Response to Ledford and Sperber

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OVER the last decade, historians have made steady inroads into the frequently static social sciences as they are trying to understand the changing post-Cold War order and the even more rapidly changing global and domestic political economies. Such softening of disciplinary boundaries is also observable in the other direction. Jonathan Sperber's work on nineteenth-century electoral politics and Kenneth Ledford's study on German lawyers offer two examples among many of historians borrowing concepts and methods from the social sciences.[1] Yet, these encouraging signs of disciplinary trespassing cannot mask the fact that these two disciplines continue only infrequently to publish in each others' journals, intelligently review each others' works, or jointly reflect on the payoffs of interdisciplinary scholarship. Given this limited dialogue, it is a particular pleasure to reply to two such thoughtful and constructive respondents. In subtly tackling the problems inherent in comparing, Kenneth Ledford ventures into the disciplinary borderlands of history and the social sciences while Jonathan Sperber stays more closely in the historical corner and — to use Ledford's apt characterization of his colleagues — "picks cautionary holes in the applicability" of comparisons.

Let me begin with the issue of comparison since it is Ledford's central theme and Sperber alludes to it on two occasions. Ledford nicely explicates the professional disincentives and epistemological qualms underlying the historians' reluctance to compare. I think Ledford is correct with respect to cross-national comparisons but I wonder whether he is not selling historians short, given their frequent comparisons across multiple points in time. Such so-called longitudinal comparisons structure Sperber's analysis of quadrennial voting choices in Imperial Germany, Ledford's study of the German bar associations in Imperial and Weimar Germany, and many other historical studies. The question therefore is not whether one discipline compares while the other does not; but

1. Kenneth Ledford, *From General Estate to Special Interest: German Lawyers, 1878–1933* (Cambridge, 1996); Jonathan Sperber, *The Kaiser's Voters: Electors and Elections in Imperial Germany* (Cambridge, 1997).

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instead it is what sort of comparison is appropriate for a given subject matter.[2]

Both respondents imply — Sperber more directly than Ledford — that my cross-national, cross-temporal and large-number-of-cases comparison represents the universalizing, scientific ambition of mainstream social science that emphasizes testing the sort of monocausal theories that historians understandably dislike as static and reductionist. However sweeping my comparison may seem to historians, it is not intended to partake in any universalizing theory testing. Its primary and admittedly ironic purpose is to show that the historians, whom I labeled pessimists and skeptics for their respective negative assessments of the Reichstag's parliamentarization, engage in that favorite pastime of social scientists, namely "lumping" (e.g., overaggregating) analytical categories. My comparisons follow the historians' inclination of splitting and thereby analytically differentiating degrees of parliamentary sovereignty.[3] The idea behind Tables 2 and 3 is the following: the rows disaggregate parliamentary sovereignty by specifying the particular government-forming and legislative tasks that legislatures assume. The columns roughly differentiate the range across which individual legislatures vary on these particular tasks.[4] The placement of individual countries in the various cells, in turn, illustrates the actual frequency distribution of individual cases across this range of parliamentary sovereignty, thus clarifying how exceptional or unexceptional the Reichstag was. The tables thus serve to define more precisely what constitutes the norm of parliamentary sovereignty against which exceptions and degrees of exceptionalism can be assessed. The large number of cases I compare thus are not meant to establish some broad, universalistic generalization about parliamentary sovereignty but to differentiate its multiple dimensions and facets. The sort of longitudinal comparisons, at which historians excel, are of limited use for explicating the benchmarks that implicitly define exceptionalist debates. Such comparisons are appropriate for determining the degree of change between two points in time, but they are less effective in assessing the significance of any such changes and where a particular point in time places a country on the larger developmental trajectory.

While my broad comparisons helped to differentiate analytical categories, they are — and here I agree with my respondents — limited in explaining the actual parliamentarization process. By exclusively focusing on the period after

2. Theda Skocpol and Margaret Somers, "The Uses of Comparative History in Macrosociological Inquiry," *Comparative Studies in Society and History* 22 (1980): 174–97.

3. The distinction between lumpers and splitters was made by Jack Hexter, *On Historians* (Cambridge, 1979), 241–42.

4. These tables are drawn from a large multinational research project that Herbert Döring conducted in large part to move the debate concerning governing institutions beyond the stale, overly abstract differentiation between presidential and parliamentary systems. Herbert Döring, ed., *Parliaments and Majority Rule in Western Europe* (New York, 1995).

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1900, my analysis treats Germany as a single, temporally undifferentiated and, hence, static case; it says little about the level of parliamentarization beforehand or what accounts for the noted changes over time. Analyzing continuities and discontinuities across time is the central preoccupation of historians and such analysis is far more effectively conducted through single-country, longitudinal comparisons or through same-period, cross-national comparisons.[5] It would be helpful if skeptics and pessimists produced such national, longitudinal comparisons to illustrate more effectively the Reichstag's alleged immutability or inconsequential transformation. Conversely, optimists, that is historians arguing that the Reichstag's sovereignty expanded, could strengthen their case if they were to elaborate on Rauh's still fairly broad, national, longitudinal comparison. By contrast, the sort of late nineteenth century, cross-national comparisons, which both Ledford and Sperber suggest, would throw into clearer relief the Reichstag's exceptionalism vis-à-vis contemporary legislatures. Such comparisons also enhance the countries' historical background conditions, thus helping to isolate causal variables which account for varying national rates of parliamentarization. In short, there are different ways of comparing and their respective payoffs vary with what one tries to understand.

Let me quickly address two more comparison-related issues raised by Ledford. He claims that my comparison of works written at different points in time mischaracterizes the evolution of the debate and ultimately overlooks that "historians of Germany have simply moved beyond the [*Sonderweg*] debate." As for the historiographical evolution, Ledford correctly points out that over the last twenty years a number of important studies of state (*Länder*) parliaments have been published that I acknowledged in footnote 7. Thomas Kühne favorably reviews these studies in his 1998 article but still concludes that no similar work on the Reichstag and Bundesrat has been forthcoming since the 1980s.[6] Christoph Schönberger opens his recent article stating that, "Since Manfred Rauh's work in the 1970s and the controversy it provoked, scholarship has largely moved away from the discussion of the parliamentarization."[7] A closer look at the *Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien*, which Ledford cites to support his case, reveals that of the fifty-six volumes published since 1985, only four directly deal with the Reichstag[8] as compared

5. I discuss this point more fully in *Institutions and Innovation: Voters, Parties, and Interest Groups in the Consolidation of Democracy — France and Germany, 1870–1939* (Ann Arbor, 2001), 15–21, 157–69. See also John Lewis Gaddis, *The Landscape of History* (Oxford, 2002), 28–32.

6. Thomas Kühne, "Parlamentarismusgeschichte in Deutschland: Probleme, Erträge und Perspektiven einer Gesamtdarstellung," *Geschichte und Gesellschaft* 24, no. 2 (1998): 325–26.

7. Christoph Schönberger, "Die überholte Parlamentarisierung," *Historische Zeitschrift* 272, no. 3 (2001): 623.

8. After 1985 volumes 103, 112, 116, 133 dealt with the imperial Reichstag; before 1985, it was volumes 7, 20, 47, 50, 54, 58, 59, 60, 67, 72, 73. I did not count biographical studies focusing on individual deputies or party monographs.

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to eleven volumes out of eighty-one volumes published between 1952–1985.[9] Finally, an admittedly impressionistic citation analysis further underscores my point. I compared the average annual citations of Rauh's two books with works of related scope that did not deal with the Reichstag. The results show relative disinterest in parliamentarization. Rauh's two books were cited on average 0.8 times per year compared to 1.6 citations for Hans-Dieter Puhle's *Agrarische Interessenpolitik* (1966), 1.7 citations for Dieter Langewiesche's *Liberalismus im 19th Jahrhundert* (1988), 2.3 citations for Dieter Groh's *Negative Integration* (1973).[10]

I emphasize the stagnation of Germany's parliamentarization debate because it directly relates to Ledford's point that German historiography has largely moved beyond the *Sonderweg* thesis. I do not contest Ledford's claim that the new scholarship stimulated by Blackbourn and Eley twenty years ago has moved German historiography beyond the *Sonderweg* thesis. But given the aforementioned disinterest in the Reichstag, I question whether his claim equally holds for Germany's governing institutions.[11] However, should these institutions receive renewed scholarly attention, the new research, one hopes, will move in the same post-*Sonderweg* direction that it has in German history at large. Ledford points out that recent German historiography casts off the stifling liberal triumphalism, which for too long defined, and hence confined, the discussion of Germany's political development, and focused instead on the limits of liberalism itself and not just its absence or presence. Schönberger's important recent article might be the opening salvo in this direction as he invites historians to compare Germany's parliamentarization not exclusively against the liberal Westminster model but also against the nonliberal or so-called consociational constitutional traditions found in smaller European nations like Belgium, Netherlands, Switzerland, and Austria.[12]

Unlike Ledford, Sperber is less concerned about the broader issue of how to compare and is more interested in checking the accuracy of my examples. Such

9. For the unwillingness of German historians, even when they study the Reichstag, to engage the parliamentarization debate, see Herman Butzer, *Diäten und Freifahrt im Deutschen Reichstag* (Düsseldorf, 1999), 20.

10. I used First Search's Art and Humanities database. Manfred Rauh, *Die Parlamentarisierung des Deutschen Reiches* (Düsseldorf, 1977); idem, *Föderalismus und Parlamentarismus im Wilhelminischen Reich* (Düsseldorf, 1973); Dieter Langewiesche, *Liberalismus in Deutschland* (Frankfurt am Main, 1988); Dieter Groh, *Negative Integration und revolutionärer Attentismus* (Frankfurt am Main, 1973); Hans-Jürgen Puhle, *Agrarische Interessenpolitik und preussischer Konservatismus im wilhelminischen Reich, 1893–194* (Hanover, 1967).

11. Furthermore, if Hartwin Spenkuch's recent review article is any indication, the debate still seems to be alive in Germany, even if Americans have grown tired of it. "Vergleichsweise besonders? Politisches System und Strukturen Preussens als Kern des 'deutschen Sonderwegs'," *Geschichte und Gesellschaft* 29 (2003): 262–93.

12. Schönberger, "Parlamentarisierung," 659–64.

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fact checking is valuable especially for social scientists who lack the historians' detailed knowledge. Yet I would like to add a few cautionary reservations with respect to checking the factors about German parliamentarization. It is the factual deficit more than factual inaccuracy, I believe, that is the primary obstacle to advancing the parliamentarization debate. Given the aforementioned disinterest, we know surprisingly little about the Reichstag's internal workings and even less about the Bundesrat. This factual deficit is particularly pronounced in the less visible power relations between the Reichstag, Bundesrat, and Reichsregierung. As I point out in the article, the frequent veto bargaining among these three governing institutions took the form of veiled veto threats being exchanged for behind-the-scenes policy concessions. These general caveats notwithstanding, the accuracy of existing knowledge certainly matters, and here Sperber finds fault with some of the information on which my interpretation of the powers of the Reichstag and those of the Bundesrat were based.

Regarding the Reichstag, Sperber claims that my analysis overstates its role in the dismissal of governments and in shaping legislative outcomes. He argues that the fact that the members of the government were senior state servants (Staatssekretäre) rather than regular politicians somehow lessened the Reichstag's dismissal powers. It is unclear to me how a minister's civil service status protects him against parliamentary votes of no-confidence. Sperber further contends that Frauendienst provides little evidence to support his claim that the Reichstag had dismissal powers. There is an inherent epistemological problem in measuring the importance of negative legislative powers, such as ministerial or governmental dismissal powers, since such powers are more frequently threatened than actually used. So, if we look for the number of resignations, they may be infrequent, while political concessions in exchange for withdrawing threats of dismissal might be frequent. Such hidden concessions are difficult to observe, much less count. It is with respect to such concessions that Frauendienst provides most of his examples, and these ultimately led him to conclude that "no minister had any prospect of working successfully, let alone remain in office, if he did not enjoy a reliable parliamentary majority."[13]

Sperber also contends that I overstate the Reichstag's legislative powers. He asserts that I underestimate the Bundesrat's importance and that my two examples fail to back my case because the 1909 bill on value-added tax for land failed to be accepted while the anti-Jesuit law of 1903 was not fully ratified for another fourteen additional years. As for the examples, Sperber may be right but I cannot verify his claim since he does not give any sources against which I could compare mine (Rauh). This unresolved claim notwithstanding, even

13. Werner Frauendienst, "Demokratisierung des deutschen Konstitutionalismus in der Zeit Wilhelms II.," *Zeitschrift für die gesamte Staatswissenschaft* 113, no. 4 (1957): 738.

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skeptics like Blackbourn do not contest the Reichstag's powers to enact legislation and detailed legislative studies agree.[14] The article cites the reversal of anti-Socialist laws, the rejection of antilabor Sedition and Penitentiary Bills, and successful pressuring for parliamentary diets, travel subsidies, shortening military appropriations, and secret ballots as examples for the Reichstag's legislative clout. Sperber appears unimpressed by these examples and still seems to adhere to his 1998 claim that "the powers of the Reichstag were less than impressive. [. . .] The Reichstag did not even have the right of initiative; all proposed legislation had to come from the Bundesrat."[15]

While Sperber accentuates the Bundesrat's powers to block the Reichstag, he questions my claim that it curbed state authority. He does so in two ways. First, he points out that it could not prevent the Kulturkampf, antilabor legislation, and anti-Socialist laws. He is right that the Bundesrat failed to curb majorities from acting tyrannically in these three instances. But did it want to? Just because federalism is not a perfect antityrannical cure-all (witness U.S. Jim Crow South) does not vitiate its checking potential on many other issues. The Bundesrat's failure to check violations of workers' civil rights is not surprising given its undemocratic makeup. However, when it came to state interests, I cite evidence that it acted as an effective check indeed. Second, Sperber questions the validity of using the French Second Empire to illustrate how pure democracies without significant checks and balances like federalism often end up producing tyrannical governments. He is correct in pointing out that I mischaracterized the authoritarian Second Empire as a democracy on a par with the First and Second Republics. Yet in making this point, he overlooks that the Second Empire's authoritarianism was assisted by the absence of any French federal institutions that could check Napoleon III's coup d'état and subsequent manipulation of electoral politics. So the fact that the Second Empire was more authoritarian than I presented it underscores my central point that federalism checks political power, be it democratic or authoritarian.

In sum, Ledford and Sperber shed two different yet still complementary lights on the benefits and limitations of interdisciplinary scholarship.[16] I am grateful that they read my article with a constructive skepticism and resisted the all too common dismissive reprimands levied against disciplinary interlopers for failing

14. See for example: Woodruff Smith and Sharon Turner, "Legislative Behavior in the German Reichstag, 1898–1906," *Central European History* 14, no. 1 (1981): 3–29; Cheryl Schonhardt-Bailey, "Parties and Interests in the 'Marriage of Iron and Rye'," *British Journal of Political* Science 28 (1998): 291–330; Alastair Thompson, *Left Liberals, The State, and Popular Politics In Wilhelmine Germany* (Oxford, 2000).

15. Sperber, *Kaiser's Voters*, 2.

16. For an attempt to explore the payoffs of such interdisciplinary research see Kreuzer *Institutions and Innovation*, 8–15.

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to meet this or that element of the critic's disciplinary catechism. Such intellectual parochialism impedes interdisciplinary dialogue and extracts a considerable intellectual price.[17] The narrow-mindedness of social scientists manifests itself in dismissing historical narratives as lacking operationalized variables, unsystematically gathering and analyzing data, and ultimately failing to generate predictive theories. Historians, in turn, display their parochialism by belittling theory-driven and methodologically grounded arguments for relying on secondary sources, overlooking a dozen factors or more, and making simplistic generalizations. This mutual disdain comes at a heavy price. For social scientists, the disinterest in history makes it easier to define the study of a subject in terms of methodological imperatives and to advance explanations that are unnecessarily reductionist and static. For historians, the ignorance of social science prevents them from incorporating useful theoretical and methodological insights into their research practices, from comparing across countries and from effectively differentiating between systematic and spurious correlations, thereby advancing causal arguments that are slightly less baroque. Hopefully, constructive commentaries, as those offered by my two respondents, will help to reduce the price each of our disciplines pays for turning a cold shoulder toward each other.

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17. An incredibly stimulating and thoughtful contribution to such a dialogue is Gaddis, *Landscape of History*. Among other things, he makes the provocative claim that historians, without really trying, are methodologically more in tune with the natural sciences than the social sciences with their outdated, Newtonian view of the world.

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