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Dr. Seuss and Dr. Stinchcombe: A Reply to the Commentaries

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Taken as a group, the three commentaries reveal many of the difficult challenges, as well as opportunities, that confront social scientists exploring issues of temporality. Although the three authors take quite different stances, each highlights important issues for further work sensitive to historical process. Here I begin with Amy Bridges's more critical commentary before turning to the responses of Jervis and Thelen.

Bridges's essay contains two major claims.[1] The first is that "self-reinforcing processes are rare in politics and society." The basis for this assertion appears to be her observation that there are markets in economics, but not in politics. I completely agree that one needs to carefully attend to the distinctive features of different social settings, and that facile analogies between economics and politics should be avoided. But why Bridges believes that only markets can generate positive feedback remains a mystery. The traditional view in economics, of course, was that markets served to generate "diminishing returns" at the margins, so one could safely dismiss issues of positive feedback and hence ignore questions of timing and sequence. Thus Bridges's claim should be turned around: if we have strong reasons to believe that *even* market processes often generate positive feedback, it is highly plausible that they occur frequently in other social processes as well.[2]

There certainly are good logical reasons to anticipate self-reinforcing tendencies in specific, frequently occurring political processes. Having flatly stated that only markets tend to generate positive feedback, Bridges ignores my discussion of the possible sources of path dependence in processes of institutional emergence, the development of public policy, collective action dynamics, and the development of ideological orientations. Equally telling, I point to numerous studies that have (implicitly or explicitly) stressed the role of positive feedback. Jervis and Thelen, exploring a wide range of empirical settings, point to many more.[3] Inexplicably, Bridges herself approves of Terry Karl's argument about path dependent development in oil-rich states, which I cite as a good example of an analysis of self-reinforcing processes. One is left wondering why Bridges thinks that this particular empirical application to politics passes muster while the many others that she dismisses out of hand do not.

Bridges' second claim is that arguments about path dependence do not capture what is theoretically illuminating in historically-grounded research. This is a

1. Bridges reading of my essay assumes a take-no-prisoners stance. She says that I claim that "path dependence is what 'the most prominent substantive claims about the role of timing and sequence really are.'", p. 109 I wrote: "the most prominent substantive claims about the role of timing and sequence . . . have almost always been grounded *at least in part* on claims about . . . path dependent processes" (emphasis added; p. 74). Bridges says (fn. 1, p. 109) that I argue that path dependence is a theory of technology or technological development. What I wrote (p. 76) was that the "large emerging literature on path dependence initially focused on the development of technology but rapidly spread to other concerns." I make it clear that in my view theories employing the concept of path dependence are about self-reinforcing processes, not about any specific empirical domain. Indeed, the claim that path dependent processes occur across a range of empirical settings is precisely what Bridges takes me to task for elsewhere. Bridges says that "Pierson's conclusion" regarding the long-term impact of bureaucratization's timing relative to democratization is "just silly" – ignoring my cautionary statement that space constraints and my own lack of expertise made it impossible for me to assess the merits of particular empirical claims in the literature. Bridges also vigorously rejects views that she presumably attributes to me, but which I did not and would not advance, for example, that "path dependence is . . . a theory of society, history, or politics" (111) or that "timing and sequence . . . constitute history" (111).

2. This issue is discussed at length in Paul Pierson, "Increasing Returns, Path Dependence and the Study of Politics," which also includes a detailed analysis of how to think about the differences and similarities between path dependence in economic and political processes.

3. For many additional examples see James Mahoney, "The Uses of Path Dependence in Historical Sociology"; Robert Jervis, *System Effects;* and Kathleen Thelen, "Historical Institutionalism."

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tantalizing opening. Arguments about path dependence do not constitute some grand theory. Rather, they offer one entry point into an examination of how processes unfold over time, and one way to critique those who try to understand social processes exclusively through synchronic analyses. Given this, one is of course eager to hear other, potentially complementary ways to proceed. Unfortunately, Bridges's attempt to explicate her alternative is a disappointment. Oscillating between the vague and the implausible, her account highlights why it is so important to think through the position of historical scholarship in the social sciences.

Much of her attempt to establish what historically-grounded research has to offer is difficult to pin down. Consider two examples: "Most timing and sequence arguments in comparative politics are . . . about how an event or series of events changes the setting for subsequent ones," and "In historically grounded comparative analyses the most prominent substantive claims . . . are about the relation of social forces to political outcomes, and about the consequences of the relations between social forces and political institutions." These statements are not wrong, but they are far too general to offer us any analytical traction on the problems at hand.

Where Bridges becomes more concrete her position is problematic. She first rejects the idea of identifying distinct mechanisms or specific types of processes that might have more general application: "[F]or the analysis of politics and society . . . the whole is prior to the parts not the aggregate of the parts. More, the parts are themselves relational (and thus not moveable from whole to whole)." Bridges then offers a suggestion that is stunning given her own premises: one can draw generalizations directly from these complex historical cases, taken as wholes. According to Bridges (111–112, emphasis added), "[t]he caution to be historically specific (rather than ahistorical) is . . . *to specify the set of cases of which one's study is an example.*" What Bridges seems to be suggesting is that historical cases, taken as wholes, provide a strong basis for illuminating other cases, also taken as wholes, from quite different historical periods.[4] If I understand her correctly, Bridges maintains that we can usefully employ studies of working class formation in eighteenth- and nineteenth-century Europe (Zolberg et al.) to inform our understandings of newly industrializing countries in the 1970s, or Ertman's study of state-building from the twelfth to eighteenth centuries to think about the construction of military power in the twentieth century.

Bridges does not explain how such sweeping comparisons might be carried out, or how they might be evaluated. Wisely, this is not the kind of task that comparativists usually take on. A moment's reflection suggests the difficulties of trying to generate explanations from a comparison of say, eighteenth- and nineteenth-century German working-class formation, taken as a historic whole, and twentieth-century Iranian working-class formation, or from a comparison of early modern French state-building and contemporary Indonesian state-building. The basic problem is the difficulty of handling comparisons among large, complex cases that vary on many dimensions simultaneously – which we would have to expect will be true when we jump from the eighteenth (or fifteenth!) century to the twentieth. Indeed, one of the main advantages of the "turn to history" is to increase our sensitivity to what might be called "period effects" – that particular causal relationships only hold given certain background circumstances (boundary conditions) that exist in some settings and not others.[5] If, as Bridges says, "the whole is prior to the parts . . . [and] the parts are themselves relational" then the difficulties are compounded. How would we know which aspects of Ertman's analysis could inform a study of twentieth century state-building?

It is worth dwelling on this point for a moment, because social scientists engaged in studying the past often seem to rely on the kind of argument that Bridges tries to make explicit. Here, the justification for a turn to history is to identify "cases" that can then be contrasted with other cases from different historical contexts. Historical analysis then becomes at its core a form of the comparative method. Not only is this a questionable methodological strategy, as already indicated. It also does not address the need to capture the temporal dimension of social processes. As Craig Calhoun has argued, "this version of historical sociology can . . . be surprisingly ahistorical. It problematizes neither temporal processes nor the specificities of time and place but rather amounts to doing conventional sociology with data drawn from the past."[6]

If this strategy is rejected, as I think it generally should be, then analysts would seem to have three (compatible) options. Two strike me as completely respectable, but they reflect a different agenda from my own. First, we might study a particular historical process because we are interested in it for its own sake. Alternatively, we might study it in order to carry out comparisons among cases that we think are sufficiently similar to overcome the problems such comparisons entail. Given the magnitude of period effects, this will usually mean that they are drawn from roughly the same point in time.

4. This raises an obvious question: If, as Bridges seems to suggest, everything in the social world is connected to everything else, and impossible to analyze outside that specific context, how would one establish the boundaries of her "historically-specific" cases?

5. Most mainstream social science is sloppy about specifying such boundary conditions, asserting "general" relationships based on empirical observations drawn from quite narrow stretches of time.

6. Craig Calhoun, "The Rise and Domestication of Historical Sociology," in *The Historic Turn in the Human Sciences,* ed. Terrence J. McDonald (Ann Arbor: University of Michigan Press, 1996), 310.

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The third option motivated my own essay and, in my view, the commentaries of Jervis and Thelen. We may study historical processes because we are trying to identify particular mechanisms that we believe have general features – that is to say, the mechanisms recur even though the specific temporal and spatial contexts we are studying obviously do not. Here the turn to history is fueled primarily by theory, not method. It reflects the belief that certain fundamental social processes can only be apprehended through analyses that are intensely attentive to the passage of time. As Thelen puts it, the goal is "to study time in as timeless a way as possible."

Robert Jervis employs his commentary to introduce readers to a range of issues explored in greater depth in his important recent book *System Effects*. In doing so, he makes a number of significant contributions. Consistent with my account, he demonstrates that processes of positive feedback are common, although far from ubiquitous, in international relations. In this respect, Jervis helps to refute Bridges's assertion that self-reinforcing dynamics are rare in politics. At the same time, he makes a number of pointed criticisms concerning the limits of mainstream analyses that pay short shrift to issues of historical context, timing, and sequence. And he emphasizes that a central agenda for social scientists should be to establish the conditions that are likely to generate different types of feedback in particular kinds of social contexts.

Given space constraints, Jervis devotes less time to how we might proceed with that agenda. In Thelen's commentary, by contrast, the issue of how to move the discussion forward is central. In this respect, her essay represents the latest in a series of contributions notable for their efforts to employ theory to address the practical demands of historically-oriented social scientists. Her audience is those who seek to do justice to the concrete processes that they study *and* to explore the possibility that aspects of those processes may in some respects represent specific instances of more general social patterns.

Thelen develops two major points. First, she offers what she terms "a friendly amendment" and I would call a much-needed clarification. At issue is my discussion of sequencing arguments involving the order in which different political alternatives emerge in a given polity. She stresses that we need to distinguish the question of "who arrives first" from the claim that "groups able to consolidate early advantages may achieve enduring superiority." Her discussion provides the basis for a useful elaboration of the argument.[7]

Thelen rightly insists that being first may not be sufficient. Sometimes early arrivers are able to consolidate their position; sometimes they are not. Such a failure might reflect either, or both, of two factors. First, a particular context may not be especially conducive to positive feedback. Second, a particular "early arriver" may not be well-placed to exploit whatever opportunities for positive feedback exist.

Thelen suggests that

> . . . understanding when early arrival matters will hinge crucially on an analysis of the "fit" between the organizational forms that develop and the political and economic context in which they emerge – *in particular, the extent to which the latter promotes or interferes with . . . positive feedback*. . . (103, emphasis added)

This is absolutely right. What we want to examine is whether or not features of a particular context promote (or impede) self-reinforcing processes. Addressing this question can increase the precision of claims like "organization A was successful because it 'fit' well in that particular context." Work already done on positive feedback in social processes offers some clues about where to look for answers. We would want to ask, for instance, whether start-up costs for potential alternative organizations (or policies, or institutions) were relatively high or low, whether organizations achieving early success would be able to exercise significant political authority, and if so, how effectively this political authority could be used to impede the progress of potential competitors, and whether other actors will face significant pressures to adapt their own behavior in light of the early success of a particular group, policy, or institution. Along all these dimensions (as well as others discussed in my article) there is likely to be considerable variation, opening the prospect of establishing stronger claims about the particular contexts that may give early arrivers enduring advantages.

Even if a particular environment is conducive to positive feedback, it obviously does not follow that *any* group (or policy or institution) will be able to ex-

7. To simplify, I will refer here to groups and organizations, but the same basic logic applies to other social outcomes that may be subject to positive feedback. Thelen quite rightly emphasizes the need to consider how path dependent arguments might operate in different ways when applied to groups, policies, or institutions. She thinks these arguments are more likely to apply to policies (and I suppose institutions more generally, since if we define institutions as sets of rules, then public policies are really a particular sort of institution) than to groups. This might be true, but no clear reason is given. Her nice argument about selection bias would seem to apply to "failed" (and hence forgotten) policies and institutions as well as to "failed" groups. What *is* clear is that the factors likely to generate or impede positive feedback will be quite different if we are discussing the potential consolidation of an organization than if we are discussing the consolidation of, say, a formal institution or public policy. This would be an excellent topic for further investigation. I would also emphasize, however, that positive feedback may involve a configuration made up of both organizations and institutions, in which the development of particular organizations reinforces the development of particular institutions, and vice versa. More macro-level path dependent processes of this kind are commonly invoked in the literature. In such cases it may make less sense to focus on whether organizations are more or less susceptible than institutions to self-reinforcing processes.

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ploit those opportunities. This is another way of saying, as I originally suggested, that events at the beginning of a path dependent process are not totally contingent – there may be only a few (perhaps only two) plausible alternatives. To employ the language of evolutionary theory, arriving first will not matter unless there is an ecological niche in which the group can prosper.

While not just any group can benefit from arriving first, however, it is crucial to this whole line of argument that there be *more than one* plausible line of development.[8] If there is only one viable option, then we are not talking about a path dependent process. In such cases, the sequence in which potential contenders arrive is either irrelevant or its relevance would need to be established through some other kind of sequencing argument. In path dependent arguments, the crucial role of sequencing hinges on the availability of multiple options. Given those circumstances, the prospect of positive feedback means that the temporal order in which these potential pathways present themselves is critical. In such a setting, an option that could have been viable if it had arrived prior to some alternative will not be viable if it arrives too late. This important point could easily get lost in Thelen's useful insistence on distinguishing different claims. The claim that particular processes are conducive to positive feedback and the assertion that sequence matters are indeed distinct, but there is also an essential analytical link between them.

Thelen's second point focuses on the issue of change at later stages in a path dependent process. As I argued in my article, claims about positive feedback do not imply that once past a critical juncture a particular set of arrangements is permanently locked in place. Thelen does not embrace this common misunderstanding. Her concern is the more subtle one of exploring how one could connect arguments about path dependence or positive feedback more explicitly to the study of social change. As I noted, Thelen has already argued very persuasively for one such connection. Clearly specifying the mechanisms that reproduce a particular institution, policy, or organization over time may give strong clues to potential sources of change – namely, emerging factors that disrupt those specific mechanisms.

In her commentary, Thelen opens another promising line of argument: emerging actors may turn existing organizations or rules, including deeply institutionalized ones, to new purposes.[9] As she shows, this has been a significant theme in historically-grounded research in comparative politics as well as the study of American political development. This argument about "functional conversion" is compelling; it captures very important elements of how change occurs in well-institutionalized polities. I would only want to underscore that Thelen's point about functional conversion is not inconsistent with the arguments about positive feedback advanced in my essay.

Path dependent processes, I stressed, do not generate a frozen landscape. Actors are constantly seeking to change the world, and sometimes they succeed in doing so. What self-reinforcing processes tend to do, however, is increase the obstacles to particular courses of development. Often, extended periods of positive feedback remove altogether certain options from the list of viable alternatives. Actors operate within a set of constraints that are often profoundly shaped by long processes of institutionalization. To repeat the passage from North cited in my article, "path dependence is a way to narrow conceptually the choice set and link decision making through time. It is not a story of inevitability in which the past neatly predicts the future." (p. 76)

Arguments about positive feedback will not tell us everything we would want to know about the episodes of institutional reform that Thelen discusses. Yet just as path dependence arguments may help provide some insight into institutional breakdown (by specifying potentially vulnerable mechanisms of reproduction), they may help us to understand aspects of functional conversion. Actors seeking to "convert" organizations, policies or formal institutions must recognize which aspects of the current arrangement are deeply embedded (perhaps because of entrenched constituents or because of strong connections to other organizations and institutions). The range of choices available is shaped by these constraints.

To make this more concrete, let me briefly refer to collaborative research on the politics of pension reform carried out with John Myles.[10] In the face of slow economic growth, demographic change, and the maturation of extensive welfare states, all the OECD

8. How the plausibility of this counterfactual is established is a different (and difficult) question. It may be derived from theory, or from comparison with other domains within the same society. In comparative politics, the usual strategy is to employ comparisons with actual developments in other countries that are viewed as roughly similar in other respects.

9. In developing this argument about conversion, Thelen highlights the role of unintended consequences. Organizations, policies and institutions may be turned to uses quite different from those anticipated by their creators. Again, this is an important subject for further investigation. The idea of unintended consequences is a frequently recurring, but poorly analyzed concept in research on temporal processes. This is unfortunate, because it is surely one of the weak links in the "synchronic" analyses that dominate mainstream scholarship. By taking "snapshots," most social scientists render the problem of unintended consequences invisible. Typically the analyst seeks to "explain" the initial creation of a particular organization, policy, or institution, in which case its long-term effects are unexamined. Alternatively, the analyst may seek to explore the on-going effects of some extant organization, policy, or institution, in which case the creators' intentions are irrelevant. Only by studying the process over time, incorporating the period of initial choice and the issue of long-term functioning within the same analysis, does the possibility of recognizing unintended consequences emerge.

10. John Myles and Paul Pierson, "The Comparative Political Economy of Pension Reform," in *The New Politics of the Welfare State,* ed. Paul Pierson (Oxford: Oxford University Press, forthcoming).

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countries confront considerable pressures to revamp their public pension systems. Powerful actors have called for a shift to advance-funded private (or quasi-private) schemes. Yet the capacity to undertake such initiatives is sharply constrained by path-dependent processes. Most countries operate pensions on a pay-as-you-go basis: current workers pay "contributions" that finance the previous generation's retirement. Once pay-as-you-go systems have been in place for a long time, they may face incremental cutbacks and adjustments, but they are highly resistant to radical reform. Shifting to private, funded arrangements would place a massive burden on current workers, requiring them to finance the previous generation's retirement while simultaneously paying for their own. The problem is that over time both current workers and current retirees have accumulated deep commitments to pay-as-you-go arrangements.

Indeed, even partial privatization has generally proven possible only in the relatively few countries lacking extensive and mature pay-as-you-go systems. If social scientists want to know the prospects for the development of large, publicly-mandated, funded pension systems in the affluent democracies, the question to ask is not "how strong is the left?" or "how fragmented are political institutions?" – the standard "variables" in ahistorical analyses of comparative public policy. Rather, we want to ask: "Did the polity in question develop an extensive pay-as-you-go pension system before roughly 1970?" If the answer is yes, then the opportunities for introducing an extensive system of funded pensions are likely to be very limited.

This emphatically does not mean the pay-as-you-go systems are not subject to change. In fact, facing new pressures and in a context where the balance of power among political actors has shifted considerably, almost all these systems are undergoing quite significant restructuring. Mostly, this has taken the form of significant cuts in benefits, introduced with lag-times measured in decades, and a gradual shift to link individual pension benefits more tightly to individual contributions. Yet even where advocates of conversion are very powerful they have had little choice but to operate within the constraints that are constitutive of mature pay-as-you-go systems, particularly those stemming from the precommitment of massive resources over decades to come. Here we have a clear example of how path dependent arguments, sensitive to historical process, can identify mechanisms that illuminate general patterns of change in contemporary polities.[11]

11. Strikingly, our finding about pension reform in affluent democracies has been supported by a new statistical analysis of pension reform in Latin America, Western Europe, and postcommunist Eurasia. The authors find that the level of "implicit pension debt" in existing pay-as-you-go systems is the most important determinant of pension reform outcomes. Sarah Brooks and Estelle James, "The Political Economy of Pension Reform," paper presented to the World Bank Conference on New Ideas About Old Age Security, Sept. 14–15, 1999, Washington, DC.

It is appropriate to close with a brief exploration of the broader implications of Thelen's line of inquiry. Thelen's agenda, explored in her thoughtful commentary and elsewhere, is to incorporate arguments about positive feedback into more complicated claims about historical processes. She wishes to capture change as well as stability, surprise as well as intentionality, and the interaction among realms as well as the unfolding of processes within a particular realm. I have a lot of sympathy for that agenda. Indeed, far from attempting to reduce everything to analyses of straightforward positive feedback, as Bridges asserts, one of the main goals of my essay was to highlight prominent efforts to embed arguments about self-reinforcing dynamics within broader analyses of historical processes. Many of the arguments about "reaction-inducing event sequences" explore what happens when positive feedback fails to occur in a particular setting (Luebbert), or what happens when a previously-institutionalized arrangement breaks down or is ill-equipped to address some new set of challenges (Collier and Collier). Many of the arguments about self-reinforcing sequences (Ertman, Shefter) generate insight by situating those dynamics relative to the timing of other historical processes.

Neither Thelen nor I (nor the other contributors for that matter) squarely address the difficulties such an agenda entails. The more complicated these arguments become, the more we move onto unstable ground. Analysts understandably wish to make arguments about temporal processes more complicated in an effort to more fully capture the richness of the social phenomena that interest us. Yet as we do so, we are increasingly forced to rely on *ex post* accounts that identify certain factors as just having happened to go together in a particular sequence in a particular case. Jack Goldstone has nicely described this as a turn to "Dr. Seuss-style" explanations: "it just happened that this happened first, then this, then that, and is not likely to happen that way again."[12]

Aron's story of the falling tile and the unfortunate pedestrian, outlined in my original article to introduce the discussion of conjunctures, captures the key difficulty. Aron's account, by necessity, is a Seussian one. If our goal is to create an "*ex post* model" of a particular outcome of interest then these "Cournot effects" may not create such a terrible problem (although the methodological challenges involved in the persuasive development of such *ex post* models of single cases are severe). But generally that is not all social scientists want. At a minimum, they want to be able to take something from that specific social setting that illuminates other social processes. We know that radical disjunctures as well as instances of functional conversion occur. We also know that different

12. Jack A. Goldstone, "Initial Conditions, General Laws, Path Dependence, and Explanation in Historical Sociology," *American Journal of Sociology* 104 (1998); 833.

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social realms may collide and abrade. What we want to know, however, is "under what circumstances are such occurrences more or less likely?" We want more than Seussian accounts.

In my view, this is the stumbling block for the otherwise appealing research agenda on temporal processes outlined in the joint work of Karen Orren and Stephen Skowronek.[13] This agenda focuses on disjunctures and interactions among distinct social realms, in part reflecting the different historical moments at which these realms are institutionalized. This is an exciting line of inquiry. It reveals a sensitivity to issues of temporal process, focuses on the thorny but central problem of explaining change, and strives to incorporate large-sale social processes and considerable historical reach within a single analysis. It is an approach that has held considerable appeal for the comparative historical sociologists who have played a prominent role in "the historic turn" among social scientists.[14] Both Thelen's essay and my own hint that efforts to build greater dynamism into path dependence arguments constitute an attempt to reach towards the Orren/Skowronek agenda. Yet the remaining gulf appears to be vast. It is a major challenge to demonstrate claims about such interactions among realms even in an analysis of a single time and place. And it is even more difficult to establish what more general propositions one could take from such an analysis, beyond the sound advice to pay attention to such potential interplay among distinct social realms.[15]

It may be useful to imagine a continuum of social processes. This continuum is anchored at one end by fairly simple and reasonably well-ordered ones (by which I mean contexts where we have well-founded theoretical and empirical expectations about the causal connections among key features). At the other end are Cournot-style, Seussian processes in which we have no basis, *ex ante,* for expecting anything in particular. One strategy of how to proceed – I am not suggesting that it is the only strategy – is to build out, as best we can, from the fairly simple and reasonably well-ordered to the less so.[16] In proceeding this way, we need to recognize that the prevalence of Cournot effects mean that our successes necessarily will be only partial.

Three decades ago, the sociologist Arthur Stinchcombe outlined a framework for advancing this strategy that remains of direct relevance to those investigating the temporal dimensions of social processes. He argued that an analyst's explanation will often rely on a form of "historical causation" – some initial event or process generates a particular outcome, which is then reproduced through time *even though the original generating event or process does not recur.*[17] As Stinchcombe puts it a "historicist explanation . . . is one in which an *effect* created by causes at some previous period *becomes a cause of that same effect in succeeding periods.*" In such arguments, "the problem of explanation breaks down into two causal components. The first is the particular circumstances that caused a tradition to be started. The second is the general process by which social patterns reproduce themselves."[18] To explain processes where forms of historical explanation are appropriate one needs to examine considerable stretches of time.

Stinchcombe's analysis should be required reading for historically-oriented social scientists. It offers a clear justification, as well as some useful hypotheses, for the investigation of path dependent processes. As Thelen and Jervis confirm, these processes are widespread in social life, and we have some pretty good general ideas about factors that generate them. Thanks in no small part to their contributions, we are making some progress in moving towards more complex arguments about temporal processes. Much of what we know about the disruption of path dependent processes, or the prospects of "functional conversion" stems from what we have learned about how positive feedback works. By contrast, we have few, if any, *general* ideas about when and how different social arrangements within a polity are likely to "collide and abrade." It would be nice if we did, but nobody has

13. Karen Orren and Steven Skowronek, "Beyond the Iconography of Order: Notes for 'New Institutionalism'," in *The Dynamics of American Politics: Approaches and Interpretations,* eds. Lawrence C. Dodd and Calvin Jillson (Boulder, CO: Westview Press, 1994).

14. See for instance, the quotation from Roy Aminzade in my essay (84). Although Theda Skocpol's *States and Social Revolutions* is generally viewed as an exercise in historical sociology as a comparative method, it contains a similar appeal:

> Social-scientific analyses of revolutions almost *never,* as far as I can tell, give sufficient analytic weight to the conjunctural, unfolding interactions of originally separately determined processes. Yet both the causes and the development of revolutions probably have to be understood in this way, which of course means that analyses and explanations must be historically grounded. (Theda Skocpol, *States and Social Revolutions: A Comparative Analysis of France, Russia, and China* [Cambridge: Cambridge University Press, 1979], 320)

15. Again, this is a topic deserving greater attention. As I suggested previously, we may be able to identify distinct historical processes that logically "go together" but where the particular pacing and timing of their relative development generate different conjunctures with distinct outcomes. Such claims may allow generalizations across a number of cases, although the need to demonstrate that the distinct processes are logically related will limit the range of such generalizations. Ertman's analysis (linking military threats, literacy, and state-building) and Shefter's (democratization and state-building) would be examples. For more on this type of argument see Mahoney, "Uses of Path Dependence."

16. A skeptic could ask whether there is anything in the social world anchoring the "reasonably well-ordered" end of the continuum. In my view, the extensive literatures built on path dependent arguments suggest a more optimistic conclusion.

17. Jervis makes the same point: "Irreversible changes can confound traditional ways of measuring the impact of independent variables because the removal of the cause does not lead to the ending of the effect" (97).

18. Arthur L. Stinchcombe, *Constructing Social Theories* (Chicago: University of Chicago Press, 1968), 102–3.

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yet shown how that might be possible. Accounts of this sort can be extraordinarily vivid, and may generate compelling *ex post* interpretations of particular conjunctures. These are genuinely valuable attributes. We are still, however, in the realm of Dr. Seuss. If our goal, as Thelen nicely puts it, is to try and "capture the impact of time in as timeless a way as possible" then we may fare better by building on the work of Dr. Stinchcombe.

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