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The Study of Policy Development

What do we mean by the term “policy history”?¹ In conventional usage, “history” refers to one of two kinds of investigation: the study of something that happened at some point in the past, or the study of how something came to be what it is. It is this second usage—the idea of policy history as an unfolding story of *policy development*—that I want to examine in this essay. Understanding the sources of policy often requires that we pay attention to processes that play out over considerable periods of time.

Thinking systematically about how social processes unfold over time has fallen into disfavor in much of the social sciences. Contemporary social scientists are more likely to take a “snapshot” view of political life, especially in areas of inquiry where “large-N” statistical methods and the analytic tools of microeconomics and game theory have been ascendant. Although it is not inherent in the use of these techniques, in practice they lend themselves to inquiries that focus on the “moves” of particular “actors” at a moment in time.

Recent scholarship on policy development, by contrast, reveals the very high price that social science often pays when it ignores the profound temporal dimensions of real social processes. Attentiveness to issues of temporality highlights aspects of social life that are essentially invisible from an ahistorical vantage point. Placing politics in time can greatly enrich the explanations we offer for social outcomes of interest. Indeed, it can expand our vision of what is worth explaining in the first place.

Of course, the social sciences have had a rich tradition of historical research. Scholarly communities devoted to extending such traditions flourish in parts of the social sciences. Indeed, some claim to witness a “historic turn” in the human sciences as a whole.² Yet in spite of this activity there has actually been surprisingly limited attentiveness to the specifically temporal dimensions of social processes. In contemporary so-

cial science, the past serves primarily as a source of empirical material rather than as the site for serious investigations of how politics happens over time.

Too often, the adoption of a historical orientation has failed to exploit its greatest potential contribution to the more systematic understanding of social processes. Especially in the field of American political development, the turn to history has been a turn to the study of what happened in the past. Here analysts study particular historical events or processes, with a focus on offering convincing explanations of specific outcomes of interest. Such investigations often greatly increase what we know about particular facets of American political history. What is less clear, however, is how particular studies fit into some broader research program. Little effort is made to suggest what, if anything, might “travel” from one investigation to another. Indeed, many historically-oriented analysts are uninterested in this question, assuming the stance of most historians—that the rich particularities of each event or process render it unique. Alternatively, these analysts seem to assume (usually implicitly) that a discussion of, say, how social movements contributed to policy outcomes in the 1930s, generates clear implications for our understanding of contemporary policymaking. Such an assumption is highly problematic. Moreover, this kind of “historical” analysis can be profoundly ahistorical in practice. Inquiry may focus on the past but nonetheless zero in too narrowly on a particular moment of time. In doing so, it risks replicating many of the limitations of social science work that ignores the past entirely.

We do, however, have another basis for connecting history to the social sciences. We can turn to an examination of history because social life unfolds over time. Real social processes have distinctly temporal dimensions. Exploring these dimensions can lead us to assess prominent areas of inquiry and conventional practices in new and fertile ways. Often we will be led to new hypotheses regarding important subjects and exciting possibilities will be opened for extending existing theoretical work in new directions. Focusing on how social processes unfold over time suggests new questions and reveals new outcomes of interest—questions and outcomes that are linked to, but distinct from, those that command attention in existing lines of inquiry.

These general comments about social analysis apply to the study of public policy. Here as well, a “snapshot” orientation has become increasingly evident in the efforts of social scientists to understand important social outcomes. In the study of public policy, ahistorical investigation takes two distinctive forms. Most obviously, it may entail a focus on *policy enactments*—the moments of policy choice. From case studies of individual

policy battles to broader studies that examine how a range of factors influences legislative productivity, it is the birth of public policies that most often captures scholarly attention.³ Alternatively, an analyst may take a snapshot of *extant* public policies. In this case, the analyst works backward from existing public policies in an attempt to understand why they take the form they do—an approach I will call functionalist. It is particularly prevalent in rational choice analyses, which have become prominent in contemporary political science. Neither concentration on enactments nor the turn to functional explanation is an unreasonable starting point for an investigation of public policy. Each, however, has serious limitations that can be laid bare and addressed by focusing on the development of policy over time.

Studying Policy as a Moment of Choice

I shall take up the study of policy enactments first. It may seem natural to direct our attention on moments of policy choice. Isn't that obviously where the action is? Martha Derthick, in her magisterial *Policymaking for Social Security*, provides the appropriate response:

Policymaking is a compound of exciting, innovative events in which political actors mobilize and contest with one another, and not-so-exciting routines that are performed without widespread mobilization, intense conflict, or much awareness of what is going on except among the involved few. . . . The absence of conflict . . . does not signify the absence of change, and what is routine, though it may not be interesting to analysts at a given moment, is cumulatively very important.⁴

Focusing on the dramatic moments of policy choice blinds us to two broad aspects of policy development: what happens before the moment of choice and what happens after. First, let me discuss the “after” part of these unfolding processes. In a recent essay, Eric Patashnik provided a powerful critique of the way in which focusing on dramatic enactments can be highly misleading if analysts are not also attentive to aftermaths.⁵ He examines several large-scale “public interest” reforms in the past twenty-five years: airline deregulation, the Tax Reform Act of 1986, and the 1996 reform of agricultural subsidies. Each of these policy enactments commanded attention because of the surprising triumph of diffuse interests over concentrated ones. Each has provided the basis for broad claims about the character of American policymaking.

Patashnik presses us not to be misled by these dramatic moments of choice. Instead, we need to see what happened after the dust settled. We need to think not only about policy enactment, but what he calls *policy sustainability*. And in fact, only one of these reforms—airline deregulation—proved to have staying power once its moment in the public eye passed. The Tax Reform Act of 1986 and the 1996 farm bill, by contrast, were undercut over time as powerful, concentrated interests reasserted themselves. Indeed, as we approach the twentieth anniversary of the heralded tax reform, one has to look closely to locate the scattered remnants of this dramatic “simplifying” initiative. Once again, the tax code sags under the ever-increasing weight of particularistic benefits.

Sustainability, Patashnik argues, depends in part on whether new policy initiatives effectively undercut the reassertion of traditional interests (e.g., by eliminating their institutional bases of support). Perhaps even more, sustainability turns on whether or not these reforms generate self-reinforcing dynamics of social adaptation, leading to the development of new supportive interests. Indeed, a core claim of recent work that has begun to focus self-consciously on policy development is the significance of “policy feedback.” Once enacted, policies with specific qualities can produce social effects that reinforce their own stability.

Political scientists working on a range of empirical issues have shown growing interest in how public policies can reconfigure politics in this way.⁶ Among comparativists, much of the focus has been on “policy regimes” that engender distinct patterns of interest-group formation and distinctive distributions of public opinion, thus reinforcing divergent and enduring political coalitions. In the comparative study of welfare states, Gosta Esping-Andersen’s path-breaking work emphasized the distinct policy configurations generated in the “liberal,” “social democratic,” and “conservative” worlds of welfare capitalism.⁷ Although Esping-Andersen traced these different regimes to distinct constellations of political power, he argued that the establishment of these policy regimes had enduring political effects.

Arguments about regime effects have become standard in this literature.⁸ Huber and Stephens, in their remarkably comprehensive treatment of welfare state development, strongly emphasize the impact of early policy initiatives in shaping long-term courses of policy development. “As each policy is put into place,” they note, “it transforms the distribution of preferences; as the regime increasingly entrenches itself, it transforms the universe of actors. The economic and political costs of moving to another regime become greater, and conversely the returns of staying on the same track become greater.”⁹ Here is their account of the impact of early policy initiatives on public opinion, which they term “the policy ratchet” effect:

Until the era of retrenchment, it was rare for secular conservative parties to roll back welfare state reforms instituted by social democratic or Christian democratic parties. Indeed they generally accepted each new reform after it has been instituted, and the new center of gravity of the policy agenda became defined by the innovations proposed by the progressive forces in society. The reason for the change in posture of the conservative parties was that the reforms were popular with the mass public, especially the broad-based policies in the areas of pensions, education, and health care. . . . The support for policies quickly broadened once citizens enjoyed the benefits of the new policies, and thus the mass opposition to cutbacks in the policies was much broader than the mass support for their introduction. *Thus, the new policy regime fundamentally transforms the preferences of the population.*¹⁰

Comparative research has also begun, at least haltingly, to assemble “micro” data, focusing on the behavior of individuals, which explores and documents the links between policies and important political variables. This is an important development because much of the previous research had simply sketched out what might be happening at the level of individuals without investigating these connections directly. Iversen and Soskice, for instance, argue that strong social insurance states promote the development of specific skills in the workforce.¹¹ They demonstrate, in turn, that this fact has consequences for political behavior. Countries with large groups of workers with these skills, who need social insurance to protect against risks associated with their investments in nontransferable skills, will exhibit higher levels of public support for expansive social programs.

Research in American politics has also begun to generate substantial evidence of large-scale policy effects. Not surprisingly, given its focus on processes unfolding over substantial stretches of time, this has been especially true in the field of American political development. Theda Skocpol’s work on early initiatives in social policy shows that they had significant political effects.¹² Jacob Hacker’s recent study breaks important new ground in demonstrating the scope of policy effects.¹³ By focusing on the regulatory and tax policies that support the “private” or “hidden” welfare state, he extends an appreciation for policy effects beyond the range of previous studies. By systematically tracking the implications of early policy initiatives over roughly five decades, he provides a particularly compelling examination of the ways in which initial policy arrangements profoundly influenced interest-group structures, the preferences of key political groups, and the range of possibilities open to future policymakers.

Hacker's analysis of the interests of employers is particularly revealing. Political economists often present the policy preferences of business actors as almost hard-wired—something innate in their status as employers. Yet Hacker shows that in the United States the same employers developed dramatically different viewpoints on the desirability of social insurance in health care (vehemently opposed) and retirement (largely supportive). Early divergences in policy structures were key factors, Hacker demonstrates, in altering the developing stances of employers in these two areas. This was not only because established policy structures changed what employers thought they could get, but because these structures gradually changed what employers perceived as desirable.

Just as in the study of comparative politics, analyses of how these processes operate at the individual level have recently appeared in research on American politics. Andrea Campbell's study of Social Security policy and the elderly and Suzanne Mettler's analysis of the G.I. Bill of Rights both offer strong evidence linking policy structures to political behavior.¹⁴ Campbell demonstrates that the expansion of Social Security heightened levels of political activity among the elderly (and, most strikingly, among the less affluent elderly). Mettler's survey of World War II veterans reveals significant effects of the G.I. Bill on levels of participation and political attitudes.

Thus we have considerable empirical research, focused on both broad trends in policy development and more intensive examination of how individuals respond to specific government programs, suggesting that policy effects can be very substantial. There is now strong support for E. E. Schattschneider's insistence that "new policies create a new politics."¹⁵ This suggests the considerable prospects for thinking not just about what grand policy enactments may occur at a moment in time, but about how those policies develop—whether they are or are not likely to become sustaining elements of a durable policy regime or, as with the TRA, initiatives that have a much more fleeting impact on patterns of governance.

Indeed, this research on the feedback effects of policy has another considerable benefit. Examining the aftermath of enactments, as suggested by Patashnik, highlights not only the checkered long-term records of much-heralded policy initiatives. Just as an exploration of self-reinforcing features of legislation may reveal that policy enactment does not ensure policy sustainability, it also suggests that we must be attentive to the possibility that policy initiatives may start small but grow substantially over time. For instance, recent research on what Christopher Howard has aptly termed the "hidden welfare state" of tax expenditures, as well as regulation of private social welfare activities, reveals a story of small or at least unheralded initiatives whose true significance only becomes apparent over

an extended period.¹⁶ These key programs developed in a pattern quite different from the grand legislative dramas that capture the attention of “snapshot” analysts. As a result, explanations for many of the programs that give the American welfare state its highly distinctive mix of private and public social welfare initiatives were simply missing prior to recent work on policy development.

Thus Patashnik and others have rightly emphasized that we should not see policy enactment as the end of the story. Approaching public policy as a matter of policy development suggests, moreover, that we should not necessarily treat enactments as the beginning of the story either. If it is crucial to consider what happens after enactments, it is also important to think carefully about what happens *before*.

Here again, the snapshot orientation of much social science creates big problems. Too often, it leads social scientists seeking explanations of policy outcomes to focus their inquiries on aspects of causal processes that unfold very rapidly and immediately prior to the outcome of interest. Yet many things in the social world take a long time to happen. The fact that something happens slowly does not make it unimportant. A wide range of processes cannot be understood unless analysts remain attentive to extended periods of time. Snapshot analyses of moments of choice will often miss important elements of the real story, or even make drastically inaccurate interpretations of the parts of the story that do attract their attention.

“Snapshot” views of major policy events typically focus on the immediate sources of change—the catalysts. They will often have a hard time identifying the role of structural factors. These, by their very nature, will typically show little variation within a limited period.¹⁷ Studies of enactments, unless carefully designed, are unlikely to highlight structural factors that influence outcomes only with substantial lags, or by restricting the range of possible outcomes. In many contexts, however, a long, slow erosion of the status quo may be a crucial factor in generating policy change. What may seem like a relatively rapid process of reform is in fact only the final stage of a process that has in fact been under way for an extended period.

Doug McAdam’s *Political Process and the Development of Black Insurgency, 1930–1970* (1982) presents a powerful example. McAdam places great weight on the role of big but slow-moving processes that established the preconditions for successful black mobilization and the onset of the “Rights Revolution” in the United States:

The Montgomery bus boycott of 1955–56 . . . [and] the 1954 Supreme Court decision in the Brown case . . . were landmark events.

Nonetheless, to single them out serves . . . to obscure the less dramatic but ultimately more significant historical trends that shaped the prospects for later insurgency. Especially critical . . . were several broad historical processes in the period from 1933 to 1954 that rendered the political establishment more vulnerable to black protest activity while also affording blacks the institutional strength to launch such a challenge. Later events such as the 1954 decision and the Montgomery bus boycott merely served as dramatic (though hardly insignificant) capstones to these processes.¹⁸

At the heart of McAdam's analysis is the decline of the cotton economy in the quarter-century after 1925. This decline simultaneously diminished the strength of forces opposed to black insurgency and generated patterns of migration that boosted the organizational capacities (e.g., massive expansion of black churches, colleges, and southern chapters of the National Association for the Advancement of Colored People [NAACP]) of a long-oppressed minority. It was these gradual interconnected social processes that created conditions ripe for a set of triggering events.

In part because studies of policy enactment make it possible to examine moments of change in fine detail, the role of particular actors in initiating such movements is likely to be highlighted. Yet these studies have greater difficulty in identifying those features that facilitate, impede, or channel entrepreneurial activity. Broad, structural features, as well as long, slow-moving processes, which may be crucial preconditions for policy change, recede from view.

To see these features, we will typically need to examine a greater stretch of time in advance of a particular enactment.¹⁹ Some causal processes occur slowly because they are incremental—demographic shifts, for instance, simply take a long time to add up to anything. In others, the critical factor is the presence of “threshold effects”: some social processes may have little significance until they attain a critical mass, which may then trigger major change. Other social processes involve considerable time lags between the appearance of a key causal factor and the occurrence of the outcome of interest. This may be true because the outcome depends on a “causal chain” that takes some time to work itself out (*a* causes *b*, which causes *c* . . .). Alternatively, causal processes may turn on “structural” features that involve transformations that are probabilistic during any particular period, which means that several periods may be necessary before the transformation occurs. Under conditions such as these, the social outcome of interest may not actually take place until well after the appearance of key causal factors.

Analysts who fail to be attentive to these slow-moving dimensions of social life are prone to a number of serious mistakes. They may ignore potentially powerful hypotheses. They are particularly likely to miss the role of many “sociological” variables, like demography, literacy, or technology. Their explanations may focus on triggering or precipitating factors rather than more fundamental structural causes.

It can get worse. By truncating an analysis of processes unfolding over an extended period of time, analysts can easily end up *inverting* causal relationships. Daniel Carpenter’s *The Forging of Bureaucratic Autonomy* presents a striking demonstration of how attention to a long-term sequence of causes can turn our understandings of policy outcomes on their heads.²⁰ He criticizes the large and influential literature on relations between legislatures and bureaucrats grounded in Principal-Agent theory.²¹ The literature argues that policy outcomes will typically reflect the preferences of congressional “principals,” because they have substantial political resources to assure the acquiescence of their bureaucratic “agents.”

Carpenter transforms the question from one about policy selection to one about policy *development*. By doing so, he flips this analysis upside down. He persuasively demonstrates how conventional analyses substantially underestimate the potential for bureaucratic autonomy because they adopt a cross-sectional approach to studying what should be understood as a long-term historical process. Working under favorable conditions and over extended periods of time, ambitious and entrepreneurial bureaucrats were able to enhance their reputations for innovativeness and competence and develop strong networks of support among a range of social actors. These achievements created a context in which Congress, facing pressure from below and deferring to the expertise of leading bureaucrats, essentially asked for what the bureaucrats wanted. Viewed as a moment in time, one sees what looks like congressional dominance; viewed as a process unfolding over time, *the same cross-sectional evidence* provides indications of substantial bureaucratic autonomy.

All of these examples suggest some of the ways in which analysts can gain insights from studying policy development as an unfolding historical process. The shaping of public policy is more than a matter of “policy choice” at a particular moment in time. That moment of choice is framed by prior and later events and processes that we exclude from our analyses at considerable cost.

Functionalist Explanations of Public Policy

There is a second strand of work on policy that also takes a “snapshot” approach. Indeed, a focus on the rational selection of policy designs by strategic actors has, in various guises, become perhaps the dominant approach to explaining policy outcomes in the social sciences. I want to explore this approach in more detail, both because it has become so prevalent and because it usefully demonstrates some additional advantages of thinking more systematically about policy development.

When social scientists have sought to explain policy outcomes, they have had a strong tendency to employ “functionalist” interpretations—outcomes are to be explained by their consequences. In particular, what I term “actor-based functionalism” typically rests on the claim that policies take the form they do because powerful actors engaged in rational, strategic behavior are seeking to produce the outcomes observed. Analysts focus on the choices of individual and collective actors who select public policies, and fashion explanations through reference to the benefits those actors expect to derive from particular policy designs. In fact, in most cases they work *backwards* from extant policy arrangements to develop an account of how they were (or might have been) rationally chosen.

Taken alone, however, these arguments are at best incomplete and at worst seriously misleading. Again, in reducing a moving picture to a snapshot, we run the risk of missing crucial aspects of the processes through which public policies take shape, as well as the ways in which they either endure or change in constantly shifting social environments. This is not to say that functional explanations of policy arrangements are not often plausible. They may be, but the adoption of an extended time frame reveals numerous problems for such accounts. Functional interpretations of politics are often suspect because of the sizable time lag between actors’ actions and the long-term consequences of those actions. Political actors, facing the pressures of the immediate or skeptical about their capacity to engineer long-term effects, may pay limited attention to the long term. Thus the long-term effects of policy choices, which are frequently the most profound and interesting ones, should often be seen as the *by-products* of social processes rather than embodying the goals of social actors.

A second issue is that of unintended consequences. Even where actors may be greatly concerned about the future in their efforts to design public policies, they operate in settings of great complexity and high uncertainty. As a result, unanticipated consequences are likely to be widespread. Of all the limitations of the rational design argument, this is perhaps the most significant. Anyone engaged in empirical research in the social

sciences knows that even the canniest of actors cannot hope to adequately anticipate all the consequences of their actions. Policies may not be functional because designers make mistakes.²²

Those who study the evolution of public policy are typically struck by the difficulty political actors have of exercising effective control in an increasingly complex world. The profound implications of the high and increasing social complexity that typifies modern polities need to be underlined. As the number of decisions made and the number of actors involved proliferate, relations of interdependence—among actors, organizations, and institutions—expand geometrically. This growing complexity has two distinct consequences. First, more prevalent and complex political activity places growing demands on decision makers, generating problems of overload. Time constraints, scarcities of information, and the need to delegate decisions may promote unanticipated effects.²³ At the same time, increasing social complexity leads to growing interaction effects. Initiatives often will have important consequences for realms outside those originally intended. We should expect that social processes involving large numbers of actors in densely institutionalized societies routinely generate elaborate feedback loops and significant interaction effects. In such settings, decision makers cannot hope to fully anticipate all of the major implications of their actions.²⁴

Nor is it just that social contexts are extremely complex; the difficulties are exacerbated by the fact that the abilities of individuals to draw inferences and judgments from their experiences have systematic biases. Levitt and March provide an excellent summary:

[I]ndividual human beings are not perfect statisticians. . . . They make systematic errors in recording the events of history and in making inferences from them. They overestimate the probability of events that actually occur and of events that are available to attention because of their recency or saliency. They are insensitive to sample size. They tend to overattribute events to the intentional actions of individuals. They use simple linear and functional rules, associate causality with spatial and temporal contiguity, and assume that big effects must have big causes. These attributes of individuals as historians all lead to systematic biases in interpretation.²⁵

For all these reasons, social activity—even when undertaken by highly knowledgeable and instrumentally motivated actors—should typically give rise to significant unintended effects.²⁶

Here we can see a definite limitation of the “cross-sectional” or snapshot view of public policies typical in actor-centered functionalism. De-

pending on whether the analyst's starting point is the moment of policy selection or the examination of some extant policy, either the long-term development of public policy or the original factors generating the choice of policy in the first place will be outside the scope of the analysis. So, of course, will be any recognition of disjunctures between the two. Focusing *either* on policy origins or on the eventual outcome—that is, on a snapshot of a policy removed from time—the issue of unintended consequences simply vanishes from view.

As Jacob Hacker emphasizes in his contribution to the current issue, this is just one way in which functionalist analyses succumb to the “temptation to conflate intentions with effects.” Operating from the premise that policies reflect the preferences of powerful actors, these approaches too easily impute a comfortable “fit” between policy outcomes and structures of political interests. In fact, there is room for dramatic slippage between the desires of powerful actors and the actual policy structures that develop over time.

Moreover, even where we do see a “fit,” in which currently powerful actors endorse current structures of public policy, it is entirely possible that a functionalist interpretation will be seriously off the mark. Indeed, the emphasis I have placed on the downstream social and political consequences of policy arrangements—the effects of policy feedback—contains a double irony for functionalist accounts. First, it suggests that functionalism will often have the causal arrow backwards. Rather than actors necessarily selecting policies that fit their needs, policies, once in place, may “select” actors. This would occur through two processes, familiar to those interested in evolutionary arguments.²⁷ First, actors adapt to policy environments, by adopting new agendas, strategies, and mobilizing techniques. In the long run, policy arrangements can powerfully shape actors' very identities. Second, individual and collective actors who do not adapt will often be less likely to survive. Through processes of adaptation and selection, actors whose strategies do not “fit” well in a particular policy context may become less common over the long haul.²⁸

The second and broader irony is that a snapshot view of such a process will mistakenly be viewed as a confirmation of actor-based functionalism. When an analyst cuts into a process of policy development at any moment in time, he or she may indeed see a relatively nice “fit” between the preferences of powerful actors and the functioning of policies. This might seem to suggest that we are in the realm of functionalist policy design, but in fact such an assertion would get the causality exactly backward. Rather than these powerful actors generating the policy, the policy arrangements may have played a substantial role in generating the properties of the actors.

Functionalist arguments that start from the benefits particular actors derive from institutions and infer that their power accounted for those arrangements typically ignore important feedback processes that may generate the same observed outcome in a completely different way.²⁹ As just emphasized, an understanding of policy-generated, self-reinforcing dynamics may suggest alternative explanations for policy arrangements that are *too easily* attributed to power relations.

At the same, we also need to consider the possibility that self-reinforcing processes may magnify power imbalances over time, while simultaneously rendering those imbalances less visible.³⁰ In the famous community power debate of the 1960s and 1970s, Bachrach and Baratz and Lukes argued persuasively that power asymmetries are often hidden from view; where power is most unequal, it often does not need to be employed openly.³¹ Pluralist critics essentially countered that it was impossible to systematically evaluate such claims.³² Although he did not frame the issue quite this way, Gaventa demonstrated that such power asymmetries can reflect positive feedback processes operating over substantial periods of time.³³ Processes of positive feedback can transform a situation of relatively balanced conflict into one of great inequality. Political settings where one set of actors must initially impose their preferences on another set through open conflict (“the first face of power”) may change over time into settings where power relations are so uneven that anticipated reactions (“the second face of power”) and ideological manipulation (“the third face”) make open political conflict unnecessary. Thus positive feedback over time may simultaneously increase asymmetries of power and, paradoxically, render power relations less visible.

Where certain actors are in a position to impose rules on others, the employment of power may be self-reinforcing. Actors may use political authority to change the rules of the game (both formal institutions and various public policies) to enhance their power. These changes may not only shift the rules in their favor, but may increase their own capacities for political action while diminishing those of their rivals. And these changes may result in adaptations that reinforce these trends, as undecided, weakly committed, or vulnerable actors join the winners or desert the losers. Many political conflicts, from the Nazi seizure of power to the gradual process through which the Labour Party supplanted the Liberals in Great Britain in the early twentieth century, reveal this sort of dynamic. Disparities in political resources among contending groups may widen dramatically over time as positive feedback sets in. In the process, political conflict and imbalances of power may become less visible, not more.

The disenfranchisement of African Americans in the post-reconstruction American South provides a clear and poignant example of how shifts

in political power can be self-reinforcing. In Alexander Keyssar's marvelous study of the history of suffrage in the United States, the end of Reconstruction precipitated a dynamic process of shifting power relations that played out over a considerable period of time.³⁴ In 1876, a contested presidential election led to the removal of federal troops from the South. In 1878, Democrats won control of both houses of Congress for the first time in twenty years. "The upshot of these events," Keyssar writes, "was to entrust the administration of voting laws in the South to state and local governments."³⁵

These breakthroughs, it must be stressed, did not result in immediate victory for Democratic "Redeemers" in the South. Instead, they ushered in a "period of limbo and contestation, of participation coexisting with efforts at exclusion." In many parts of the South, the Republican Party "hung on, and large, if declining, numbers of blacks continued to exercise the franchise":

Periodically they were able to form alliances with poor and upcountry whites and even with some newly emerging industrial interests sympathetic to the probusiness policies of the Republicans. Opposition to the conservative, planter-dominated Redeemer Democrats, therefore, did not disappear: elections were contested by Republicans, by factions within the Democratic Party, and eventually by the Farmers' alliance and the Populists. Consequently, the Redeemers, who controlled most state legislatures, continued to try to shrink the black (and opposition white) electorate through gerrymandering, registration systems, complicated ballot configurations, and the secret ballot (which served as a *de facto* literacy test). When necessary, they also resorted to violence and fraudulent vote counts.³⁶

Social and political power was used over time to reinforce and consolidate political advantage. By the early 1890s, major challenges to the Redeemers began to dissipate, giving way to a durable system of planter hegemony. As late as 1964, only 10 percent of African Americans in Mississippi would be registered to vote.

These observations point to deep-rooted difficulties in the treatment of power in contemporary social science: sometimes analysts see power when it is not there, and sometimes they do not see it when it is. "Snapshot" views will often miss important elements of power relations. In short, shifting to the study of policy development provides the basis for a revitalized effort to investigate issues of power in political life.

The Study of the Past and the Study of the Present

One final implication of this focus on issues of policy development deserves emphasis. Students of policy history should vigorously contest the tendency to separate the study of the past from the study of the present, to draw sharp distinctions between students of “policy history” and those who study contemporary politics. There are many reasons to study policy history, and scholars can do so very profitably even if their research has no particular implications for the present. But good work on policy development will often have crucial implications for contemporary issues. Even more important, it is often extremely useful to treat the present itself as but a specific moment within a larger dynamic process. As I have argued, central to the notion of development is the recognition that the social world is marked by processes that unfold over time. Studying the present as a “snapshot” of a moment of time can distort what we see and how we understand it in profound ways. Shifting to a developmental perspective presses us, even when we are focusing on the present, to pay more attention to the long-term sources of policy change, to address the central issue of policy sustainability, to consider the possibilities that in the long run “small” outcomes may end up being very big, while “big” ones end up being small, and to adapt our analyses to the reality of ubiquitous unintended consequences.

Thinking systematically about how policies develop through time can also refine our expectations about the possibilities and constraints of contemporary politics. I have argued that a sensitivity to processes of policy development allows us to identify the factors that provide the “glue” for a particular policy arrangement. This in turn can be extremely helpful for identifying likely paths of policy reform. If we know, for example, which elements of policy arrangements have generated important adaptations for which sets of actors, we are more likely to be able to identify which kinds of revisions they would regard as acceptable and which they would view as problematic.

Consider the following example: the deeply institutionalized arrangements of public pension systems in mature welfare states.³⁷ Most of these pension systems became well institutionalized by 1980. Yet a series of profound social, economic, and political changes have generated a climate of austerity in which virtually all national pension systems are undergoing substantial revisions. Because different national systems are starting from different points of departure, with greatly differing degrees of institutionalization, the menu of options available to reformers varies dramatically. Where generous, earnings-related pay-as-you-go pension systems

have been in place for a substantial stretch of time, established precommitments make a shift to a funded, individualized system of retirement accounts virtually impossible. Instead, policymakers have pursued reforms designed to control costs and link contributions more closely to benefits. In systems without mature pay-as-you-go plans, more radical reform options remain on the table, and have in fact been implemented in some cases (e.g., the United Kingdom). In short, a clear understanding of where countries are in a course of policy development give us a much sharper sense of what kinds of reform are most plausible.

There are enormous benefits to be gained from bringing the orientations and insights of work on policy development to bear on issues central to contemporary politics—a step that will help us to identify new questions and contribute to the understanding of old ones. On this score there are encouraging signs. For example, recent years have seen a number of impressive works that directly address the contemporary American polity, but approach it through the study of policy development over time. Jonathan Skrentny's analysis of the minority rights revolution, Suzanne Mettler's research on the long-term political effects of the G.I. Bill, and Jacob Hacker's investigation of the divided welfare state are telling examples.³⁸ These works all rest on the explicit assertion that historical investigation focusing on unfolding processes can greatly illuminate our understanding of the present. If work in this vein continues, it will mark the end of a strict separation between our efforts to comprehend "the historical" and "the contemporary" in public policy. This will be good news for the study of policy history, and even better news for the social sciences.

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Notes

1. This essay presents an introduction to issues explored in much more depth in Paul Pierson, *Politics in Time: History, Institutions, and Social Analysis* (Princeton, 2004). I would like to thank Julian Zelizer for very helpful comments on an earlier draft.

2. Terrance J. McDonald, ed., *The Historic Turn in the Human Sciences* (Ann Arbor, 1994); Robert Bates et al., *Analytic Narratives* (Cambridge, 1998).

3. See, for example, Sarah Binder, *Stalemate: Causes and Consequences of Legislative Gridlock* (Washington, D.C., 2003), and David Mayhew, *Divided We Govern* (New Haven, 1991).

4. Martha Derthick, *Policymaking for Social Security* (Washington, D.C., 1979), 9.

5. Eric Patashnik, "After the Public Interest Prevails: The Political Sustainability of Policy Reform," *Governance* 16 (2003): 203–34.

6. Arguments about what is now often termed "policy feedback" are usually traced back to sources such as Theodore Lowi's classic article, "American Business, Public Policy, Case Studies, and Political Theory," *World Politics* 16 (1964): 677–715. Lowi's argument, however, is really about the structure of issues and the associated (diffuse or concentrated)

winners and losers. It is not an argument about how specific structures of public policy can influence politics. Recent strands of work on this topic stem largely, in my view, from lines of argument set down by Theda Skocpol and her collaborators in the 1980s. See especially Margaret Weir and Theda Skocpol, "State Structures and the Possibilities for 'Keynesian' Responses to the Great Depression in Sweden, Britain, and the United States," in Peter B. Evans, Dietrich Rueschemeyer, and Theda Skocpol, eds., *Bringing the State Back In* (Cambridge, 1985), 107–62.

7. Gosta Esping-Andersen, *The Three Worlds of Welfare Capitalism* (Princeton, 1990).

8. Duane Swank, *Global Capital, Political Institutions, and Policy Change in Developed Welfare States* (Cambridge, 2002); Evelyn Huber and John Stephens, *Development and Crisis of the Welfare State: Parties and Policies in Global Markets* (Chicago, 2001); and Paul Pierson, ed., *The New Politics of the Welfare State* (Oxford, 2001).

9. Huber and Stephens, *Development and Crisis of the Welfare State*, 32.

10. *Ibid.*, 28–29, emphasis added.

11. Torben Iversen and David Soskice, "An Asset Theory of Social Policy Preferences," *American Political Science Review* 95 (2001): 875–94.

12. Theda Skocpol, *Protecting Soldiers and Mothers* (Cambridge, Mass., 1992).

13. Jacob Hacker, *The Divided Welfare State: The Battle over Public and Private Social Benefits in the United States* (Cambridge, 2002).

14. Andrea Louise Campbell, *How Policies Make Citizens: Senior Political Activism and the American Welfare State* (Princeton, 2003); Suzanne Mettler, "Bringing the State Back In to Civic Engagement: Policy Feedback Effects of the G.I. Bill for World War II Veterans," *American Political Science Review* 96 (2002): 367–80.

15. E. E. Schattschneider, *Politics, Presures, and the Tariff* (New York, 1935).

16. Christopher Howard, *The Hidden Welfare State* (Princeton, 1997); Hacker, *The Divided Welfare State*.

17. Dietrich Rueschemeyer, Evelyn Huber Stephens, and John Stephens, *Capitalist Development and Democracy* (Chicago, 1992), 32–34.

18. Douglas McAdam, *Political Process and the Development of Black Insurgency, 1930–1970* (Chicago, 1982), 3

19. For a detailed discussion, see Paul Pierson, "Big, Slow-Moving, and . . . Invisible: Macrosocial Processes in the Study of Comparative Politics," in James Mahoney and Dietrich Rueschemeyer, eds., *Comparative Historical Analysis in the Social Sciences* (Princeton, 2003), 177–207.

20. Daniel P. Carpenter, *The Forging of Bureaucratic Autonomy: Reputations, Networks, and Policy Innovation in Executive Agencies, 1862–1928* (Princeton, 2001).

21. Matthew D. McCubbins, Roger G. Noll, and Barry R. Weingast, "Administrative Procedures as Instruments of Political Control," *Journal of Law, Economics and Organization* 3 (1987): 243–77; Matthew D. McCubbins and Thomas Schwartz, "Congressional Oversight Overlooked: Police Patrols versus Fire Alarms," *American Journal of Political Science* 28 (1984): 165–79.

22. Of course, this was a standard conclusion of implementation research, which of course was well placed to examine unintended consequences precisely because it examined what happened *after* policies were enacted—although typically considering only a limited stretch of time. The classic in this genre is Jeffrey L. Pressman and Aaron Wildavsky, *Implementation* (Berkeley and Los Angeles, 1973).

23. James March and Johan Olsen, *Rediscovering Institutions: The Organizational Basis of Politics* (New York, 1989); Herbert A. Simon, *Models of Man* (New York, 1957).

24. Robert Jervis, *System Effects: Complexity in Political and Social Life* (Princeton, 1997).

25. Barbara Levitt and James G. March, "Organizational Learning," *Annual Review of Sociology* 14 (1988): 323

26. Friedrich Hayek, *Law, Legislation, and Liberty: Rules and Order* (London, 1973); Fred Hirsch, *The Social Limits to Growth* (Cambridge, Mass., 1977); Thomas Schelling,

Micromotives and Macrobehavior (New York, 1978); Charles Perrow, *Normal Accidents* (New York, 1984); Jervis, *System Effects*.

27. Miles Kahler, "Evolution, Choice, and International Change," in David A. Lake and Robert Powell, eds., *Strategic Choice and International Relations* (Princeton, 1999), 165–96.

28. Huber and Stephens, *Development and Crisis of the Welfare State*; Jacob Hacker and Paul Pierson, "Business Power and Social Policy: Employers and the Formation of the American Welfare State," *Politics and Society* 30 (2002): 227–325.

29. Hacker and Pierson, "Business Power and Social Policy."

30. Terry Moe, "Power and Political Institutions," paper presented at the Conference on Crafting and Operating Institutions, Yale University, New Haven, 2003; Kathleen Thelen, "Historical Institutionalism and Comparative Politics," *Annual Review of Political Science* 2 (1999): 369–404.

31. Peter Bachrach and Morton S. Baratz, "The Two Faces of Power," *American Political Science Review* 56 (1962): 947–52; Stephen Lukes, *Power: A Radical View* (London, 1974).

32. Nelson W. Polsby, *Community Power and Social Theory* (New Haven, 1963); Raymond A. Wolfinger, "Nondecisions and the Study of Local Politics," *American Political Science Review* 65 (1971): 1063–80.

33. John Gaventa, *Power and Powerlessness: Quiescence and Rebellion in an Appalachian Valley* (Urbana, Ill., 1980).

34. Alexander Keyssar, *The Right to Vote: The Contested History of Democracy in the United States* (New York, 2000), 107–16

35. *Ibid.*, 107.

36. *Ibid.*

37. John Myles and Paul Pierson, "The Comparative Political Economy of Pension Reform," in Pierson, ed., *New Politics of the Welfare State*, 305–33.

38. John D. Skrentny, *The Minority Rights Revolution* (Cambridge, Mass., 2003); Mettler, "Feedback Effects of the G.I. Bill"; Hacker, *The Divided Welfare State*.