Comparing Comparisons: Disciplines and the Sonderweg

Kenneth F. Ledford

Inspired in part by a recent exchange in *Central European History* in which Volker Berghahn and Margaret Lavinia Anderson debated trends in the historiography of the Wilhelmian era of the Kaiserrreich, Marcus Kreuzer offers to historians the perspective of a political scientist on what he calls the neglected issue of the “parliamentarization” of the imperial regime over its last two decades. From the starting point of an interpretive dispute about German exceptionalism among historians, a dispute that to him seems easily solvable, Kreuzer compares the various arguments that historians have made about whether by 1914 the Imperial Reichstag was already, or was developing toward becoming, a truly parliamentary regime; he next categorizes historians into schools of interpretation; and he finally resolves the issue for historians by comparing the imperial Reichstag with an array of other parliaments, showing that, based upon evidence contained in the cumulative scholarship produced by historians, it in most cases ranks toward the more powerful end of the spectrum. In the end, he remains puzzled as to why this easily available method has remained too difficult for historians to grasp, concluding that it has been rather easy to drive a stake through the heart of the German Sonderweg, as it is with any exceptionalism.

In the CEH exchange, Berghahn lamented that the outcome of the Sonderweg debate has been for historians of Germany to turn away from inquiries that might help answer the question of how Germany got itself into the catastrophe of the First World War toward examinations of cultural developments at the grass roots of German society in the decades before that war. He exhorted historians of Germany “to take stock of research on the German Empire since the 1960s and to try to reintegrate the fruits of recent work with scholarship that


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has not been invalidated simply because we no longer bother to look at it."² Anderson defended post-Sonderweg scholarship as neither so fragmented nor so decentered as Berghahn suggests, arguing that a conception of the "future of the German Empire as more open than we once thought" may actually help lead to better explanations for the war's origin.³ Like Kreuzer, Anderson sounds a little surprised that the specter of the Sonderweg is still haunting the historiography of the Kaiserreich.

Kreuzer's brave foray out of his disciplinary fortress onto the terrain of the historians provides a useful lesson to our discipline in three ways. First, he shows that, although historians have called for meaningful employment of comparative methods for decades, and although a central part of historians' critique of the Sonderweg has been the desire to make explicit the comparison that was always implicit in that normative argument, as a discipline we still have much to learn from other disciplines who compare as a matter of course. Boundaries of training, career survival strategies, and even contours of the job market, as well as historians' temperamental and sometimes even philosophical historicism, still inhibit us from the daring use of comparison so frequently made by political scientists, sociologists, and economists. Indeed, our first response, as I demonstrate below, is to pick cautionary holes in the applicability of examples chosen for comparison, yet his message persists that comparison nonetheless remains worthwhile. Second, he raises the outsider's question of a scholar interested in politics and political institutions as to why detailed monographs studying political institutions have become rare in recent years. While I contest the premise of that question, I suggest that the explanations for the circumstance that leads Kreuzer to pose it lie in the very critique of the Sonderweg that he joins, and that the routes that historians have chosen to escape the sterility of the Sonderweg debate often trace paths different from those that political scientists would choose. And third, he forces historians to articulate the reasons for the actual outcome of the Sonderweg debate in ways that are more clearly accessible to scholars from other disciplines, who often look, as does Kreuzer, to our research for the raw material for their own discipline-embedded applications.

Kreuzer focuses narrowly on the historiography of Germany's parliamentarization to reassess its prevailing exceptionalist interpretation. Categorizing the existing historical research into schools of "optimists," "pessimists," and "skeptics" with regard to the development of the Reichstag toward "true" parliamentary power, Kreuzer brings to the table from political science the broad comparative perspective of comparative politics and comparative parliamentary systems. Measuring five different axes of parliamentary power, and comparing

² Berghahn, 81.
³ Anderson, 90.
the Reichstag with a wide array of European parliamentary regimes over a breathtaking time scale, he concludes that by each measure, the Reichstag fit into a "normal" spectrum of parliamentary robustness in an utterly unexceptional way. By means of a consequent comparison of measurable features, and by shifting the inquiry from what the Reichstag could not do to what it could do, Kreuzer again refutes the Sondersieg, vanquishes the skeptics, and aligns himself unequivocally with the optimists.

From the very beginning of the Sondersieg debate, formally initiated with the publication in Germany of the Eley-Blackbourn critique in 1980 and spread more broadly with their amplified English version in 1984, critics of the "pessimism" articulated by many, but most prominently by Hans-Ulrich Wehler, made the modest but indisputable observation that any claim of exceptionalism or peculiarity is inherently comparative. To be exceptional or peculiar is necessarily to be different from everything else; the real questions therefore are whether the standard against which to compare the subject under study, in this case the political institutions and political culture of the Kaiserreich, is explicit or only implicit, and whether the comparison is carried out consequently, with as much effort and space devoted to the study of the measure as to the subject to be measured. Thus historians can improve all of their work by employing careful, thorough, and nuanced comparison.

Now this agenda is easy to proclaim but very difficult for historians to carry out, and the number of comparative histories written by historians that have been critically well-received by specialists in both fields is very small.5 There are several reasons besides fear of criticism to explain why historians tend not to engage in comparison. First, the nature of graduate training results in our becoming by and large country specialists. Historians of Germany no doubt train as historians of Europe, but structures of funding for research, archival organization, and realistic limits of time that can be devoted to graduate school contribute to our proclivity to define our expertise largely as one-country


specific. Second, the structure of the academic historical profession still promotes the idea of "country expertise." Although larger departments have the luxury of tenure-track slots for "comparative historians," and smaller departments increasingly value colleagues whose teaching interests include comparison and encompass wide geographic scope, most job searches still define themselves in one-country specific terms. Likewise, although publishers also embrace and encourage comparative history, their catalogs still advertise their British, French, German, and Russian lists. Indeed, the organization of scholarly historical journals into a world of _Central European History, French Historical Studies_, and _The Journal of British Studies_ reinforces our comfort with country-specific definitions of scholarly expertise.

Finally, and perhaps most importantly, historians’ reluctance to engage in extensive, explicit comparison flows from the very historicism that forms the philosophical foundation of our modern professional ideology. The celebration of particularity that lies at the heart of historicism, the lingering notion that the best posture for understanding any historical subject is to view it as _unmittelbar zu Gott_, actively discourages deeply rooted faith in the efficacy of explicit comparison. Even as we realize that as a logical matter any claim of particularity or exceptionalism is inherently comparative, even as we open ourselves to the use of theory and methods from surrounding disciplines and seek to transcend the limits of historicist particularism, we still carefully delimit and circumscribe the scope of the claim to validity of our research findings, ever aware, especially in Germany, that things are just different in the next valley over.

A close examination of Kreuzer's own article from the perspective of a historian demonstrates the kind of critique that discourages historians from attempting broad and deep explicit comparisons over time and space. In his comparative charts that illustrate how the Imperial Reichstag fit at various places on a spectrum of European parliaments (and indeed often place it toward the more powerful end of that spectrum), Kreuzer includes contemporary regimes in the Federal Republic of Germany, the French Fifth Republic, the United States without any reference to what era, the British Parliament in Westminster without any time referent, many other European states, presumably in their current twenty-first century forms, as well as the Reichstag during the Weimar Republic. Indeed, the only comparison that is contemporary to the Imperial Reichstag is the parliament of the French Third Republic (Tables 2 and 3). Few historians, even the most outspoken proponents of comparative history, are truly comfortable that such transhistorical comparisons really can be dispositive of complex issues such as parliamentarization in Wilhelminian Germany.

Further Kreuzer's choice of historical works to examine in order to establish his three schools of historians falls into two errors of comparison. First, he treats as timeless, and hence equivalent, works conceived, researched, and written at
wildly different times and thus addressing different concerns. Table 1b encompasses works as various as Walter J. Shepard’s contemporary account of 1911 together with research as recent as the 1990s, without considering the advantages (such as access to newly-accessioned archival sources) enjoyed by subsequent research and the realities of development in the scholarly conversation in the literature over a span of at least a quarter century.¹ This essay’s very mode of comparison of historians’ accounts of parliamentarization is rife with errors that distort the nature of the scholarly debate that has occurred. Second, Kreuzer compares works of fundamentally different kinds from eras in which questions were of fundamentally different character. The “map of historiographical positions” embraces textbooks (Berghahn [1994], Craig [1978], Nipperdey [1992], and Wehler [1985 but really dating to 1973]), monographs relating results of empirical research (Rauh [1977]), thematic essays (Blackbourn and Eley [1984, originally 1980]), lectures in a Feitschift (Ritter [1999]), a political science monograph (Fraenkel [1964]), and a recent historiographical essay (Schnberger [2001]). None of this is to suggest that Kreuzer mischaracterizes the position of any of the works that he cites; rather, his choice of items to compare requires some accounting for the different nature of the various species of scholarship and the time-sequence over which the body of work was produced. His comparison has opened him to contestation about his choices. Anticipating all of this criticism, historians who employ comparison remain much more circumspect and less daring, not always to the benefit of the salience of our scholarly claims.

As a second theme, Kreuzer notes quizzically (pp. 333–34) that the historiographical debate has evolved little over time, and argues in support that research into the question of parliamentarization of the Reichstag has ceased in recent years. Here I think that he seriously underestimates the work that has been actually done. He cites the important electoral studies of Jonathan Sperber and Margaret Lavinia Anderson, as well as an important historiographical essay by Thomas Kühne. Yet he neglects to mention the many contributions in the series Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, published by the Commission for the History of Parliamentarism and Political Parties, and now extending to some 127 volumes since 1952, fifty-six of which have appeared since 1985. While not all of these works attack the issue of parliamentarization of the Reichstag explicitly and directly, the totality of the scholarship certainly contributes to a discussion that has by no means ceased.

¹ Kreuzer compounds this problem of telescoping time by using a reference system that refers to the date of the versions of the books that he cites; for example, he makes the Wehler book appear more recent than it is by citing the 1985 English translation of a book originally published in German in 1973; likewise, he dates as 1999 the Bückenforde essay on the “German type of constitutional monarchy,” an essay unchanged since it first appeared in 1967.
particular, Kreuzer's focus on the question of parliamentarization of the Reichstag directs his attention away from following the very important research into state-level political institutions, such as Kühne's own book about the three-class suffrage system and electoral culture in Prussia and Hartwin Spenkuch's important book on the Prussian Herrenhaus.  

Nevertheless, despite this large literature that Kreuzer neglects, it certainly remains true, as Berghahn also pointed out, that the center of gravity of historical research since the late 1970s no longer focuses on political institutions and political culture and thus on parliamentarization, the subject about which Kreuzer has chosen to write. A number of reasons lie behind this shift, reasons that contradict his assumption that the larger historiographical debate on the Sonderweg has evolved little and rather suggest that, as he concludes, the pessimists have lost, and while the optimists may not have won, even skeptics have shifted the focus of their work away from the labeling and categorizing that characterized the Sonderweg debate to a focus on experience and agency. Whether couched in terms of the "social history of culture" or the "cultural history of society," historians such as Anderson have shifted their focus from the workings of the Reichstag to the voting behavior of the public, the experience of the franchise, and as indicated above, to political institutions other than the Reichstag. The focus of historical research has also tended to shift from sweeping interpretation and master narrative to the inner workings of institutions, the social construction and cultural meaning(s) of categories such as religion, class, and gender, and to a tolerance of ambiguity and the absence of a master narrative. Historians have thus by and large accepted that Imperial Germany and its institutions, whatever their peculiarities, simply ranked somewhere on a spectrum, and need not be measured against some hypostatized, normative measuring rod simply because of the Sonderweg of 1933–45. Rather than failing to evolve, the historiographical debate about the Sonderweg has indeed largely resolved itself against a pronounced particularity of German history, and historians have thus been freed to conduct research without the need to label and categorize that the Sonderweg had imposed. This is Anderson's point, and it is the cause of her surprise that Berghahn had resurrected the "old" categories of debate. Research interest, spurred first by social history and then by the cultural turn, led historians often away from political institutions toward experience, and even work that addresses political institutions focuses more on meaning and experience than upon labeling and categorizing. Historians of Germany have simply moved beyond the debate.

Finally, Kreuzer's questions return historians of Germany to articulate what is in fact now the larger argument in which we are engaged and that remains of interest to scholars in other disciplines. From the very beginning of the Eley-Blackbourn critique of the Sonderweg, there was explicit a quest for bigger game than simply the refutation of one interpretive school in the historiography of the Kaiserreich. The aim was to "normalize" German experience, to view it more as a point on a spectrum rather than an outlier that offered no lessons for larger interpretations of the history of Europe during industrial capitalism in order to emphasize the peculiarity of the Anglo-American model of liberalism-democracy-capitalism that had been rendered normative by modernization theory. Indeed, Kreuzer himself notes "the peculiarity of the British" Westminster model of parliament (pp. 334–35). Instead of arguing that Imperial Germany went wrong because it "failed" to become liberal, to develop a real parliamentary system, historians of Germany now use the German experience to emphasize the limits of liberalism, the fragility of parliamentary systems, and to undercut the triumphalism of neo-liberal dogma as it emerged over the last decades of the twentieth century. The intellectual shift that occurred was from a Sonderweg that stressed German peculiarity in the past to strengthen democracy in Germany in the present, to stressing Anglo-American peculiarity or exceptionalism in an effort to guard against the neo-liberal messianism that proclaimed "the end of history" and the inevitable triumph of liberal dogma in an age of globalization. Historians have sought to "normalize" the history of Imperial Germany in order to exemplify the limits of liberalism, both before and after 1914. Yet the noise and turmoil of the Sonderweg debate, and the absence of a new metanarrative for the history of the Kaiserreich, have prevented scholars in other disciplines from perceiving this fundamental and important shift.

So Kreuzer has added his own contribution to the endgame of the Sonderweg, and while historians of the Kaiserreich can quibble, as I have, with some of the precise methods of his comparisons, we will not quarrel with his conclusion, for the discovery that the Sonderweg debate has ended is not new for us. He challenges us rightly to consider again avenues by which we can move toward consequent comparative history, and he calls upon us to articulate more clearly, and not just for internal disciplinary consumption, precisely where our historiography of German exceptionalism or normality stands. And Kreuzer brings good news to the historical discipline for dialogue with social science disciplines. As he emphasizes in his conclusion, social sciences have returned recently to

8. The trial that Geoff Eley seeks to separate differs slightly from this formulation; see idem, "The British Model and the German Road: Rethinking the Course of German History before 1914," in Blackbourn and Eley, The Peculiarities of German History, 37–155, especially "Bourgeois — Liberalism — Democracy: Some Necessary Distinctions," 75–90.
historical approaches. The happiest message for historians is that when we write that more comprehensive and clearly articulated account of Imperial Germany's position on the spectrum of normalities, there will be a wider audience of disciplines that we can reach.

Response to Ledford and Sperber

Marcus Kreuzer

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. 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


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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

³ The distinction between lumpers and splitters was made by Jack Hester, 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).
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. 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. 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." 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 as compared

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.
to eleven volumes out of eighty-one volumes published between 1952–1985. 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 Puhl’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).

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. 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.

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, Däten und Freiheit im Deutschen Reichstag (Düsseldorf, 1999), 20.
10. I used First Search’s Art and Humanities database. Manfred Rauh, Die Parlamentarierung 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 Aktivismus (Frankfurt am Main, 1973); Hans-Jürgen Puhl, Agrarische Interessenpolitik und preußischer Konservatismus im wilhelminischen Reich, 1893–1914 (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. “Vergleichweise besonders? Politisches System und Strukturen Preußens als Kern des ‘deutschen Sonderwegs’,” Geschichte und Gesellschaft 29 (2003): 262–93.
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 parliamentization. It is the factual deficit more than factual inaccuracy, I believe, that is the primary obstacle to advancing the parliamentization 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 Frauenhöfer 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 Frauenhöfer 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.”

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

skeptics like Blackbourn do not contest the Reichstag's powers to enact legislation and detailed legislative studies agree. 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."  

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. 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


16. For an attempt to explore the payoffs of such interdisciplinary research see Kreuzer Institutions and Innovation, 8–15.
to meet this or that element of the critic's disciplinary catechism. Such intellectual parochialism impedes interdisciplinary dialogue and extracts a considerable intellectual price. 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.