how courts in general might respond to events such as the imposition of authoritarian rule.[5] As a basis for rejecting a model of inquiry that conveys what they consider to be an implausibly mechanistic or naturalistic portrait of politics, those who engage in more historical or ethnographic work assume that political behavior is a product of contingent decisions made by purposeful actors who are not only embedded in particular historical and cultural contexts but who are also capable of deliberating about, and even transforming, their motivations and situations (see Smith 1992). While most behavioralists work hard to exclude from their analysis any account of the actual lived experience of those whose behavior is being investigated (on the grounds that the data represented by such accounts is "subjective" and "unreliable"), interpretivists argue that,

[3] The first conspicuous group of political scientists who could be characterized as postpositivist empiricists was affiliated with the Amherst Seminar on Legal Ideology and Legal Process and included John Brigham, Christine Harrington, Lynn Mather, Austin Sarat, and Adelaide Villmoare (see the bibliography provided by Trubek & Esser 1989; apologies in advance for any omissions). Other political scientists have affiliated themselves more informally with the so-called "new institutionalism" in that they (we) are interested in examining relatively autonomous and historically contingent institutional and ideological structures (see Smith 1988, 1992; Robertson 1993; for a criticism of this method of inquiry from a defender of positivist social science, see Gates 1991). Much of the empirical institutional work conducted by these scholars has found an outlet in journals such as *Studies in American Political Development, Law and Social Inquiry,* and *Journal of Policy History.* In the American Political Science Association, the growing presence of these scholars is reflected in the increasing size and prestige of the Politics and History section and in a greater number of political scientists joining the Social Science History Association. For an admirable discussion by a political scientist of "postempiricist" public law scholarship that emphasizes (among other things) the inevitability of interpretation in observation, and thus overlaps with some of the arguments I will be making herein, see Sarat 1990.

[4] Of course, postempiricists would deny that social scientists who make use of hypothesis testing and quantitative analysis are "moving beyond narratives." Instead, they are viewed as constructing a different sort of narrative—one that conveys an image of politics that is largely deterministic or naturalistic, where behavior is produced by exogenous forces rather than by purposeful choices.

[5] Almond & Greco (1990:36) write: "Social scientists who—for whatever philosophical or methodological reasons—deny [that political affairs have ontological properties that are different from natural events] and view human behavior as simply reactive and consequently susceptible to the same explanatory logic as 'clocklike' natural phenomena are trying to fashion a science based on empirically falsified presuppositions."

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since behavior is produced by particular goals and purposes (rather than by general dispositions) that are meaningful only in relation to a specified context, we can only begin to understand or explain behavior if we take into account the actual intention states of participants and the circumstances within which they operate—after all, similar behaviors (e.g., raising a hand at a meeting) might be motivated by any number of purposes and might have any number of different political implications, depending on the context, just as different behaviors (e.g., pulling a lever and punching a card) might have the same political implications. Moreover, if behaviors are intentional and contextual rather than the product of immutable and mechanical dispositions, then it is reasonable to assume that political behavior will be historically contingent and therefore resistant to the development of general hypotheses or scientific theories about what sorts of things individuals or institutions will do under specified circumstances.

Other criticisms have been lodged against positivistic social science by interpretivists or postempiricists, but I would like to demonstrate how the two charges against social science scientism that I just mentioned—the limits on analysis associated with an exclusive reliance on quantifiable behavioral indicators and the futility of constructing scientific theories of institutional or individual behavior—might be leveled against the approach that Tate and Haynie adopt in their investigation of comparative judicial politics. The publication of one study would not normally be the occasion for drawing attention to these larger issues relating to the nature of social science inquiry. But Tate and Haynie's article is a careful and expert example of the sort of work interpretivists have rebelled against, and consequently their invitation for scholars to follow their lead is likely to be influential. Moreover, most discussions of social science epistemology and practice take place among theorists, and only rarely are these discussions made accessible to practitioners via debates about the strengths and weaknesses of particular research strategies. Therefore, it seems useful to take this opportunity to think about whether it would be best for students of comparative judicial politics to follow Tate and Haynie's lead as we proceed to cultivate this rich terrain.

While this is not the place to have a labored discussion of epistemology, it should be noted that underlying the differences that separate my conception of social science inquiry from Tate and Haynie's are some well-rehearsed debates relating to the epistemology of the social sciences. These debates have generated endless permutations, but most of the discussions have centered on the question of whether, as Hempel (1965) suggests, the social sciences should adopt the "logic of explanation" of the natural sciences and strive for the identification of empirical

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"covering laws" that accurately predict behavior, or whether, as Taylor (1985) suggests, inquiries into the behavior of subjects rather than objects demand approaches that are nonnaturalistic and essentially hermeneutical, so that they focus on the development of a defensible interpretation of the intentional states of participants and the cultural meanings associated with the behaviors at issue. While my sympathies are with the Taylorites, in this article I will adopt the more generous view of Rorty (1980, 1982) that there is no essential methodology for the social sciences. Rather, in good antifoundationalist and pragmatist fashion, we should choose those methods that best help us cope with the particular questions or problems before us. On this view, one might agree that there are all sorts of questions where it is reasonable to offer a depiction of judges as acting like atoms or proteins—that is, as entities with certain natural predispositions to act in certain ways under certain conditions. Using this metaphor, we can construct stories of how a set of measurable inputs (e.g., appeals from prisoners) triggers a set of measurable outputs, with different sorts of judges (Democrats vs. Republicans) having different sorts of dispositions. Of course, we would not want to take these stories too seriously, since judges' dispositions are not "natural"; they are socially constructed, historically contingent, susceptible to imaginative transformations, and often resistant to tidy and stable scientific categorizations. Still, at any one period of time, and with respect to any number of issues, these dispositions can be fairly fixed and predictable, and there are all sorts of reasons why it might be useful to take measurements and calculate correlations for the purpose of offering some reliable descriptions of these short-term trends. Nevertheless, it seems to me that there are good reasons to think that the dispositions of judges and crisis rulers during transformations to authoritarian rule are unlikely to be fairly fixed and predictable, and there is no reason to think that these dispositions will conform to the predictions offered by any scientific theory, no matter how full-fledged. On this point I second James Bohman's argument in *New Philosophy of Social Science* (1991:vii–viii) that "social phenomena are shot through with indeterminacy and open-endedness" and this means that "good explanation in vital research programs must find ways to deal with the problem that this indeterminacy raises."

Let me begin, then, by discussing how a court's performance of different political functions cannot be measured in the way that Tate and Haynie would prefer, and then move to a discussion of why the quest for a full-fledged scientific theory of judicial performance during transitions to authoritarianism is unreasonable. I will end by suggesting that this is the sort of inquiry that demands precisely the sort of historical, ethnographic, and inter-

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pretive approaches that Tate and Haynie want to exclude from the analysis.

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Tate and Haynie's research is prompted by a very important question: What is the impact on the behavior of previously powerful and independent courts when democratic regimes fall victim to authoritarian rule? They begin by identifying three political functions Shapiro ascribes to courts—conflict resolution, social control, and administration—and then present a test of certain hypotheses about how the imposition, consolidation, and breakdown of Marcos's authoritarian rule might have affected the Philippine Supreme Court's performance of these different functions. For example, they hypothesize that authoritarians are unlikely to trust independent courts with the performance of important social control functions in the early stages of a crisis regime, but that as part of an attempt to achieve some measure of legitimacy, authoritarians may be willing to let courts continue to engage in routine dispute resolution. As a way of measuring changes in the performance of these functions, these authors count the number of civil, criminal, and administrative cases handed down by that Court across a period of time. They then use a Box-Jenkins time series analysis to see if they could find a statistical relationship between certain markers or "interventions" on a time series (beginning before the imposition of martial law and continuing through the time that Marcos was forced to leave the country in February 1986) and changes in the number of civil, criminal, and administrative cases handed down during that period. They found that "authoritarianism had no impact on the Court's performance of its conflict resolution function; authoritarianism's onset increased and its breakdown decreased the Court's performance of the routine administrative function; and authoritarianism's onset decreased but its consolidation increased the Court's performance of the social control function." Tate and Haynie make sense of this data by explaining that because authoritarians want to maintain some sense of legitimacy, they will allow courts to continue to exercise relatively "routine" or unimportant functions such as conflict resolution and administration, but because of the emergency they also have an interest in wresting from courts the responsibility of engaging in social control, at least until the crisis situation is stabilized, and they become interested once again in reestablishing more legitimate mechanisms of social control. Apparently, included in this story are certain "theories" about the concern of authoritarians for both legitimacy and control, and the data are supposed to represent some measure of verification of these theories. The authors thus conclude that the study "sheds light on the possible and ac-

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tual interactions that govern the relations between courts and executives, especially when those executives are authoritarians who may find courts useful but potentially troublesome or dangerous competing institutions" (p. 736).

One can see why these authors would be tempted to use Shapiro's discussion of the various functions performed by courts as a basis for constructing their preliminary scientific theory of judicial politics. One of Shapiro's goals was to point out how courts can be observed performing a variety of functions that are not normally associated with courts in liberal theory, including social control, legislation, administration, and mediation. A scientist would naturally view this observation as an opportunity to conduct a more systematic investigation of the circumstances within which different courts performed different sorts of functions. But while it is understandable why they would read Shapiro as a source of testable hypotheses, it is also a bit ironic. Shapiro certainly has some general things to say about courts, but he is not in the covering-law business; if anything, his narratives demonstrate how much judicial behavior is a product of distinctive cultural and historical contexts. Moreover, Shapiro often used the power of his historical interpretations to blur the functional categories that Tate and Haynie want to use as the basis for offering distinct scientific classifications of empirical referents. Shapiro's point was that "once we encounter the substitution of judicial office and law for spontaneous consent, the intermix of conflict resolution, social control, and lawmaking in most courts, and the frequent integration of judging with administrative or general political authority, a substantial share of courts and judges seems to be engaging in politics" (Shapiro 1981:63). Throughout the book, in fact, he seems less interested in attaching the right label to a specific instance of judicial power than in pointing out how easy it is to attach many different separation-of-power labels to the behavior of a court—how quite often what looks like judging is at the same time an act of administration and social control.

Still, for these authors, the sort of "historical examples and speculation" that characterizes Shapiro's discussion of courts (p. 719) is ultimately a weak foundation on which to build a reliable understanding of judicial politics; hence they write that a "major purpose of our analysis" is to operationalize three of the functions that Shapiro ascribes to courts so that they may be investigated more scientifically (p. 718).[6] If these operationalizations are to live up to the promise that they represent a species of "ac-

[6] Apparently, one of the authors has had second thoughts about whether the political functions that Shapiro describes can be quantified. Previously Tate wrote that Shapiro's functional typology is "far from easily operationalizable. One can easily illustrate the performance of the conflict resolution, social control, and administrative processing functions by particular courts: examples will abound. But it will be very difficult to measure the performance of these functions by courts in a manner which will lead to useful cross-national research" (Tate 1987:24).

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tual data" (p. 707) that is somehow more reliable than Shapiro's contestable textual interpretations and historical narratives, then they must resemble what Taylor (1985:28–29) refers to as "brute data identifications," which are measures that are "beyond fear of interpretative dispute"; in other words, Tate and Haynie need to convince us that the performance of a particular function is unambiguously implicated when we observe a particular empirical referent. For Tate and Haynie the brute facts that embody the performance of conflict resolution, social control, and administration are the number of civil, criminal, and administrative cases handed down by the Court. These operationalizations are characterized by the authors as "valid on their face" (p. 719) apparently because, as a matter of simple definition, we are in the habit of viewing these sorts of cases as implicating the functions ascribed to them. The data are considered reliable because everyone agrees that one is more likely to find independent verification of results when it comes to counting numbers of cases than when it comes to interpreting the political significance one should attach to judicial opinions. With the data in hand, changes in functions become a simple matter of counting changes in the numbers of cases.

One quibble with this operationalization is that, despite the authors' claim, it does not quite capture what Shapiro means by these three functions. According to Shapiro, appellate courts of last resort are not so much involved in conflict resolution, social control, or administration as they are involved in protecting and promoting the policy interests of the central regime (by issuing instructions to lower courts and supervising the bar and the bench) and providing an outlet for those who believe that an injustice has been done in a lower court (Shapiro 1980, 1981: 49–56); in other words, they perform functions more closely associated with legislation and legitimation than the ones that Tate and Haynie seek to test.[7] It is true that every civil case decided by a court of last resort represents an act of conflict resolution for the parties involved, but the function of such an appeal has less to do with an interest in resolving conflicts than in promoting

[7] It is noteworthy that the authors did not attempt in their article to operationalize the "legislative" functions of courts, particularly since their focus of attention was the Philippine Supreme Court. It is tempting to speculate that this choice was made in part because the legislative function is implicated in virtually all appellate decisions and thus could not be captured by simply counting certain categories of cases. In other words, identifying instances of legislation would require a higher degree of contestable interpretation of actual decisions than these authors thought wise, given their concern about limiting the analysis only to the most reliable (that is, reproducible) data. The authors note in note 12 that an analysis of the "policymaking" function "requires operationalizations that are quite different from those for conflict resolution, social control, and administration," and they promise to offer such an analysis in other work. Those of us who question the ability of positivists to offer "brute data identifications" of activities such as "judicial policymaking" will have to wait and see whether they offer a set of behavioral indicators to act as evidence of judicial policymaking and whether those indicators are resistant to the charge that they mask rather than resolve problems of interpretation.

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the policy interests of the central government. Similarly, while appellate court policymaking often has implications for social control, it is nevertheless the case that courts of last resort are not directly charged with imposing social control, unless they also act as courts of original jurisdiction in criminal cases—and even then, like most trial courts, they are in a position to participate in social control only as part of a larger contingent of state actors, including police, prosecutors, and generals. More commonly, though, courts of last resort do not so much perform social control as influence how it is performed by others via appellate policymaking. Finally, it is true that when Shapiro speaks about "administration," he briefly mentions the role played by appellate courts in managing a hierarchy (Shapiro 1980:641–45), and it appears to be this aspect of administration in which Tate and Haynie are interested (even though their understanding of the political implications of this role differs from Shapiro's).[8] However, Shapiro more commonly uses the concept of administration to refer to the condition of being one of the "front-line social controllers for more distant governing authority," such as a trial judge or a local magnate, both of whom are responsible for applying "general rules to particular situations on a case-by-case basis," and both of whom typically engage in conflict resolution and social control (Shapiro 1981:20–22).

While it may be misleading for the authors to say that they are operationalizing three of the functions that Shapiro ascribes to courts, their operationalizations might be defended on the ground that they capture some related functions. But it seems to me that there is a more basic question about whether there is anything to learn about how courts perform different functions by simply counting cases, without any attention to the substance of the decisions being rendered. Consider first the authors' discussion of the judiciary's social control function. They are testing two hypotheses—that the onset of authoritarianism would "initially decrease the performance of social control activities by the Philippine Supreme Court" (since the authoritarian does not want to take any chances and leave this important function to a potentially "independent" institution) and that the consolidation of authoritarian rule would increase the performance of this activity (since after a while the regime has a greater "interest in appearing to be constitutional," and this means a gradual transformation of these responsibilities away from the military and back to more familiar and legitimate institutions) (p. 716)—and they consider it a verification of these hypotheses that the Philippine Supreme Court handed down fewer criminal cases in the

[8] As I will show in a moment, Tate and Haynie treat this function of administration as relatively unimportant and unrelated to the policy interests of the regime, and this is certainly inconsistent with Shapiro's understanding of the purpose behind appellate court management of a judicial hierarchy.

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period after the onset of martial law and more criminal cases after the regime had been consolidated.[9] Because courts of last resort do not themselves perform social control, but merely make it more or less easy for others to do so, it may be appropriate to translate the prediction so that we are asking whether the Court at different times was more or less involved in, or supportive of, social control. However, the problem is that there is no reason to believe that a smaller number of appellate criminal cases means that the Court was less involved in social control and that a higher number means that it was more involved in social control. It is not hard to imagine a circumstance where a court of last resort helps a crisis regime impose social control more effectively by handing down just a few appellate cases that make it easier to prosecute regime opponents in civilian trial courts. If this were to happen, then it would seem appropriate to say that the Court was involved more actively in social control even though it was handing down fewer cases.

Of course, in the context of this study there are reasons to accept as reasonable Tate and Haynie's interpretation that the numbers imply less involvement in social control. Still, this has more to do with how our familiarity with the historical context shapes our reading of the data, and it has nothing to do with the power of ostensibly face-valid, logical inferences from the numbers: that is, in this case, the "imposition of authoritarian rule" refers to the imposition of martial law, and martial law *means* that civilian courts and routine procedures are being circumvented (and thus the judiciary is less involved in social control), just as the "consolidation of authoritarian rule" means that martial law has been lifted and hence civilian courts are getting back to business.[10] If one's judgment was prepared by a different historical context, then it is easy to see how one might immediately interpret these numbers in a way that was the *opposite* of what Tate and Haynie expect; for example, if it was discovered that the U.S. Supreme Court handed down more decisions relating to criminal procedure in the 1960s than it did in the 1950s, it would not be surprising to hear someone say that the higher number meant

[9] The authors do not adequately explain the theoretical basis for another hypothesis that was not confirmed, which was the expectation that the level of social control activity in the courts would drop once the regime transformed from consolidated authoritarianism to democracy. What makes this hypothesis curious is that under both consolidated authoritarianism and democracy, the judiciary is performing social control (in cooperation with members of the executive). Under authoritarian regimes, there's less "freedom" and therefore more social control in that respect, but that's not what's being measured when you simply count the number of criminal cases heard. One might even predict that the number of criminal cases would go up as societies become more "free"; but on the basis of the authors' measures this would suggest that more "social control" was being performed by courts. I think this means that there is some confusion in the way the authors use and then operationalize the concept "social control."

[10] One might go so far as to say that the prediction that there would be fewer criminal cases decided by courts is not so much a test of a theory about interbranch relations during the imposition of authoritarianism as it is a simple description of martial law.

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that the Court was become less involved in (or cooperative with) social control. In one context a higher number of criminal cases suggests something like "martial law is being relaxed and courts are getting back to social control," and in the other it suggests (to some) "making it more difficult for police and prosecutors to engage in social control." Still, despite the historical context that gives shape to Tate and Haynie's interpretation, we do not know for sure that their numbers support their theory, since we have no information about what the Philippine Supreme Court was actually saying in the cases it was handing down.

The inability of case counting alone to capture changes in the performance of various functions is also evident in the authors' discussion of conflict resolution and administration. As for the former, the authors theorize that, unlike totalitarians, authoritarians "are much concerned to present an appearance of acting moderately and constitutionally," and this means that "the regular judiciary may be left to 'its traditional degree of independence' " when it comes to the performance of routine conflict resolution (p. 715).[11] The authors verify that the Supreme Court made no changes in the performance of its conflict resolution function by reporting that the Court handed down roughly the same number of civil cases both before and after the imposition of authoritarianism. But it is literally impossible to tell whether these similar numbers represent continuity in the performance of this function without some reference to what the court was saying in these cases. This is particularly true since the authors define this function as an effort to resolve "specific conflicts . . . in a manner which may have some chance of being acceptable even to the loser" (p. 713). It is conceivable that the Court might have handed down roughly the same number of cases while at the same time changing the way it performed its conflict resolution function so as to hand down decisions that were more consistent with the crisis regime's agenda—for example, by being

[11] The authors note that crisis regimes will most likely allow courts to perform these routine functions while at the same time shifting "political relevant cases" to special courts that are less independent of the regime (p. 715). It should be noted, though, that because the authors' data set includes all civil cases—even those involving disputes with the government—and because they predict no changes in the way the Court was allowed to approach these cases, the authors are in effect assuming that civil cases are, by definition, ordinary and uninteresting to a crisis regime. It is unclear, then, what the proffered theory would have to say about crisis regimes that are born out of conflicts between workers and employers or landowners and peasants—conflicts that might take the form of competing claims about rights to organize or rights of property. In note 17 the authors seem to suggest that they recognize that some civil cases might be "politically sensitive" and in such cases an authoritarian ruler might be inclined to remove these cases from ostensibly independent courts. However, the authors make no attempt to distinguish the way the Court treated ordinary civil cases and politically sensitive civil cases; they simply include in their data "[civil] suits of all kinds between or among individuals, groups, and [the] government" (p. 718). This leaves open the possibility that the authors have incorporated into their conflict resolution data many "politically sensitive cases" that the Supreme Court was nevertheless allowed to address despite what their hypothesis predicts.

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less sympathetic to rights asserted by classes considered troublesome (e.g., workers or peasants).

As for the Court's performance of routine administrative tasks, the authors suggest that "[authoritarian] regimes would probably be inclined to turn the courts toward an ever increasing routine administrative load and orientation to ensure that they could not pose a policy challenge to the regime" (p. 717). Apparently, the theory is that authoritarians have an interest in keeping high court judges busy with relatively unimportant ("routine") administrative matters, and thus the authors predict that the high court will hand down more of these sorts of cases once an authoritarian regime is imposed.[12] Interestingly, the authors seem to part company with Shapiro on the politics of administrative review, since they suggest that supervision of the bar and judiciary is unrelated to the policy interests of a regime, and he suggests that this sort of supervision is usually represents an effort on the part of the central government to coordinate policymaking by decentralized decision makers (Shapiro 1980). Whether these sorts of administrative cases represent uninteresting busywork or politically sensitive policy coordination depends entirely on what types of cases are being handed down and what the judges are saying in those cases; for example, it would be interesting to know whether dissident judges and lawyers were being purged. But this is precisely the sort of information that these authors want to exclude from the analysis on the grounds that it would require subjective interpretation rather than an objective look at the indisputable facts. In light of this omission, the information we do have relating to increases in the number of administrative decisions is impossible to interpret.

In the case of each of these functions, the authors' interpretation of the data is premised on two assumptions: first, that certain categories of appellate cases are, by their very nature, either routine or politically charged; and second, that increases or decreases in the numbers of cases means that the court is increasing or decreasing its performance of a particular function. These assumptions are essential to the goal of limiting the analysis to careful counting and excluding contestable interpretations of what the judges were actually saying and doing. But unfortunately, these assumptions are also, if you'll pardon the expression, unreliable.[13] Without attention to actual states of mind and

[12] The authors do not explain why they predicted that the Court would hand down more administrative cases but only the same number of conflict resolution cases. Both of these functions are characterized as "routine" and therefore of little interest to the new regime, but in one case the authors theorize that the regime would want the Court to hand down the same number of cases and in the other it is theorized that the regime would want the Court to spend even more time on the issue.

[13] I am using "unreliable" in the conventional sense to mean "you can't count on these assumptions being true," not in the specialist sense of the scientist to mean "data collection is reproducible if other researchers follow the described procedures."

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prevailing circumstances, the limited data produced by categorizing and counting is simply too indeterminate to allow us to draw any conclusions about whether courts are changing the way they perform certain political functions.[14] This is because the performance of political functions is rarely, if ever, a matter of engaging in a fixed *behavior* more or less frequently; instead, it is usually a matter of engaging in a particular *activity* in a particular way, and one changes how one performs an activity not only by doing the same activity more or less but by doing it differently. In fact, at the risk of putting too fine a point on it, it is also the case that in light of changing circumstances, the choice to continue to engage in the *same behavior* can also amount to engaging in a *different activity*; for example, prior to the imposition of authoritarianism a judge might view civil litigation as routine and relatively insignificant for larger political purposes, but after the onset of authoritarian rule the choice to continue to resolve conflicts as they had been resolved before might be intended as a powerful message to crisis rulers that she intends to struggle to maintain her judicial independence (in the face of, for example, new expectations that she will be less sympathetic to workers' assertions of civil rights). This is just to say that the performance of a political function is a purposeful activity and not the mindless manifestation of a functional script, and as such it can only be adequately examined, understood, and explained with reference to the intentional states of the participants and the shifting contexts within which they operate.

No doubt some behavioralists will respond that a problem with a particular set of measurements should be corrected not by tainting the project with contestable interpretations of intentions and contexts but by counting different things. In order to better illuminate the central hypotheses of the analysis, some might suggest that rather than count high court opinions in criminal appeals, it might be more useful to count the number of criminal convictions in trial courts. Someone may also propose a coding scheme that distinguishes routine criminal, civil, and administrative cases from politically charged ones, as a way of getting beyond the unreliable generalizations about the extent to which crisis regimes care about certain categories of cases. But before

[14] For the record: Since all quantitative studies are premised on a set of contestable inferences made from observed behavior, they are equally vulnerable to the charge of subjectivity that behavioralists level against historical and ethnographic studies. These inferences, though, do tend to be less fully justified, since they tend to be derived from general assumptions rather than induced from a particular context. Still, the charge of subjectivity has no sting against either mode of inquiry, since the charge makes sense only if it could be contrasted to a way of knowing that is unmediated by interpretation, and my argument assumes that there is no such way of knowing, despite the methodological calisthenics that are often produced by the search for unmediated access to Reality. As Bohman (1991:103) puts it, "the realization that we are interpreting whenever we describe actions has led many to reject empirical and causal approaches entirely and announce that the social sciences have taken an 'interpretive turn.' "

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scientists run to refine these sorts of analyses, we should consider the larger question: Is it possible to construct a general scientific theory about the functions performed by courts during the rise and demise of authoritarian rule?

* * *

To believe in this project, one needs to believe that the interaction of traditionally independent courts and newly authoritarian regimes unfolds according to an immutable institutional logic or to some sort of governing law—one that flows either from a set of programmed behaviors imprinted on (or adopted by) actors who occupy these institutions or from some other fixed dispositional source, such as "rationality," that somehow standardizes the behavior of actors embedded in specified contexts. If actors in institutions have no inevitable dispositions, or if the guiding force of rationality (to the extent that the term meaningfully refers to anything at all) is not sufficiently strong to overwhelm the centrifugal forces produced by competing political purposes and distinctive contexts (see Smith 1992), then there are no such governing laws or predictable patterns to discover, only (at best) imperfect generalizations or rules of thumb culled from contestable interpretations of familiar histories.

When we examine the sorts of generalizations that animate Tate and Haynie's analysis—generalizations like "crisis rulers are sufficiently concerned about legitimacy that they have an interest in letting existing courts carry on relatively routine decisionmaking" or "courts with a reputation for independence will be considered potentially troublesome by new authoritarians"[15]—it is fairly easy to see that they are either too unreliable or indeterminate to serve as the basis either for a full-fledged scientific theory or for a set of predictions about how particular courts and authoritarians will behave. Even Tate and Haynie are willing to admit (in a footnote) that some crisis rulers, such as Fujimoro in Peru, are not at all shy about closing down courts (note 18); and as I write, this Boris Yeltsin has just announced that he is disbanding Russia's constitutional court, even though arguably he is "much concerned to present an appearance of acting moderately" and has "[justified seizing] dictatorial power as temporary but necessary to preserve the nation . . . in a time of crisis" (p. 715). It is not that Fujimoro and Yeltsin deviated from an otherwise reliable script; it is that there is no script from which they can deviate. Judges and authoritarians are not the "passive bear-

[15] These are paraphrases. What Tate and Haynie actually say is this: "[Crisis governments] are much concerned to present an appearance of acting moderately and constitutionally. That appearance would clearly be harmed by too obvious an effort to take control of independent courts, which are likely to be perceived as defenders of constitutionalism and the rule of law" (p. 715).

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ers" (Bohman 1991:13) of the purposes, goals, and calculations ascribed to them by social scientists. It is, of course, reasonable to assume that some crisis rulers and judges will exhibit the predicted intentions and dispositions, but this is merely evidence of the fact that general theories are often constructed from familiar historical events (what other origins would they have?); the appearance of generality is easily produced by the removal of proper names. The point, though, is that these generalizations are not offered as defensible lessons extracted from particular cases; they are presented as statements about the goals and dispositions of certain types of powerholders, applicable as a general rule in every case where powerholders possess certain defining characteristics. Once a generalization is transformed from an imperfect and politically charged comparison to a scientific governing law, it develops the weighty responsibility of predicting behavior (rather than merely making sense of behavior that has already been examined), and unless powerholders are governed by the same sort of fixed dispositions that characterize the objects studied in the natural sciences, it is inevitable that the contingencies of history, culture, and personality will expose the indeterminacies of the model. Among other things, this is because key terms in the model, such as "routine decisionmaking," mean different things in different contexts and because key categories, such as "crisis rulers" or "courts with a reputation for independence," include within them powerholders who do not share a fixed set of goals, purposes, or capabilities.[16]

In the article under discussion, perhaps the most telling example of how a set of confident generalizations becomes vulnerable when we pay careful attention to local circumstances can be seen when we examine the linchpin of Tate and Haynie's argument: the claim that the Philippines Supreme Court was an independent institution forced to adapt to the imposition of authoritarianism. The authors suggest that the available historical evidence supports their claim that for decades prior to the imposition of martial law the Court was "independent," "above the struggle of partisan politics," but nevertheless "often in the thick of important public policy disputes" (pp. 708–9). The list they provide of "politically explosive conflicts" in which the Court participated lays the groundwork for their hypotheses, all of which

[16] Geertz (1983:57–58) argues that interpretive concepts tend to be "experience-near" while those of macrosociology are "experience-distant." The former emerge from "thick" descriptions of social life—that is, descriptions that are designed to give a rich account of lived experience—while the latter are used in the sort of "thin" descriptions that are characteristic of general theories. My point is not that we should not make use of concepts that are "experience-distant"; some distance from experience is essential for critical analysis (Bohman 1991:218–27). My point is only that we should not use "experience-distant" concepts as if they were accurate descriptions of actual intentional states, and we should only use those experience-distant concepts that we can defend in light of our familiarity with actual events.

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are premised on the assumption that the crisis regime wanted to maintain some legitimacy but was also interested in preventing this ostensibly independent Court from exercising power over "politically relevant" issues. Surprisingly, though, this central premise is seriously undermined when the authors point out at the very end of the article that "even before he declared martial law, Ferdinand Marcos had appointed a majority of Supreme Court justices," and that "it was clear that some Supreme Court justices accepted the president's definition of the national crisis that justified his declaring martial law" (p. 735). In light of this it is no longer obvious that this is a case study of how authoritarians cope with "potentially troublesome or dangerous competing institutions" (p. 736). It is certainly possible that the justices on the Court at the time of the imposition of martial law were not so close to him as to be completely trusted during the emergency, and that this uncertainty about the Court led him to treat it as potentially dangerous.[17] But without more historical or ethnographic information about the actual relationship between Marcos and these justices, and without more textual information about what the justices actually said in the decisions they handed down, we are also free to speculate that Marcos felt free to impose martial law because he knew that a majority of the Court would support him. If we replace Tate and Haynie's assumption with this one, then much of the information they report takes on a very different meaning: The lack of change in the number of conflict resolution cases means that Marcos trusted the justices to address both routine and politically important civil cases; the short-term reduction in the number of criminal cases either reflects the simple fact that there is less crime under martial law and thus fewer cases to be appealed, or it suggests that the Court cooperated in the social control by handing down a handful of very supportive precedents; and the increase in the number of

[17] The only information Tate and Haynie provided in this regard is that after martial law Marcos was no longer required to present his appointees to the Congress-based Commission on Appointments, and this meant that around the time of the consolidation of his rule he had "ensured that a substantial majority of the justices were his personal and political cronies" (p. 735). It is not stated but the impression is left that the justices who were appointed prior to the imposition of martial law were his partisans but were not quite his cronies. It is unclear, though, whether it is part of the authors' theory that crisis rulers who face courts that have been staffed with their own appointees will act the same as crisis rulers who face courts that have been staffed with appointees from other parties.

Also, a problem of interpretation arises once we are told that it did not take long for Marcos to pack the Court with his cronies. Tate and Haynie interpret the increase in social control activities by the Supreme Court after the consolidation of authoritarianism as a manifestation of the crisis ruler's "need to be perceived as acting legitimately" (p. 716). However, this reestablishment of the Court's social control function could just as easily be explained as resulting from the fact that Marcos's cronies now dominated the Court and thus the Court was no longer potentially troublesome. In fact, if it was widely understood that Marcos had packed the Court with his cronies, then it is unlikely that he would have been perceived as acting legitimately when he gave them more authority to engage in social control.

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administrative cases reflects Marcos's interest in having the Court purge dissidents from the bench, bar, and bureaucracy.

I am not suggesting that this is a more accurate reading of the data than the one offered by Tate and Haynie; in fact, as will become clear in a moment, I am fairly confident that Tate and Haynie are correct when they suggest that, at least for a time, Marcos considered the Court independent and potentially troublesome. I am only suggesting that any effort to resolve these competing interpretations must pay more attention to actual intentions and contexts and less attention to states of mind that are predicted by abstract theories; and I think it is safe to say that Tate and Haynie's conclusion (and I dare say their general theory) was shaped more by their familiarity with the specifics of Philippines history than by their ability to draw logical inferences from the case counting. I am also suggesting—as Shapiro tried so hard to point out—that there's usually more to a concept such as "judicial independence" than meets the eye, and it certainly should not be treated as a fixed state that some courts find themselves in and that results in judges having certain predictable dispositions, such as a reflexive hostility to martial law; even U.S. Supreme Court justices have failed to follow that script.[18] This is just to say that we are more likely to advance our understanding of judicial politics if we begin with history, ethnography, and textual interpretation and then induce whatever generalizations we can defend than if we begin with abstract theories and then try to verify derived predictions by quantifying behavior while ignoring the meanings that participants attached to their behavior.

We are more likely to advance our understanding this way because, as Bohman (1991:6–7) puts it, "The social sciences are indeed 'sciences of indeterminacy' whose theories do not succeed by predicting unique and determinate outcomes. . . . The proper form of explanation in the social sciences is both non-reductionist and non-determinist, treating phenomena that are not only diverse and irregular, but intentional and complex." Or, in Smith's words (1992:10–11), if even "our own daily experiences of making choices do not seem reducible to single-factor or even multifactor determinist explanations," then "we might insist firmly on the autonomous importance of human choices, particularly political decisions, on their irreducibility to explanation by exogenous factors, and on their role in reshaping all such factors." Importantly, though, neither Bohman nor Smith suggests that the end of naturalistic, positivistic, Hempelian social science means the end of rigorous inquiry in favor of either the anarchy of irresponsible subjectivism or the banality of accounts

[18] Tate (1987, 1993) demonstrates greater sensitivity to the problems associated with operationalizing "judicial independence" than is evident in this article.

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that report just one-damn-thing-after-another.[19] Smith (ibid., p. 29) suggests that some generalizations, at least historically contingent ones, might be possible, but whether they "will prove reliable enough to be worthwhile must be answered at least in part by experience"—and, I would add, with reference to the purposes we have in mind when we ask the questions in the first place. Bohman argues that in our post-Kuhnian world standards of appropriate evidence and adequate explanation can no longer be derived from facile and unselfconscious references to objectivity or neutral methods of procedure. Instead (and this tends to happen automatically anyway), practitioners need to directly discuss which research projects seem to work best, or otherwise seem satisfying or useful for certain purposes. The debates that are generated by these conversations can shape the practice of social science, not by generating strict methodological guidelines (of which there should be none) but by nominating "Kuhnian exemplars," that is, " 'paradigmatic' cases that exhibit the general features of an adequate and complete explanation of a certain type" (Bohman 1991:5).[20]

For the comparative study of judicial politics, my candidate for an exemplar would not be Tate and Haynie's search for a general theory but another article recently published by one of the authors. In "Courts and Crisis Regimes: A Theory Sketch with Asian Case Studies," Tate (1993) tells a story that, to its credit, reads more like Shapiro than Hempel. He suggests that one useful way to think about how crisis regimes cope with courts would be to isolate how they respond to four different features of judicial politics: judicial independence, which is the degree to which they interpret the law in opposition to the preferences of others who have political power; judicial impartiality, which is the extent to which judges decide cases based on expressed rules rather than the preferences of interested parties; the scope of judicial decisionmaking, which is the range of subject matters over which courts are authorized to assert authority; and the depth of judicial decisionmaking, which is the extent to which courts are able to change or invalidate the rules that apply in specific cases.

[19] Irresponsible subjectivism is never a problem because the practice of social science—like all purposeful activities—is constituted by an evolving set of assumptions and expectations about what counts as a defensible example of the practice. These assumptions and expectations are always contestable but at any one time a set of them is always in place; otherwise nothing could be recognized as an instance of social science (see Fish 1989). The purpose of this article is to participate in the process by which practitioners negotiate the future shape of the practice of comparative judicial politics. That was part of the purpose of Tate and Haynie's article as well.

[20] Bohman (1991:14) also advises that practitioners should not bestow on themselves "the authority to eliminate alternative directions and theories." Instead, we should judge "research programs and their typical explanations to be better in some respects and worse in others. While such comparative criteria of evaluation are not strong enough for many epistemologies, they are sufficiently normative for the social sciences and the practical knowledge that they warrant."

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Rather than try to operationalize these concepts, Tate uses them as lenses through which to interpret the interactions between crisis rulers and courts in India, Pakistan, and the Philippines. The result is a rich yet rigorous narrative of three interbranch dramas, one that draws freely from press reports and other contemporaneous sources and that is quite attentive to the importance of specific events and personalities. He demonstrates how in each case crisis rulers chose not to assault the structure of courts but instead moved to limit the scope and depth of their decisionmaking authority in order to insulate important regime initiatives from judicial review. He also shows how, at some point, each regime responded to the judiciary's "timid defiances of the regimes' wishes" by attacking the courts' independence and impartiality (ibid., p. 336). Tate's generalizations are persuasively induced from these case histories and raise important questions about "whether courts actually can play more impressive roles" when battling authoritarianism (ibid.). At the same time Tate shows how the distinctive political histories of these three countries meant that all crisis rulers were not equally interested in using courts to nurture a sense of constitutional legitimacy and were not equally tentative about assaulting the judiciary's independence and impartiality. For example, while "Pakistan's history of military involvement in politics" laid the groundwork for Zia's claim that "the armed forces have a higher duty to the nation that may occasionally require them to act extraconstitutionally," the prevailing practices of Philippine politics led Marcos to openly assign "the Supreme Court the role of guaranteeing the constitutionality of his crisis-generated authoritarianism" (ibid., p. 331), with the result being that Zia and Marcos adopted very distinct strategies with respect to courts as they attempted to consolidate their power.

Predictably, Tate's review of the history of the Philippines crisis in this article resolves many of the questions left open by the more narrow data used by Tate and Haynie. For example, we learn that, at the time of the imposition of martial law, Marcos made it a point to defend "himself against charges of dictatorship by pointing out that his actions were subject to judicial review by the independent Supreme Court" (ibid., p. 326). At the same time, Marcos dramatically limited both the depth and the scope of the Court's decisionmaking by excluding from judicial review all decrees and actions taken by him or his representatives during martial law and by giving the military the jurisdiction to try almost any criminal case it wished to hear. In response the independent-minded Supreme Court agreed to hear petitions challenging the validity of martial law decrees. It was not long before 6 of the 10 justices supported a decision "rebuking the President for the manner in which he had declared the new constitution to be in effect." On the other hand, 2 of the 6 were unwilling to go

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as far as "to declare the new constitution *not* in force," and this meant that the Court as a whole did very little to stop Marcos (ibid., p. 327). Tate concludes that Marcos's "support on the Supreme Court was sufficient in the early days of martial law to cause key decisions not to go against him, even if the Court did not strongly endorse his crisis rule. . . . [After some time] the President's consistent appointment of close associates to the many new vacancies on the Court, caused by the expansion of its size and the adoption of lower retirement age under the provisions of the new constitution, made the Supreme Court no longer a potential source of opposition to the President" (ibid., p. 328).

This account of the relations between Marcos and the Court, which is chockfull of the sort of "historical examples and speculation" that Tate and Haynie treat as second-rate data, is actually much more persuasive and seemingly *reliable* than the one that is anchored solely in what Tate and Haynie characterize as "actual" reliable data. Not only are we made more familiar with the actual political goals and strategies of the participants, but we can also see that the interactions between Marcos and the Court proceeded, not in accordance with some inevitable institutional logic or behavioral pattern, but for reasons that were actually quite contingent: there was nothing necessary about the fact that the Court Marcos faced included four judges who were firmly for martial law, four judges who were firmly opposed to martial law, and two who were ambivalent or cowardly, and there is no doubt but that events would have proceeded quite differently had Marcos known (for example) that he could have counted on six cronies on the Court or, alternatively, that there were eight judges who simply would not stand for his actions. The ultimate outcome might not have been too different—Marcos's regime probably would have been consolidated one way or another—but the lessons relating to the judicial response to authoritarianism would have been different, and that is the topic about which we are trying to learn more.

Tate made it a point in the title of this article to emphasize that he was merely offering a "theory sketch." But he is misguided if his intention was to denigrate the accomplishments of his more historical and interpretive work simply because it did not conform to a flawed blueprint for scientific inquiry in the social sciences. A topic as interesting and important as the relations between courts and new authoritarians deserves better than case counting and abstract models. Tate has been studying the Philippines for 20 years now (Tate 1974), and we have a lot to learn from his familiarity with these events and his expert judgment about what they mean. Let's not require our experts to hide most of what they know on the false promise that we will learn more, or at least something deeper and more fundamental,

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if we look only at reliable machine-readable data. Let's not pretend as if we can construct explanations without reference to the experiences of participants and the circumstances within which they made their choices. Let's use careful counting when appropriate, in cases, for example, where we want to know whether judges with military backgrounds hand down more severe penalties for draft evasion than other judges (Cook 1989). But in examining the judiciary's performance of political functions, let's proceed by telling real stories and extracting whatever general lessons we can defend, and let's not worry so much about the construction of full-fledged scientific theories.

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