# Goodbye to Pluralism?
# Studying Power in Contemporary American Politics

Prepared for Presentation at the Wildavsky Forum for Public Policy, Goldman School of Public Policy, April 2015. This essay draws in part on Pierson 2015 and Pierson forthcoming, and is deeply indebted to my on-going collaboration with Jacob Hacker. I am grateful for discussions of these issues with Terry Moe, John Stephens and Kathleen Thelen.

The efforts of contemporary political scientists to understand American politics reveal a striking paradox. On the one hand, "Americanists" have documented a rapid increase in the inequality of political resources. On the other, they have largely failed to detect inequalities of *power*. Most political scientists continue to insist that we lack clear evidence that the inequality of political resources translates systematically into markedly unequal influence over American government.

For most non-inhabitants of the political science village the inference is probably obvious: so much the worse for political science. I'm inclined to agree, but as a fellow villager I'm forced to pause. These are very smart folks working at this very hard, and they are inclined to let their data do the talking. Moreover, many of them *went looking* for unequal influence. Nonetheless, they were unable to track it down in a manner that met their standards of evidence. That leaves two possibilities. One is that they're largely right – which would suggest that concerns about rapidly rising political inequality are overblown. Or they're mostly wrong, which is the argument I want to make today.

This isn't just a quarrel among villagers. Explaining why I think the political scientists are mostly wrong requires a careful examination of how power is exercised in the American political system. Political scientists have mostly missed unequal influence because they've looked in the wrong places. If influence is hiding then we need to know where it is hiding. Looking in the right places reveals not just political inequality, but a distinctive take on the processes that are at the heart of modern governance. This in turn has implications for how we think about whether influence has become *more* unequal, and what needs to be done if we in fact consider inequalities of power to be a problem.

After briefly describing the central paradox in a bit more detail, the bulk of this essay explores how best to think about influence and how best to assess its distribution. I then briefly apply the argument to one important realm in American society, the provision of health care, before drawing out a few tentative conclusions about the distribution of influence in contemporary American politics.

*The Growing Inequality of Political Resources*

Let's start back in the mid-1970s, a moment that many would later identify as the turning point. At the time, few observers of American politics would have denied that there were substantial inequalities in political resources. This would have been true even among committed pluralists who nonetheless argued that the more important point was that power was widely dispersed. Indeed, it was during this period that two of the discipline's most prominent pluralists, Robert Dahl and Charles Lindblom, essentially defected to the side of their previous critics. In 1976,

revisiting their earlier work (which had been central to the pluralist cannon), they expressed growing frustration with the American polity. In particular, they suggested that they had underplayed the significance of economic inequality for our politics:

> [W]ealth and income, along with many values that tend to cluster with wealth and income, such as education, status, and access to organizations, all constitute resources that can be used in order to gain influence over other people. Inequalities with respect to these matters are therefore equivalent to inequalities in access to political resources. Inequalities in access to political resources in turn foster inequalities in influence, including influence over the government of the state. More concretely, the present distribution of resources in the United States presents a major obstacle to a satisfactory approximation of the goal of political equality" (Dahl and Lindblom 1976, pp. xxxi-xxxii).

Dahl and Lindblom went on to argue that their pluralism had also dramatically underemphasized what they called the "privileged position of business":

> "we made another error – and it is a continuing error in social science – in regarding businessmen and business groups as playing the same interest-group role as other groups… Businessmen play a distinctive role … that is qualitatively different from that of any interest group. It is much more powerful than an interest group role" (Ibid p. xxxvi)

For Dahl and Lindblom the conclusion was obvious: economic inequalities needed to be *reduced* substantially to fulfill the pluralist promise of a reasonably democratic polity. Needless to say, this didn't happen. The mid-1970s, when these leading pluralists shifted to a much more critical stance, coincided with the *low point* in modern economic inequality in the United States. As is now well known, the United States has undergone a very substantial growth in inequality. Moreover, it is an inequality of a very particular kind – one where the distributional winners are highly concentrated at the very top of the income distribution. Since the early 1970s the income share of the top 1% has more than doubled, reaching roughly 20% of total national income. The income share of the top .01% (excluding capital gains) has more than quintupled (Piketty and Saez 2015).

Just as Dahl and Lindblom (along with most everyone else) would have anticipated, growing inequality of income translated into growing inequality of other political resources. Campaign spending has gone way up over the past generation. Campaign contributions have become dramatically more concentrated. The top .01% accounted for 10-15% of federal campaign contributions until the early 1990s. In 2012 they accounted for *40%* (Bonica et al 2013). Even that extraordinary figure excludes "dark money" contributions to 501c(4) "charitable"

organizations that are almost certainly coming overwhelmingly coming from the same group.

The shift of campaign finance to the wealthy continues to gain speed. In late 2014, for instance, the Koch brothers network – essentially a "rich peoples' movement" organized by the country's second wealthiest family – announced that it planned to raise almost $1 billion for the 2016 campaign. This figure is comparable to the recent election-year war-chests of the two major political parties. By early 2015, headlines that would have seemed bizarre a decade before became so commonplace they escaped notice or were met with shrugs. Jeb Bush requested that individual donors not give *more than $1 million* "for now" (presumably because of the "optics"); million-dollar bundlers complained that they were being frozen out of the "invisible primary" because candidates only had time for the billionaires.

Campaign spending gets the headlines, but it is (revealingly) not the main story. If corporate investments are indicative, lobbying is a far more important domain for the exercise of influence. Drutman reports that the ratio of reported corporate lobbying to corporate PAC spending has fluctuated around a figure of 13-1 since 1998 (Drutman 2015, p. 17). That is, for every dollar these corporations spend on campaigns, they spend thirteen on efforts to influence whoever ends up in power.

Here, too, the imbalance of political resources is huge and growing. Unfortunately, serious (if still limited) reporting requirements did not kick in until 1998. As a result, we only have data on roughly the last third of the period that has been broadly marked by growing inequality and growing business mobilization. In real terms, reported lobbying has more than doubled in the last fifteen years.

Drutman calculates what he calls the "countervailing power" ratio of spending by corporations and trade associations vs. spending by labor unions and "diffuse interest groups" such as environmental and consumer groups. Since 1998, that ratio has grown from 22-1 to 34-1 (Drutman 2015, p 13). Many groups lobby, but the concentration of reporting lobbying effort is extreme. The Sunlight Foundation, for instance, tracked the lobbying expenditures of 200 giant corporations from 2007-2012. They accounted for just 1% of the entities reporting lobbying during the period, but 26% of the lobbying expenditures.

Lobbying has not only changed in scale; it has shifted to involve more intimate linkages between those seeking favors and those who govern. Lobbyists with prior government experience in Congress or the Executive Branch now represent 44 percent of all active lobbyists, up from less then 18 percent in 1998. Roughly half of members of Congress now become lobbyists, up from just 3 percent in 1974. Perhaps this reflects a growing need for specialized knowledge, but there is significant evidence suggesting that it is more about access than expertise. The market price of congressional staffers on K Street, for instance, drops considerably when their former boss vacates a key legislative position (cite).

Thus the first part of Dahl and Lindblom's formulation has held true – more unequal economic resources means more unequal political resources. Not surprisingly, the public is convinced that power has followed the money. In 1964, Americans agreed, by 64-29%, that government was run for the benefit of all the people. By 2012, the answer had flipped, with voters saying by 79-19% that government was "run by a few big interests looking after themselves." (Edsall)

*Political Scientists Dissent*

From this conclusion mainstream political science dissents, exhibiting skepticism that unequal resources translate into unequal influence. Most empirical studies have failed to support the popular wisdom and cast doubt on the power of organized interests. More fundamentally, political scientists have gravitated away from the very concept of power (Moe 2005). In the field of American politics – which plays a leading role in shaping the contours of the discipline as a whole – power and influence remain elusive, unhelpful, and marginalized concepts.

Let's start with the empirical findings. When Americanists have gone looking for "power" – decisive political advantages for those with more resources – they mostly haven't found it. The results can be summarized in a few basic points:

- There is very little evidence showing that campaign contributions systematically affect roll call votes in Congress (Hall and Wayman).

- Jim Snyder and Stephen Ansolabehere provocatively asked, "Why is there so little money in American politics?" They found that although this was partly because donating raised big collective action problems, it was equally because money seemed to make little difference to electoral outcomes (Ansolabehere, de Figueredo and Snyder 2003).

- The supposed clout of big interest groups has also met with skepticism. A systematic analysis centering on the nation's leading business association, the Chamber of Commerce, found that when the business community was unified on an issue that reached the political agenda they were not particularly likely to be successful (Smith 2000).

- Lobbying is overrated. There is little systematic evidence suggesting that it influences roll call votes. At most it seems to "buy time" encouraging legislators to put more effort into things they already supported [Hall and Wayman] This result was echoed recently in a broad and sophisticated study of lobbying from some of the leading scholars of interest groups (Baumgartner et al 2009). They found "virtually no linkage between [group] resources and outcomes."

Nor is the elusiveness of power just an empirical matter. More fundamentally, power doesn't really fit in the leading frameworks for studying American politics. Crucially, these frameworks typically start with the American voter and work their way out from there. The electorate's views are usually regarded as a strong constraint on policy-makers. Those views fluctuate back and forth over a moderate policy space. Thus voter preferences operate like a thermostat, bringing the political system back to the middle – a feature that is reinforced by a tendency for the public "policy mood" to lean against the orientations of the President (Erikson, MacKeun, and Stimson 2002).

Once you place voters at the center of political analysis, talking about power doesn't make a whole lot of sense. As I will explore later, "power" is a concept that is typically built around strategic behavior, often of groups. It is a matter of moves taken (or not taken) in anticipation of their effects on outcomes, given the expected moves of other groups. Voters, for the most part, are not engaged in that kind of strategic behavior – actions are individual and private. And of course (apportionment and gerrymandering issues aside) voters get more or less an equal say. Blocs of votes carry the day because they are larger, not because they are more powerful. If voters are sovereign, it makes more sense to talk about the balance of preferences than the balance of power.

A key consequence is that the frameworks political scientists have built out from voters typically depict politics as fluid or "plastic." Elections (and much else) follow a Downsian logic; this cycle's loser adjusts and becomes next cycle's winner. Take out incumbency, David Mayhew observes, and presidential elections over the past century or so have been essentially a coin toss between the two parties (Mayhew 2002). Legislatures are under the sway of Arrow's paradox of voting so that losers in any legislative struggle are well positioned to cycle back into the winner's position. Whether the focus is on voters, legislatures, or parties, temporary rather than durable advantages appear to be the rule. Jacob Hacker and I have suggested that the dominant frameworks treat politics like the movie Groundhog Day. After each day, Bill Murray wakes again to find himself in Punxsutawney, nothing important has really changed, and all the participants just start over (Hacker and Pierson 2014).

It is all a long way from Harold Laswell's famous definition of political science as "the study of who gets what, when and how."

*Community power revisited*

The marginalization of power was not always characteristic of political science. On the contrary, the debate between pluralists and their critics over the nature and distribution of political influence is one of the most famous in the discipline's history. Even after a half century, the "community power debate" remains the best place to begin a reexamination of the topic of political influence.

Not only did the debate highlight mistakes that still undermine much research on the subject, but by specifying key ways in which power operates, pluralism's critics provided a basis for placing power back at the heart of political analysis.

The argument over pluralism remains sufficiently familiar that the broad contours need only to be quickly recapped here. Pluralists such as Dahl and Lindblom maintained that power was widely dispersed in modern polities (Dahl 1961; Dahl and Lindblom 1953). They stressed that the existence of a variety of political resources and the potential access to diverse venues of political activity (especially in the American separation-of-powers system) prevented the concentration of power. Influence was not equally distributed, but it was widely dispersed.

Critics countered that this analysis rested on an overly narrow conception of power (Bachrach and Baratz 1962; Crenson 1971; Lukes 1974) – specifically, forms of influence that were visible in open contestation over political alternatives. The anti-pluralists insisted that this open contestation was only the "first" dimension of power. They argued that there were other dimensions that were less visible but more significant. Typically, these are called the second and third dimensions.

The second dimension refers to cases where competing interests are recognized (at least by the powerless) but open contestation does not occur because of power asymmetries. This dimension, encapsulated in the overarching term mobilization of bias, was more than a bit fuzzy in most formulations. It can usefully be divided into two quite distinct components, which highlight different dimensions of potential influence. The first is what can be termed "non-decisions." It refers to the ways in which formal or informal decision rules may favor some actors' concerns over others. In coining the term, Bachrach and Baratz follow E. E. Schattschneider, whose original formulation remains worth quoting:

> A conclusive way of checking the rise of conflict is simply to provide no arena for it or to create no public agency with power to do anything about it … All legislative procedure is loaded with devices for controlling the flow of explosive materials into the governmental apparatus. All forms of political organization have a bias in favor of the exploitation of some kinds of conflict and the suppression of others because organization is the mobilization of bias. Some issues are organized into politics while others are organized out. (Schattschneider 1960, 69)

In contemporary social science we would say that this dimension of influence refers to agenda control. It is now well understood that this is one of the principal ways in which institutions may advantage particular actors. McKelvey's (1976) pathbreaking work demonstrated that, given realistic assumptions about the distribution of preferences, the structure of agenda control could determine the final outcome. McKelvey's work catalyzed a rich literature. The allocation of agenda

control can indeed effectively organize some issues (or groups) into politics while others are organized out.

The other central mechanism in the second dimension is that of *anticipated reactions*. Here, too, potential issues are "organized out" of politics, but the way in which this happens is fundamentally different. Sometimes open contestation does not occur because the weaker actor rationally chooses not to engage in light of their weak position. Contestation is costly, both because of the need to expend resources and, if you are weak, because of the prospect that the powerful will retaliate. To underscore what we are talking about, retaliation can mean the loss of a job, social ostracism, or physical violence against you, your family, or friends. Given these costs, choosing not to act may be completely reasonable if defeat seems likely.

The crucial point is that the decision not to contest takes place in the shadow of power relationships. Looking at "open conflict" reveals a tiny sliver of power, and a misleading one at that. If a slave chooses not to rebel, we should not take the absence of open contestation as a sign that there is no power involved. Again, this dynamic is widely appreciated in some modern contexts – anticipated reactions feature prominently in standard game theoretic analyses (such as those employed in the study of international bargaining, or the analysis of presidents' veto powers). It is not, however, well-integrated into core understandings of political influence in democratic polities, because it has limited relevance to the act of voting that is seen as the cornerstone of these systems. Voting is private, and for the most part voters don't have to worry about the reactions of the powerful to their use of the franchise.

Finally, critics of pluralism pointed to what is typically termed the third dimension concerns ideational elements of power. Powerful actors can gain advantage by inculcating views in others that are to their advantage. In essence, this involves what Marx termed false consciousness. Those with influence over the media, schools, churches, think tanks, or other key cultural institutions may foster beliefs in others (about what is desirable or possible) that serve the interests of the powerful. Again, what looks like consensus on the surface may reflect underlying inequalities of influence.

I am going to say nothing more about this third dimension today. This is not because I think it is unimportant – on the contrary I'm increasingly convinced that it is very important – but because we will have plenty on our plate without getting to the thorny issues involved in the study of power and ideology.

Pluralists and their critics disagreed about how power is distributed in large part because they disagreed about where to look for it. Pluralists had insisted that the focus should be on open conflict. As the pluralist Nelson Polsby (1980) argued, looking at who prevailed in decision making "seems the best way to determine which individuals and groups have 'more' power in social life, because direct conflict between actors presents a situation most closely approximating an experimental test of their capacities to affect outcomes."

Articulation of the three dimensions of power represented a powerful assault on this conception of influence. The core theme of the anti-pluralists was that surface appearances were just that – appearances. If taken at face value, they were likely to be highly misleading guides to the structure of power in a society.

The force of the anti-pluralist critique rested on a critical insight: the exercise of power will often not take the form of open contestation. Indeed, the point can be put more strongly: *on issues where the distribution of power among competing interests is quite unequal we should expect to see little or no open contestation*. Instead, some combination of agenda control, anticipated reactions, and cultural manipulation mutes conflict and restricts it to a much narrower and less fundamental subset of potential issues.

Of course, there will still be open conflict. But most of the time clashes occur only on those matters, and between those political actors, where the balance of power is (believed to be) relatively even. If a society is marked by considerable inequality, the visible subjects of open contestation will often be matters that are unrelated to those power distributions, or affect them only at the margins.

It is thus unsurprising that on this skewed subset of possible conflicts empirical research would reveal no clear pattern of outcomes. Pluralists were, and still are, looking for power in all the wrong places. Their methodological insistence on studying open conflict – which most Americanist studies of influence have followed – systematically biased their results. As the Swedish sociologist Walter Korpi (1985) summarizes, "since the probability of manifest conflicts decreases with increasing differences in power resources between actors, to focus the study of power on situations involving manifest conflicts considerably increases the likelihood of discovering 'pluralist' power structures."

The original critics of pluralism were right on a crucial point: most of the iceberg of power hides below the waterline. Consider one of the skeptical results I discussed earlier, Mark Smith's (in many ways excellent) study of the limited success of the business community, even when unified, in open political contests. Smith is focusing on the iceberg's tip. He is not examining *all* issues where other social actors might oppose the interests of a unified business community. He is looking only at the much more restricted set of issues on which (once anticipated reactions, tilted playing fields, and other obstacles are taken into account) other political actors believe they have a reasonable prospect for success and in fact manage to push their concerns onto the political agenda. A moment's reflection suggests that it will be a pretty unusual set of issues that are going to pass that test. In the absence of a big shock, which alters the balance of power in fundamental ways, we should expect high-visibility political conflict to emerge *only* where the power resources of contending forces are relatively even. Thus, when examining the smallish visible tip of the iceberg, we should *expect* to see no clear pattern.

Incisive as it was, the critics' insight proved to be a double-edged sword. The pluralist counterattack, launched through a series of influential rebuttals, boiled down to a single formidable response: you can't study what you can't see (Polsby 1980; Wolfinger 1971). Because anti-pluralists seemed to focus on what didn't happen, they could not systematically observe the mechanisms they asserted were operating. To the pluralists, their critics' claims that the strong were silently dominating the weak were little more than ideological conceits masquerading as social science – a series of assertions about all the (progressive or radical) things that mass publics would implement in the absence of hidden structures of power.

By the late 1970s, the debate seemed to have reached an impasse, and most political scientists were ready to move on. As Moe (2005) notes, "The community power debates of the 1960s, combined with the large and contentious philosophical literature on power, seem to have convinced much of the discipline that power cannot be defined or studied rigorously." Equally important, the behaviorist and rational choice revolutions were shifting the discipline's focus to a much more atomized vision of politics, emphasizing individual choice as well as cooperation around mutually beneficial institutional arrangements. The rise of experimentalism, by encouraging political scientists to focus on individual behavioral responses to various "treatments", has reinforced this broad shift in theoretical orientations.

## A Structural and Historical Approach to Studying Power

When the anti-pluralists argued that a focus on open political conflict was likely to miss most of the story about power relations, pluralists' most effective response (you can't study what you can't see) was methodological. It is worth noting at the outset that this is a pretty defensive posture. The pluralists didn't really deny that such subterranean inequalities might exist; they simply maintained that there was no way to know. Ironically, even as power has receded as a concern within the discipline we are actually in a much stronger position today to identify the kinds of influence explored by pluralism's original critics. Theoretical progress has made some of the claims of the anti-pluralists more tractable. Social scientists now have the capacity to see much more of what lurks below the waterline.

Regrettably, much of political science has turned in exactly the wrong direction. It has moved away from more "structured" frameworks toward ones that are more atomized and fluid, drifting from exploring systems of organized interest intermediation toward a behaviorist and electoralist focus on the links between voter preferences and their representatives. Most political scientists now see the interface between politicians and voters, mediated by the structure of electoral and legislative institutions, as the heart of politics – indeed, almost its entirety. At the same time, the discipline as a whole has a new infatuation with experimental and quasi-experimental methods that strongly orient research toward the investigation of a restricted set of immediately observable micro-level phenomena.

For the study of power, I will argue, these are unfortunate turns. Power is built into core institutional and organizational structures of societies. This kind of influence can be made visible, but only through theoretically grounded analysis and appropriate research designs attuned to what lurks below the immediately observable behavior that preoccupies most contemporary political science. The methodological implications are clear. Open conflict is not the best place to look for evidence about the distribution of power, unless those conflicts are treated as a subset of the observable implications of theories attentive to more subterranean processes as well.

Why has influence become a more tractable problem? Theorists have largely succeeded in unpacking and investigating the two distinct dimensions of the second face of power (agenda control and anticipated reactions) discussed above. We have a much richer appreciation for the importance of agenda control and how particular rule structures allocate authority over agendas. We now know that particular institutional arrangements will systematically favor the representation of certain views and interests. Consider two fundamental and well-researched examples:

- The construction of independent central banks is likely to durably shift monetary policy in predictable ways, by empowering particular sets of actors and reducing their vulnerability to particular kinds of political pressure (e.g., Franzese 1999).

- Legislative leaders can use their power of "negative agenda control" to keep items off the agenda that would divide their coalitions, obtaining outcomes that would not be sustainable otherwise (Cox and McCubbins 1993).

The same holds true for the idea of anticipated reactions. Recognition of the phenomenon obviously predates the rise of rational choice institutionalism (Friedrich 1963). Still, game theory has given social scientists a more sophisticated understanding of the role of anticipated reactions in politics. This in turn has encouraged the development of techniques for studying bargaining power that treat "non-decisions" as a completely expected and researchable aspect of politics (Cameron 2000). In practice, systematic attentiveness to anticipated reactions can provide political scientists with a powerful means of identifying shifts in the distribution of influence in important settings (Broockman 2012; Hacker and Pierson 2002).

For the most part, however, these theoretical developments have failed to reinvigorate the study of power. Instead, they have uneasily coexisted with the broader turn toward an atomized, micro-oriented and power-free political science. They have been applied in a limited way to a limited set of problems, operating more or less at the margins of discussions emphasizing cooperation, responsiveness to citizen preferences, and the general fluidity of political arrangements (Hacker and Pierson 2014).

Rather than shoved to the margins, power should be at political science's core. How to do this is the subject of the rest of this paper. I argue that a revitalized study of influence requires two main intellectual moves. The first is to shift the focus of inquiry to contestation over durable structures of governance – institutions and public policies. These structures represent the institutionalization of advantage. The second is to recognize that such a focus requires more historical methods, examining the development of these structures over time.

*The Institutionalization of Advantage*

At the heart of political power are the efforts that winning coalitions will typically make to institutionalize their advantages. That is, they use their power to change "the rules of the game" to create further advantages down the road. These rules include both formal and informal institutions, as well as public policies. This claim is a theoretical one, but it has important methodological implications.

The idea that power in politics is generally about the institutionalization of advantage was the core of Terry Moe's broad critique of rational choice institutionalism a decade ago (Moe 2005). He argued that the variant of institutionalism rational choice scholars imported from economics subordinated questions of power. Instead, they stressed how institutions facilitated coordination, enforced commitments, and facilitated gains from trade, rested on an assumption of *voluntary* exchanges. Although some might gain more than others, everyone was made better off (or at least not worse off) as a result of these arrangements. If individuals weren't better off, they would simply choose not to participate.

Moe countered that while these frameworks generated crucial insights about how institutions were valuable to particular political coalitions, they ignored a crucial feature of politics. Unlike the case of market exchanges, in politics a winning coalition gets to use political authority, and it can use it to impose outcomes on losers. These losers often have no viable exit option. Ignoring (or downplaying) this crucial difference from (idealized) market transactions misses much that is at the heart of politics.

The implications of Moe's insistence that in politics winners can exercise authority over losers run deep (Gruber 2000). Most fundamentally, it suggests the need to recognize that political contestation is *both* a battle to gain control over political authority *and* a struggle to use political authority to institutionalize advantage – that is, to lay the groundwork for future victories. In short, it calls for an appreciation of how political influence is often invested. The exercise of authority is not just an exercise of power; it is potentially a way of generating power.

In politics, the most famous of these institutional arrangements dictated by victors is democracy itself. Democracy was a new configuration of authority that – where it became stable – durably altered the rules for allocating political authority.

New decision rules diminished the value of political resources based on the possession of property or coercive capacity and increased the value of resources based on sheer numbers (Rueschemeyer, Stephens and Stephens 1993). Daron Acemoglu and James Robinson have recently developed a popular version of this argument. They identify the establishment of democratic institutions as the decisive "cut-point" in political history, institutionalizing a set of durable advantages (at least compared to previous political regimes) for ordinary citizens (Acemoglu and Robinson 2012).

Specific constitutional arrangements can have similar effects of durably advantaging particular actors, for instance by creating super-majority requirements for revision (Starr 2015). A large literature in comparative politics has developed around the crucial institutional divide between electoral institutions that enshrine majoritarian and proportional representation systems. Considerable research has demonstrated how particular coalitions chose to entrench one system or the other, depending on their prognostication of the long-term political effects (Iversen, Cusack and Soskice 2007).

In the United States, key constitutional choices at the founding involved a similar institutionalization of advantage (Dahl 200x; Robertson). Notable were those surrounding federalism. Too often, we mythologize the founding and treat its core features as results of some consensus view of optimal design. Far from it. The battle over the role of the small states was fierce, and the man usually celebrated as the constitution's main craftsman ended up on the losing end. Madison argued for the principle of equal representation of citizens in the Senate – his opponents defended their interests to the hilt, and won. Indeed they won so big that they made sure the rules for constitutional amendment essentially insured the permanence of a system of mal-apportionment unequaled in modern democracies.

Yet the basic point about institutionalizing advantage extends well beyond basic constitutional rules, or the direct impact of rules on how votes are counted. In modern democracies *the main mechanism for institutionalizing advantage is public policy*. Winners get to impose their policy preferences on losers. Often, this means imposing arrangements to which losers must adjust *even if their side wins future elections*. Policies create facts on the ground, durably altering resources and incentives. Policies can strengthen supporters and weaken losers. In extreme cases, policies can effectively eliminate the losers as a serious force altogether.

The establishment of new policy arrangements may constitute a kind of mini-constitution in a particular domain of social life. Eskridge and Ferejohn (2001) coined the term "super statutes" to distinguish extraordinary laws that exert a strong gravitational pull on jurisprudence and norms. When one looks more broadly at the capacity of policies to remake political circumstances the ranks of mini-constitutions expand dramatically (Pierson 2006). In Eric Patashnik's *After Reform*, for instance, airline deregulation was cemented in part by eliminating the Civil Aeronautics Board, the regulatory venue where the old-line airlines had their

greatest leverage. At the same time, the new legislation unleashed market forces that induced a war of attrition, steadily removing the high-cost airlines (who were deregulation's strongest opponents) from the playing field (Patashnik 2008).

This basic insight about policy coalitions—once so deeply held that analysts felt little need to make it explicit—is at the heart of long traditions of more macro-oriented work, both in comparative politics and American political development (Gourevitch 1986; Skowronek 1993). Shifting coalitions of interests battle to exercise authority in order to impose their preferences through governance. The potential for policy trajectories to be highly path-dependent makes these efforts profoundly important. It is why comparativists can identify distinct "regimes" covering huge areas of public life like the welfare state and a nation's model of capitalism (Esping-Andersen 1985; Esping-Andersen 1990; Hall and Soskice 2001; Huber and Stephens 2001; Pierson 2004; Thelen 2005). These regimes are grounded in durable policy arrangements, resulting from fierce contestation among organized interests.

Elections matter in many of these cases, but they are only one part of a much broader and deeper political process. Major policy enactments *are* the mobilization of bias. Although these policy initiatives are often strongly connected to one party or another at the outset, these arrangements are sustained over time by supportive coalitions that have transcended and outlasted any specific electoral majority. Their endurance is testament to the capacity of long-lived political actors to use government authority to refashion economies and societies in enduring ways.

New institutions or policy regimes are often the main prizes awarded to the victors during critical junctures (Hacker and Pierson 2014). These new arrangements create advantages for certain actors over others, organizing some issues in and other issues out. They can often generate feedback effects that reinforce the advantages of winners over time, transferring resources, necessitating or underwriting social investments, and sending signals about likely outcomes that can encourage individuals to switch sides or adapt (Pierson 2015).

It is worth noting explicitly that this discussion of policy coalitions exerting (and building) power through control of governance shifts the focus of political analysis from voters to organized groups (Hacker and Pierson 2014). Most of those involved in politics in a sustained way participate because they care what government does. Again, politics is a contest where some gain the authority to make decisions of fundamental significance for others. This makes the exercise of authority a central object of political contestation.

Yet effectively exercising political authority to remake the structures of opportunity is a daunting challenge. To do so requires the capacity to overcome collective action problems, mobilize resources, coordinate actions with others, develop extensive expertise, focus sustained attention, and operate flexibly across the multiple domains of political authority. Moreover, all of this must typically be

done over long periods of time, across shifting partisan environments, despite considerable turnover of elected officials, and in the face of dogged resistance from other resourceful actors. These are not capacities we usually associate with voters. They are the comparative strength of organized interests.

Here again one can see why the shift in political scientists' focus from groups to voters has gone hand in hand with the subordination of an analysis of power. The subterranean character of power relationships means that it is simply impossible to see if one focuses primarily on elections and voting behavior. It is, instead, illuminated by the examination of group-based, long-term contestation over policy outcomes.

Consider one brief illustration from the field of comparative political economy. A central expectation of those studying the topic of inequality from a perspective that emphasizes the preferences and behavior of atomized voters is that of Meltzer and Richard (1981). Rising inequality skewed to the highest income groups should produce more egalitarian policies, as the median voter faces growing incentives to vote for redistribution. As Huber and Stephens have recently noted (Huber and Stephens 2012, p. 11), the logic may be elegant, but the empirics are "plain wrong." It is the *most egalitarian* societies that make the greatest efforts to equalize income. Moreover, as societies become more unequal they often decrease, rather than increase, their redistributive efforts. The reason, as Huber and Stephens emphasize, is that "a greater distance between the median and the mean income tends to be accompanied by a more skewed distribution of political power and thus lower responsiveness to demands for redistribution."

Here Huber and Stephens draw on the central ideas of power resources theory, with its emphasis on what Korpi (1985, p. 36) calls "'the Matthew effect' in exchange: to him that hath, shall be given." The example clarifies why historically-oriented students of comparative politics have typically emphasized the importance of distinctive policy regimes, which either enhance or discourage pressures for egalitarianism. More fundamentally, it points to the need to focus on how power is built into durable social structures, rather than operating exclusively at the level of open conflict. Only by explaining how outcomes at key junctures produce durable (but not permanent or unchanging) shifts in social arrangements can we make inequalities of influence visible.

*The Need for Historical Analysis*

Let us return again to the central riddle of contemporary political science – the difficulty researchers have found connecting unequal resources to unequal influence. One of the most intensive of the recent attempts was an extended study of lobbying over multiple issue conducted by a team of leading interest group scholars (Baumgartner *et al.* 2009). As already noted, these researchers could find no

evidence that the side with greater resources had any discernible advantage in the policy fights they studied.

Much to their credit, Baumgartner and colleagues do not stop with the reporting of this striking finding. Instead, they emphasize a fundamental limitation to their analysis. Just as economists say that the stock price of a company may embody all the information there is about the company's value, Baumgartner *et al* suggest that the policy status quo may be said to embody the inherited distribution of power. If some groups have had greater influence over time, we should expect that the status quo *already reflects* this. Again, contemporary conflict is the tip of the iceberg. Unless the relative power of the groups that supported that policy arrangement is continuing to grow, we shouldn't expect that they will win additional open conflicts going forward, although they are likely to benefit from the advantages already institutionalized.

Existing policy is an equilibrium among contending forces. In short, Baumgartner and his colleagues conclude that their findings of little advantage stemming from the open deployment of greater political resources are consistent with a view of politics that sees underlying power resources as very unequally distributed. The implication, however, is that political scientists need to devote tremendous attention to the construction of those policy equilibria. This requires historically grounded research that explores the evolution of policy options, which groups favor particular outcomes, the conditions that allow particular alternatives to triumph, and the long-term effects of those policy enactments on the distribution of political resources and policy preferences. This is how politics happens. Moreover, it provides a treasure-trove of observations about power-in-action for an analyst who knows how to look for it. By considering multiple rounds of contestation it is possible to collect a variety of observations that allow an analyst to evaluate alternative hypotheses about the underlying political processes, including those related to "non-decisions" and anticipated reactions (Carpenter 2001; Hacker and Pierson 2002; Broockman 2012).

It is thus no accident that studies that examine policymaking over time have been much more likely to appreciate the dynamics of political power than those focusing on the electoral seesaw. Studies of this sort have been prevalent within the field of American Political Development, which has frequently focused on the efforts of political coalitions to institutionalize favored arrangements. I have already mentioned Patashnik's simple but telling example of how airline deregulation quickly drove its biggest opponents, the high-cost airlines, out of business. Moe's recent analysis of public sector collective bargaining has a very similar dynamic (Moe 2011; Moe 2015). Such processes operate on a grander scale as well. Jacob Hacker and I have sought to understand the neo-liberal turn in American public policy since 1975 as a sequence of pitched battles, policy victories (and defeats) and downstream adaptations that have broadly favored the economically privileged (Hacker and Pierson 2010).

Similarly, research in American political development on race and ethnicity has repeatedly emphasized the role of institutionalized hierarchies, cemented through policy, that proved stubbornly resistant to liberalizing developments in other domains of politics, precisely because they were deeply embedded in durable coalitions of organized actors hostile to emancipatory changes (King and Smith 2012). The collapse of Reconstruction after 1876 provides an excellent example. The bargain of 1877 (in which Republicans won the White House in a contested election, in return for a commitment to remove troops from the South) represented the institutionalization of advantage for segregationists. It led to a series of statutory and constitutional changes, consolidating a Jim Crow regime that locked southern blacks (and many poor whites) out of politics for nearly a century (Keyssar 2000). Victory over core institutional arrangements tilted the playing field for future rounds of contestation, increasing the probability of victory (or the likely scale of victories) for one of the contending parties. The bargain of 1877 was critical in bringing Reconstruction to an end because it assured that future conflicts over the Southern political economy would occur in a different arena (the states), freed from federal intervention. This new venue was heavily slanted in favor of segregationists. While they did not achieve instantaneous victory, they steadily gained the upper hand.

*Power and Powerlessness*

Again, historical analysis of this kind is vital because power is something that develops over time and simultaneously becomes less visible as it does so. The use of historical analysis to tackle this challenge was the central contribution of John Gaventa's marvelous 1982 book *Power and Powerlessness: Quiescence and Rebellion in an Appalachian Valley*. There, Gaventa develops an astute defense of the anti-pluralist position, countering the pluralists' objection that you could not study what you could not see. He presents a careful empirical study of political conflict in a setting – a poor mining community simultaneously marked by ostensibly pluralist political institutions and vast economic inequalities – conducive to identifying how influence is deployed.

*Power and Powerlessness* is a sustained methodological answer to the pluralists. Gaventa argues that one could study what wasn't happening if one clearly explicated the mechanisms through which these dimensions of power should operate and specified what the observable implications of power's exercise might be. Crucially, Gaventa highlights that these observations would have a pronounced temporal dimension. We could uncover the "hidden" dimensions of power through historical analysis. Over time, open rebellion would give way to quiescence in predictable ways, and we could study that historical process systematically.

Unfortunately, Gaventa's incisive argument came too late to exert much influence among political scientists. The conversation had already shifted away from issues of power to the study of institutions. Yet Gaventa's analysis provides the essential bridge between the community power debate – where critics of pluralism

rightly insist that political power is akin to an iceberg, with most of its mass lying under the waterline – and contemporary efforts to build theories more attentive to inequalities of influence.

Gaventa's turn to historical analysis was not just a methodological move. True, he persuasively argues that we could detect political influence through historical process tracing. Gaventa's analysis was also a theoretical move, because it stressed that important forms of influence often became amplified over time. It is in making these dynamics more explicit that we possess the best opportunities for extending Gaventa's and Moe's insights.

**Who Governs American Health Care? A Case-Study[1]**

In contrast to the pluralist approach of examining open conflict and seeing who wins, the current analysis suggests an alternative: we should strive to identify the coalitions that undergird crucial policy arrangements that distribute resources within American society. This is partly an exercise in policy analysis – an attempt to uncover "who gets what." In turning to the political question of "why", this approach draws heavily on historically grounded analysis that identifies key stages in the development of those policy structures and assesses evidence for the influence of various actors over the durable outcomes institutionalized at those stages. That assessment, in turn, must include substantial attention to the multiple dimensions of power. At a minimum, it requires systematic examination of the possibility that influence may be exercised through anticipated reactions or agenda control as well as through open political conflicts.

This alternative approach is, admittedly, cumbersome. Because every policy realm has distinctive features and complexities, it does not lend itself to easy, broad generalizations. The inferential task is daunting, involving the assembly and comparison of multiple strands of evidence to assess alternative hypotheses. In contrast to the experimental or quasi-experimental techniques that are regarded as the coin-of-the-realm in many quarters of contemporary social science, it amounts to something that is more like a detective's approach to knowledge. We need to carefully assemble clues of varying quality and rule out possible suspects.

Set against these limitations, however, are two important advantages. First, this strategy of inquiry is consistent with strong theoretical reasoning (well-supported in numerous empirical settings where it has been applied) about how we should expect influence to operate. Recall the old story about the drunk who looked for his keys under the streetlight because the light was good – even though it wasn't where he lost them. The approach outlined here consciously chooses to go where the light is dim but where we think the keys actually might be hiding. In revisiting the pluralist/anti-pluralist divide over methods, it sides with the latter, bolstered by

[1] This section draws on Hacker and Pierson forthcoming and is heavily beholden to the policy expertise of my co-author.

the recognition that we now are better equipped to meet the challenge than the anti-pluralists of fifty years ago were.

Second, the strategy of inquiry is consistent with observations about the behavior of the most highly knowledgeable participants in politics – namely, durably organized interests. Recall the description of trends at the beginning of this paper. Well-resourced interests have greatly increased their efforts over the past few decades. They direct the bulk of their efforts towards the exertion of pressure on governance – on shaping actual policy – rather than electoral contests. If we assume that these highly knowledgeable and long-time participants in politics more or less know what they are doing, perhaps we should follow their lead. That means focusing on sustained efforts to influence policy development rather than fixating on the electoral contests and open conflicts over marginal matters that they treat as a secondary priority.

The following brief sketch focuses on American healthcare policy. I really mean "sketch." Rather than develop and fully support the claims made here my goal is simply to illustrate how the approach to the study of influence advocated here might allow us to update our views about the distribution of power in American politics.

There are several reasons for choosing to focus on health care. First, health care is a big case – involving a sixth of the American economy. Although it remains "just a case", findings in this area are so substantively significant that they provide a credible starting point for thinking about the broader issue of influence. Second, ample comparative research facilitates a relatively clear assessment of how the America policy regime distributes benefits. In this case we do not have to rely on difficult counterfactuals to identify how policy structures influence "who gets what." We can simply compare the United States to other rich democracies. Finally, there have been numerous episodes over the past seventy years involving substantial attempts to adjust the healthcare policy regime. These episodes are well-documented, and there have been serious efforts by journalists, historians and social scientists to trace the relevant processes in ways that are attentive to organized power.

So let's briefly examine health care as an exploration of political influence. I will stress three main points. First, *with respect to overarching policy outcomes, the American system is an extreme outlier among rich democracies.* Viewed comparatively, American healthcare outlays are staggering. In 2012, U.S. health spending topped $2.8 trillion, which means that our medical-industrial complex is larger than the *entire economies* of all but five nations. At the personal level, health expenditures per capita (almost $9,000 in 2012) are roughly twice the levels found in our richest trading partners (the German number, for example, is $4,800).

These cost differences add up. In 1980, Switzerland and the United States had comparable per-capita spending, but Switzerland then moved more

aggressively to control costs (as well as expand coverage to all citizens). Thirty years later, the Swiss are spending about a third less per person than we are. That may not seem impressive; Switzerland spends substantially more than other European nations. Yet had the United States followed the same trajectory since 1980, Americans would have collectively saved a whopping *$15 trillion*—enough to finance a four-year college degree for more than 175 million Americans, or have eliminated all federal deficits over the same period, with room to spare.

Health spending is a function of two factors: how much care patients receive and how much that care costs. With regard to the amount of care, the United States does not look all that exceptional. It has fewer nurses, physicians, and hospital beds per capita than the OECD norm. Americans visit hospitals and physicians less frequently than do citizens of other wealthy nations, and their hospital stays are much shorter.

Rather, the core culprit is American health care prices. Consider a normal delivery. On average, U.S. insurers pay over $10,000 when a patient gives birth. Compare that with a standard price of $2,824 in the Netherlands—the country with the next highest share of its economy devoted to health care (12 percent versus the United States' 17 percent). The story is the same with hip replacement. The Dutch price is $11,187—the same for all patients. The American average is $40,364. Heart bypass surgery? $75,000 on average. Compare that with a price of $15,742 in the Netherlands. And that $75,000 is the price that *insurers* pay after negotiating down providers' opening bids.

This leads to the second key point: *Our high costs are primarily driven by extremely high payouts to health care providers, resulting from a failure to use effective countervailing power resting in public authority.* Doctors, hospitals, pharmaceutical companies and equipment manufacturers charge vastly higher prices than those providing comparable or identical services in other countries. Comparative research suggests the reason is clear: the normal price mechanism breaks down in health care, because the health care market doesn't work like other markets. Prices are mostly set through negotiations between providers and insurers—in both the public and the private sectors. The main difference, it turns out, is that public price-setting is much more effective than the private alternative.

Within circles of elite opinion, there's surprising resistance to this obvious conclusion. Every conceivable alternative gets endlessly recycled, even when the evidence is weak or disconfirming. Meanwhile, even thoughtful observers ignore or downplay the basic fact that information asymmetries make health care a sellers' market in which (absent sufficient clout on the other side) the sellers will charge much higher prices than needed to ensure the supply of high-quality services. Consider the most persistent explanation for why we spend so much more: Americans just don't have enough "skin in the game." There's a big problem with arguing that over-insurance explains our high costs: *Americans bear much more of the direct cost of care than do citizens of other nations.* So while patients with

insurance are less sensitive to prices than patients without insurance (that's the point of insurance), our high prices simply can't be blamed on the excessive protections that Americans enjoy.

No doubt the United States is doing *something* with the extra trillions that it has poured into the medical sector over the last few decades. On the available evidence, however, what it is mainly doing is paying higher rents to health care providers. Drug prices are a case in point. According to a study by McKinsey & Co., drugs used in the United States are 50 percent more expensive than the same drugs used in other nations. Branded drugs are 77 percent more expensive. All told, Americans are spending in excess of $100 billion more on drugs every year than you would expect given our national income. The same staggering excess is visible in virtually every realm of American healthcare provision.

Third point: *the failure of the U.S. to institute sufficient countervailing power to manage health care costs reflects repeated, intensive applications of power on the part of health care providers to prevent such policies from taking hold in the American healthcare system*. Over the past half-century, popular pressure (combined with escalating costs) have repeatedly encouraged government action to increase healthcare access. In this context, elections have mattered – a point that deserves emphasis. Major expansions of access in 1965 and 2010 followed huge electoral victories. These shifted the balance of power and placed pressure on the existing policy equilibrium (a similar initiative was attempted in 1993, also in the aftermath of a major electoral victory, but it failed). Yet in these cases, effective efforts to mobilize political authority to combat astronomical prices to healthcare providers failed. Indeed, they largely fell by the wayside before Congress even began to write legislation.

Analyses of the politics surrounding the Affordable Care Act have made this exceedingly clear. Steven Brill's excellent, detailed reporting provided the gory details (Brill 2015). Powerful interest groups were fine with expanding access. It meant more sales. They were even willing to negotiate some "contributions" since they would still end up ahead. But the price of their support was made clear. Knowing that Republicans were already committed to a strategy of gridlock, health care providers needed only the support of one Democrat to block reform. Essentially providers told the administration that they were confident they could block reform if they didn't get a package they liked.

The result was a clear case of anticipated reactions. Indeed the administration (eager to repeat the defeat of 1993-94) seemed to understand this from the outset. As one of the plan's key architects, Jonathan Gruber, put it: "you can either try to expand coverage or you can try to do something to control costs. But trying to control costs too much dooms whatever you do, because the lobbyists will kill you. That's what happened to Hillary in 1993."

Thus the health legislation incorporated concession after concession—most of them stripping out tougher cost-controls or moving authority to the states, which were viewed as less threatening to providers. Before the debate even began in earnest, for example, the White House and congressional leaders brokered a peace treaty of sorts with the health care barons, or at least two of the most powerful: the hospitals and pharmaceutical manufacturers. Promising not to directly regulate drug prices or significantly restrain hospital charges, Democrats received in return a promise by the two industry players for modest givebacks. Far more important, the hospital and drug organizations promised not to use their huge war chests and lobbying arms to take on the health plan.

The White House also signaled it would jettison the so-called public option, which would have created a public plan using rates based on Medicare's that would compete with private insurance plans. The CBO had projected big savings from a strong version of the plan, which was popular with the public. But within a few months, it was gone. Health care stocks shot upward on the news.

Whenever the administration tried to put in place serious cost control, observes the journalist Steven Brill, "They were stopped by the lobbyists...The only way that a bill this big will pass in Washington is if the powers that be decide that it should pass." Brill notes that at one point in the process the White House began looking for more "contributions" from the drug companies, and PhRMA's Tauzin refused. As Brill reports "Tauzin didn't budge. He knew they could never get sixty votes in the Senate if the drugmakers switched sides and began financing a different set of ads, and he said so." On this as on so much else, PhRMA won.

In short, despite major reforms to other parts of the health care system, the equilibrium of permissiveness towards astronomical prices remained. It is this outcome, the subject of no roll-call votes and largely invisible to the American voter, that helps us put the health care industry's massive presence in Washington in context:

- Since 2008, according to the Center for Responsive Politics, a remarkable 131 members of Congress have lobbied for the industry And the health care sector employs more of these revolving door lobbyists than any sector besides finance.

- In 2012, forty former staff members of Senator Max Baucus, the Democrat who chaired the Senate Finance Committee during the debate over the Obama plan, were registered lobbyists.

- Since federal lobbying disclosure began in 1998, pharmaceutical manufacturers, medical device makers, health insurers, hospitals, and medical professionals have reported spending more than $6 billion on lobbying. The American Hospital Association, PhRMA, and America's Health Insurance Plans are among the biggest

> heavyweights in Washington, rivaled only by Wall Street as a lobbying superpower.

Variants of the ACA story – occasional wins for access, but defeats for any serious effort at cost containment – are the main theme of healthcare policy development in the United States. Most of the time, victories on access are dependent on major Democratic electoral victories. The one exception is revealing. Republicans passed an expensive prescription drug program in 2003. With Democrats pushing to expand Medicare coverage to include prescription drugs, Republicans took advantage of their control of the House, Senate, and White House to push through their own plan—a huge and hugely costly expansion of a program they had once opposed. Why? Former Reagan adviser Bruce Bartlett offers a frank explanation:

> Republicans were keen to make sure that the legislation enacted was theirs, because the Democrats were certain to include cost containment for drugs in their legislation. It was widely believed that if the federal government used its buying power to pressure drug companies to cut drug prices, the cost of providing drugs to Medicare recipients would be substantially reduced.
>
> But forcing down drug prices would diminish the drug companies' profits and Republicans were adamantly opposed to that. Consequently, despite their oft-repeated opposition to new entitlement programs, they got behind the new drug benefit, now known as Medicare Part D, and made sure there was no cost-containment provision.

As I warned, this is just a sketch. A full assessment of how the structure of American health provision reflects a historically-generated distribution of influence would require a much more detailed and carefully documented investigation. It would need to lay out the extensive set of entrenched interests among provider groups developed over a long period. Most of all, it would require attentiveness to anticipated reactions. Positions espoused in open conflict may reflect concessions to hard political realities rather than revealing sincere preferences. My goal here was simply to offer a brief illustration of how such an approach to the study of influence can be implemented, how it might bring more of what's below the water-line into the analysis, and how as a result it might change our assessments of how influence is distributed.

## A Quick Comparison with Survey-Based Approaches to the Study of Influence

I have yet to mention another major body of work in recent political science that addresses similar themes and reaches conclusions broadly consistent with mine. This is a series of studies on "responsiveness" to public opinion fashioned by Martin Gilens, Larry Bartels and a number of their colleagues (Bartels 2008; Gilens 2010; Gilens and Page 2014). The core of this work compares enacted policy

changes with the views of voters derived from a vast collection of public opinion surveys. It suggests that the views of the affluent are far more likely to generate policy change than those of middle or low-income citizens, which appear to have little or no impact. A brief comparison between this "responsiveness" literature and the approach I am advocating here provides a useful point of contrast as well as transition to the final set of issues I wish to discuss.

At a surface level this responsiveness literature – marshalling doubts that there is a strong link between ordinary voters' preferences and policy outcomes – seems consistent with my take here. It. One way to connect that work and the approach I have outlined would be to say that the latter attempts to supply some of the mechanisms (anticipated reactions, institutionalization of elite preferences in existing policy structures) that account for the findings of the former. The preferences of the powerful are often institutionalized in existing policy structures, protected by factors like agenda control and anticipated reactions, and so on.

This recent work on responsiveness is incredibly important and impressive – and not just because it is broadly supportive of the argument advanced in this lecture. Nonetheless, I have some real concerns about how far we can go with the survey-based approach to studying influence. By focusing on the mass public-policymaker connection it runs the risk of replicating many of the problems in the work I've been criticizing – even as it reaches different conclusions. Surveys often reproduce the blindness to structure I've been discussing. That is, they focus largely on the issues and proposals that make it onto the agenda, typically ones that reach a high level of visibility because they seem politically plausible. There are many surveys about a minimum wage hike, but not so many about serious steps to curb executive pay or, say, to eliminate the carried interest tax provisions that provide enormous benefits to some of the wealthiest Americans (a provision about which most citizens are completely ignorant).

There is also the question of whether what looks like consensus across income groups is nonetheless a reflection of unequal influence. This could be because of the third dimension of power (ideological manipulation). It could also be because of elite control over the policy agenda. Indeed – one of Gilens's most surprising findings (that congruence with mass preferences was *relatively* high during the George W. Bush administration) highlights both possibilities. He considers the Iraq War responsive to public opinion – but arguably it shows "congruence" only because the Bush administration and its allies effectively advanced highly deceptive arguments as a rationale for the war.

Gilens reaches a similar positive assessment of the Bush tax cuts. There is considerable evidence that this result stems not from genuine consensus but from agenda control. When asked "would you like a tax cut?" people say yes, but careful examination of polling that compares the Bush tax cuts with alternative proposals (like reducing the deficit or shoring up the finances of Social Security) indicates a strong popular preference for the alternatives (Hacker and Pierson 2005; Bartels

2005).

His suspect coding of the Bush administration's two most important policy initiatives is not the only reason to question Gilens's conclusion that responsiveness has not gotten worse over time. Equally problematic is the way Gilens deals with the issue of worsening gridlock. It is well understood that gridlock is increasing – a result of both intensifying partisan polarization and greatly expanded use of the filibuster. The result is that fewer and fewer policy changes actually occur. Ironically, this gridlock increases responsiveness as Gilens measures it, because the items that do pass are more likely to be broadly popular. (Set a higher threshold for passage—80% agreement, say, rather than 60%, and of course you see broader agreement). Not only does it seem perverse to say gridlock increases responsiveness to the least advantaged. The impact of gridlock is the main basis for Gilens's conclusion that things haven't gotten worse over time. The over-time trend is going to be greatly driven by the decreasing proportion of items that pass.

Tellingly, all of these points suggest that Gilens's pessimistic conclusions about responsiveness may not be pessimistic enough (Bartels 2013). But the issues raised in this brief section clearly require more exploration than I can give them here.

**Goodbye to Pluralism?**

To say that power often is not expressed in visible conflict does not mean we cannot study it. On the contrary, we have made progress on multiple fronts over the past few decades that make exploration of what is under the waterline more feasible than it was for community power theorists. To do that, we need to look in the right places, and particularly at the persistent efforts of groups to make, sustain or remake particular policy regimes.

What does this argument imply for conversations about the distribution of influence in American politics today? I want to close by briefly noting three important implications. First, political scientists need to reboot the discussion of power. Broadening our discussion of power means recognizing that many of the discipline's empirical findings count for less than many assume. Looking only at the tip of the iceberg is bound to be misleading. At a minimum, the fixation of many analysts on voters and public opinion and the deep skepticism of most political scientists about the power of interest groups rest on a fragile foundation.

Second, the framework outlined here is in principle agnostic about the true distribution of power in American society. Knowing that much of influence is already "baked into the pie" of existing governance doesn't tell us who those political actors were. In fact, existing policy equilibria may be the result of any number of political forces, and in the massive and massively complex structures of modern governance it makes sense to think of "pockets" of power that may be quite diverse. Moe, for instance, has made a strong case that public K-12 education

constitutes such a pocket of power, built primarily around teachers' unions (Moe 2011, 2015). One could make a similar case about the clout of environmental groups embodied in major statutes like the Clean Air Act, or groups like labor and the Social Security Act.

Nonetheless, what we know about the distribution of organized power suggests that such cases may be exceptional. E.E. Schattschneider famously noted that "the flaw in the pluralist heaven is that the chorus sings with an upper-class accent." We now know just how unequally "voice" is distributed in American political life (Brady, Schlozman and Verba 2012). It seems likely that this is reflected in most policy regimes – that is certainly my view – but it is a subject for research and debate.

Third, a crucial challenge is to link the general account of power relationships I have offered to a systematic analysis of American political institutions. The vital issue to explore here is the ramifications of our unusually veto-ridden governing structure (Linz and Stepan 2011). American politics is highly biased towards the preservation of the status quo. That bias has grown even larger with the dramatic spread of the filibuster. One obvious consequence (which complicates the analysis offered here) is that policy equilibria generated under some prior set of power relationships may persist for a long time, even if the forces that created them have weakened considerably.

There is a less obvious consequence, however, that I think reinforces concerns about the "unheavenly chorus." *A strong institutional bias against enacting new exercises of political authority is not neutral among all parties*. It favors those who don't need political help, who can act on a large and coordinated scale without the use of that public authority, and who would prefer an unrestricted playing field. In short, such a system is biased in favor of economic elites. All else equal, extensive veto points favor coal companies over environmental groups. Indeed, as Hacker's important work on "policy drift" demonstrated, even established exercises of political authority will often gradually erode or even collapse without successful efforts to update them. That is, effective authority requires regular renewals of authority.

One of the most important sources of policy drift is market pressure. Hacker and I have argued that many of the big gains of the affluent over the past generation are grounded primarily in this kind of process. The blocking of efforts to update policies (related to unions, financial regulation, the minimum wage, etc.) have arguably been more important than the passage of new legislation (Hacker and Pierson 2010). Here again, much of the exercise of influence involves "non-decisions." As income becomes ever more concentrated in executive suites and on Wall Street the two big winners in our new winner-take-all economy, are content to have government do as little as possible. The difficulty of coordinating exercises of political authority in such a system differentiates it from traditional notions of oligarchy, but it is a considerable distance from any reasonable notion of pluralism.