

CONTINGENCY V. STRUCTURES IN EXPLAINING JUDICIAL BEHAVIOR

A COMMENT ON STUART CHINN'S
*RACE, THE SUPREME COURT, AND THE JUDICIAL-
INSTITUTIONAL INTEREST IN STABILITY*

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Stuart Chinn has written an interesting – and truly informative – article on the role that the United States Supreme Court has played in stabilizing the American socio-political order following periods of transformation. Almost inevitably, he suggests, the transformations are less extensive and go less deeply than their proponents might have wished, not least because the Court, for a variety of reasons, attempts to integrate these transformational changes into an existing status quo in order to produce minimal disruption. Chinn, who is a political scientist as well as lawyer, is interested in explaining, as set out in the very first sentence, “[w]hat factors influence judicial behavior.” That is, it is not enough simply to describe what the Court has done. Chinn, and the rest of us, are curious as to *why* they behaved as they did (which implies, among other things, that there might have been alternatives).

After canvassing a variety of explanations, including one proffered by Jack Balkin and myself that focuses on the “partisan en-

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trenchment” of judges committed to one or another of the great “high political” views of what the Constitution means and what role courts should play in protecting that meaning, Chinn offers his own candidate, which is the “institutional interest” possessed by the Court in “stability.” “[I]n the particular context of post-reform periods, the Court has been inclined at these moments to stabilize, delineate, and clarify the boundaries between competing governing authorities and competing sets of rights within the recently-transformed policy domain.” Even more striking (and potentially important) is his “additional claim . . . that this judicial-institutional interest in stability has manifested itself in three specific types or ‘modes’ of adjudication that recur in American constitutional history.” This allows us to see deep patterns in decisions over time, in what may first appear to be quite disparate eras and doctrinal areas, that can be explained, in significant measure, by placing them within the structure that he has identified, i.e., a prior time of significant transformation (initiated by other branches or social movements) that is then “tamed” (my word, not his) to fit into what remains a largely (even if not completely) untransformed legal polity.

Chinn has clearly mastered a great deal of the relevant literatures, in history, political science, and law, and it is an impressive achievement by any measure. My role as a commentator, however, is not simply to offer applause, however merited, but also to indicate any concerns I might have. My major concern is *not* his unwillingness to accept in toto the Balkin-Levinson “partisan entrenchment” thesis; he is certainly fair in describing it and in offering some of his own reservations. Rather, my concern is that Chinn’s own thesis tends to dampen our recognition of the importance of contingency and sheer historical happenstance because of the emphasis on deep structural forces which are seemingly destined to triumph.

Kenneth Scheppsle many years ago emphasized that Congress is a “they,” not an “it.” It’s not only that there are two quite different Houses of Congress, but, equally important, each House is subdivided into lots of smaller institutions and groups, each with its own interests and incentives. And, of course, finally there are the individual members of the House and the Senate, whose interests, con-

tra to Madison's suggestion in Federalist 51, may be quite different from the ostensible interests of "the place," whether because they are hyper-party-loyalists or because they rather desperately wish to be re-elected (or, these days, be hired by K St. lobbying firms). Similarly, even the Supreme Court, with its (usual) nine justices, is also very much a "they"; members of the Court will often disagree both on what "the law" means and, one must assume, also on what counts as the specific institutional interests of the Court at a given moment in time.

One might be most confident about "institutionalist" explanations – and, for that matter, what might be termed standard-form "legalist" explanations – when decisions are unanimous. And institutionalist explanations are often dispositive when, for example, the Court refuses to grant certiorari in cases that are clearly hot potatoes. Or, even if cert. has been granted, one might offer an institutionalist explanation for the majority's actual behavior in a case like *Newdow*, where it almost shamelessly (and, for some, shamefully) invented a wildly implausible theory of standing to avoid having to admit that the Ninth Circuit Court of Appeals was correct in pronouncing "under God" in the Pledge of Allegiance to be unconstitutional. That would undoubtedly have provoked calls for a constitutional amendment, as well as, perhaps more importantly, made the Court itself a central focus of the 2004 presidential campaign. One can easily understand why most justices believed that almost certainly would not have served the Court's institutional interests (any more than would the Court's declaring not only that William Marbury deserved his commission as justice of the peace, but also that the Court stood ready to order James Madison to deliver it).

But Chinn, by and large, is not dealing with unanimous opinions, or with the crafty denial of certiorari or the use of what Alexander Bickel famously called the "passive virtues" to avoid institution-threatening hot potatoes. Instead, with some frequency, cases featuring bitter divisions between a five-justice majority and four angry dissenters are also explained by reference to the structural imperatives, and so the obvious question is why the dissenters were so blind to the institutional interests in a way that was not true of the

majority. Perhaps they had a different calculus of “interest”; less plausible for many analysts today is the possibility that they were blithely indifferent to such pragmatic concerns and, instead, devoted themselves, a la a version of Dworkin’s “Hercules,” to articulating what they deemed the single best answer to the question of who actually enjoyed a legal right to a favorable outcome, quite independent of any implications in might have for the institutional position of the Court.

In any event, Chinn writes, altogether accurately, that his findings are not designed to bring pleasure to those who view the Court as a likely partner in “liberal expansion of open-ended dismantling reforms.” This is yet another articulation, using a quite different methodology, of the view that it is basically a “hollow hope” to look to the judiciary if one really wishes transformation. I have no trouble agreeing with much of his “bleak suggestion” about the limits of the judiciary as an agent of change. But I must say that I want to look at other explanations for this reality instead of (or, at the very least, as a complement to) the particular kind of argument that Chinn offers.

Let me suggest, for example, that it is a fundamental error to underestimate the importance of life tenure on the United States Supreme Court, which means, among other things, that the “partisan entrenchment” emphasized by Balkin and myself is a function not only of who wins specific elections, e.g., Ronald Reagan instead of Jimmy Carter, but also of whether the president in question has the opportunity to make appointments that will presumably further his agenda. It is a notorious truth that Jimmy Carter is the only elected (one-term) President in our history to go through a four-year term without having a single opportunity to name someone to the Supreme Court. (One reason for this, a recent biography of William J. Brennan suggests, was Brennan’s basically egoistic desire to remain on the Court. He did suggest around 1978 to his clerks that he was thinking of retiring, but one suspects this was basically designed to elicit the anguished cries of “no, you can’t,” which he certainly received.) Richard Nixon, on the other hand, got to name four members to the Court during his six-year term. Most interest-

ing, in many ways, was FDR, who had no appointments at all during his first term and then a full 8 appointments (including boosting Harlan Fiske Stone to the Chief Justiceship after Hughes retired) in the next seven years. Some of us still believe that Al Gore “really” won the 2000 election, but it was, obviously, George W. Bush who was ultimately able to name two extraordinarily conservative members to the Court. Had Lyndon B. Johnson not been so eager to name his good friend Abe Fortas as Chief Justice (or, for that matter, to put him on the Court in the first place), then there would have been no vacancy for Harry Blackmun to fill. Or think of what might have been had Arthur Goldberg not proved so subject to LBJ’s cajoling him to leave the Court. Similarly, Prof. Yalof has suggested that if Howard Baker had not asked for a night to think it over, he would have joined the Supreme Court instead of William Rehnquist (so memorably identified by the appointing President, Richard Nixon, as “Renchburg” and “that clown” with long sideburns who dressed, according to Nixon, somewhat like a hippie).

I don’t want to argue that whirl is all and contingency is king (or queen). It is surely not the case that presidents could have named just any lawyer to the Court, as manifested in the successful filibuster against Fortas and the defeat of two of Nixon’s nominees to the Court. The structural limitations facing even very strong presidents is a necessary caution against overestimating the power of a given individual. That being said, though, I’m not sure about the strength of a theory that is built on so many 5-4 decisions. (See only the list of cases set out near the beginning of the text [p. 4 of the manuscript]. Chinn is obviously aware of the frequency of “closely-divided Supreme Court votes” on many of these issues, but I’m not sure he pays adequate attention to his own insight. Instead, he is determined to make the case for “larger, structural explanations of judicial behavior.”

What is probably most truly distinctive about Chinn’s thesis, and its greatest contribution, is his emphasis not only on the result of given cases, but also on the doctrinal forms within which they were argued. Most political scientists look only at results – in the confidence, often debatable, that it is easy to discern the meaning of a

particular result for the wider political order – and rarely at the internal logics of argument. Here is where Chinn is most lawyerly, for he believes that what an opinion contains by way of argument is at least as important as the particular result. Theories of partisan entrenchment, for example, help to explain results along a liberal/conservative axis. They do not, in any uncomplicated way, help to explain why the Court would or would not decide to adopt an “originalist” posture or accept or reject legislative history when attempting to discern the meaning of statutes. But Chinn does offer a mode of analysis that purports to explain “how the judicial-institutional interest in stability manifests itself in specific modes of adjudication that recur – in precise order – in the context of post-dismantling periods” (p. 14).

In particular, Chinn locates as “core judicial values” the provision of “settlement, notice, and predictability.” But all of these words are extremely mixed in their specific messages. Consider the notion of “settlement.” Arguably, there are many equilibria that could provide a “settlement” of sorts; more to the point, there are inevitably many such ostensible “settlements” that break down, whether in short order or in the long run. Similarly, “predictability” could be satisfied by practically any stark declaration. Consider, for example, an announcement by a court, perhaps in an opinion written by Justice Lewis Powell, that “hereafter in suits brought by labor unions against management, we will always find for management.” There may be few instances of such crass predictability – and no instances, presumably, of such clearly articulated predictions – but, presumably, repeated instances union defeats and management victories will lead most unions to refrain, at the very least, from filing petitions for certiorari regarding losses below.

That American constitutional development might in fact be more subject to purely contingent forces may require a tempering of what might be termed “structuralist exuberancy.” That, however, does not diminish the contribution of the close readings of many classic cases and their placement within very real political contexts within which judges were almost undoubtedly concerned with the kinds of institutional concerns identified by Chinn. That there might have

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been alternative histories does not allow us to ignore the actual events that occurred, and the extent to which they indeed helped to shape the contours of the overall American political system. □