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TAKING POLITICAL SCIENCE SERIOUSLY

H.W. PERRY, JR.*

One can tell that Professor Merrill spent time in the Office of the Solicitor General. His Childress Lecture¹ resembles a product from the S.G.'s Office: it is intelligent, thorough, careful, and nuanced. It provides guidance on how to think about an issue, but, importantly and refreshingly, it also acknowledges its limitations and is willing to confess error. There is no hiding the ball. Few other institutions or scholars do that so forthrightly. There are many things about his lecture to praise. For example, his argument to show why a simple liberal/conservative distinction does a poor job of capturing differences between Justices, notably Justice Thomas and Justice White, is excellent. Others have criticized a liberal/conservative categorization as too simplistic, especially when critiquing political science scholarship, but they often merely assert the claim. Here, Professor Merrill systematically demonstrates why this is so. Relatedly, he treats the Justices differently, showing that they vary on many dimensions in terms of their multi-faceted goals and behaviors. Again, one might think such a notion obvious, and yet much analysis of the Court, especially by political scientists, often tends to differentiate Justices simply by their preference for outcomes. Additionally, his analysis of Justice Scalia on federalism is provocative and probative, and changes the terms of the debate about Justice Scalia. Similarly, his general notion of a Court being in stasis or in flux is a very helpful way to think about this Court in particular, as well as a promising way to think about future Courts. Much more could be praised. The role of commentator, however, is usually best fulfilled by raising questions and criticisms. That this Essay is so focused should not be misinterpreted. My overall opinion is that the lecture is a tour de force. Unlike Professor Merrill's careful argument, however, I shall luxuriate in the role of commentator and oversimplify, cite sparingly, make broad characterizations, and offer suggestions that are really beyond the scope of his Lecture, with the goal of highlighting the small bit of corrective that I think is needed for the argument.²

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1. Thomas W. Merrill, *The Making of the Second Rehnquist Court: A Preliminary Analysis*, 47 ST. LOUIS U. L.J. 569 (2003).

2. I ask, in advance, for a blanket pardon as I proceed to characterize scholarship, disciplines, trends, and the like with a broad brush while making little effort at documenting my

As a professor of both political science and law, I spend much of my time urging my colleagues in one discipline to take more seriously the work in the other discipline. My reaction, then, to Professor Merrill's earnest attempt to take political science seriously led me to a somewhat ironic reaction: do not take us quite *that* seriously.³ More precisely, be selective in what one imports and "learns" from political science, including the fact that public law scholarship in political science might not be the best source for insight or represent the best of political science.⁴ Apart from that caveat, it is a joy to see a law professor engage the discipline of political science in a serious way. Political scientists are often aghast at the willingness of legal scholars to ignore (or to be content with a superficial knowledge of) scholarship from other areas.⁵ For example, one of the criticisms of critical legal studies by political scientists was not so much the general premises, but rather the superficiality of some of the scholarship.⁶ "Doing" philosophy or serious Marxist analysis requires more than simply being smart and having a passing familiarity with

position, let alone carefully noting exceptions or drawing distinctions. My purpose here is, in fact, to make and report broad characterizations.

3. For this essay, I wear my political science hat.

4. Perhaps a better way of saying it is that while I believe scholarship in the law can benefit greatly from what political scientists know, one would not want to limit oneself to public law scholarship as the only or best source of insights from political science. Additionally, what political scientists "know" is often a subject of much contention.

5. Elsewhere I have taken political scientists to task for ignoring and trivializing what it is law professors, judges, and legal practitioners know. See, e.g., H.W. PERRY, JR., *DECIDING TO DECIDE: AGENDA SETTING IN THE UNITED STATES SUPREME COURT* (1991). That said, I think that most political scientists who study law and legal institutions are far more familiar with the legal literature than many law professors and judges are with the relevant social science literature. Moreover, I would assert that political scientists (or historians or sociologists) are less likely to assume that with a little bit of reading in an area that they are then competent to hold forth. There are, of course, many notable exceptions on both sides. Some of the best political science done on law is done by law professors, and some of the best legal analysis is done by political scientists. To name a couple of examples, political scientist Edward S. Corwin had a few important things to say about constitutional law. See, e.g., 1 *CORWIN ON THE CONSTITUTION: THE FOUNDATIONS OF AMERICAN CONSTITUTIONAL AND POLITICAL THOUGHT, THE POWERS OF CONGRESS, AND THE PRESIDENT'S POWER OF REMOVAL* (Richard Loss ed., 1981); 2 *CORWIN ON THE CONSTITUTION: THE JUDICIARY* (Richard Loss ed., 1987); 3 *CORWIN ON THE CONSTITUTION: ON LIBERTY AGAINST GOVERNMENT* (Richard Loss ed., 1988). Conversely, legal scholar Lucas A. Powe examined the Warren Court in a broad political context that is rarely seen these days in political science. See LUCAS A. POWE, JR., *THE WARREN COURT AND AMERICAN POLITICS* (2000). Powe's book received special notice from the public law section of the American Political Science Association as one of the best books published in public law.

6. Criticism came on many fronts, but it usually took the form of what is interesting is not new. For trenchant commentaries, see Sanford Levinson, *On Critical Legal Studies*, 1989 *DISSENT* 360; Rogers M. Smith, *After Criticism: An Analysis of the Critical Legal Studies Movement*, in *JUDGING THE CONSTITUTION: CRITICAL ESSAYS ON JUDICIAL LAWMAKING* 92 (Michael W. McCann & Gerald L. Houseman eds., 1989).

major writings. Another example is less esoteric. Legal academics and judges opine about the presidency, separation of powers, federalism, representation, and the like with little reference to generations of scholarship on such topics, much of which demonstrates conclusively that things simply do not work, or could not work, in the way that is being portrayed. Perhaps one area where the legal academy itself came to realize that legal training, legal reasoning, intelligence, and familiarity with the American regime were not sufficient to the task was in the initial forays of constitution building in new or emerging democracies.⁷ The importance of such things as electoral structures, political parties, clientelism, civic culture, history, and ethnic and social cleavages, to outcomes—“Poly Sci. 101” for comparative political scientists—was often ignored, but, unfortunately, not with impunity. Later waves of constitution building that took more seriously the knowledge and expertise of political scientists in comparative politics were certainly more productive.

These general observations, however, are not apposite to the work of Professor Merrill. One wishes that all of one’s political science students had as deep a knowledge and understanding of public law scholarship in political science as Professor Merrill does. Not only did he take public law scholarship seriously, but also he clearly understood it, employed it fairly, and has improved and advanced it. Nevertheless, Professor Merrill may have a bit too much enthusiasm for some of our theories and paradigms and perhaps may not fully appreciate how much contention, and, in some cases, disdain, surrounds some of our best known theories and models.

Professor Merrill first turns his attention to the attitudinal model in public law scholarship, which he refers to as “[t]he dominant hypothesis of the political scientists.”⁸ He applies it fairly to the Rehnquist Court, and he is persuasive in demonstrating its inability to explain the phenomena he has observed. Yet many (I dare say most) public law political scientists, let alone political scientists generally, have never taken the attitudinal *model* that seriously. That is not to say that we do not take the attitudes and preferences of Justices seriously, or that we do not take seriously the work of the proponents of the attitudinal model. Indeed, most political scientists would start from the point that any decision maker’s preferred outcome would play an important role in any decision process. Most political scientists, however, would not believe that attitudes are the sole determinant, or that they play as singular a role as propounded by the so-called “attitudinal model.”⁹ It is true

7. See VICKI C. JACKSON & MARK TUSHNET, *COMPARATIVE CONSTITUTIONAL LAW* (1999); Bruce Ackerman, *The New Separation of Powers*, 113 HARV. L. REV. 633 (2000).

8. Merrill, *supra* note 1, at 590.

9. The number of essays and papers on this topic are numerous. See, e.g., *SUPREME COURT DECISION-MAKING: NEW INSTITUTIONALIST APPROACHES* (Cornell W. Clayton & Howard Gillman eds., 1999).

that the attitudinal model has dominated much of the debate in the professional journals. That is, however, in part, a function of how peer reviewed journals work.¹⁰ It is also because the proponents of attitudinal explanations have done rigorous quantitative work, which is what is most valued in the most prestigious political science journals, and quantitative work generally seeks refutation by more quantitative work. Generally speaking, the most vociferous opposition to the attitudinal model has come from non-quantitative political scientists. Therefore, one should not conclude that the model's dominance in some journals makes it the dominant paradigm of public law political scientists. Nevertheless, as previously stated, most political scientists, quantitative or not, would accept that attitudes matter greatly. For at least half a century, we have demonstrated the malleability of the law and have assumed that the law is often manipulated to achieve outcomes that will reflect attitudes or preferences.¹¹ Moreover, most would not believe that differences in outcomes can be explained solely by different philosophies of judicial interpretation, even though there is often a correlation. Legal scholars would probably not disagree that attitudes matter, but I think it is fair to say that the centers of gravity are in different places. Political scientists and lawyers generally do part company when it comes to the degree that law matters, or, more precisely, that preferences matter vis-à-vis the constraining nature of the law and judging.¹² Still, most political scientists would see decision processes as more complex than the attitudinal model suggests. Most political scientists understand that decision making involves multiple and often contradictory

10. The differences between the law review world and the peer reviewed journal world are dramatic. There are only a handful of top professional journals in political science, and there is a strong pecking order among the handful. The rejection rates of the top journals are often well over 90%. Professional journals often reflect (enforce) a particular worldview or a methodological bias. Depending upon one's view, of course, that worldview is often seen as either the cutting edge of the discipline, the well-reflected views of the mainstream of the discipline, or an entrenched paradigm. One can debate the advantages and disadvantages of peer reviewed journals. Legal academics, however, must be aware of the jokes and derision aimed at the legal academy by other disciplines because the primary publishing venue is not peer reviewed. On the other hand, the multitude of good law reviews increases the likelihood that ideas of all sorts will get out and that they will be tested in a broader court of academic opinion. This may be especially important for ideas, paradigms, and methodologies that are not currently in vogue. There are pros and cons to both worlds.

11. See *infra* notes 17-19 and accompanying text (discussing Robert McCloskey's description of *Marbury v. Madison*, 5 U.S. (1 Cranch) 137 (1803)). No attitudinalist, McCloskey, nonetheless saw Marshall's preferences driving the result. Much of the behavioral work of the 1950s-1980s, as well as normative works, assumed the importance of attitudes at some level.

12. This is certainly reinforced by the different training one gets for a J.D. as opposed to a Ph.D.

beliefs and behaviors.¹³ For example, many of us would accept that judges, like any decision maker, are products of their own socialization. That socialization would usually see an attitudinal model of decision making as inappropriate. Indeed, one of the most frequently cited admonitions comes from one of the early public law behavioralists, C. Herman Pritchett, whose work spawned much subsequent public law behavioral scholarship. He argued:

Again, political scientists who have done so much to put the “political” in “political jurisprudence” need to emphasize that it is still “jurisprudence.” It is judging in a political context, but it is still judging; and judging is something different from legislating or administering. Judges make choices, but they are not the “free” choices of congressmen. . . . Any accurate analysis of judicial behavior must have as a major purpose a full clarification of the unique limiting conditions under which judicial policy making proceeds.¹⁴

Whether or not the attitudinal model is the “dominant hypothesis” of political science as Professor Merrill suggests, he is correct to note that it has structured much debate, and he would have been remiss not to have addressed the role of attitudes in explaining the Justices’ behavior. Interestingly, his analysis and conclusions would probably comport with what most political scientists would believe, notwithstanding the impression that one might get from reading the *American Political Science Review*.

Professor Merrill next turns to another area of public law political science: strategic decision making. Before examining his argument, however, an observation that distinguishes between the attitudinal model and the strategic model is in order. The former is idiosyncratic to the public law subfield in political science. Its purpose, after all, is to debunk legal models of decision making. To my knowledge, it has had no influence in other areas of political science.¹⁵ Strategic models of decision making, however, are part of a major

13. See JOHN W. KINGDON, CONGRESSMEN’S VOTING DECISIONS (2d ed. 1981); PERRY, *supra* note 5, at 271-284; JUDITH N. SHKLAR, LEGALISM: LAW, MORALS AND POLITICAL TRIALS, at x (1964).

14. C. Herman Pritchett, *The Development of Judicial Research*, in FRONTIERS OF JUDICIAL RESEARCH 42 (Joel B. Grossman & Joseph Tanenhaus eds., 1969). Professor Merrill notes the Pritchett warning, but his own analysis does not take it or the constraining nature of the law as seriously as do many in political science.

15. In fact, to wash a little dirty laundry in public, a continual frustration of public law scholars generally is how much their work is ignored by the larger discipline of political science. Most political scientists know the work on other institutions and draw from it. Whether or not this one way street is justified, it is fair to say that the best work on how members of Congress, or Presidents, or foreign regimes, or military opponents make decisions is richer and far more multifaceted. See, e.g., ROBERT JERVIS, THE LOGIC OF IMAGES IN INTERNATIONAL RELATIONS (1970); KINGDON, *supra* note 13; RICHARD E. NEUSTADT, PRESIDENTIAL POWER: THE POLITICS OF LEADERSHIP (1960); MARK A. PETERSON, LEGISLATING TOGETHER: THE WHITE HOUSE AND CAPITOL HILL FROM EISENHOWER TO REAGAN (1990).

intellectual movement in political science and other social sciences generally, a point to which I shall return.

Do Justices act strategically? Of course. Are they influenced by both internal and external actors? Undoubtedly. This is accepted even by the most traditional understandings of judicial decision making short of mechanical jurisprudence, and it did not take political scientists to introduce the idea of strategy into judicial decision making. Justices are expected to try to write opinions that will encourage others to join them, while other Justices know how to hold out. That is strategy. Strategic models are not new, though they have found a new resurgence given the rise of rational choice in political science generally in the 1980s. However, it took it awhile to make it to the public law subfield. The key word is resurgence. Strategic understandings of judicial behavior in political science actually predate the attitudinal model. Even the early behavioral work that gave birth to the attitudinal model envisioned strategies for developing coalitions. The most sustained analysis of strategic decision making by Justices, however, came with Walter Murphy's classic *Elements of Judicial Strategy*.¹⁶ The burden that most public law political scientists face is to say much more about Supreme Court decision making than did Walter Murphy almost forty years ago. The title of the book describes its contents, and, while there has undoubtedly been change in the Court as an institution, not to mention the role of government generally and other governmental actors, the book may still be the best single description of Court behavior and potential behaviors. It is full of examples of strategic actions and considerations. He notes both internal strategies and external influences. Strategic accounts of judicial behavior even precede Murphy and the early behavioralists. Though Robert McCloskey's classic work *The American Supreme Court*¹⁷ is not generally seen as a book about strategic decision making, he certainly demonstrates it. Witness his account of *Marbury vs. Madison*.¹⁸ McCloskey, of course, depicts a wily and strategic John Marshall who is fully aware of, and responds to, external constraints, namely Thomas Jefferson.¹⁹ McCloskey's own prescription for the proper role for the Supreme Court in the American system involves strategic calculations about when the Court should insert itself into difficult issues. Despite preoccupation with the attitudinal model in the journals, strategic understandings have never really been abandoned.²⁰ The point of this brief foray into public law

16. WALTER F. MURPHY, *ELEMENTS OF JUDICIAL STRATEGY* (1964).

17. ROBERT G. MCCLOSKEY, *THE AMERICAN SUPREME COURT* (1960). This was the only work that I was assigned to read in both law school classes and political science classes. It would be interesting to note what literature is routinely assigned today in both places.

18. 5 U.S. (1 Cranch) 137 (1803).

19. MCCLOSKEY, *supra* note 17, at 40-44.

20. My book *Deciding to Decide*, though often seen as solely about certiorari, is largely about the interplay of attitudes, strategy, and the perceived constraint of the law. Though the

scholarship history is simply to point out that the understanding of strategic behavior that Professor Merrill is testing is of one sort. His understanding is gleaned from the work of Lee Epstein and Jack Knight, two scholars who are part of this Symposium. It is certainly a logical place to start. The aforementioned burden of saying anything new after Murphy was met by these two scholars. Their book, *The Choices Justices Make*,²¹ pays homage to Murphy, but, more importantly, they have refocused attention of public law scholars on strategic decision making. They build on the scholarship of the attitudinalists, and they also bring to bear insights from the rational choice literature. Epstein and Knight give us concrete examples, as well as more of a theoretical base, of understanding how a strategic decision making process might work in the Supreme Court.

Professor Merrill's lecture demonstrates problems with using internal and external strategic calculations to try to explain too much. His findings seem right. Strategy is important, but not all important. Others, however, will be less persuaded. He will be accused of not really testing strategic explanations. Unlike the attitudinal model, modern strategic analysis is broad-based within the discipline of political science, and it crosses disciplines, including, of course, economics and law. Modeling strategic behavior has become a very sophisticated endeavor, and it is much more theoretically driven. Institutions and rules matter, but the theories of decision making are general, and the unique aspects of an institution simply must be interpreted to fit the more general terms. In short, challenging a strategic explanation is a more formidable task. I dare say that most rational choice scholars would not see Professor Merrill's counterfactuals as much refutation. Rational choice approaches are, by no means, universally accepted by political scientists. Debates about it have been furious and have torn departments and subfields asunder. Professor Merrill's effort actually goes to the heart of much of the debate about rational choice. Oversimplifying, its critics assert that the proponents can assume away any difficulties, particularly in how one defines a preference, and so it is difficult ever to disprove empirically. A related criticism is that a model, though mathematically consistent, is so far removed from reality that it is no longer helpful. Again oversimplifying, defenders assert that even very sophisticated empirical statistical analysis rarely amounts to more than "barefoot empiricism." Scientific knowledge must be theory driven, otherwise all one has is some interesting correlations that might have

context of the book is largely the certiorari process, it is really an argument about decision making by the Justices generally. In the mix, strategy often plays an important role. See PERRY, *supra* note 5, at 113-284.

21. See LEE EPSTEIN & JACK KNIGHT, *THE CHOICES JUSTICES MAKE* (1998).

some predictive ability.²² Theory, here, basically means the theories of economics and the related theories of games and public choice.

The foregoing remarks are not really a criticism of Professor Merrill's effort. Indeed, he acknowledges limitations and notes that his purpose is to encourage political scientists and those more sophisticated in methods to pursue the questions that he has raised.²³ Still, taken on its own terms and without going to rigorous mathematical modeling, there is one problem that is apparent. Most notably, one cannot just look at the strategy of one player. Professor Merrill acknowledges this when he looks at the tit-for-tat scenario, and he does talk about the need to look at what other players are doing. Before he can dismiss strategic explanations, however, he must take into account more fully the strategy and the responses of other players. Likewise, one would need to examine not only the preference ordering of Justices, but the intensity of the preferences. Interestingly, Professor Merrill does this more than much rational choice scholarship, even if he has done it in a more informal way. A strategic analysis would also need to look more at decision options within a set of rules. For example, it would require an analysis of when, how, and who could exercise vetoes, at what cost, and in what ways. One would want to understand the extent to which one is engaged in repeat play. So, for example, it may be incorrect to assume that Justice Scalia would prefer to shape a majority opinion à la Justice Brennan rather than be a powerful dissenter. That strategic choice may depend upon his time horizon. In short, to test and refute or qualify strategic explanations that would satisfy most political scientists, one would need to engage in a slightly more sophisticated analysis, even if not held to the standards of those who do rational choice for a living.

Finally, Professor Merrill's notion of a Court in stasis or flux is a very helpful concept. In fact, it sits well with all sorts of decision theory. A strategy game is definitely influenced by the ability to identify the preferences and strategies of others, and a Court in stasis is going to enable that in ways that a Court in flux will not. In less esoteric terms, some have suggested that one of the reasons for the "failed revolution" of the Burger Court was the

22. Many outside observers have seen the rational choice wars as having been about quantitative versus qualitative research. In fact, the fight was more between empirical statistical research versus theory-driven research. The quantitative vs. qualitative debate has existed since the behavioral revolution, but for the most part quantitative research long ago won that battle in terms of journal publications and prestige in the profession. That said, non-quantitative research continued to be published, especially in books, and now is actually having some resurgence in the journals. Moreover, the second wave of rational choice scholarship often tries to verify empirically its models. Nevertheless, fighting over methodological approaches is congenial in political science departments.

23. See Merrill, *supra* note 1, at 572-73.

inability of the conservatives to predict outcomes on the merits.²⁴ They could get four votes for certiorari, but often miscalculated for the fifth vote on the merits.²⁵ More traditional non-rational choice decision theory also accounts for group composition in understanding collegial decision making. As one example, norms of reciprocity develop and evolve.

Now for my own hobbyhorse. If we truly want to understand decision making behavior by individuals, we might all need to learn more about cognitive decision making processes. We know from cognitive psychologists that it is a very complex phenomenon. Political science undoubtedly could learn much from cognitive psychology. This, however, is the dilemma of all academic research. There is always more that we need to know, and it is impossible to keep up with one's own discipline, let alone others. That is why Professor Merrill's attempt here is so impressive and why one hopes that political scientists will take up his challenge. Even if they do not, however, from his cogent analysis, we have a far better understanding of this Court, and we also have a new and better understanding of the problems and possibilities of public law political science when trying to explain the behavior of Justices.

24. VINCENT BLASI, *THE BURGER COURT: THE COUNTER-REVOLUTION THAT WASN'T* (1983).

25. PERRY, *supra* note 5, at 198-212.

