The Pitfalls and Promises of a New Legal Realism Rooted in Political Science

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Introduction

The new legal realist movement seeks to foster inter-disciplinary studies that combine the best of legal scholarship and the social sciences. This goal is laudable but results can vary. Sometimes fresh eyes and different skills discover new insights and avenues of inquiry, but sometimes scholars imperfectly translate theory, methods or data from outside their fields. In other instances, progress is piecemeal, as scholars cobble together substantive findings across theoretically and methodologically diverse work that address complementary questions and topics. The accumulation of empirical insights can be useful but falls short of a theoretically integrated, inter-disciplinary research agenda.

Political science seems a natural partner in the new legal realism project and, indeed, political science and legal studies have a long tradition of productive cross-pollination. Yet the combination of law and political science has yielded mixed results from an inter-disciplinary perspective. On the plus side, it has produced reams of publications using original data and sophisticated methods as well as massive data sets, which are models of clarity and the provision of public goods to the scholarly community. This work has helped dispel the myth of "mechanical jurisprudence"—the idea that judges simply find the law and apply it as written—and the "myth of rights"—the notion that rights are self-executing and somehow "above" politics. These are significant achievements.

Yet studies of law rooted in political science have produced dilemmas that present ongoing challenges for a vibrant new legal realism. The first is what I call the "positivist dilemma." Many legal scholars with a political science bent have embraced positivist methods for studying the law and courts. This approach emphasizes the need for clear, falsifiable hypotheses and concepts that can be translated into quantitative data. Using this approach, it often makes sense to treat law as prescriptive commands and measure the degree to which they shift behavioral and policy outcomes controlling for other factors.

While acknowledging the methodological appeal of positivism, critics flatly reject its restrictive definition of the law. The result has been a stalemate in which scholars agree on many of the basic empirical findings of positivist studies—such as the existence of a robust statistical relationship between judges' attitudes and their votes—but disagree on their interpretation and significance. The tension between the discipline's methodological commitments to causal inference and more sophisticated (but harder to operationalize and falsify) theories about the nature of law is likely to persist, as the field increasingly emphases using the logic of randomized experiments in *all* types of empirical work. This experimental template encourages political scientists—whether located in disciplinary departments or law schools—to treat the law as an independent stimulus or "treatment" in politics and policy-making at a time when many scholars outside the discipline see law, politics and public policy as deeply intertwined and interactive.

The second challenge is the "substantive dilemma." For all of the theoretical discord underlying legal studies based on political science, they frequently stress how judicial decision-

making and policy-making resemble other forms of politics and policy-making; for instance, the notion that judges are merely "politicians in black robes." Cutting through legalese and the formality of legal procedures to identify the law's underlying political and policy functions also represents a significant contribution. However, normalizing judicial decision-making and policy-making arguably makes them much less interesting to study on their own (Burke and Barnes 2015; Barnes 2016). If law is just another form of politics, why not drop the "legal" from new legal realism and fold the study of law and courts into a broader research agenda on political decision-making and policy-making? This tension might become more urgent as new legal realism looks to contemporary political science, as political scientists who study the modern American administrative state are increasingly seeing litigation as just another mode of social benefit provision.

This essay argues that the positivist and substantive dilemmas are challenging but not insuperable. There are promising footholds for a new legal realism that combines legal studies and political science, which address these dilemmas and invite a range of scholars using multiple and mixed methods to participate. One prominent example is scholarship that explores the administrative, political and social consequences of relying on policy regimes that depend on courts and litigation to varying degrees. These studies avoid the positivist dilemma by treating the law and courts as a distinct yet comparable institutional setting for politics and policy-making as opposed to a set of prescriptive rules that take the form of "thou shall," "thou shall not" and "thou may." They address the substantive dilemma by emphasizing *both* the similarities and differences of politics and policy-making in legalistic and non-legalistic (or less legalistic) settings.

This essay develops this argument in stages, beginning with a discussion of the judicial behavior literature and its critics to illustrate the positivist dilemma and then offering a parallel analysis of "gap studies" to illustrate the substantive dilemma. It ends with a discussion of paths forward. A final caveat: this essay is not a summary. Legal scholarship that uses concepts and methods from political science constitutes a vast literature and this essay only addresses a tiny portion of it. Providing a comprehensive account of even a small sample of this literature is beyond the scope of a short review essay. Instead, this essay offers several case studies that reveal underlying challenges and opportunities for new legal realism that leverages political science. Inevitably, condensing a large and sprawling literature will exclude noteworthy studies and gloss over the nuances of others. The hope is that distilling different debates within this literature with an eye towards lessons for future inter-disciplinary work will somewhat compensate for its descriptive limitations.

Judicial Behavioralism, Post-Positivism and Positivist Dilemma

Judicial behavior studies—studies that model the determinants of judicial votes—offer a cautionary tale for new legal realism, which illustrates some of the pitfalls of applying rigorous methods at the expense of theoretical nuance. These studies trace their roots to traditional legal realism scholarship. Indeed, while law professors might assume that Critical Legal Studies are legal realism's natural heirs, one would be hard pressed to find scholars more fully attuned to the general tenets of traditional legal realism than judicial behavior scholars (Gillman 2001a). They repeatedly echo Holmes' (1897:465) famous formulations that "you can give any conclusion

logical form" and "behind the logical form lies a judgment as to the relative worth and importance of competing legislative demands." Like legal realists, judicial behavioralists argue that the law's inherent malleability *requires* scholars of judicial decision-making to focus on how judges act and not what they say. As Oliphant (1928:159) argued, "[n]ot the judges' opinions, but the way they decide cases will be the dominant subject matter of any truly scientific study of the law." Or, in the words of Llewellyn (1930:443, 448, 464), "legal scientists" should adopt an approach that "turns accepted [jurisprudential] theory on its head" and concentrates on the "practices of the courts, and not 'rules' at all."

Some trace the roots of legal realism within political science to Pritchett's analysis of voting blocs on the "Roosevelt Court" (1930), but Schubert's work in the late 1950s and early 1960s more clearly marks the shift from the study of "public law" to "judicial behavior" (1959, 1963). The initial wave of judicial behavior scholarship took traditional legal realism to its logical extreme by entirely dispensing with legal variables in analyzing judicial decision-making. It concentrated instead on more readily measurable variables, particularly attributes of judges, like their political party affiliations and backgrounds (e.g., Schmidhauser 1959; Nagel 1961) and then more sophisticated measures of their attitudes, values or personal policy preferences (e.g., Murphy and Pritchett 1974; Segal and Spaeth 1993; George 1998). This was intentional. It reflected a conviction that legal reasoning is *ad hoc* rationalization and no more apt for scientific inquiry than "creative writing, necromancy, or finger painting" (Spaeth 1979:64).

Using this approach, judicial behavioralists repeatedly found a robust statistical association between a judge's political ideology (variously measured) and their votes. The fact

that justices regularly dissented—that they voted differently when applying the same laws to the same facts in cases with the same lawyers, briefs and oral arguments—seemed additional evidence that they have significant discretion. Moreover, because phrases like "unreasonable search and seizure," "equal protection," and "due process" are inherently vague, the idea that law limits judicial discretion seemed fundamentally misplaced in many instances (Gibson 1991). For judicial behavioralists, the fluid nature of law combined with findings that judges' ideology reliably predicted their votes seemed to seal the deal for legal realism's claims about the negligible (at best) role of law in judicial decision-making, the deeply political nature of the courts, and the need to assess judges' actions not words. With typical punch, leading judicial behavioralists Segal and Spaeth summarized what became to be known as the "attitudinal model" within political science as follows: "Simply put, Rehnquist votes the way he does because he is extremely conservative; Marshall voted the way he did because he is extremely liberal" (Segal and Spaeth 1993:32-33).

The attitudinal model has been wildly successful, producing publications in the top disciplinary journals, law reviews and university presses, sophisticated and comprehensive data sets and an exemplary commitment to analytic rigor. Yet the emergence of a "new instititutionalism" in political science gave rise to critics, sometimes known as "post-positivists," who sought to take the law more seriously. For post-positivists, judicial behavioralism rested on antiquated conceptions of law and decision-making and tested a form of mechanical jurisprudence long-abandoned by legal scholars (and rejected by practitioners) (George and Epstein 1992; Smith 1994). In an insightful review essay, Gillman (2001a: 471) explains as follows:

> What is particularly noteworthy about the long-standing behavioralist aversion to legal variables is that it was premised on a fairly controversial conception of law as a set of clear, determinate rules. It was the same sort of formalist conception that had been the object of realist ridicule, and to some extent it found renewed expression in H. L. A. Hart's reformulation of legal positivism. This conception of law allowed many behavioralists to discount the significance of legal influences whenever it could be demonstrated that different judges reached different decisions—and, since that could almost always be shown, it was an article of faith that no additional justification had to be given for focusing on other [non-legal] variables (footnotes omitted; see also Caldiera 1994; Baum 1994).

In making these arguments, post-positivists (illustrating a benefit of inter-disciplinary work) inserted long-standing theoretical debates about the nature of law and decision-making under rules from leading legal scholars into the discussion. They explicitly drew on Dworkin's critiques of Hart's arguments about judicial decision-making. Hart (1961) claimed the rules matter when judges decide routine cases under well-settled law, but that judges enjoy unfettered discretion when the law's clear prescriptions end. In response, Dworkin (1978, 1985, 1986) called for a more nuanced conception of discretion under rules as well as a broader definition of law that included more open-ended principles and policies as well as prescriptive rules. According to Dworkin, some rules are clear but others provide decision-maker strong discretion (and little constraint). So, for example, strong discretion arises when a sergeant is ordered to "pick any five soldiers" for a mission. This rule sets clear boundaries on the number of soldiers

to be selected (five, not more or less), but the decision of which soldiers to select is left entirely to the sergeant. In the abstract, any five soldiers under the sergeant's command would be eligible for service. Under these circumstances, decision-makers are indeed free to impose their personal preferences when applying rules.

By contrast, some rules are neither wholly prescriptive nor open-ended. They require decision-makers to apply standards that offer some guidance but defy mechanical application. These types of rules yield weak discretion (and some constraint), as when the sergeant is ordered to "pick the *best* five soldiers." Under this command, not all soldiers under a sergeant's command are likely to be equally qualified and thus not all would be eligible for the mission. At the same time, different sergeants might choose different soldiers while following this order in good faith, as the concept of "best" is subject to multiple interpretations. (So, for example, one sergeant might select five soldiers based on particular skills, another might choose different soldiers based on combat experience and a third might stress readiness.) Of course, sergeants can still disobey the order by, for example, knowingly choosing five unqualified soldiers to spite their commanding officer or protect their favorites.

Under this view, Supreme Court justices might vote differently in the same case based on conflicting good faith interpretations of flexible legal principles. Of course, they might also differ because they are using the law in bad faith to pursue divergent policy preferences or partisan agendas. The critical difference lies not in their votes but in the motivations underlying their decisions (Gillman 2001b). Put in more positivist terms, there is a problem of observational equivalence when simply counting judicial votes: split decisions and even a robust association

between judicial attitudes and voting patterns are consistent with both realist and more sophisticated legalist accounts of judicial decision-making.

The result is an impasse. Behavioralists want to test law as an external constraint using large data sets and advanced quantitative methods. Post-positivists want to test the law as an internal constraint using interpretive, historical and ethnographic methods (e.g., Whitehead 2014). This impasse can be seen in reactions to Spaeth and Segal's subsequent efforts to test the legal model by analyzing the degree to which Supreme Court justices adhere to precedent (1999). In setting up their study, Spaeth and Segal acknowledge that justices may apply precedent because they feel constrained by it or because they agree with it as a matter of policy or partisan inclinations. To address this issue, they examine the behavior of dissenting justices over time, assessing whether they follow the majority opinion in subsequent cases. The idea is that dissenting justices reveal their true policy preferences by publicly rejecting the majority opinion in the original decision. If these justices fall into line with the majority's reasoning in future cases, then they are constrained by precedent. As is typical, Spaeth and Segal collect heaps of data and reject the legal model of judicial decision-making, as dissenting justices generally maintain their positions. While results varied across justices, between 80 to 100% of the votes and opinions by dissenting justices resisted precedent, some vehemently.

Post-positivists were not convinced (e.g., Gillman 2001a). The problem, they contended, is not the accuracy of empirical findings but the definition of stare decisis. Specifically, post-positivists argued that stare decisis is typically not seen as requiring dissenting justices to adhere to the majority position held by their colleagues. From this vantage, Spaeth and Segal's research

design was ill-suited for probing good faith judicial decision-making. That requires independent evidence of the justice's motivations when rendering their post-dissent decisions. In fact, dissenting justices' adherence to their original position might be evidence of their steadfast sense of obligation to follow what they see as the proper interpretation of the law!

The debate over Segal and Spaeth's painstaking efforts to test stare decisis reflects the difficulty of trying to resolve deeply conceptual disagreements over the nature of law and judicial discretion by counting votes. One might counter that surveys could bridge this gap. But judges are not promising survey subjects on the topic of their underlying motivations. As Shapiro (1981) once noted, the underlying social logic of the courts requires that judges appear to be neutral. One way to maintain the appearance of neutrality—and the "triadic relationship" between the judge and the litigants—is for judges to maintain that they are merely applying the law as written, even if this is largely a myth. As a result, Shapiro (1996:100) argues that "[a]ll politicians lie, but judges lie more consistently and pervasively than any other set of politicians. …. What judges persistently lie about, and are supposed to lie about, is the very [political] nature of the courts themselves and the decisions they reach. They do so for reasons that have to do with the very foundations of courts as legitimate institutions." If we cannot ask judges about their mindset when rendering decisions—whether they are in fact rendering decisions in "good faith"—it becomes hard to assess post-positivists claims using the types of quantitative data preferred by many behavioralists.

As noted at the outset, the positivist dilemma will remain a potential obstacle to using political science as part of a thriving inter-disciplinary new legal realism. The reason is that the

"causal inference revolution" in political science places a premium on meeting strict analytic assumptions, requiring empirical studies to rest on systematic comparisons between "treatment" and "control" groups to estimate average treatment effects. This implies, among other things, that researchers identify a discrete, independent treatment and find ways for accounting for unmeasured confounding variables, which typically requires randomization, "as if" randomization or natural experiments (see generally Dunning 2012).[1] This ideal creates pressure to envisage the law as a "treatment" that is distinct from politics and policy-making at a time when many inter-disciplinary scholars see law, politics and policy-making as mutually constitutive. This tension could isolate what is seen as cutting edge political science from the new legal realism project.

Gap Studies, Legal Mobilization and the Substantive Dilemma

Whereas judicial behavior studies seek to test whether law-as-commands shift judicial decisions, gap studies seek to test whether judicial decisions-as-commands shift policy outcomes. (The name "gap studies" derives from the distance often found between the lofty ambitions of progressive judicial decisions and persistent social and economic inequalities on the ground.) The debate between Rosenberg's *The Hollow Hope* (1991[2007]), which is perhaps the most widely cited gap study (and, indeed, is one of the most prominent books in all of political science), and Michael McCann's *Rights at Work* (1994), which is often cited as a foil to Rosenberg's analysis, exemplifies the substantive dilemma. In *The Hollow Hope*, Rosenberg asked whether the Supreme Court can create significant social change at the national level. To explore this question, he analyzes some of the most celebrated Warren and Berger Court

decisions, including *Brown v. Board of Education,* 347 U.S. 483 (1954), and *Roe v. Wade,* 410 U.S. 113 (1973), on the grounds that these seem "likely cases" for finding Supreme Court influence. He tirelessly delves into the record, plotting change in policy outcomes and discourse after these decisions, and typically finds little evidence of significant short-term change. Even the famous *Brown v. Board of Education* decision falls short, Rosenberg argues, as levels of school integration in the South significantly improved years *after* it was handed down and only when the President and Congress used their powers of the sword and purse to coerce change at the local level.

Given this type of evidence in multiple policy areas, Rosenberg concludes that litigation is typically a "hollow hope" for directly improving social conditions. More specifically, he argues that the Supreme Court can make policy only under very narrow circumstances, which he summarizes under the Constrained Court Model. That Model predicts the Court will make social change only when its decisions have (1) ample precedent, (2) congressional and executive support, (3) some public support (or low opposition) and (4) one of the following at the local level: (a) positive incentives for compliance, (b) costs for non-compliance, (c) market incentives for compliance, or (d) extra-judicial allies seeking change (typically using the Court's decisions as cover for pursuing their own agendas). He adds that the "fault lies not merely with the message but the messenger itself" (1991[2007]:213), implying that the doctrinal, institutional and cultural constraints on the Court's coercive powers render it inherently ineffective as a policy-maker. Even worse, Rosenberg contends that litigation can engender backlashes and serve as "political flypaper," reversing hard won gains in the courts while forcing advocacy groups to divert precious resources from other supposedly more effective forms of advocacy, such as

lobbying and grassroots activism. As a result, groups become mired in the courts, relying on a costly and often fruitless mode of policy-making that can backfire (compare Rosenberg 1991[2007]: 341 and Forbath 1991 with Klarman 2012; Keck 2009 and Gash 2015).

As with judicial behavioralism, questions emerged. Some could not replicate Rosenberg's findings (e.g. Hall 2011). Others challenged the theoretical foundations of gap studies. Like post-positivists who objected to judicial behavioralism's overly simplistic accounts of the law in judicial decision-making, critics argued gap studies rested on a similarly impoverished conception of the role of judicial decisions in reform processes. Instead of seeing decisions as policy commands to be followed or disobeyed, these scholars contended that judicial decisions represent a source of ideas and normative claims for activists, who use court decisions and the language of rights as a part of ongoing battles to disrupt accepted status quos and pursue new policies in multiple forums. For these scholars, judicial decisions are "perhaps best viewed as the beginning of a political process in which power relationships loom large and immediate" (Scheingold 1974: 83; see also Epp 2009).

Testing the efficacy of judicial decisions as political resources hinges on assessing how activists deploy them (and the language of rights) in long-term struggles as opposed to whether they quickly shift policy outcomes. By definition, this type of analysis requires tracing how litigation and rights-based advocacy are used over time and their often subtle influences on political and policy-making process (e.g., Silverstein 2009). Equally important, the impact of judicial decisions and rights-based advocacy should be assessed at various stages in the change process, including raising consciousness, mobilizing interests, setting agendas and eventually

building coalitions for negotiating and implementing new rules. Failure or only partial success at the final implementation stage should not discount success at other stages.[2]

McCann's *Rights at Work* exemplifies this approach, although his analysis is nuanced, recognizing that the language of rights can be both a resource and constraint. Specifically, it examines the pay equity movement, which began in the 1970s when unions and individual workers started to file "comparable worth" lawsuits. Unlike prior litigation that centered on women being paid less than men for the same job, these suits focused on pay discrepancies between job categories dominated by women versus those occupied by men, arguing that women should be paid equally for jobs of comparable worth. In a few high-profile cases, including a 5-4 Supreme Court decision (*County of Washington, Oregon v. Gunther,* 452 U.S. 161 (1981)), judges seemed sympathetic to this argument. These partial judicial victories, however, were short-lived, the wage gap persisted and, by the time McCann wrote his book in the 1990s, many felt that the movement had run its course.

In evaluating rights-based advocacy in this policy area, which seems a "less likely" case for finding judicial influence, McCann argues that judicial decisions and the language rights played a significant and constructive role. So, for example, political activists used publicity surrounding the early court victories as part of mobilization efforts that used slogans like "Raises, Rights, Respect" and "Help Defend Working Women's Rights" (McCann 1994: 67). These rights-based campaigns helped shift the expectations of some women by identifying grievances and common interests as a matter of judicially recognized (and thus legitimated) rights. Equally important, activists were not ensnared in the myth of rights or the flypaper of

litigation; they understood that lawsuits were only one part in their struggle for equality and that they could be combined with other forms of pressure and advocacy in a range of forums. Consistent with this view the role of courts and litigation, some unions used the threat of litigation and lingering legal uncertainty to gain concessions and advance legislative campaigns, thereby using partial court victories as leverage in public and private settings (see also Melnick 1994). While the final policy results were not a complete success—McCann (1994: 43-44) insists that the wage gap has significantly closed at least as an indirect result of the comparable worth movement even if it has not been eliminated—the contributions of judicial decisions and related rights-based mobilization campaigns in raising consciousness, building coalitions and changing women's perceptions of fairness in the workplace were significant and lasting.

McCann sums up his findings in a "process-based Path Model" of legal mobilization. Under this Model, court decisions can catalyze movements when political opportunities and organizational resources align. Within the right opportunity structure, advocates can use even modest court victories and appeals to rights to identify common interests that facilitate collective action. McCann (1994: 137) stresses "both the specific meaning and relative power of particular legal conventions are shaped by extralegal discourse and situational factors. The relationship among [these] factors is dialectical and interactive rather than linear and mechanical" (see also Epp 2009).

Because gap studies and legal mobilization studies rest on incompatible theoretical definitions of judicial decisions, they illustrate the positivist dilemma. Yet the result of this debate has not been a complete stalemate because these studies address different aspects of

multi-faceted social, political and policy processes. Under these circumstances, their substantive findings can be aggregated to some degree despite their divergent theoretical assumptions. Specifically, when viewed from the vantage of a policy-making cycle that includes mobilization, agenda setting, rulemaking and implementation, these studies converge on the idea that litigation and rights-based politics closely resemble other kinds of policy-making and politics. So, stepping back, gap studies tell a fairly generic story of policy implementation, the culminating stage of policy change. This story identifies factors influencing the efficacy of judicial implementation that seem to apply equally to other, less court-centric modes of policymaking (Epp 2008; Barnes and Burke 2015; Silverstein 2009). Presumably the implementation of legislation or regulations would benefit from support from the other branches of government, public support (or low opposition), a small number of veto points, incentives for compliance and local allies. The one factor in gap studies that on its face seems particularly legalistic—that judges ground their policy interventions in well-established legal precedent and constitutional values—parallels the familiar public policy principle of incrementalism: namely, the less the level of change demanded by officials, the more likely it will be immediately implemented (Sabatier 1986).

The analysis of law as a political resource in *Rights at Work* dovetails with the general public policy literature, which has long-recognized that laws and regulations are not self-executing and that short term, before-and-after analysis is not the only (or even the most appropriate) test of their policy impact. As a result, public policy scholars have prominently called for studies of how laws and regulation mobilize "advocacy coalitions" that seek change in multiple forums (Sabatier and Jenkins-Smith 1999). Indeed, McCann acknowledges that his

process-based Path Model explicitly builds on McAdams' (1982) more general model of social movements (see also Epp 2009).

If implementing judicial decisions parallels implementing statutes and regulations and legal mobilization follows the same trajectory as social mobilization, why not just eschew the tired law/politics distinction entirely (as traditional legal realist once argued) and incorporate the courts into a broader analysis of policy and social change? One could argue that this is happening in political science, as new institutionalist accounts of the American administrative state broaden conventional conceptions of social policy to include things beyond welfare programs, such as tax policy and the regulation of private insurance and pension benefits. In the process, it has broken down the distinction between public and private remedies (e.g., Hacker 2002). This "welfare state nobody knows" (Howard 1999) surely includes litigation, as groups on both the left and right routinely use constitutional, statutory, administration and common law litigation to pursue a variety of policy goals (Nolette 2015; Keck 2014; Erkulwater 2006; Melnick 2018, 1994, 1983). From this vantage, we should not be striving for a new *legal* realism in political science but a new *political* realism in legal studies that treats law, courts and litigation as just one part of the complex matrix of social and policy making that underlie modern administrative states.

Towards a New Legal Realism Rooted in Political Science

The positivist and substantive dilemmas represent significant obstacles to integrating political science into a truly inter-disciplinary new legal realist movement. Yet one can accept that law, litigation and courts play political and policy functions while still recognizing their

distinctive character. Even scholars that forcefully argue that courts serve basic political and policy functions also contend that judges perform these functions in a distinct manner. Shapiro (1968:44), a law professor who has had an enormous impact on political science as the architect of "political jurisprudence," argues that courts perform similar roles as agencies in policy formulation and implementation but adds that judicial and administrative policy formulation and implementation are likely to differ. Judges tend to be generalists as opposed to specialists, they enjoy greater protections from removal than political appointees in agencies, and judges often exercise negative power by striking down laws through judicial review as opposed to positively shaping policy through the promulgation of specific regulations. Feeley and Rubin (1998) (also law professors with broad disciplinary impact) make a similar point in their analysis of the prison reform cases, arguing that policy-making is a routine judicial function. Yet they maintain judicial policy-making differs from other modes of policy-making, in part because the rule of law requires judges to use specialized modes of legal reasoning to convince other judges to adopt their decisions (Feeley and Rubin 1998: 242).

From this perspective, the challenge is framing a new legal realism rooted in political science that addresses what is distinctive and consequential about law and judicial policymaking in methodological rigorous ways while avoiding the positivist and substantive dilemmas. The first step is adopting conceptions of the law that are viable across disciplines. Put bluntly, any new legal realism that uses the tools of social science must avoid methods-driven definitions of the law that reduce it to a set of prescriptive rules. The next step is to broaden the substantive focus. To date, many legal studies drawing on political science aim to highlight the political and policy functions of law and courts. New legal realism should correct that bias and aim to

examine the similarities *and* differences between legalistic and non-legalistic (or less legalistic) modes of politics and policy-making. The irony is that this type of analysis might partially revive the law/politics distinction derided by traditional legal realism by identifying how law and its alternatives systematically differ.

There is a variety of work that makes significant strides towards these goals. Kagan's (2001) widely cited book on the style of American law offers a prominent example, which is already used across disciplines (Burke and Barnes 2018). Kagan's work rests on a typology of policy-making, which can be set forth in a simple 2x2 table, like the one below. Each cell in the box represents an ideal-type of policy-making regime, which connotes a distinct form of authority for creating and implementing injury compensation policy. The horizontal axis is the level of *formality* in defining and determining the underlying claim, meaning the degree to which decision-makers use pre-existing rules in resolving disputes. The use of rules in dispute resolution involves all the paraphernalia of legal processes: precedents, records, documents, and written procedures. Informal processes, like negotiation among parties or decision-making by appointed professionals and elected political officials, do not require pre-existing rules because decisions are made on a case-by-case basis in light of the relevant parties' judgment and bargaining power. The *vertical* axis is the degree to which centralized actors drive the process from the top down as opposed to the parties and their representatives from the bottom up. The result is four ideal types of policy-making and dispute resolution:

[Insert Table 1.1 here]

Professional or political regimes in which a designated government official makes decisions based on their judgment on a case-by-case basis. The governors of the Federal Reserve provide an example in which designated officials make decisions on interest rates based on their assessment of the economy. They are not bound by prior precedents or approaches.

Negotiation regimes in which stakeholders or their representatives bargain over policy solutions, as in Congress.

Bureaucratic legalism is formal and hierarchical and connotes an ideal Weberian bureaucracy that centers on civil servants implementing formal rules from the top down, as in the case of government-run social insurance programs where government officials determine compensation according to pre-existing medical criteria and payment schedules.

Adversarial legalism features formal but participatory structures, meaning that the parties to the underlying dispute drive the decision-making process. In these regimes, parties dominate policy construction from the bottom up by arguing over the meaning of substantive standards and procedural rules, the application of those rules to the decision at hand, and even the justice of the relevant rules and procedures as in the American tort system.

Kagan argues that these structural differences tend to correspond to distinct styles of decision-making and policy implementation, which generate different levels of legal uncertainty and administrative costs. In adversarial legalism, for example, the decision-makers (judges and juries in the American civil litigation system) are not tightly bound to a centralized higher

authority and a premium is placed on tailoring decisions to specific circumstances. In bureaucratic legalism, civil servants who are bound to a centralized authority and emphasis is placed in the uniform application of rules across cases. Given its decentralized nature, the rules are constantly in dispute and changing under adversarial legalism, as the parties argue of what rules should apply and how they should be interpreted. As a result, adversarial legalism is likely to be more unpredictable and administratively costly than bureaucratic legalism.

My own work with Thomas F. Burke (2015) squarely falls within the new legal realist approach by adding an explicitly political twist to Kagan's work. While Kagan compares the administrative trade-offs associated with relying on adversarial versus bureaucratic legalism, we compare the interest group politics of policies that are structured around rights, courts and litigation with policies that do not have this structure. Specifically, the analysis focuses on the field of injury compensation, which, like many areas of American social policy, includes a vast array of policies of diverse design, some based on adversarial legalism, others on bureaucratic legalism. Applying this comparative strategy to quantitative data on four decades of congressional hearings and three historical case studies, we find that by organizing social issues as discrete disputes between parties, adversarial legalism seems to individualize politics by assigning fault to specific entities, and creating a complex array of winners and losers. That over time creates a distinctively fractious politics, in which interest groups associated with plaintiffs and defendants fight not only each other but amongst themselves as well. This pattern is particularly pronounced when we compare it to the political trajectory of bureaucratic policies that compensate for injuries. There we see moments of great contention, but long periods of relative peace, and greater solidarity among interests.

The common thread is that these studies are (a) inherently comparative across settings that rely on law and courts to different degrees; (b) focus on the consequences of institutional choice; and (c) allow for multiple and mixed methods approaches for analysis, including the use of natural experiments (e.g., Barnes and Hevron 2018). These elements are promising ingredients for a new legal realism that uses political science to speak across disciplinary boundaries.

Conclusion

The "old" legal realism inspired political scientists to apply rigorous methods to study law and courts and examine the ways that law and courts are "political." This work has yielded significant achievements by creating new data sets, applying advanced methods in legal studies and rejecting a simple law/politics dichotomy. After reading this literature, it is impossible to believe in the ideal of syllogistic legal reasoning in which judges find the law and mechanically apply it or the myth that rights are self-executing and above politics. Yet legal studies rooted in political science sometimes have stressed methods over theory, resulting in the positivists dilemma in which scholars rigorously test simplistic conceptions of law-as-prescriptions and engender theoretical standoffs over the interpretation of their findings. Moreover, by emphasizing the political dimensions of law and courts, political science-oriented legal scholars sometimes have lost sight of how law and courts provide a distinctive medium for pursuing politics and policy and how that medium might matter. The resulting substantive dilemma does not yield a conceptual impasse; it produces an existential crisis. If law is just politics, why study the law as a separate topic at all?

A new legal realism that seeks to leverage political science should heed these lessons. One path forward is building on comparative work that traces the consequences of relying on different modes of policy-making, some more legalistic, some less so. This type of analysis avoids the positivist dilemma by treating the law and courts as a particular institutional setting (as opposed to a set of formal commands) and the substantive dilemma by assessing how more legalistic policy regimes engender distinct administrative, political and social trade-offs. An added bonus is that these issues lend themselves to qualitative and quantitative analysis as well as observational and experimental methods. The ensuing research agenda would invite multiple and mixed-methods scholars to contribute to our understanding of how and why the law matters in politics and policy-making, whether these scholars happen to be in political science departments or law schools.

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Endnotes

[1] Whether comparison between control and treatment groups is appropriate for causal inference on two assumptions: Strongly Ignorable Treatment Assignment (SITA) and Stable Unit Treatment Value Assumption (SUTVA). SITA requires the absence of unmeasured confounding variables. Assuming SITA is met, SUTVA requires two further requirements to be satisfied for making a causal inference based on a comparison between the treatment and control (or non-treatment) groups. The first is that there is non-interference between units, meaning that the treatment received by the treatment group does not affect the units in the control group. The second is that the treatment is consistent within groups.

[2] It is worth noting that Rosenberg explicitly recognizes this possibility in *The Hollow Hope*, acknowledging that courts can have *indirect* effects on social policy. However, Rosenberg generally argues that groups are unlikely to mobilize around decisions if they are unaware of them and many judicial decisions are obscure. Using this logic, he controversially dismisses the potential indirect effects of *Brown v. Board of Education* on the grounds that it garnered little media attention at the time—a point that has been vigorously disputed by subsequent analyses.