Path Dependence in Studies of Legal Decision Making*

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Introduction

Judges, law professors, and, yes, even political scientists are creatures of their training and professional socialization. Although we all come to the table with unique concerns, perspectives and insights about legal decision making, those views are inevitably shaped by how we first come to understand the endeavor and the assumptions of relevant colleagues with whom we interact on a daily basis. Legal academics and social scientists are perpetually steeped in the literature of their home disciplines. We constantly think about seminal debates and recent articles of import to frame our own research (or sometimes help our students understand some case or phenomenon from a new perspective). Judges, of course, deal in the primary language of legal argument. They read legal briefs or motions for summary judgment, delving into the language of cases themselves to dissect litigants' arguments and seek doctrinal guidance for their judgments.

It used to be that someone would peek over the disciplinary fence that separates doctrinal from empirical approaches once only every so often to offer a critique, borrow an intuition or organize a conference – but such encounters are becoming more and more frequent in recent years. Indeed, over the past decade political scientists have started annual "workshops" to train legal academics in empirical methods to encourage interdisciplinary exchange. Moreover, the number of articles where legally socialized individuals have criticized empirical studies or called for more attention to legal considerations seems to have increased exponentially. Calls of this type are not necessarily a new development – witness the a series of articles by Wallace Mendelson (a political scientist with a Harvard Law degree) in the early 60's (e.g. Mendelson 1963, 1964, 1966) – but the number and frequency of such commentaries seems to have spiked in the past ten years (See for instance, Cross 1997, Edwards 1998, Cross, Heise and Sisk 2002, Friedman 2005, Shapiro 2009, Tamanana 2009).

Ironically, although all of the more recent commentaries raise issues of their own, there are several reoccurring critiques are quite similar to those mentioned by Mendelson over 40 years ago.[1] This is true for at least two reasons. First, after raising early concerns about the quantitative study of judicial

[1] Commonly occurring critiques include the fact that empirical studies fail to take the complexity of legal decision making into account by focusing on case outcomes (Mendelson 1963; Friedman 2005, 266-8) and assuming cases involve a single "dominant" issue in scaling analysis (Mendelson 1963; Tannenhaus, 1966). Closely related are concerns that have been raised about subjectivity in data collection and the classification of cases into one issues area or another in order to perform quantitative analyses (Mendelson 1963; Tannenhaus 1966; Cross, Sisk Heise 2002; Freidman 2005 270-2, Shapiro 2009).

behavior -- there was no sustained interest from legal scholars in "following up" on such critiques. Judges and legal academics seemed to view empirical work as the product of number crunchers who did not "understand" legal decision processes. As such they were largely dismissive of quantitative approaches to understanding legal decision making.[2] Second, empiricists went about their business without caring much what legal types had to say. Political scientists involved in behavioral research took pride in studies tending to demonstrate the influence of extra- legal factors in decisional behavior. They pointed to findings in studies by Prichett (1948), Schubert (1962) and Speath (1961, 1963, 1964) as evidence that doctrinal reasons judges gave for their case votes were not as useful as ideological factors in explaining such decisions. Theories of judicial behavior invoking political ideology and attitudinal factors were certainly more parsimonious than traditional accounts of legal reasoning. Moreover, they were easier to quantify and test. [3]

Thus, two disciplines that are interested in similar phenomena have existed in a state of "mutual indifference" if not outright distain for the past half century or so. [4] In that time each has evolved in terms of theory and conceptualization of concepts related to legal decision making. Perhaps inevitably a "division of labor" has developed based on what some have characterized as the "comparative advantage" of each approach (Ford 2006 citing Swisher, et al. 1946 APSA report on directions for research in Public Law). Most legal academics continue to explore doctrine though esoteric rules of legal decision making.

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[2] In an attempt to "raise discussion to a higher level than…prevail[ed]," in 1964 Tanenhaus anecdotally noted "broadside attacks on [the] quantitative investigation" of legal decision making. Some of the more colorful sentiments he attributed to lawyers' characterizations of empirical work at the time include, "thinkers don't count and counters don't think" and "figures don't lie, but liars do figure" (1964, 505).

[3] There were instances where individual researchers responded to concerns raised by Professor Mendelson in particular studies (ex. Spaeth 1966). But the exchanges that occurred in scholarly journals did not do much to change this trend in thinking. Indeed, such exchanges tended to validate the usefulness of quantitative approaches for many social scientists notwithstanding the concerns raised by scholars who were more doctrinally inclined.

[4] One could point to instances where each discipline has been somewhat patronizing of the other in important respects. There are legal academics (most recently Tamanaha 2009) who argue that many political scientists who do work on the court are "agenda driven" and have overcharacterized evidence of extra legal factors in decision making based, at least in part, on a fundamental misunderstanding of the way legal reasoning works. Just as troubling for the prospect of meaningful and respectful interdisciplinary exchange is the critical stance some political scientists have adopted that legal doctrine matters little in how courts, particularly the Supreme Court (see e.g., Segal and Spaeth 1993, 2002), make decisions. The implication is that judges and legal academics who analyze doctrine to discern how that court, and perhaps others, will decide future cases are at best, wasting their time and, at worst, diluting themselves if they believe the law actually matters in how judges make decisions.

They are primarily interested in normative questions and how distinct jurisprudential approaches converge on desirable outcomes. Text, intent, precedent and the persuasive force of the principles that underlie them are the primary tools of their trade. Behavioral scholars on the other hand, deal in the currency of testable propositions. They are primarily concerned with data, hypotheses and effective operationalization. Their comparative advantage has been the use of quantitative methods, statistical techniques and more recently formal models to theorize about and empirically demonstrate factors significant in determining outcomes. After a half century of such research behavioral scholars have developed their own notions of how judges make decisions that are at odds with formalistic portrayals of legal reasoning and also quite distinct from how most legal scholars conceive of extra legal influence in terms of mechanism and degree (Braman 2009).

Of course such "comparative advantages" are subject to change, evolve and diminish. This is especially true in times like this when "interdisciplinary efforts" are more encouraged than frowned upon (Martinek 2009). There is no reason legal academics cannot master statistical techniques and test their own models. Indeed, there are some excellent examples of such work in recent years (Cross 2007, 2009; Lindquist and Cross 2005; Czarnezki and Ford 2006). Also the number of behavioral scholars with formal legal training has significantly increased in recent years (see, Posner 2008 citing specific examples). This means that the easiest critiques levied across interdisciplinary lines -- that researchers don't "understand" statistical logic or decision making norms – are becoming less available.

We believe that greater attention to the assumptions, approaches and findings of researchers across the disciplinary divide is a very positive development that has significant potential to improve our understanding of the factors influencing legal outcomes ***and*** the normative implications of such knowledge. And it is worth stating that we think that this sort of interaction is not only important because it is currently en vogue – fundamentally it is the best chance we have of getting things "right." Behavioral scholars need to take the *content of law* and how the decision makers they are studying *actually think about cases* more seriously. Legal academics cannot exist in the heady domain of normative theory and "doctrine in the abstract" without greater attention to the political considerations and extra legal factors that influence judges in real-word cases. The worlds of "how things are" and how they "should be" need to converge if we are to have a well rounded understanding of this institution of law we all purport to care so much about.

This sort of interdisciplinary exchange is bound to fail, however, if legal and behavioral scholars do not come to the table in the spirit of mutual respect with a sincere openness and appreciation for what they have to learn from one another. At bottom, this means not only an appreciation of the methods and insights scholars might borrow across disciplinary lines to aid their own research in isolated instances, but a critical assessment of what knowledge and findings accumulated across disciplinary borders means in terms of our own assumptions and the very the questions we are asking. This involves the difficult task of acknowledging shortfalls and limitations in our own understandings that might be illuminated by the insights of others with different perspectives. This is perhaps the most painful part of interdisciplinary exchange – but it is only though the exposure of our most fundamental flaws that real progress can be made in such respects.

An important corollary is that ***just offering criticism across disciplinary lines is not enough to advance our understandings of normative and empirical phenomena related to legal decision making.*** Indeed, at this point in our collective knowledge one could argue that raw criticism without constructive suggestions as to how to do things "better" is somewhat counterproductive. For interdisciplinary exchange to ***move forward*** concrete suggestions about how to conceptualize empirical findings in the context existing normative theory are fundamentally important. On the other side, legal scholars need to advise behavioral researchers about how to empirically capture aspects of legal approaches they see as missing from our models. Clearly it is time to move past the idea that legal decisions are determined by "wholly legal" or "wholly attitudinal" factors toward a more complex understanding of how such considerations *interact* in the minds of judges. Greater interdisciplinary exchange should help us build a research agenda enabling us to do that.

At its best interdisciplinary work should not look like "business as usual" in law or political science. Scholars that work at the intersection of these fields need to take findings and assumptions from each domain seriously to develop projects that contribute significantly to knowledge in both disciplines. This might seem like a tall order, but in some instances it could be that scholars just have to be more explicit about what hypotheses and findings mean for how we should think about broader normative questions of democracy and/or conceptions of judicial authority. For scholars that deal primarily with legal doctrine it could mean more explicit acknowledgment that the lines of argument they are developing through statutes, case law and principle will likely operate with differential force depending on factors

such as the level of court they are dealing with and/or the extent to which decision makers agree with the desirability of outcomes and contested principles they are utilizing in making such arguments.

While we see this sort of attention to the concerns and implications of findings across disciplines as a step in the right direction – it is not all that is necessary for meaningful and sustained interaction. And so it is time to "get into the weeds" and offer some of the sort of concrete suggestions we advocate above. In doing so, we acknowledge that understandings in both fields have developed more or less independently in path dependent ways over the past half century. Many political scientists employ the concept of path dependency for understanding the historical development of political institutions and processes – but the idea can easily be applied as a framework for understanding the development of a scholarly literature over time.

At a very broad level, path dependence means "that what happened at an earlier point in time will affect the possible outcomes of a sequence of events occurring at a later point in time" (Sewell 1996, 262-63; quoted in Pierson 2004, 20). Most political scientists have preferred a somewhat narrower and more precise definition: "[P]ath dependence refers to dynamic processes involving positive feedback, which generate multiple possible outcomes depending on a particular sequence in which events unfold" (Pierson 2004, 20; citing Arthur 1994; David 2000). This latter understanding of path dependence focuses on "self-reinforcing or positive feedback processes . . . that reinforce the recurrence of a particular pattern into the future" (Pierson and Skocpol 2002, 699). Positive reinforcing feedback at different points of development not only push us further along the path chosen, but it makes "the costs of switching to some previously plausible alternative to rise" (Pierson 2004, 21); thus the "'roads not chosen' may become increasingly distant, increasingly unreachable alternatives." Timing, sequence, context and conjunctures are all central to understanding the slow moving causal processes that lead to certain social and political conditions or outcomes (see Pierson and Skocpol 2002, 699-704).

Political scientists typically use path dependency frameworks for understanding how certain institutional arrangements came to be and what the consequences of those arrangements are for political and policy outputs, often with normative reference points. Unfortunately, our scholarship can be prone to path dependent processes as well. Each fork in the road presents us with epistemological, theoretical, methodological and conceptual choices and each of these choices occurs in a specific temporal context. When a particular choice enjoys positive feedback from peers, reviewers, editors and others in the field, the path chosen is reinforced and we move further in that particular direction.

There are certainly strong intellectual arguments for proceeding in this manner. Social scientists, for example, endeavor to produce knowledge incrementally by building on what we know within an accepted research paradigm in the name of "normal science" (see e.g., Farr 1988; Kuhn 1962). Therefore, the reference point for any piece of scholarship is usually going to be that which came before it in a specific disciplinary field or subfield – until or unless a paradigmatic shift occurs. But on the other hand, as scholars trod along a given path, they have a responsibility to be self reflective, vigiliant and deliberative before choosing to follow the path further. As we discuss below in specific contexts, intellectual laziness can result in improperly transporting concepts form one research project to the next, blindly accepting assumptions from previous research despite a lack of empirical support for those assumptions, or using improper measurements or operationalizations from existing datasets, among many other pitfalls.

Additionally, there are many professional factors that can serve as positive reinforcement for pushing a line of research further along a given path. The incentives for young scholars' professional development and advancement revolve round publication of their research. Editors of presitigious journals and presses often show predilections toward research with particular theoretical, methodological or ideological orientations. Where journals are refereed, manuscript reviewers tend toward approving work within a research tradition with which they are most familiar. To the extent that editors choose reviewers from a mainstream or favored research orientation, that work is most likely to be published. The same holds true for publishing with reputable book publishers, such as university presses, as well as prestigious grants and fellowships. For academics for whom tenure, promotion, salary and hiring are dependent on publishing – especially in certain outlets - the message is clear. Thus, the reward structure of the academy may serve to stymie innovation and promote path dependent research with self reflection and vigilance.

In the case of judicial decision making literature, then, much of the failure to cross disciplinary lines is in some part a result of the path dependent nature of our scholarship. For the last several decades, the most reputable political science journals have reinforced a certain approach to understanding judicial decision making, one rooted in the attitudinal model and which most commonly involves large-N quantitative empirical analysis or game theoretical approaches to understanding how so-called attitudes are or are not constrained (APSR centennial issue and Kristen Monroe's edited volume on Perestrokia.) On the other hand, the most common route to publication and career advancement in the legal academy

involves doctrinal analysis that is rooted in a particular judicial decisions or set of decisions – analysis that attacks the legitimacy or application of certain interpretive methods, or understandings of precedent.

We also think a key aspect of social science research that gets left in the dust as scholars proceed down a particular path is that of conceptualization and concepts. The problem seems to have been especially acute in quantitative studies that focus more on operationalization and measurement issues.[5] Indeed, today it seems that political scientists who are associated with qualitative methodology may be paying closer attention to conceptualization and concepts than quantitative methodologists.

Goertz has provided the most careful guide to concept construction, arguing that we must understand that concept are "causal, ontological and realist" (2006, 5). Goertz and Mazur provide ten guidelines for "creating, evaluating and modifying concepts that are applicable to concepts in general" (2008, 15). While a complete review of those guidelines is unnecessary here, we highlight just a few that are particularly relevant for our inquiry. One important consideration in developing and using concepts is that of causal relationships within and between concepts. Another involves the accepted name of the concept and whether that name is used in a consistent manner semantically among those in the field. Moreover, must ask whether the concept necessarily consists of multiple dimensions, and if so, explicate the interdependence between dimensions. And lastly, it is crucial to consider whether the concept "travels" well across time or other contexts. This last point is particularly important when considering the path dependent nature of research, as scholars attempt to extend and expand previously formulated concepts to new aspects of a research agenda.[6] The further we progress down a path, and the more difficult it is to reverse course, the more dangerous it is to rely on concepts that might have been faulty to begin with or which have been extended in questionable ways. Therefore, the importance of evaluating how we conceptualize concepts cannot be understated.

Mertz (2008) specifically discusses the task of "translation" between law and social science. We think this is a useful concept as sometimes scholars across our disciplines talk about similar concepts in very different ways. As we begin this sort of exchange it is important to consider how concepts that pass

[5] See generally Goertz 2006, 2-3, and Goertz and Mazur 2008 14-15, noting that even the efforts of King, Koehane and Verba to impose more rigorous, quantitative-type standards on qualitative methodology had little to say about "concept methodology." See also Sartori 1970.

[6] For an example of this type of path dependent extension of a concept in the comparative context, see Collier (1995).

in our own field may "play" across disciplinary lines. This is especially true as empiricists interested in measurement and operationalization can fail to capture the "essence" of what legal scholars see as relevant and/or important in our analyses. Similarly, in developing lines of authority that lead outcomes that they portray as desirable, doctrinal scholars can treat legal goals and democratic principles as absolutes that political scientists understand are contestable. From this perspective decision makers' susceptibility to legal arguments may have less to do with the authoritative force of the doctrine that scholars cite as the extent to which decision makers agree with desirability of such principles, or more cynically, the extent to which those arguments help them reach specific policy outcomes in particular instances.[7]

As we are both political scientists we proceed in the remainder of this paper with a critical discussion of specific empirical measures and conceptualizations that we believe would benefit from greater interdisciplinary attention. Specifically, we mention two areas of research: the conceptualization of concepts related to legal decision making and those involving interactions between judicial and legislative actors. We also engage in a candid discussion of what findings from our own discipline, which has admittedly avoided explicit normative discussions, has to contribute to debates about judicial authority in the context of larger democratic theory.

In doing so, we acknowledge that the trajectory of thinking and empirical research on legal decision making in the past century has been shaped by more realistic understandings of the influences on

[7] This is not to say that legal scholars do not acknowledge democratic principles are contested. Indeed this is the life's blood of our adversarial process. Moreover, there is much more diversity in conceptions of what is "desirable" in legal academia than in other disciplines, like say, economics. Our larger point here is that building doctrinal theory and argument often involves arguing that some principles should be elevated over others in specific domains. Consider, for instance, debates about intellectual property law. Is the primary goal of patent and copyright law to encourage innovation or to ensure that those who engage in creative endeavors receive due compensation? Obviously, these goals are not mutually exclusive and the original purpose of such provisions was to ensure one desirable outcome (innovative creativity) through the other (exclusive economic rights). But as doctrine develops and case scenarios become more specific there are unquestionably instances where the two are in conflict, perhaps most visibly in recent debates about the "fair use" doctrine. As a result there are competing legal arguments about how cases should be decided depending on which goal scholars see as most important (see, Cohen 2007 for discussion). Legal scholars on each side make doctrinal and principled arguments about how to achieve "desirable" outcomes in quite different respects depending on which goal they see as essential to the development of sound doctrine. The findings of political scientists suggest that the persuasiveness of such arguments has as much to do with the extent to which *decision makers' agree* about how goals should be ordered as anything else. Legal scholars tend to discount such considerations focusing on the persuasive "force" of their arguments without considering characteristics judges as the receivers of persuasive messages.

judges' decisional behavior. Current scholarship in political science ***and*** legal academia challenge conceptions of "legal formalism" in its strictest sense. However, neither political scientists nor legal theorists have sufficiently rectified these more nuanced understandings of legal behavior with a democratic theory that requires constraint of judicial actors in terms of the most basic functions they perform in our government system. Although legal scholars have attempted to create a jurisprudence that speaks to such concerns, these approaches fail to *address the range of what judges do in our constitutional democracy.* Specifically we observe that in the absence of meaningful constraint the promises of fairness, predictability and equality before the law are severely compromised and the legitimacy of judges' authority to make distributive outcomes is rightly called into question. Ironically, political scientists who are supposed to care deeply about authority and "who gets what, when and how" have been largely content with demonstrating the influence of extralegal factors in decision making without dealing with larger theoretical implications of their findings.

We offer these observations not to criticize how approaches in each discipline have evolved over the years, but to highlight points of similarity, points of departure and areas of opportunity where interdisciplinary exchange can improve our studies and theorizing so that we can move forward in a truly productive fashion.

Operationalization of Concepts Related to Decision Making

Most of the critiques levied against empirical scholars by judges and law professors involve the way behavioralists conceptualize and measure concepts related to legal decision making. As referenced above, the tone of commentaries on quantitative work across the disciplines varies. Some scholars remain incredulous, unconvinced empirical approaches capture anything useful or different than what we could find out from asking judges how they make decisions (Edwards 1998; Tamanaha 2009); others seem sincerely interested in detailing what empirical scholars should do to make studies more relevant and/or credible in the eyes of those with legal backgrounds (Friedman 2005; Shaprio 2009); still others have made comments in the context of law review symposia or scholarly exchange with quantitative scholars about what such approaches have to add to our knowledge about legal institutions (ex. Cross, Heise and Sisk 2002 exchange with Epstein and King 2002).

All of these "perspectives from the outside" are useful. If nothing else they give empirical scholars an idea of how their questions, methods and findings are perceived by those with different

training and operating assumptions about how the law works. At this juncture however, we think it may be useful to engage in this sort of critical assessment from the "inside," taking account of what judges and legal academics have said about empirical work. We do so with a full appreciation of the operating assumptions in our own discipline and a meaningful understanding of the benefits and difficulties attendant in empirical research.

At the outset we would like to clear up what is perhaps a widely held (if less widely expressed) misconception about empirical scholars: that we don't care about our operationalization of concepts related to legal decision making beyond the fact that they are quantifiable, or can be measured across a wide array of cases so we can include them in our analyses. This is not true. Empirical scholars understand better than anyone that our findings are only as good as our measures. Early in our training we are introduced to the concept of "construct validity," referring to how well measures of particular variables "capture" phenomena of interest. Indeed, one might observe that effective operationalization is the *sine qua non* of excellent social science research – not significant findings. The most successful scholars understand this. The rest of us are constantly reminded of the importance of careful operationalization though the exercise of peer review (although, as detailed further below this can be a double edged sword in terms of creative innovation in the conceptualization of concepts).

Admittedly social scientists have not been perfect in translating concepts relevant to legal decision making into our models, but it is not for lack of effort. Those involved in empirical research take great care in developing and improving the independent variables that we theorize are related to judicial behavior. This can be seen in numerous iterative studies aiming to refine some of our most central variables including ideology (Segal and Cover 1989, Baum 1989, Segal, Epstien, Cameron and Spaeth 1995, Martin and Quinn 2002) and case salience (Slotnick 1978; Maltzman and Wahlbeck 1996, Epstien and Knight 1998; Wahlbeck, Spriggs and Maltzman 1998; Epstien and Segal 2000). Judicial politics scholars are also engaged in the constant assessment and reassessment of the state of our knowledge in periodic review articles and book chapters (see for example, Dixon 1971; Tate 1983, Shapiro 1991; Whittington, Keleman and Caldeira 2008; Segal 2008). These reviews typically include critical evaluations of how we are capturing concepts and how we may strive to improve our measures.

That said, we would also like to acknowledge that behavioral scholars have been narrow minded in thinking about concepts related to decision making in ways that are closely related to the issue of path dependence in the discipline. For all our efforts to measure and capture the role of "political ideology" in

studies of judicial behavior there has been much less sustained attention to the operationalization of legal considerations in judges' choices. Rather the force of law is often conceived as a "constraint in the abstract." The assumption in many empirical studies is that if law is operating to constrain legal choices, systematic differences observed in the voting behavior of judges should not obtain. One may question whether this conceptualization of law as the "null hypothesis" is justified in large N studies and if there may not be better ways to test for its influence (see, for example Braman 2010, for alternative suggestions about how to conceptualize legal constraint in empirical research).

Another issue is the predominance of large N studies, themselves, in studies of judicial behavior. In these studies political scientists typically utilize widely available data sets (such as Speath's Supreme Court dataset or Songer's Court of Appeals dataset) to investigate the influence of their variable of choice on the expression of ideology in judges' case votes. While we fully acknowledge the significant contribution of the scholars (and research assistants!) who provide such data, often with the help of NSF grants which tend to highlight rather than detract from the worth of such efforts, we are concerned by the hegemonic status of this sort of research in the discipline.[8] First, there are many choices judges make in the process of thinking about cases that may more closely reflect particular aspects of legal reasoning (like attention to precedent or statutory construction). For instance, one might ask how judges conclude a particular line of doctrine is determinative in cases involving multiple issues, or why a specific case or statute is deemed authoritative in regard to a particular litigant or set of case facts.

Yet the easy availability of data makes case votes the primary dependent variable of choice in quantitative studies. This is true even though case facts can (and do) differ substantially across cases used in empirical analyses. Such differences are assumed to "cancel out" in the aggregate. Although many legal scholars discount empirical work for exactly this reason – we note that this is not a fatally unrealistic assumption. The strong and constant influence of ideology in judicial voting behavior cannot be summarily dismissed. Clearly ideology "matters" in judges' outcome choices. Still we know very little about *why this is true*. Looking at decisions other than case votes may help to shed light on how judges are able to reach conclusions consistent with their preferences in the context of thinking through legal

[8] Note: we leave aside the issue about whether such heavy reliance on case coding by a single scholar or research team is a good idea, although the question has been raised with respect to the Spaeth dataset. See specifically, Shapiro 2009, arguing that variables of particular interest to legal scholars may be miscoded, undervalued and/or ignored in existing coding schemes; and Harvey 2009 arguing there may be bias in coding schemes.

authority. Moreover, this sort of inquiry could lead us to a more sophisticated understanding of where the law can act as a meaningful constraint on legal decision makers and where attitudinal factors are most likely to hold sway.

There are variables in large data sets that could be utilized to start investigating these questions, such as those indicating the number and types of issues raised in litigation and which lines of authority particular judges deemed determinative in opinions they wrote or endorsed. But these variables are largely underutilized – and hardly ever explored as *dependent* variables. [9] Perhaps more problematic, are the subtle ways in which reliance on available large data sets confine our thinking and hypothesis testing. For instance, we have all read studies where researchers confine their analyses to periods for which data are "available," without sufficient attention to the theoretical justifications for limiting or "cutting off" studies at seemingly arbitrary points in time because the scholars providing such data are not as current as ideas motivating the scholarship. There is inevitably going to be a lag between when cases are decided and when variables are coded. Moreover, updated datasets need to be checked for errors before they are made available for mass consumption. Unfortunately the tendency of many empirical scholars is to provide their analyses for "all" available data or cases that are most current. Usually it would be better for scholars to *think about reasons* to limit their studies to a subset of dates for which data are available rather than utilizing arbitrary cut-off dates without sufficient theoretical justification. Of course, researchers *could* code variables of interest in cases themselves to include more recent court decisions in their analyses where it is justified, but as a practical matter most do not, further illustrating the problem of such heavy reliance on these datasets.

Intimately related to such concerns is the idea that prevalence of large N statistical studies and widely accepted operationalizations inhibit scholars from thinking creatively about other ways to capture phenomena related to legal decision making. Arguably there are other empirical techniques that could advance our understanding of factors relevant in legal decision making behavior. Experimental methods seem an especially promising avenue to investigate biases and constraint in normative decision making

[9] To their credit, however, empirical scholars are thinking harder about the relationship between substantive and procedural issues in decision making (see, Baum 1977 for classic study of use of discretionary jurisdiction by California Supreme Court to hear and avoid criminal cases consistent with their preferences and Goelzhauzer2009 for more recent take on justiciablity issues as gate-keeping measure by the U.S. Supreme Court).

though they have not been widely utilized except by a handful of scholars doing work at the intersection of law and psychology (see for example, Guthrie, Rachlinski, Wistrich 2001; Wistrich, Guthrie and Rachlinski 2005; Holyoak and Simon 1999; Braman 2006; Braman and Nelson 2007; Ferguson, Babcock and Shane 2008). There are also examples of scholars using content analyses techniques that significantly shed light on group and individual decision making processes on collegial bodies like the U.S. Supreme Court (Tetlock, Bernzweig and Gallant 1985; Gruenfeld 1995). Obviously each of these alternative approaches has its own difficulties attendant to data collection, the operationalization of concepts related to legal reasoning and the extent to which findings can be generalized to different contexts. But the significant effort entailed in using and justifying alternative methods of inquiry may often be warranted where they can get us "closer" to phenomena of interest in understanding legal decision making. Obviously theoretical considerations should drive scholars' choices regarding their methods of inquiry; still, we think as a general rule behavioral scholars could be more creative in thinking about the data and methodology appropriate for particular questions.

Also, we observe that there is an overreliance on the specific operationalization of variables related to decision making (usually developed by prominent figures in the discipline) without thinking about how concepts may be "better captured" in specific empirical studies or whether or not the inclusion of such variables is theoretically appropriate. Fundamentally, a large part of this problem is driven by the difficulties of getting scholarly work through the peer review process.[10] The fact is it is really hard to get new or "different" conceptualizations through the process. Once particular measures are used in studies that have been published they take on a degree of "legitimacy" that is more and less warranted across variables. Setting aside this issue (for now) there are certainly benefits to using common measures across studies. The availability of such variables cuts down on the work of empirical scholars because it reduces the need for primary coding and data collection. Moreover, it is often useful to have common measures used across studies because it allows for the effective comparison of concepts across different courts and/or decision making contexts. In these respects scholars who take the time to develop and test such operationalizations are performing a service to the discipline that we in no way mean to diminish.

[10] We are by no means arguing that we should do away with the peer review. We see it as an (often painful but) essential part of the accumulation of knowledge in our discipline. But there are certain "externalities" attendant in the practice it is worthwhile to make clear.

Our larger concern is with the utilization of existing variables in empirical research. No matter how widely utilized or "accepted" a particular measure is it is always important to consider how the variable captures the construct of interest and whether or not there may be more appropriate ways to do so in the context of the particular questions empirical scholars are investigating. In essence we are concerned that the availability of such measures fosters a degree of "thoughtlessness" in our own discipline, much like the "thoughtlessness" Daniel Lazare (1996) and Stanford Levinson (2008) have identified with respect to citizens blind reverence for the Constitution. They may inhibit us from thinking about how to do things more appropriately, especially as our ability to capture phenomena of interest in develops and the questions we are asking related to legal decision making evolve and change.

Compounding this difficulty is the defensive stance empirical scholars, particularly those early in their training and socialization, often take in presenting and submitting work for publication. The tendency is to control for every possible factor discussants and/or anonymous reviewers might be concerned about. There is much less attention to how variables are captured than the fact that they are included in the model. Quite often researchers include available measures as controls without sufficient theoretical justification or any attention to how these concepts are conceptualized other than parenthetical cites to published studies where they have been used before as a "proof" of their validity. This is a problem. Somewhat ironically, the best hope we have of correcting these tendencies is through the sort rigorous peer review that gives rise to them in the first place. But admittedly scholars have different "tolerance" for this sort of behavior and for many reviewers parenthetical cites might serve as heuristic cues that measures are adequate even where they would take issue with these variables in the context of particular inquiries if they were given more information about how they were captured.

Another area where we need much more creative thought and innovation is in tightening our conceptualizations ***and*** operationalizations of how legal text affects decision making. The so-called "legal model" has most commonly been conceptualized and operationalized by quantitative-minded political scientists in terms of *stare decisis* and precedent (Spaeth and Segal 1999). This has led to more sophisticated empirical explorations of citation patterns (Spriggs, Wahlbeck and Johnson 200x). While we do not question the sincerity of the researchers or the importance of understanding the role of precedent, the focus on precedent and citation seems to us one borne partially out of the convenience of available variables in large-N datsets and other electronic resources, such as *Shepards citations*.

It seems to us more central concept and analytical starting point for understanding "law" in legal decision making involves legal text. The battle in the court, especially appellate courts, is almost always over what a particular text means. And this seems a fruitful endeavor for interdisciplinary collaboration between law professors, who believe and write about how legal text matters (or should matter) in particular cases, and political scientists trained to test such propositions systematically. Scholars could identify similar statutes the text of which varies. A very simple example would be to identify statutes that have been the subject of litigation that declare an executive official *may* take a certain action and others that direct that an executive official *shall* take a particular action. One could easily code for these types of differences in semantics then analyze judicial behavior in adjudication involving these different statutes. Moreover, we should be able to devise ways to determine legal clarity beyond the rather crude conceptualization that complex cases are those with multiple legal provisions. One might anticipate that the clearer the text of a statute, the less ideology or preferences would matter and the more unified judges on a collegial bench would be. One possibility is might be to conduct experiments or quasi experiments with carefully chosen subjects who are proficient in the English language to determine their views on the clarity of the meaning of particular statutory language and compare the results with judges' votes in cases applying those statutory provisions; if the clarity of legal text matters, we would expect judges to be more unified in the application of statutes the clearer the text of the law (see e.g., Pickerill 2009).

Finally, sometimes the way particular measures are developed can influence their broader use and theoretical relevance for scholars interested in legal decision making from different disciplinary perspectives. Here we borrow from Friedman's insight that there are widely asked questions "where those outside the subfield of law and courts would be justified in scratching their heads and wondering precisely why anyone cared" (2005, 263). Friedman makes the point with respect to studies of voting fluidity on the Supreme Court, but we think it can be extended to how we think about concepts related to legal reasoning. In developing our measures related to decisional behavior empirical scholars can sometimes "miss the forest for the trees." To make this point we specifically point to the trajectory of thinking with regard to the one of the iteratively developed, well worn concepts mentioned above, "case salience."

Judicial scholars have measured "case salience" in a number of ways. Sometimes scholars presume that cases involving broad issue areas like "civil rights" are salient (ex. Flemming and Wood 1997) in other instances they use more objective case specific indicators of the concept. Slotnick (1978), for instance, considers whether cases decided by the Supreme Court were subsequently included in

Constitutional Law case books signifying their importance, Maltzman and Wahlbeck (1996) use the number of amicus briefs filed in a case. Most recently Epstein and Segal (2000) offer a measure indicating whether the Supreme Court decisions appear on the front page of the *New York Times*. All of these measures conceptualize salience by whether some relevant external audience (law professors, interest groups or the general public) is "paying attention" to what the Court is doing. While this conceptualization is justifiable, and may have been appropriate where researchers were considering internal institutional practices like opinion assignment, it is far from perfect. Most significantly, because existing measures of "cases salience" are assumed to operate the same way across judges, they fail to capture an aspect of "personal relevance" inherent to the concept that is arguably most important (and interesting) with regard to decisional behavior.

The term salience comes from cognitive psychology; it is used to describe characteristics of objects that "stand out" in the perceptual field. In this respect it is correct to assume that there are "salient" cases that receive disproportional attention across decision makers. But there are also individual factors that may make cases stand out in the minds of some decision makers but not others. For instance, a judge might have particular experience litigating, writing or teaching in an area of law making cases involving that issue particularly important to her in ways that do not apply to colleagues. Consider for instance, Justice Souter's experience writing on copyright issues. Moreover, psychological research shows that individual responses to salient stimuli often differ depending on traits and cognitions of the perceiver (Johnson, Jackson and Smith 1989). Along these lines, one might hypothesize that the experiences of Justice Ginsberg litigating on behalf of minority interests causes her to think "differently" about cases involving these matters than the other justices on the court because of her prior personal involvement with such issues. Extant measures fail entirely to capture these differences.

We note that in other subfields of political science concerned with decision making salience is treated as an individualistic characteristic. Consider public opinion studies that measure issue salience by probing what respondents see as the "most important" problem or issue facing the nation (see, for instance Lavine et al. 1996). The assumption in such studies is that these considerations will have disproportional influence in the way citizens make decisions. The concept of salience is most useful when there is some relevance to the relationship between perceived objects in terms of the decision task. For instance, just knowing that the economy is "salient" in today's political climate tells us *nothing in and of itself* about how individual citizens think about policy related to the issue. But knowing that a particular person lost

his job making the economy salient in his mind vis a vis other political issues like the environment or global warming, gives us a pretty good guess about the relative importance that individual might place on such considerations in evaluating candidates' or the president's performance (Rabinowitz et al. 1982).

In a similar vein, we wonder how much just knowing that a case is salient or that "people are watching" tells us about legal decision making phenomena, especially where cases are decided in *seriatim*. It seems akin to knowing that the economy is important to everyone these days. It tells us nothing at all about how the justices think about authority in the confines of salient cases, [11] or how particular justices might express differential reasoning styles with respect to issues that are most relevant to them. As such, much of the *promise of the construct* for explaining decisional behavior gets lost in operationalizations now prevalent in the discipline.

Moreover, empirical scholars talk about the measure "as if" it captures some aspect of personal relevance where one could argue that existing conceptualizations are, at once, over and under inclusive. We don't know, for instance, whether all the justices are concerned about the public attention given to cases that is assumed to make cases salient in their minds. And there are surely cases our operationalizations miss that are salient to one or more the justices for other reasons. Because we are empirical scholars, and sincerely sympathetic to the difficulty of measuring psychological constructs of this sort "from a distance," we understand how the trajectory of thinking about case salience has evolved. But at this point we think it's time to take a step back from subfield specific debates about things like relevant compendiums in developing such measures (Epstein and Knight 1998; Wahlbeck, Spriggs and Maltzman 1998) so that we may notice the forest instead of the trees.

The question is: are there alternative measures that we could use that may "capture" the construct of salience in our empirical studies. Yes -- one may see how some aspects of the justices' experience

[11] In many behavioral studies case salience is used as an independent variable to see whether it moderates the expression of ideology in the justices' case votes as compared to cases that are not salient. Arguably there is some useful information in knowing whether judges are being more or less ideological in cases likely to get attention from outside audiences. We note however that findings have been mixed on this question depending on the specific operationalization of the concept. Moreover, empirical scholars "spin" conflicting findings in different ways. Where judges are not ideological in salient cases they are portrayed as caring about the institutional legitimacy of the Court and adherence to legal norms; where ideology is significant it is taken as evidence of the prevalence of attitudes in the justices decisions. Our larger point is that the construct has greater potential to explain decisional behavior in ways that would be more interesting and perhaps relevant to scholars in other disciplines like law and/or psychology.

mentioned above interact with case characteristics, or as alluded to by Epstein and Segal (2000) the number and types of questions the justices ask in oral arguments may be used as a proxy that the justice see the issues involved in particular cases as important (alternatively one could use the proportion of time justices speak in oral arguments as a measure). Are these measures perfect? No -- it could be that in using such measures behavioral scholars have to limit ourselves to cases in particular issue areas and may lose information with respect to some decision makers. Justice Thomas, for instance, is notoriously silent in oral arguments so any measure used to track salience using such data would fail to capture issues that may "stand out" in his perceptual field. Is it worth the effort and cost to consider these alternative operationalizations? Of course, it depends on the questions researchers are interested in – but we would guess that in many instances the answer will be, yes. The fact that personal relevance can differ across justices seems like it could be fundamentally important in how they think about cases.

That said, behavioral scholars need to think harder about *how and why* salience should make a difference in decisional behavior and why it matters. In studies looking at opinion assignment in cases where relevant audiences are watching – the logic seems obvious. For other aspects of decision making the normative connection is less clear. It might be useful to look beyond how salience moderates the expression of ideology in case votes. One obvious hypothesis involves the role of salience in cert decisions. An inquiry along these lines may tell us something about the interaction between personal and normative considerations in decisional behavior. Where conference notes are available one might look at whether justices make more, or different kinds of arguments in the cases that are of particular relevance to them. Also one might be able to discern if they are more persuasive in the decision process by looking at vote coalitions and/or intercourt memoranda. We should be able to insights from psychology on group decision making and opinion leadership in formulating theory and hypotheses with regard to these questions.

For as long as behavioral scholars have been trying to measure salience in judicial decision making there has been an interest in seeing how judges might think differently in cases that "stand out" in their minds. Our only point is we could, and in many instances should, be thinking differently about theory and measures related to the concept in ways that are less path dependent to improve our understanding of decision making phenomena.

Conceptualizing Congress/Court Relations

Another area of research in which judicial behavior scholars have not been as careful in their conceptualization and operationalization of concepts as perhaps they should be involves Court-Congress relations. The nature of the relationship between the two branches is an important one for judicial scholars because there are good reasons to think that the judiciary's place in a system of separation of powers may affect how judges decide cases and provide insights on judicial independence. But as we will discuss in this section, this line of research has followed a path which has transported concepts in problematic ways, resulting in questionable operationalizations and quite probably invalid conclusions.

Although scholars have long observed that the Court and Congress interact in important ways and that Congress might respond to judicial decisions in numerous ways (e.g., Murphy 1962; Stumpf 1965), it was in the late 1980s and early 1990s that political scientists, empirical minded legal academicians and game theoreticians began analyzing the relationship in earnest in a systematic and rigorous fashion. In short, the relationship was conceptualized as one in which two policy-maximizing institutions sought to implement their preferred policies. These scholars accepted the attitudinal model's presumption that Supreme Court justices decide cases based on policy preferences, or something like ideology. However, drawing from rational choice theory, scholars theorized that the separation of powers creates exogenous constraints on the justices, who want their decisions implemented by the other branches of government. Therefore, the justices might be likely to moderate their positions to the extent necessary to avoid confrontations with the other branches, and especially with Congress, which can pass legislation to "reverse" or "override" a court decision. These insights led to the development of Separation of Powers (SOP) games. [12] From the beginning, we would submit these approaches were built on a shaky foundation. There has never been any strong empirical support for the *assumption* that judges approach cases before them with these factors in their mind (See e.g., Segal 1997). Despite a shaky foundation, these concepts have been extended and transported to additional research project in problematic ways.

The logic of Congressional reversals of the Court's statutory interpretation cases has also been extended to the court's *constitutional decisions* (see e.g., Gely and Spiller 1992; Eskridge 1993).

[12] A related line of SOP research examines how the possibility of future court action affects decision-making and the passage of legislation in Congress (e.g., Ferejohn and Weingast 1992; Eskridge 1991; Ferejohn and Weingast 1992; Martin 2001; Vanberg 2001). Although we think much of our critique in this section may apply to this line of research as well, we do not address it herein order to focus on the SOP implications for judicial decision-making.

Essentially then, congressional responses to the Court's exercise of judicial review have been conceptualized as "reversals" of the Court's constitutional decisions invalidating legislation. In an oft-cited study, Meernik and Ignagni (1997) operationalized congressional reversals of the Court's judicial review decisions as amendments to the invalidated law or statutes passed in Congress that appear to revive the policy struck down by the Court. Following the lead of the SOP research on statutory interpretation, they coded any amendment to the legislation that had been struck down by the Court or new legislation involving the same policy as "reversals." Meernik and Ignagni further concluded that their findings supported the theory of coordinate construction of the Constitution advocated by a number of constitutional scholars (citing Agresto 1980; Burgess 1992; Choper 1980; variations of coordinate construction are also discussed in a much broader literature as "departmentalism," "tripartite theory," "constitutional dialogues). That is, a congressional enactment in response to the Court's judicial review decisions is conceptualized as a reversal of the decision and as an instance of coordinate construction of the Constitution by members of Congress.

Problems with SOP Models of Statutory Interpretation

In the area of statutory interpretation, scholars theorized that justices would compromise their true preferences if their ideal point was too far from the ideal point of the median member of each house of Congress. Conceptually, the models expect justices to apply statutes in a manner that minimizes the likelihood of adverse action in Congress and maximizes their policy preferences (e.g, Marks 1989, Eskridge 1993). Similarly, congressional responses to court decisions are conceptualized as reversals motivated by the ideological distance between the median member Congress and the judicial outcome (see e.g., Gely and Spiller 1990; Eskridge 1991; Ferejohn and Shipan 1990; see also Haussagger and Baum 1999). In order to facilitate empirical research on the subject, a number of "common space scores" have been developed in which the ideology of the justices is scaled within the same space as members of Congress (See e.g., Segal and Spaeth 2002, 320-328). The few empirical studies testing theories have yielded mixed results. For example, cases with stronger majorities appear to be less likely to lead to a congressional reversal, suggesting that the justices might work toward more consensual decisions to avoid being overridden by Congress and that Congress might view less consensual decisions as more vulnerable or even less legitimate (Eskridge 1993; but see Melnik 1994). On the other hand, the leading empirical research indicates that the ideological distance between justices and key members of Congress does not

constrain justices from voting their true policy preferences (Segal 1997; see also Segal and Spaeth 2002, 326-349).

There are a number of important implications of these SOP games and empirical studies for our understanding of legal decision making. Because the research on Supreme Court decision making supports the importance of policy preferences, it is assumed that attitudinal voting on the Supreme Court can be equated with attitudinal voting in Congress and common space scores along single issue dinsmeions can easily and validly be computed. Thus, it is assumed that the content and purposes of laws stimulate and evoke the ***same types*** of attitudes along identical issue dimensions across the two institutions. Additionally, no distinction is made between an understanding of the law prospectively and in the abstract and one that is retrospective and in the context of concrete facts. And the conceptions of who the relevant players are in Congress to whom justices are supposedly paying close attention are unconvincing. Lastly, and closely related to previous points, the models assume that text does not matter in SOP games – that is, the text of the law does not influence the justices' decisions and the text of the Court's opinion does not influence legislators' responses to Court decisions.

In the first place SOP models have not adequately differentiated the different ways judges look at a law and the ways a legislator look at a law or a court decision interpreting the law. Even if we accept that the justices are driven by attitudes, empirical studies also make clear that the justices' attitudes are responsive to the facts before them in the adjudication, i.e., based on a retrospective application of the law to concrete facts (Segal 1984; Segal and Spaeth 2002). Conversely, legislators initially consider legislation from a purely prospective and fairly abstract viewpoint. Moreover, studies of congressional decision making suggest that members of Congress make decisions based on credit-claiming, blame-avoidance, the position of party leadership, and attention to various constituencies or "attentive publics," in addition the desire to make good public policy (See Mayhew 1974, Fenno 1973; Fenno 1977; Kingdon 1989; Arnold 1990; Cox and McCubbins 1993, Jackson and Kingdon 1992). It is a very different decision making process than within the Supreme Court. It is not enough to simply characterize these factors as independent variables in a generalized decision-making model; the differences in the decision making environments and the motivations of actors in different institutions most certainly influence the perspective through which the decision maker understands the law in question.

Generally, theorists set up the decisions of the Court as part of a formal game involving the preferences of actors in other institutions. The number of relevant actors in each theory varies, but all

positivist models include some consideration of the ideal points of Congress, the President and the Court.[13] The number of relevant actors considered **within** each institution also varies from one theory to the next. The Court is uniformly treated as a unitary actor [see Segal (1997) questioning this characterization]. There are, however, numerous conceptualizations of relevant congressional players. Some theorists include separate ideal points (H) and (S) for the House and Senate (Spiller and Gely 1992). Ferejohn and Weingast (1992) follow this convention but include a third point (Q) to represent the ideal point of the "enacting legislature" to distinguish it from the preferences of actors currently in Congress. Eskridge (1991a, 1991b) and Epstein and Walker (1995) model congressional preferences by including ideal points of the median member of congress (M) and the median member of the "relevant gate keeping committee" within Congress (G) with regard to the particular issue the Court is considering. Finally, some theorists combine these approaches by including four relevant congressional ideal points, one for the House (H) and one for the Senate (S) and one each for the gatekeeping committees within each chamber (h) & (s) (Eskridge & Ferejohn 1992).

We note that the varying number and characterizations of relevant actors within Congress is a problem for positivist theories in general. It is not entirely clear that the differences can be explained by different policy domains each theory tries to explain. It is not evident for instance why the preferences of gate keeping committees would be relevant in civil rights (Eskridge 1991a & 1991b) but not labor relations decisions (Spiller and Gely 1992) which occurred during the same period. Moreover, as the number of relevant ideal points increases it becomes less and less plausible that justices actually conduct (or are even capable of conducting) an exhaustive consideration of all relevant preferences in ruling on particular legal issues.

In a critique of positivist which some authors (Brace, Hall & Langer 1999) have treated as conclusively demonstrating the error of applying such models to notions of Supreme Court decision making, Segal (1997) argues that the lack of objective evidence to support such theories combined with unrealistic assumptions of perfect information and unidimensional case and issue preferences, render positivist arguments untenable. Specifically, he questions the theoretic justification for treating the nine

[13]Several theories also included the preferences of relevant bureaucratic or agency decision makers. Spiller & Spitzer (1992); Ferejohn & Weingast (1992).

justices as a unitary actor and representing the differential preferences of 535 members of Congress as one or two ideal points in a formal model. Finally, he takes issue with the theoretical notion that justices who enjoy life tenure would ever vote strategically rather than in accordance with their true policy preferences. He provides what he characterizes as "unambiguous" evidence of sincere rather than strategic behavior on the part of individual Supreme Court justices by looking at the votes the justices case in statutory cases between 1977 and 1992.

Segal's insights and analysis unquestionably dealt positivist theories of inter-branch relations a serious blow. Regardless of whether or not one accepts Segal's evidence of sincere vs. strategic behavior on the part of the justices,[14] his theoretical criticisms of this general approach to institutional relations remain quite forceful. The problem with positivist theories of Court/Congress relations is that they are all over the map. Rather than building consensus, individual scholars have tried to carve out their own theories of institutional interactions in different policy domains. Thus there is no agreement on who the relevant congressional actors are. Such a consensus is fundamentally necessary before authors will be able to tie positivist theory to reality in anything but a post hoc manner. Clearly, there needs to be a synthesis and consolidation as to the essential elements of the theory and the conditions under which various constraints will be operable before further empirical progress can be made and the value of such theories for helping to understand institutional relations between Congress and the Court can be fairly determined. We believe the way to solve this problem is to disaggregate the theory into testable components to reveal how actors in each branch actually communicate and act with respect to one another. Due to difficulties in obtaining and correctly categorizing data as "responsive" such research has been sparse thus far, but we believe such efforts are worthwhile because they represent our best chance of discovering the nature of such interactions.

[14] The evidence itself can be criticized on several grounds. First in looking at the justices individually Segal fails to take on positivist theory on its own terms because he disaggregates the Court majority which is responsible for outputs of "the Court." Secondly he includes rather strange operationalizations of the justices preferences that do not map directly onto averaged ADA scores used to represent ideal points of median members of Congress.

Problems with Extension of SOP Models to Constitutional Contexts

Even if we accept the logic of SOP models and congressional reversals of the Court's *statutory interpretation* decisions, the extension of the concepts from to constitutional decisions is an excellent example of continuing down a path and transporting concepts in a path dependent manner at best, and a careless manner at worst. The Meernik and Ignagni study was in a certain sense a nice attempt to use the SOP logic from literature on statutory interpretation to further our understanding of Court-Congress relations in the constitutional interpretation or judicial review context. However, we only need to begin to scratch the surface to see how the concepts of congressional overrides of statutory interpretation and coordinate construction of the constitution did not travel very well.

To begin with, statutory interpretation and judicial review are two different modes of legal and judicial decision making. When the judges engage in statutory interpretation, they are applying the text of a statute to the facts in front of them, whereas judicial review involved resolving a conflict, or possible conflict, between constitutional principles or text and statutory provisions. Moreover it is important to recognize that conflicts between statutes and the Constitution may arise *on the face* of the statute rendering the facts of the case virtually irrelevant, or the conflict can arise because of the facts of the case when the statute is facially valid but *applied* to a party in a suspect manner.

Another fallacy in SOP models, and especially for the case of judicial review, involves conceptualizing congressional responses to court decisions as a dichotomous variable where either Congress fails to respond or passes legislation to reverse the Court decisions. In fact, members of Congress have more options open to them than just a dichotomous response – and this is especially the case in as Congress considers how to respond to judicial review decisions. Research shows that members of congress frequently amend "unconstitutional" legislation struck down by the Court in a manner that attempts to comply with the Court's constitutional holding – in other words, it attempts to revive the policy in the statute by complying to varying degrees with the court's constitutional requirements for delivering the policy (Pickerill 2004, 49-57; see also Whittington 2005, Blackstone 2008). Sometimes Congress repeals legislation, completely complying with the Court (Pickerill 2004, 48). What is clear when one actually reads legislation passed in response to judicial decisions, and the legislative history of that legislation, is that Congress responds in many ways other than reversing the judicial decision.

This reality is crucial for several reasons. First, SOP models have simply mis-conceptualized the nature of congressional responses to judicial review, and thus do not reflect reality. Second, the insights drawn from SOP models regarding strategic calculations of judges are suspect because they suggest judges will act to avoid reversals without consideration of other types of possible responses. Finally, the congressional enactments in response to judicial review simply cannot be accurately be characterized as instances of coordinate construction of the Constitution in any meaningful sense – and certainly not as that concept is used by scholars such as Agresto, Burgess and Choper, all cited by Meernick and Ignagni (see, Pickerill 2004; 57-60; see also Whittington 2006). The problem here is not just that the concept has been incorrectly used or the label improperly co-opted, but this is exactly the sort of thing that alienates legal scholars when they read political scientists' accounts of legal decision making. There is just little evidence, if any, that members of Congress engage in independent, meaningful debate over the constitutional issues in question, when instead, they appear to be more focused on satisfying the legal requirements of the Court.

Still the Meernik and Ignagni article is frequently cited– why? To judicial behavior scholars, indeed probably the reviewers, who are unfamiliar (or only vague familiar) with the rich debate over coordinate construction, but who ***are*** familiar with the SOP models, the research seems at first glance a logical extension of work on statutory interpretation. And, of course it should not be forgotten that the article appeared in one of the top political science journals of general interest, *American Journal of Political Science*, and so its legitimacy was not seriously questioned by political scientists (and not read by the legal scholars most likely to do so). Finally where junior scholars see senior scholars cite it, they may tend to accept its findings unquestionably.[15]

[15] Indeed, in a recent exchange between one of the authors and a junior scholar who shall remain nameless, the authoritative citation to Meernink and Ignagni without reference to works calling their conclusions into question was justified by pointing to two more senior scholars who had cited the piece in the previous two years.

Legitimacy and Normative Issues

Thus far in this paper we have mainly commented on how practices in our own discipline might benefit from greater attention to issues of concern to scholars across the disciplinary divide. We do so with (love and) respect for our disciplinary predecessors and contemporaries. We are proud to be empirical law and courts scholars; we understand that the subfield has made significant contributions to our understanding of phenomena related to decision making – our observations of made in the spirit of improving our measures and conceptualizations so that we may continue to do so. At this point we would like to transition to talk a bit about how legal scholars would benefit from taking the findings of political scientists more seriously in their thinking about normative issues related to judicial authority and the legitimacy of court outputs. In doing so we pause to notice that there is much less concentrated attention in the scholarly journals to exchange in this direction. Though we can (and do) point to many studies where legal scholars comment on the overall usefulness of quantitative approaches, and studies where legal academics are employ statistical techniques to explore questions in isolated instances, there has not been much in the way of "grand theorizing" about the implications of empirical findings for normative questions.

Clearly, part of this is the fault of behavioral scholars who have not been as explicit as they should be in drawing connections between specific findings and their consequences for how we should think about normative questions (Friedman 2005). Surely there has been an *implicitly* critical stance in the work of law and courts scholars for the past half century that has not gone unnoticed by judges and legal scholars (see for instance Edwards 1997; Friedman 2005,262 – referring to such work as "pathological[ly]" skeptical and Tahamana 2009 observing that empirical types are "agenda driven"). But political scientists who do work on the courts have been largely content with chipping away at notions of legal constraint by demonstrating the influence of attitudinal factors in judicial behavior in successive studies without offering suggestions about how we should think about findings in the larger democratic context.

And, of course, it is not at all accurate to say legal scholars have not been concerned by these issues in theorizing about decision making. As referenced below, legal and constitutional theorists were grappling with their own brand of thinking about extra legal influence in the guise of "legal realism" (Frank 1931a, 1931b; Lewellyn 1962) well before behavioral scholars had a fully developed "attitudinal model" (Segal and Spaeth 1993; 2002). We argue, however, that the sheer volume of empirical work

demonstrating the influence of extra legal influences in judicial behavior over the past half century raises the importance of dealing with such findings in a more direct and theoretically satisfying manner.

We also note here (and we promise this is the last time we will criticize those on our own side of the disciplinary fence) that where political scientists have addressed issues like "legitimacy" they have done so in an extremely narrow fashion. Gibson and Calderia, together, individually and with the help of other colleagues have amassed a truly impressive body of work on this subject looking at public support for the United States Supreme Court (Caldeira 1986; Caldeira and Gibson 1992; Gibson and Caldeira 1992) the European Court of Justice (Gibson and Caldeira 1998), and other high national courts (Gibson, Caldeira and Baird 1998). They have addressed how highly visible decisions (Gibson, Caldeira and Spence 2003) and judicial selection practices (Gibson 2008a, 2008b) can influence the esteem in which citizens hold judicial institutions. Obviously this is interesting and important work. Moreover, in developing their measures of public support for judicial institutions Gibson and Calderia have been careful to distinguish between aspects of "specific" and "diffuse" support that have helped all of us think about these issues more carefully (Calderia and Gibson 1992).

Looking at such research however, one could question whether we as a subfield have gone too far in equating legitimacy with public opinion. Surely there is more to the construct -- a normative aspect that our measures of public support fail to capture -- that is part and parcel of how judges and legal scholars think about the concept. Again the issue of translation (Mertz 2008) is relevant here. For legal scholars the concept of legitimacy evokes a host of democratic values important to the administration of justice including fairness, equality, adherence to procedural rules and attention to normative aspects of legal reasoning. Just as extant measures of "case salience" fail to capture aspects of personal relevance across decision makers, our measures of legitimacy fail to capture these important normative considerations that are inherent in the way judges and legal academics think about the concept. Certainly public support is an important aspect of legitimacy, and may be taken some evidence of legitimacy, but it is not, and cannot be legitimacy itself.[16] This does not mean that the substantial effort that has been

[16]For example, the tension between the legitimate function that judicial institutions play in protecting fundamental rights and public opinion is as old as our nation's history. Consider the following hypothetical: If 1000 randomly surveyed individuals were asked whether Congress should right to limit the Supreme Court's jurisdiction to make decisions on school prayer and 60% or 90% of respondents agreed that it should, would it mean that it would be "illegitimate" for the Court to make such decisions? We're going to go out on a limb and say no. One could go even further to argue that if a majority of those respondents agreed that the Court should be abolished tomorrow it would *still* have

invested in such measures is without value. It just means we have to be very careful in the way we discuss our findings across disciplinary boundaries acknowledging that aspects of the concept empirical scholars have been able to quantify and measure may not fully encompass the way legal scholars think about the construct.

In legal parlance much of the discussion about legitimacy involves justifying the exercise of judicial authority in our constitutional system in light of judges' "democratic deficit." Federal judges, and many state judges, are not elected but appointed by other political actors. They do not have the same democratic authority to act in accordance with their policy preferences. This raises a critical dilemma because judges are unavoidably human. The government officials who are so necessary to resolve disputes between citizens and say what the law requires in particularized circumstances are bound to have personal preferences (shaped by their ideology, attitudes and experience) with regard to the cases before them. According to classic democratic theory, the dilemma is solved with reference to the unique knowledge judges acquire through their specialized training. We abide their substantial influence in our democratic system because of the expertise they possess as "interpreters of law." On this account, judges are not free to decide cases according to their personal views because they are *constrained* by appropriate sources of legal reasoning and cannons of interpretation. [17] Technical training in the tools of legal analysis enables judges to separate reason from personal biases in their deliberations; and it is the predominance of reason that endows judge-made law with legitimacy in our constitutional democracy.[18]

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legitimate authority to make those decisions under our constitutional structure. Quite simply the majority's momentary willingness to do away with judicial institutions does not vitiate citizen's legitimate interest in having them or judges' legitimate authority to make pronouncements that might be contrary to what a majority of citizens want.

[17] Constraint can be defined as "something that limits the freedom to act spontaneously; or some physical, moral or 'other' force that compels somebody to do something or limits their freedom of action." When legal types talk about constraint they are usually referring to professional norms and/or obligations that require judicial actors to use legal presumptions, rules and authority in reaching decisions. Traditional legal theory holds that the reasoned use of these tools often compels judges to reach particular outcomes. Gillman (2001) and others have referred to the idea that law imposes an external constraint on decision makers' ability to reach particular outcomes as "legal positivism."

[18] This notion of expertise as a source of judicial authority is uniquely reflected in our constitutional structure. Federal judges are insulated from politics. They serve for life during "good behavior." The democratic justification for this protection is not to allow judges to pursue their policy preferences unchecked -- although notable judicial scholars have argued such behavior is the result of this particular institution – but rather, to allow judges to apply their expertise without having to worry about pleasing particular constituencies or government officials. Such concerns,

Obviously the conception of law as a meaningful constraint on decision makers has taken a serious blow in the last century with the rise of legal realism in legal academia and studies of judicial behavior in the social sciences. The overwhelming weight of empirical evidence demonstrates that judges vote disproportionately for outcomes that are consistent with their political policy preferences, casting significant doubt on the constraining force of legal authority they cite as determinative in their decision making. These findings are extremely significant from a normative perspective.

Perhaps the logical place to start thinking about why this is important is to consider what we are worried about -- what seems so improper about unconstrained exercise of discretion in legal decision making? The answer is, at once, surprisingly simple and complex. It is simple because it involves some of the most basic aspects of our legal system that we are taught to be concerned about in high school civics – including fairness, equity and the exercise of unelected authority in our constitutional system. It is complex these days, because not everyone is equally concerned about the lack of constraint in judicial behavior.

The argument for requiring that decision makers use and cite accepted sources of authority is at least twofold. First, where judges use their training to apply the logic set forth in sources of legal reasoning, they are encouraged to think about specific disputes in light of generally applicable rules and larger societal principles that our constitutional founders, legislative actors and other judges have deemed important and applicable in similar circumstances. In this way referencing legal considerations like constitutional text, intent and precedent helps imbue judicial decisions with democratic values. It also promotes continuity with other government institutions and creates an internal consistency in doctrine that should foster predictability for future litigants. Second, judges utilizing tools of legal reasoning are prevented from calling on *their own* values, beliefs and preferences in making decisions between adverse parties; the fact that judges are engaged in seemingly objective task of legal analysis helps legitimate the distributive choices they make in our constitutional system. If there were widespread acknowledgement

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it is assumed, interfere with judges' expert interpretation of what the law requires in particular cases. Although this might seem like a naive justification for an independent judiciary, it is the one that was given at the time of the framing, further demonstrating the close connection between expertise and notions of judicial authority inherent in the American political framework. As Alexander Bickel put it "judges have or should have the leisure, the training, and the insulation to follow the ways of the scholar in pursuing the ends of government" (1986, 25-6).

that judges were not actually constrained in the application of their expert knowledge, we would have to reassess the assumptions underlying some of our most basic (and necessary) governmental forms.

Building off the tradition of legal realism, and fueled by empirical evidence of extralegal influences in decision making, there has developed what scholars have referred to as to as a "post-realist" or "post-positivist" (Gilman 2001) jurisprudence where legal academics acknowledge that meaningful constraint imposed by law may not exist in all cases. In the absence of external constraint, a number of prominent legal scholars including Dworkin (1993) Ely (1980) Sunstein (1996) and Ackerman (1991) argue for a jurisprudence based on fundamental principles, or different conceptions of judicial (and/or democratic) values to legitimate judge made law in "hard" cases or those involving abstract constitutional issues.[19]

In what is perhaps an unfair characterization of these theories, Segal suggests that proponents of such jurisprudential approaches essentially argue that as long as judges can reasonably justify their decisions by dressing them in *some* appropriate authority, they are by definition, legally justified.[20] His point is that according to this post positivist school of thought, it does not matter whether the law is, in fact, the *primary determinant* of judicial decisions or whether decision makers are *compelled* to reach particular outcomes -- as long as judges can reasonably couch their decisions in some doctrine, attempting to do so sincerely (Gilman 2001) and/or consistently (Dworkin 1977, 88) those decisions will be deemed legitimate (see also, Friedman 2005 at n41,quoting Dworkin for a similar proposition).

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[19]We note (somewhat parenthetically) that interpretive legal theory has evolved in a highly unrealistic fashion by scholars touting schools of interpretation that justify the outcomes in seminal decisions like *Brown vs. Board of Education* (1954) and refute those in infamous decisions such as *Plessy vs. Ferguson* (1896) and *Lockner vs. New York* (1905). The merit of a particular interpretive approach is judged by its ability to justify widely acknowledged desirable outcomes. Stated simply, constitutional theory evolves "as if" there were some consistent interpretive approach that would yield such desirable outcomes. Political scientists, of course, know better; often the outcome in constitutional cases has more to do with the mix of preferences of the justices on the Court then their commitment to any interpretive principle. As such, constitutional legal theory has completely failed to consider a half century of empirical research on the influence of ideological preferences on Supreme Court decision making. That said, we observe that this is not necessarily a bad thing. Interpretive legal theory covers the normative as well as empirical ground; it is properly concerned with how decisions **should** be made as well as how they have been made at critical junctures in our history. But it underscores our larger point that it is time for political scientists to take a more active role in the debate and consider the democratic implications of how judges actually make decisions in a broader theoretical context.

[20]Segal has referred to this line of thought several times on the law and courts listserv in this manner (ex. 8/15/05). Segal and Speath also make this point in revisiting their attitudinal model (2002, 432-3).

If Segal is right in characterizing post-positivist thinking in this manner, and we believe that he is, the concept of external constraint imposed by law has lost its luster, not only for empirically minded social scientists, but also for legal academics seeking to legitimate judicial decision making by other means. Here we argue that attempts to justify legal authority without reference to meaningful external constraint are fundamentally flawed. Such arguments fail to address the *range* of concerns posed by unconstrained judges in our system. Specifically we see constraint as a "democratic good" necessary to legitimate the *distributive choices* of unelected judges. As such, scholars in all disciplines, but particularly political science, should be deeply concerned with finding constraint in legal decision making. If we come up empty in this search, or even empty in some types of cases, we, as a discipline, will need to do some very serious thinking about why judges are among those political officials that get to say "who gets what, when and how."

In fleshing out this argument, we would like to make two things clear. First, as we see it, any debates between "attitiudanlists" like Segal and Spaeth and "post-positivist" legal scholars like Dworkin and Ackerman are debates about the *degree* and *mechanism* of extralegal influence in judges' decision making, *not* debates about the fact of such influence. For the purposes of our argument, their conclusions are similar: the law does not (and perhaps cannot) constrain legal decision makers in all cases.[21] Thus, we see these literatures as largely consistent, in contrast to other scholars who have characterized arguments made by attitudinal and post-positivist scholars as oppositional in one or more respects (Smith 1994, Gillman 2001).

Second, based on some of the points we make earlier suggesting that empirical scholars have not been as thoughtful as they could in operationalizing legal considerations in our studies of judicial decision

[21] Of course, Dworkin (1993) and Ackerman (1991) go on to argue for the legitimacy of judicial decision making in the absence of constraint as long as judges rely on "fundamental" or "judicial" values. Segal and Spaeth (1993, 2002) do not attempt to legitimate judicial decisions in the absence of constraint. Indeed part of the power of their empirical investigation is their implicit critique of traditional conceptions of judicial authority. This is not to say that political scientists are not interested in such issues. Gillman (2001) argues for a conception of legitimacy that involves whether or not judges act "sincerely" in their attempt to apply legal principles to particular cases. While we are sympathetic to Gillman's argument that a lack of volition in the application of values and/or ideology takes some of the "blame" off judges engaged in the exercise of unconstrained decision making (see, Braman 2009) the subjective state of mind of decision makers fundamentally cannot resolve the issue of the legitimacy of court outputs as we see it.

making, we think the question of whether or not constraint exists is still largely an open issue. It is entirely possible (and, we think, quite probably, inevitable) that we will discover meaningful constraint exists in some cases but not others[22] or with regard to some, but not all, decision making practices designed to curb individual discretion. This does not make the search for constraint futile. Indeed for those of us interested in the *process* of decision making this is what makes the endeavor so worthwhile. Understanding how stylized norms of appropriate behavior succeed and fail to shape legal decision making will go a long way in advancing our understanding of decision making processes more generally. Moreover, knowing **how** norms and context operate in the minds of decision makers will only improve our understanding of where the requirements of democratic theory tax the boundaries of human capacity (Frank 1931a, 1931b) and where traditional conceptions of legal expertise as a legitimizing factor may yet have force in justifying court outcomes.

In thinking about why constraint is so fundamental to conceptions of judicial authority, in the best tradition of political science, we consider *what judges do* in our constitutional system. Here we are not referring to case votes as opposed to doctrinal explanations judges give for their decision making, but the actual functions judges perform in our democracy. Specifically, we focus on two relatively non controversial tasks judges perform. First, they "interpret" the law and in doing so create legal doctrine that itself has authoritative force. Second, they make decisions that are the basis for distributive allocations between adverse parties in litigation. Each of these functions involves somewhat different democratic concerns and thus requires different justification. While the concept of external constraint may be dispensable with regard to the former function, we argue it is essential with respect to the latter. Post-positivist legal scholars trying to create a jurisprudence legitimizing choices in the absence of constraint have not adequately dealt with this problem.

So based on the influence of legal realism and evidence of extralegal influences from empirical research, what happens if we relax the assumption of constraint imposed by law? If we admit there is subjectivity inherent in legal interpretation where there are equally compelling interests at stake and the law is not necessarily clear about what outcome should obtain? If we acknowledge the best judges can do

[22] It makes sense to think of constraint as a "case specific" rather than "court specific" variable. Although the Supreme Court is likely to have a much higher proportion of "hard" cases relative to its workload – those cases must start somewhere and so they are present in our judicial system at all levels, albeit in different concentrations.

in some cases is rely on "judicial values" or their view of what is "fundamental" in closely contested political environments where reasonable decision makers can (and do) come to different conclusions?

Quite simply, the answer is more problematic for some aspects of judicial authority than others. Considering judicial actors as functionaries in "interpreting law" and saying what is required in particularized circumstances – relying on judicial values judges acquire through expertise and professional experience – may be the best we can hope for (and indeed all we can ask). It makes sense that those who are trained in the law should get to say what it means as long as they are engaged in the sincere and circumspect application of their legal knowledge (Gillman 2001) and do so in a consistent fashion (Dworkin 1977).

A lack of constraint is more problematic if we consider the role judges play as third party arbiters that distribute rights and resources. This is *especially true* where there are closely contested views about *how to distribute* those resources and the role of the state in such environments. Citizens who come to the court have a right to expect fairness, predictability and equality before the law. The introduction of subjectivity into legal interpretation fundamentally alters the state's ability to deliver on such promises where the outcome *has more to do with the individual values (judicial or otherwise) of* ***who is on the bench*** *than what the law dictates*. Judicial distributions based on anything but the law are not legitimate unless and until someone can come up with a colorable theory about why judges, who are among the most unrepresentative of all political actors, get to make these distributive choices based on *their* values other than the fact that they have been to law school. Post-positivist legal theorists spend a great deal of time in debates among themselves trying to justify departures from doctrine in favor of "fundamental" or "judicial" values where there is a lack of meaningful constraint. They would have a far more difficult time explaining to ***loosing litigants*** why jurists rendering decisions in their particular "hard" cases can only rely on their own idiosyncratic conception of what is "fundamental" in contested socio-political environments – *and why this is all O.K.* -- because really, when you get down to it, it's not O.K. It comes down to arguing that judicial decisions based on decision makers' personal beliefs and values are legitimate because they are made by judges.

The absence of meaningful constraint and/or objective criteria by which judges can make distributive choices calls the legitimacy of such choices into question and enlists state authority in the enforcement of what are essentially personal preferences about litigants or competing legal (or policy) arguments. Any expectation of fairness or equality before the law becomes illusory in the absence of

external constraint. This becomes exceedingly clear when we consider that judges with different attitudes or judicial philosophies can (and do) decide similar cases in opposite directions.[23] Arguing that judges understand how to "interpret the law" is not enough to justify judicial authority where decisions *are not legally determined*. At base, these are value judgments; and there is no reason why any individual citizen's beliefs and values are "better suited" than any other's for making such determinations, number of years in law school notwithstanding.

Political scientists understand this. Indeed, many scholars in the discipline have carved impressive careers for themselves taking an intentionally critical stance showing that judges are not meaningfully constrained by legal considerations. The larger point is that if constraint does not exist, or it exists in some cases but not others, political scientists cannot be complacent or satisfied with identifying this state of affairs. It is time to ask (and start answering) some larger democratic questions given the crisis a lack of constraint poses for judicial authority and the legitimacy of distributive outputs in our constitutional system. [24]

So in the spirit of making some of the sort of concrete suggestions we advocate at the start of this paper, we offer a proposal. Quite honestly at least one of the authors of this paper has never been a fan of electing judges. There are certainly problems with the way judicial elections work in our system that many of the scholars this conference can (and unquestionably will) detail far better than us (See for instance, Geyh 2003 for an especially biting critique of the practice). Given the concerns we raise here, however, we can certainly see where a tenable argument could be made that elected judges have more legitimacy to make 'judgment calls' in contested socio-political environments. Moreover, although we have not seen this argument raised in the context of legitimizing judicial behavior given our current knowledge of the way decision making works, this sort of hybrid conception of legitimacy has some

[23] Compare Friedman (2005) at n41 citing Dworkin for the proposition that conflicting decisions in such instances are not problematic from a democratic theory perspective.

[24]The fact that more citizens are not especially bothered by this "crisis" of legitimacy we are suggesting does not solve the larger democratic problem for two reasons. First, legal decision making is a highly esoteric endeavor; judges always couch their decisions in seemingly objective legal authority. Thus, citizens are not in the best position to detect a lack of meaningful constraint in legal decision making. Second, as we argue above, legitimacy itself is a multifaceted concept. It certainly involves public acceptance of judicial authority and decisions, but there is also a *normative* aspect to the concept, involving why judges *should* exercise the authority they do in our constitutional system.

attractive qualities. In the subset of cases where there is meaningful constraint judges can rely on expertise – and where there is not they at least have *some* direct democratic authority to rule as their 'judicial values' dictate. Significantly, this sort of thinking suggests that elected democratic authority may be most important at the *highest* level of our federal court system where the greatest proportion of "hard" cases exist rather than at the level of state trial courts where most judicial elections actually occur. This means we may be *entirely wrong headed* in the way we have been thinking about judicial selection if one of out our primary concerns is the legitimacy of court outputs.

To be completely honest, we are not wedded to this proposition. As legally socialized scholars, we are somewhat uncomfortable in offering it. But we think that it is the sort of suggestion that will at least *move debates about judicial legitimacy forward* in ways that take account of what we know about decisional behavior. Moreover, increased attention to the authority of judges to make distributions with regard to specific litigants would be a welcome expansion of current thinking about legitimacy that might engage behavioral scholars more directly. At any rate it is another example of how our thinking might be improved by diverting a bit from dominant disciplinary trajectories. At this stage, with the help of thoughtful colleagues to guide, direct, and potentially refocus our thinking we may have much to gain from wondering off the beaten "path."

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