Building a Scientific Comparative Judicial Politics and Arousing the Dragons of Antiscientism

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> There are both traditionalists and behaviorists who think the gate is strait and the way narrow into the public law kingdom, but a more sensible text for all to contemplate is the old Chinese saying "Let a hundred flowers bloom."
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> —Pritchett (1968:509)

> Perhaps the single most endearing attribute of scientific research is that it is intersubjectively transmissable; that is, through reproduction, knowledge generated by the scientific method can be known independently by all. . . . [and] the *falsifiability* of propositions is possible. The hallmark of scientific inquiry is its reproducibility.
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> —Gibson (1986:142-43)

> Do not meddle in the affairs of dragons,
> For you are crunchy and good with catsup.
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> —Bumper sticker, Dallas, Texas (30 December 1993)

Following both Pritchett and Gibson, we thought we were, in "Authoritarianism and the Functions of Courts" (Tate & Haynie 1993), extolling the virtues of an infrequently grown, but potentially beautiful or even useful, floral species: falsifiable, reproducible research on the role of courts outside the United States of America. Indeed, because our bloom grew outside the well-cultivated plots of the industrialized democracies, we thought it might be of still greater interest to the horticultural community of sociolegal studies. Thus we urged the members of that community to try their own hands at growing this rare blossom.

Where we saw a flower, however, Howard Gillman saw a weed, one at least as irritating as ragweed, and possibly as dangerous as poison ivy. To call attention to this dangerous breed and to warn against its further cultivation, he has written a critique nearly as long as the original article and intricate in its argu-

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ments. As we have tried to trace and respond to Gillman's arguments, we have sensed the lurking presence of intellectual dragons we thought long slain and have felt just a little brittle and sauce-laden.

The Issues

Metaphors aside, Gillman's critique appears to raise several general issues. First, should (any) scholars follow our lead in trying to "begin the construction of empirical theory about the institutional performance of courts" (Tate & Haynie 1993:707)? Second, and subsuming the first issue, do such attempts proceed from "the mistaken assumption—inappropriately borrowed from the natural sciences—that institutions and individuals are naturally disposed to behave in certain ways under certain conditions" (Gillman, p. 3)? Third, does our work do justice to Shapiro's (1975, 1981) typology of the functions of courts, either in nominal or operational definition?

In fairness to Gillman and to ourselves, we must note that this list of issues represents our reconceptualization of Gillman's text. After several initial pages of argument regarding the possibility and desirability of attempting to construct empirical theory, Gillman says that he wishes to discuss "how a court's performance of different political functions cannot be measured in the way that Tate and Haynie would prefer," "why the quest for a full-fledged scientific theory of judicial performance during transitions to authoritarianism is unreasonable," and to "end by suggesting that this is the sort of inquiry that demands precisely the sort of historical, ethnographic, and interpretive approaches that Tate and Haynie want to exclude from the analysis."

Building Empirical Theory and Diversity of Inquiry in Comparative Judicial Politics

We began our article with the confession that "We seek to build empirically based theory about the institutional performance of courts" (p. 707). If pressed, we would probably go further and admit that we would prefer to try to conduct research on courts and judges that is as scientific (read "falsifiable and reproducible") and as explanatory as possible, that we would prefer doing what we would regard as scientific, theory-building research to any other kind of sociolegal research we might attempt. We hold this view even though one of us regularly teaches with great relish courses in constitutional law that employ the traditional tools of history and interpretation, while the other seeks to maintain the knowledge of the history and culture of the Philippines that would qualify him as a competent country specialist.

We think there are good reasons to try to conduct research that is as scientifically explanatory as possible, and we shall have more to say about those reasons below, though we think they are probably quite clear to most social scientists or even to lay observers of the scientific enterprise. For the moment, what we wish to stress is that our preference for such inquiry does not blind us to the difficulty of doing it or lead us to reject research that falls short of some arbitrary scientific or explanatory ideal.

The record of our published work on the Philippine Supreme Court documents our willingness to practice research diversity, as it runs the gamut from (interpretive?) narratives based on historical and journalistic data encumbered by little or no theoretical apparatus (Tate 1994, 1992b, 1974), through articles using similar data or multiple data sources but employing a more formal theoretical perspective (Tate & Haynie, forthcoming; Tate 1977, 1993; Tate & Sittiwong 1986), to analyses employing the full apparatus of scientific investigation—conceptualization, hypothesis formation, operationalization, measurement, quantitative data analysis, hypothesis testing, generalization (Tate & Haynie 1993; Haynie 1994; and, marginally, Tate 1972). We have practiced research diversity in spite of our commitment to scientific explanatory analysis because we recognize that (1) good description is necessary to proper explanation and (2) not every topic worth discussing, not every question needing an answer, can be addressed using a research design that encompasses "the full apparatus of scientific investigation."

We are warily flattered by Gillman's judgment that "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" (p. 4). We can hardly deny that we expressed the hope that our work might stimulate others to produce "time series models of the performance of courts that are more complete and sophisticated than [the ones] offered here" (p. 737). We thought it might be interesting or fruitful for them to do so. We do deny, however, that we sought (or seek) to encourage scholars to do *only* the sort of work represented in "Authoritarianism and the Functions of Courts." Neither of us has any immediate plans to work in Gillman's self-identified fields of philosophy of law, critical legal studies, constitutional history (see Gillman 1993), and civil rights/liberties[1] (American Political Science Association, Law & Courts Section 1992:17), but we recognize that these fields are fascinating and rewarding to many scholars, and we lack the hubris to suggest that their work is misleading or worthless.

[1] We have been interested in the scientific analysis of patterns of U.S. Supreme Court civil rights/liberties preferences across individual justices (Tate 1981; Tate & Handberg 1991) and over time (Haynie & Tate 1990).

It may also be important to note that our work is not unique, either generally, as a scientific, explanatory study of comparative judicial politics, or specifically, as a rigorous time series analysis of the functional performance of a non-American court system. Since Gillman has not, to our knowledge, been involved personally in comparative judicial research, it is understandable that his perception of the intellectual history of the field is incomplete.[2] Certainly the events and scholarly publications he identifies at the beginning of his critique represent important developments in the study of comparative judicial politics. But his history misses what were, in general, equally important developments, particularly the powerful precedent for empirical comparative judicial politics research represented by the publication of some 15 quantitative studies of comparative judicial behavior in two important symposia, *Comparative Judicial Behavior* (Schubert & Danelski 1969) and *Frontiers of Judicial Research* (Grossman & Tanenhaus 1969).[3] More specifically and more recently, our brand of comparative scientific explanatory institutional analysis has been seen in an excellent study by Giles and Lancaster (1989).[4]

Why, then, is Gillman so concerned that we might lure some scholars into trying the kind of analysis represented in "Authoritarianism and the Functions of Courts"? The principal reason is that Gillman does not accept the possibility or the desirability of attempting to construct "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" (p. 2).[5] It is when we contem-

[2] Certainly our understanding of Gillman's self-identified fields would be equally incomplete.

[3] Also important were the conceptual analysis of Becker (1970), the pioneering monograph of Russell (1969), two empirical monographs analyzing European judiciaries (Morrison 1973; Kommers 1976), and the first version of another conceptual analysis (Shapiro 1975). See Tate (1993) for a more extensive discussion.

[4] Given our focus on the analysis of the institutional behavior of a non-American court over time, it may also be worthwhile to note that *Law & Society Review* has been the preeminent publication site for such studies. For examples, see Toharia (1975), Blankenburg (1975), and the studies by Wollschläger, Van Loon & Langerwerf, Clark, and Ietswaart, all in the 1990 special symposium issue on longitudinal studies of trial courts (vol. 24, #2). Of course, we do not mean to imply that these studies necessarily share our analytical intentions or scientific perspective.

[5] The quoted phrase appears to be presented by Gillman as if it were a paraphrase of Schmidhauser (1987:240). While we believe Schmidhauser would approve of what we tried to do in "Authoritarianism and the Functions of Courts," we cannot, after several careful readings of the brief chapter Gillman cites, find the sentence(s) for which the quoted phrase appears to be an accurate summary of Schmidhauser's words. This may be an innocent mistake, but it may also represent an unfortunate tendency to exaggerate the claims being made by those trying to build a scientific comparative judicial politics. For example, in the next sentence Gillman cites Tate & Handberg (1991:461–62) in support of his statement that "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" (p. 2). While this certainly is an accurate paraphrase of the sentiments expressed by the authors as an ideal (see especially note 3, pp. 461–62), Gillman ignores the subsequent paragraph (also on pp. 461–62):

plate starting an argument concerning this point of view that we most clearly feel the hot breath of the dragons of antiscientism.

Gillman, allegedly following the lead of other interpretivists or postempiricists, apparently sees "Authoritarianism and the Functions of Courts" as an excellent example of social science scientism (p. 4). If we accept a definition of scientism as "the belief that the assumptions, methods of research, etc., of the physical and biological sciences are equally appropriate and essential to other disciplines, including the humanities and the social sciences,"[6] it is difficult to avoid falling into a childish and futile "yes you are, no we're not" exchange of accusations. Certainly we agree that the *scientific method* (as opposed to any specific method of the physical or biological sciences) is appropriate and essential to the investigation of many important research questions in the social sciences and some in the humanities. While we do not seek to speak for others, we certainly do *not* believe that the methods in question are appropriate and *essential* to the investigation of all questions in the social sciences (as normally understood) or humanities.

Leaving aside the obvious case of normative propositions, on which, for the record, we are probably best characterized as "noncognitivists" leaning toward "naturalism" (see Dahl 1976: 132–37), we recognize the existence of aesthetic questions for which the scientific method is irrelevant. We also recognize that there are in the social sciences many questions for which "qualitative" research methods represent the best or even the only feasible approach. However, we do not agree that the standards of science as we understand it are irrelevant to qualitative research. Instead, we share the view of King, Keohane, and Verba (1994:1–2):

> [T]he traditions of what are conventionally denoted "quantitative" and "qualitative" research . . . appear quite different; indeed they sometimes seem to be at war. Our view is that these differences are mainly ones of style and specific technique. The same underlying logic provides the framework for each research approach. This logic tends to be explicated and formal-

Since this ideal cannot yet be achieved in any area of political science research, researchers should expect that as they build theories of political behavior, they still may find it necessary to include the "proper names" of temporal and geographical political systems in some way. Theory that explains a given behavior well in one time period or place may not work so well in another unless it is modified to reflect differences in the principal social and political structures and processes among different political systems.

[6] The full entry in a commonly used unabridged dictionary is:

Scientism [pronunciation omitted]. *n.* **1.** *Often Disparaging.* the style, assumptions, techniques, practices, etc., typifying or regarded as typifying scientists. **2.** the belief that the assumptions, methods of research, etc., of the physical and biological sciences are equally appropriate and essential to other disciplines, including the humanities and the social sciences. **3.** scientific or pseudoscientific language. *The Random House Dictionary of the English Language*, unabridged ed. 1967.

> ized clearly in the discussion of quantitative research methods. But the same logic of inference underlies the best qualitative research, and all qualitative and quantitative researchers would benefit by more explicit attention to this same logic in the course of designing research.

As a final defense against the dragon's teeth, we must protest that we do not recognize in our work (or in that of other quantitatively oriented scholars we know) the kind of social science Gillman describes in suggesting why there has been an "interpretive turn" in our disciplines. We have not, in all honesty, given much thought to the reasons for the "interpretive turn" to which Gillman refers. More or less fascinated by our work, we assumed that those who chose to follow interpretive lines of inquiry found their work rewarding, and it had not occurred to us to warn others against pursuing it, even though we chose not to do so ourselves.

Nevertheless, the view of "our" brand of social science put forward by Gillman is stereotypical: it depicts it as seeking cast-iron covering laws.[7] Those who describe this view seem never to have heard of or at least to reject out of hand any notion of stochastic "covering laws" that are multicausal and far from immutable, mechanistic, or deterministic, yet it is precisely such laws that characterize almost all social science as we know it. Gillman suggests that "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')" and fail to take into account "the actual intention states of participants and the circumstances within which they operate" (p. 3). To this we reply that the "actual" lived experience, "actual" intentions, and "circumstances within which" political actors operate may, given appropriate theory, be extremely important in behavioral explanations of political life; the "behavioralist" merely insists that if our knowledge of these phenomena is to support valid and transmissible inferences, it must be produced (and reproducible) by "the systematic use of well-established procedures of inquiry" (King et al. 1994:4).

We are reluctant to try to follow Gillman in his brief foray into the epistemology of the social sciences. But with appropriate caveats, we can agree with him that (all quotations are on p. 5):

- "[T]here is no essential methodology for the social sciences." The methodology to be adopted depends on the nature of the question being investigated. However, when one is investigating questions amenable to scientific analysis, there is no "essen-

[7] Gillman cites with apparent approval the discussion by Almond & Greco (1990), which puts forward an especially stereotypically distorted view of a "hard" political science devoted to the adoption of extraordinarily simplistic "clocklike" approaches to the explanation of political behavior.

tial methodology for the social sciences" only because there is a basically a common method for all scientific inquiry.

- "[J]udges' dispositions are not 'natural'; they are socially constructed, historically contingent, susceptible to imaginative transformations, and often resistant to tidy and stable scientific categorizations." We do not view any of the important variables in social science as reflecting "natural predispositions" in the sense in which Gillman appears to use this term, and we agree that judges' dispositions are as Gillman describes them. We would only point out that dispositions are not always able to fend off satisfactory, if not complete, scientific explanation (see Pritchett 1948; Schubert 1965, 1974; Segal & Spaeth 1993, for examples that appeal to us).
- Gillman quotes Bohman to say 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.'" We could not agree more. We would only add that when we seek explanations concerning empirical matters, it does little good to throw up one's hands at the difficulty of the problems. Any strategy for dealing with these problems, whether it involved qualitative or quantitative inquiry, must be conducted in as scientific a manner as possible if it is to have any hope of generating transmissible, reproducible explanation. Other types of inquiry may succeed (or not) in promoting insight or inspiration, but not explanation.

Studying Courts under Authoritarian and Democratic Rule: Conceptualization, Operationalization, Measurement, Analysis

Gillman thinks it understandable that we "were tempted to use Shapiro's discussion of the various functions performed by courts as a basis for" what he refers to as "our preliminary scientific theory of judicial politics"[8] (p. 7). Apparently we all agree that Shapiro's discussion is insightful and potentially useful in creating conceptual frameworks for analyzing the institutional performance of courts. However, Gillman also thinks it ironic that we use and try to operationalize concepts from Shapiro because Shapiro is "not in the covering-law business," and because he "used the power of his historical interpretations to blur the functional categories" (p. 7) we want to use.[9] We do not find

[8] We certainly regarded our analytical objective as considerably narrower than developing "a preliminary scientific theory of judicial politics" as a whole subject of inquiry.

[9] In the succeeding sentence, Gillman notes: "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'" (p. 7–8). While we certainly

these arguments relevant. Like Gillman and like us, Shapiro presumably uses his considerable talents to explore topics that he finds interesting, using the tools he feels are appropriate or with which he feels most comfortable. We have not asked Shapiro what he thought of our use of his concepts.[10] Whether he is in the "covering-law business" or even expressly opposed to it, whether he blurs or keeps clear the functional categories he proposes, does not prevent us from exploring their utility in reproducible tests of falsifiable hypotheses about the reaction of courts to authoritarian regimes.[11]

For our inquiry, the key question is whether we can operationalize (our adaption of) Shapiro's concepts reliably and validly.[12] Here Gillman faces a dilemma in proceeding with his critique of our work. His epistemological position mandates that he reject any attempt at operationalizing (quantifying) particular concepts. Nevertheless, he feels it necessary to argue that our particular choices of operationalizations are not adequate. To bolster his critique, he argues that if our operationalizations "are to live up to the promise that they represent a species of 'actual data,'[13] then they must resemble what Taylor [citation omitted] refers to as 'brute data identifications,' which are measures that are 'beyond fear of interpretative dispute'" (p. 8). We reject this assertion.

In scientific research, it is ordinarily possible to suggest any number of operationalizations for an important concept, including operationalizations that may not be "beyond fear of interpre-

agree with this quotation, we are unable to determine how it relates to our analysis or Gillman's criticism.

[10] Of course, our vanity leads us to hope that he is fascinated by that use, whether or not he thinks we have chosen to use his concepts wisely.

[11] Perhaps we should remind readers that operationalizations of Shapiro's concepts merely serve as the dependent variables in our analyses. The hypotheses we test are not especially influenced by Shapiro's discussion.

[12] Gillman correctly observes (in his note 6) that one of us, in an earlier work (Tate 1987:24), expressed doubt about the ease with which Shapiro's concepts could be operationalized for use in cross-national research. After more than a decade of reflection (the first draft of the work in which the doubts were expressed was written in 1981), Tate has indeed had second thoughts, and has concluded that it is worth attempting to operationalize and use Shapiro's insightful concepts, despite the anticipated difficulties in doing so. The approach taken in "Authoritarianism and the Functions of Courts" suggests that the difficulties were perhaps not as great as he originally anticipated. The criticisms made by Gillman suggest that perhaps they were. What makes the latter conclusion less acceptable than the former is that Gillman's position rests on the assumption that there is no point in attempting to identify and employ any operationalizations that are reproducible and that lead to falsifiable propositions.

[13] Gillman continues the sentence containing the quoted words by stating that we must argue that the actual data "is somehow more reliable than Shapiro's contestable textual interpretations and historical narratives." We make no such argument, but one wonders how Shapiro's discussion, which does not treat the Philippines or the performance of courts under authoritarianism in a way that is relevant to our inquiry, can be seen as an alternative to our efforts at operationalization. Presumably Gillman means that we must somehow demonstrate that our operationalizations are "more reliable" than some narrative account of the Philippines that we might alternatively construct.

tative dispute."[14] What is important about scientific operationalizations is not so much that they are beyond dispute but that they are defined clearly, specifically, and publicly so that they lead to falsifiable hypotheses and reproducible research procedures. Such operationalizations obtain plausibility if they can be demonstrated to be valid and reliable.[15]

Are the operationalizations we have used valid? Gillman correctly notes that our principal argument for the validity of our operationalized measures is that they are "valid on their face," that is, they may be presumed valid because, to use Gillman's words, "we are in the habit of viewing these sorts of cases as implicating the functions ascribed to them" (p. 8). This represents what some have called the "informal" test of face validity (Kidder 1981:132). We claim only "informal" face validity because we have not assembled a formal panel of judges but relied on our own "expert" judgment and that of the sociolegal community who will read our article. Gillman's comment confirms that our operationalization has passed the informal test of face validity with even a critical member of that community.

Are our operationalizations reliable? If they are, it is not 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" (Gillman, p. 8).[16] Assessing the reliability of our operationalized measures has nothing to do with comparing them to interpretations of the political significance of the Philippine Supreme Court's judicial opinions. It might (or might not) be a good idea for some scholars (possibly including us) to engage in such interpretation for any number of purposes. But for the purpose of assessing the reliability of our measures, the key question is whether successive judges will, over repeated trials, consistently classify our given set of the Philippine Supreme Court's decisions into the same substantive catego-

[14] Assuming that this means that one person might say "I think *X* is a good operationalization of Concept *Y*," while someone else might say "I disagree; *X* does not seem to be a good operationalization of Concept *Y*." For the most constructive dialogue in these circumstances, it is very helpful if the second person then suggests a specific alternative operationalization that might be superior to *X*.

[15] It may be important to define "valid and reliable," since Gillman's use of these terms, especially "reliable," appears sometimes to employ definitions other than those most commonly used in the social sciences. A "valid" measure is one that measures what it is intended to measure. A reliable measure is one that measures the same thing time after time in repeated measurements. A measure may be reliable without being valid. A valid measure must, ultimately, be reliable, since, if it did not measure the same thing time after time, it would not, on at least some of those occasions, be measuring what it was intended to measure (see, e.g., Leege & Francis 1974:141–42; Stouffer 1962:265, as cited in Riley 1963:250; Simon & Burstein 1985:18–19; Babbie 1992:129–34).

[16] Here, Gillman does not appear to be using "reliable" in the sense of the definition we have just given.

ries (i.e., civil case, criminal case, judicial administration case).[17] Though we do not report it in "Authoritarianism and the Functions of Courts," we do have substantial information from analyses of our data that suggests that classification of the cases at the level of generality required for our purposes can be carried out in a highly reliable fashion.[18]

Of course, our arguments for the face validity and reliability of our operationalizations of the conflict resolution, social control, and routine administrative functional performance of the Philippine Supreme Court are not really relevant to Gillman. Casting such plebeian considerations aside, he first contends that our operationalizations are inappropriate distortions of Shapiro's concepts because they are measured for an appellate, rather than a trial, court. For example, with regard to our operationalization of the conflict resolution function, Gillman states: "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 the policy interests of the central government" (p. 9). The phrase before the comma represents our view of the function of conflict resolution as performed by any court, and but for the phrase after the comma, we would fail to see why our operationalization is problematic, since Gillman agrees that the resolution of civil cases does constitute "conflict resolution" in the sense we quite obviously intended. But the phrase after the comma tells us that conflict resolution in the sense we intended is not really conflict resolution if performed by an appellate court, because it has to do with "promoting the policy interests of the central government." Following Shapiro's arguments concerning "the uses of appeal" (1981: 49–56), what Gillman is saying is that "resolving conflict" can also serve the interests of the central government.

Gillman makes a similar argument about social control. He reminds us that no court can impose social control by itself but can only participate in the performance of that function with other agencies, and contends that "courts of last resort do not so much perform social control as influence how it is performed by others via appellate policymaking" (p. 9). Assuming the validity of Gillman's paraphrase of Shapiro's arguments, we do not see these arguments as damaging to our operationalizations. That appellate courts make policy and serve as a mechanism for controlling the judicial and legal hierarchies does not in the least

17 And not whether subsequent "judges" can come up with the same counts of the number of cases in each category; a computer does that, with perfect reliability.

18 Each case was actually coded twice on the key substantive variables, and when the judgments of the first and second coders differed, the case was coded a third time by an experienced "resolution coder." At the levels of generality used in our measures, first-coder–second-coder agreement percentages ranged from 70% to 95%. Our actual reliability is still higher, since we used the third or "resolution" codings for our measurement.

mean that they do not also perform conflict resolution and social control as we have operationalized these concepts.

Gillman notes and we admitted that our operationalization of the Philippine Supreme Court's performance of the "routine administration" function does not capture the full meaning embedded in Shapiro's discussion of the roles of courts in administration. Nevertheless, Gillman insists that our interpretation of this function as providing essentially a nonthreatening means of occupying a court that might otherwise be threatening to authoritarian rulers is incorrect, and that our "understanding of the political implications of this role differs from Shapiro's" (p. 9), presumably because supervising a judicial hierarchy is a primary means of insuring central policy control. Gillman seems to ignore our explanation that the kind of hierarchy supervision involved in our measure is not general policy control of lower-court judges but, rather, the discipline of judges, prosecutors, court employees, and attorneys who are alleged to have misbehaved in some way. We therefore believe careful readers will think that our operationalization means what we say it means.

Gillman recognizes that even if everyone agreed that it was "misleading" for us to "say that we are operationalizing three functions that Shapiro ascribes to courts," we could still argue that we had operationalized concepts that were worth investigating. He therefore questions whether one can learn anything interesting by counting cases, without paying attention to their substance.[19] He then tries to convince readers that one cannot by reciting a long litany of instances in which our operationalizations *might* not mean what our operationalizations say they mean. In some cases, these hypothetical circumstances suggest (implicitly) that *other* operational definitions of our key concepts might be appropriate (e.g., whether a court handed down a few appellate cases that made the imposition of social control easier). In some cases, they rest on incorrect assumptions (e.g., about the nature and circumstances of martial law in the Philippines). In some cases, they involve an apparent confusion of the meaning of the functional concepts (e.g., in his contention that when the U.S. Supreme Court handed down criminal procedure decisions in the 1960s that upgraded the rights of criminal defendants, they were less involved in social control because they were less cooperative with police). In some cases, they rest on a simple assertion that "it could be imagined that" or "it is conceivable that" our operationalizations mean something other than what we define them to mean or that they could have changed meanings

[19] Presumably this means without paying attention to their substantive outcomes or, perhaps, to the doctrines pronounced in them. We do in fact pay considerable attention to the content of the cases when we classify them into substantive categories.

over time.[20] In general, our response to all these "it could be's" is "Yes, it *could* be; indeed, we invite others to suggest and create alternative operationalizations that deal with some of these 'it could be's.'"

Gillman even anticipates[21] the response just given but chooses to continue his argument by reasserting the epistemology that denies the possibility of developing transmissible, reproducible generalizations about political behavior. Once again, his arguments appear to posit as the only option a rigid, deterministic social science that cannot accommodate exceptions to cast-iron covering laws. For example, he appears impressed that we note the existence of exceptions to a generalization that we put forward only very tentatively, because we have not tested it.[22] Once again, we are struck by the stereotyping involved in Gillman's discussion of the objectives and procedures of social science, and we do not think it necessary to rehash the argument further.

Exemplars

Gillman graciously cites Tate's "Courts and Crisis Regimes" (hereafter "Courts") (1993) as an exemplar of how we might have better invested the time and effort we expended on "Authoritarianism and the Functions of Courts" (hereafter "Authoritarianism"). We are glad that he found "Courts" insightful and useful, but we reject the effort to label these two products of our intellectual gardens as "good" and "bad" blossoms. We see them, rather, as complementary flowers, each of which adds something to the bouquet of our research on the roles of courts and judges outside the American setting.

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[20] Gillman returns to these "what if things were different" scenarios in later paragraphs, particularly those on pp. 16–17 regarding the independence of the Philippine Supreme Court and how/whether that independence was transformed under Marcos's authoritarianism. Ironically, he seems to think that the mechanisms we describe at the end of our article as possible means through which our empirical results came about somehow do damage to our findings, instead of making them more plausible because more understandable.

[21] On p. 14, in the paragraph beginning "No doubt some behavioralists . . ."

[22] Specifically, he notes that, as an exception to our very tentative generalization that it appears that authoritarian rulers do not usually shut down the courts when they shut down other political institutions, we note that President Fujimori in Peru did displace the regular courts when he staged his *autogolpe*. Why this should strike him as a significant admission of the weakness of our analytical efforts is beyond us, since our generalization was clearly stochastic, in addition to being tentative. Gillman then adds, as a supposedly relevant example, Boris Yeltsin's closing down of the Russian Constitutional Court. Was Boris Yeltsin an authoritarian ruler when he closed down the Constitutional Court? If so, Fujimori and Yeltsin do not "deviate from an otherwise predictable script"; they represent instances in which a stochastic generalization does not hold, possibly because all scientific generalizations must always be stochastic or possibly because our explanation is still incomplete, "misspecified" in the words of the methodologist (see King et al. 1994).

If one assumes that our available research time is fixed as well as finite, it is true that we could have spent the considerable amount of time we invested in "Authoritarianism"[23] on some other, less rigorously defined and executed project. We are even prepared to admit that, had we done so, we might have come up with research that even we found to be as or more worthwhile than that reported in "Authoritarianism."[24] But we doubt it.

Furthermore, we are not at all sure that the "fixed research time" assumption underlying Gillman's criticism is accurate. In the first place, for better or worse, the proposal underlying "Authoritarianism" generated substantial external funding that allowed us to execute not just the research in "Authoritarianism" but to finalize that for "Courts"[25] and several other projects, not all of which have yet been reported. Without the "wasted" effort that produced "Authoritarianism," the effort that produced the exemplar "Courts" would never have been completed. In addition, we are sure that the excitement (and frustration) we experienced as we worked on "Authoritarianism" had a synergistic effect on our other research efforts.

In closing, we would like, briefly and, we hope, constructively, to turn the tables on Gillman by suggesting how a relatively small effort to incorporate more rigorous, reproducible, and quantitative research might have improved a research exemplar for which he is responsible, *The Constitution Beseiged* (Gillman 1993) (hereafter *Constitution*). We found *Constitution* to be extremely well researched, and we think it presents an interesting and provocative "revisionist" interpretation of its topic. To try to avoid letting our debate with Gillman overly influence our assessment of *Constitution*, we take the liberty of characterizing it in the words of a sympathetic reviewer (Schultz 1994:4–5):[26]

[23] And it was considerable, as we tried to please usually sympathetic editors faced with conflicting evaluations from the disparate sets of reviewers recruited to assess this strange flower: quantitative longitudinal research on the behavior of a Philippine judicial institution. Reviewers who knew a lot about our time series methods usually knew little about comparative judicial research and nothing about the Philippines. Reviewers who knew the Philippines usually knew little about judicial research and nothing about time series methods. Reviewers who knew judicial research usually knew little about time series methods and nothing about the Philippines, etc.

[24] Perhaps we should note here that we hope we have not given readers the impression that we think the research reported in "Authoritarianism" is as good as it gets. There are numerous aspects of that research that we would have executed better had available theory and data and our own intellectual limitations allowed us to do so.

[25] The "Courts" research has its origin in a conference paper Tate originally wrote in 1978. However, its revision, its "theory sketch," and its final form are the direct result of the research funded via the "Authoritarianism" proposal.

[26] Schultz identifies himself (American Political Science Association, Law & Courts Section 1992:42) as someone interested in "philosophy of law, constitutional law, constitutional history, and civil rights/liberties" who has "just finished a book on property rights in American democracy," presumably Schultz 1992.

> *Constitution Besieged* is a fascinating and intelligent analysis of . . . what has become known as the [Supreme] Court's *Lochner* Era opinions. . . . [F]ollowing upon Justice Holmes' dissent . . . , many believe . . . [these cases were] "decided upon an economic theory which a large part of the country does not entertain," i.e., laissez faire economics. Yet instead . . . the book connects the logic of decisions like *Lochner* to an overall fear of factions and special legislation that would benefit a specific class or group[,] . . . to earlier police power jurisprudence which emphasized political neutrality and unbiased legislative decision making . . ., [and to] an effort to adjudicate economic legislation based on whether or not the Court felt that the laws were the product of valid enactments for the public good or simply the product of pressure politics meant to favor a particular class or interest. . . .
>
> [T]he crisis of constitutional doctrine during the *Lochner* Era and up to 1937 was the refusal of the Court to accommodate its jurisprudence to the forces of industrialization that rendered invalid the old assumptions about a neutral market of autonomous individuals. . . . [T]he Court tried unsuccessfully to continue to apply the old neutral police power jurisprudence and it struck down certain types of laws when it believed that they were the product of factional politics supportive of a particular class or interest. . . . Eventually, . . . [in the New Deal litigation came] recognition of the dismantling of most of the basic principles of *Lochner* Era jurisprudence and the nineteenth-century legal ideology that supported it. . . .
>
> Gillman . . . . seeks to demonstrate how judges can fashion an apparent neutral constitutional ideology out of certain visions of the market and political power.

We find this review to be both an astute and a fair summary of *Constitution*. We are also in agreement with the reviewer's mild criticisms (ibid., p. 6):

> First, to what extent did the courts in the nineteenth-century use an apparent neutral legal ideology to mask policy preferences, versus to what extent was the ideology so intertwined with other assumptions about power, politics, and the market that the Justices did not realize the biases in their jurisprudence? Gillman appears to argue the latter position, hence painting many of the "conservatives" on the Court as facially neutral. Yet Gillman's claim may be overstated. For example, the *Lochner* judiciary was also the same one that was hostile to civil rights legislation for emancipated slaves, voting rights for women, child labor legislation, income tax, and federal Commerce Clause regulation. All of these decisions suggest more of an explicit political ideology at stake than Gillman seems to suggest. . . .
>
> [A] second criticism of the book is that police power jurisprudence must be looked at in the context of decisions in several other areas of law to see if they are part of a larger legal ideology that [*sic*—than?] Gillman asserts. . . . [T]here is a possi-

> bility either that the ideology Gillman describes was used to support property rights for reasons more akin to traditional understandings [of] *Lochner* assumptions than he asserts, or a different ideology was invoked. Were Gillman to examine the police power in the context of many other legal principles, the claims he makes about neutrality, power, and legal ideology might need to be altered or modified.

Assuming he had wanted to do so, how might Gillman have changed the research in *Constitution* to avoid or blunt these criticisms? One very good way would have been to have included, as a part of his basic research design, one or more quantitative, hypothesis-testing analyses of (a sample of) the Supreme Court's 19th- and early 20th-century decisions, analyses that used well-established and understood methods of judicial behavior research (e.g., multidimensional scaling analyses of judicial voting, systematic content analysis of opinions) to attempt to determine whether the behavior of the justices was more or less consistent with the pattern implied by Gillman's interpretation, as opposed to the alternative patterns suggested by Schultz in his review. It would be particularly easy to determine the relationships, or lack thereof, between the justices' policy preferences in *Lochner*-type economic regulation/police power cases, other types of economics cases, and the types of civil rights and liberties cases Schultz mentioned: the lack of any substantial relationships would support Gillman's interpretation; substantial relationships would document the existence of a more general conservative ideology consistent with Schultz's suggested alternative interpretation.

We are bold enough to think that such analyses could have been conducted within the time budget available to Gillman for the *Constitution* project had he been willing to substitute the recommended quantitative research for what one might see as excessively detailed textual analyses that reassert Gillman's interpretive point in multiple ways without removing the doubts summarized in Schultz's criticisms.[27] Alternatively, if Gillman feels that such a strategy would have weakened *Constitution*, we would be delighted to see him undertake a rigorous corroborating analysis now that the book is completed.

In conclusion, we should stress that in "Authoritarianism," we did not see ourselves in a "us versus them" situation. We did want to encourage the expansion of more rigorous studies of comparative judicial politics, because they had, in our view, been relatively rare. We did not want to rule out or to cut ourselves off

[27] Since neither of us does serious doctrinal or interpretive research, our perceptions might differ from those of someone who does. Nevertheless, for what it is worth, we felt that Gillman's doctrinal analysis not only made his point but made it redundantly and at a greater length than was strictly necessary, though still without addressing evidence that could support doubts about the accuracy of his interpretation. This is the basis for our suggestion that he could have accommodated the quantitative analysis without expanding the research time budget required to complete *Constitution*.

from the insights we might gain from other kinds of research. Gillman's arguments move in the opposite direction, to deny that those practicing other kinds of research can gain any insights from the kind represented in "Authoritarianism" and to threaten to loose the dragons of antiscientism just when we thought they had been safely caged.

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