Comparing Comparisons: Disciplines and the *Sonderweg*

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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 Kaiserreich, 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.[1] 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

1. "An Exchange on the Kaiserreich," Volker R. Berghahn, "The German Empire, 1871–1914: Reflections on the Direction of Recent Research," *Central European History* 35 (2002): 75–81, and Margaret Lavinia Anderson, "Reply to Volker Berghahn," *Central European History* 35 (2002): 83–90.

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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.

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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 *Sonderweg*, vanquishes the skeptics, and aligns himself unequivocally with the optimists.

From the very beginning of the *Sonderweg* 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.[4] 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

4. See David Blackbourn and Geoff Eley, *Mythen deutscher Geschichtsschreibung. Die gescheiterte bürgerliche Revolution von 1848* (Frankfurt am Main, 1980) and idem, *The Peculiarities of German History: Bourgeois Society and Politics in Nineteenth-Century Germany* (Oxford, 1984). For the succinct statement of the comparison inherent in any discussion of exception from a "norm," see ibid., 10.

5. Exemplary comparative histories that fit this description include George M. Fredrickson, *White Supremacy: A Comparative Study in American and South African History* (Oxford, 1981), Peter Kolchin, *Unfree Labor: American Slavery and Russian Serfdom* (Cambridge, Mass., 1987), and Colleen A. Dunlavy, *Politics and Industrialization: Early Railroads in the United States and Prussia* (Princeton, 1994). For thoughtful reflections that place issues of exceptionalism in a comparative perspective, see the essays in Elisabeth Glaser and Hermann Wellenreuther, eds., *Bridging the Atlantic: The Question of American Exceptionalism in Perspective* (Cambridge, 2002).

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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 Wilhelmian 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

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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.[6] 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 *Festschrift* (Ritter [1999]), a political science monograph (Fraenkel [1964]), and a recent historiographical essay (Schönberger [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. In

6. 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 Böckenförde essay on the "German type of constitutional monarchy," an essay unchanged since it first appeared in 1967.

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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*.[7]

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 hypostacized, normative measuring rod simply because of the *Sonderend* 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.

7. Thomas Kühne, *Dreiklassenwahlrecht und Wahlkultur in Preussen 1867–1914. Landtagswahlen zwischen korporativer Tradition und politischem Massenmarkt* (Düsseldorf, 1994), and Hartwin Spenkuch, *Das Preussische Herrenhaus: Adel und Bürgertum in der Ersten Kammer des Landtages 1854–1918* (Düsseldorf, 1998).

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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.[8] Indeed, Kreuzer himself notes "the peculiarity of the British" Westminister 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 triad 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 "Bourgeoisie — Liberalism — Democracy: Some Necessary Distinctions," 75–90.

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historical approaches.[9] 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.

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9. Terrence J. McDonald, ed., *The Historic Turn in the Human Sciences* (Ann Arbor, 1996).

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