Fetishizing the Soviet Collapse: 
Historical Rupture and the 
Historiography of (Early) 
Soviet Socialism

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Historians of the Soviet Union and nonspecialists alike have misunderstood the relationship between the Soviet collapse and the historiography of the Soviet Union. There has been a fetishization of the collapse. Fetishize is a strong word, but lest any take umbrage I include myself among the fetishizers, and use it here to reveal, rather than to reproach. Nor do I intend, by employing the word, to suggest pathology. Rather, I mean the investing of a phenomenon—in this case the “collapse”—with unwarranted reverence and even awe. What is it about the Soviet collapse that we have so revered? For one thing, many have asserted that the collapse—including its shock waves in Eastern Europe—has been the decisive factor in generating the shifts in perspective, such as new paradigms and the attendant changes in thematic focus, and, more broadly, in providing the impetus for the field’s emergence out of not-so-splendid isolation into the historical mainstream by drawing on new theories and concepts, as well as non-Russian historiographies.1 So

I dedicate this essay to Nicholas V. Riasanovsky, teacher, mentor, and friend, and historian of Eurasia. Conversations with Sarah Stein helped me to crystallize and embolden some of the ideas presented here. For extremely valuable critiques of earlier versions, I am grateful to Eve Levin, the anonymous readers for The Russian Review, Mark von Hagen, Sarah Stein, Willard Sunderland, and Tani Barlow. For expert attention to the manuscript, I thank Kurt Schultz and Sigrid Asmus. I wish also to thank the scholars whom I interviewed for their patience, time, promptness, and good humor. My inclination to place the recent development of the Soviet field in comparative perspective was stimulated by ongoing conversations with Asia specialists in the University of Washington’s Henry M. Jackson School of International Studies and Department of History. Although the students in my graduate seminar in the spring of 2006 were (mercifully for them!) not subjected to reading a draft of the essay, they engaged in helpful dialogue with me about some of its basic premises. Some of the research was supported by the University of Washington’s Royalty Research Fund.

Glennys Young

as not to caricature the historiography, I would note that there is some intuitive understanding, and perfunctory acknowledgment, that the Soviet field began changing before the collapse. Some have wisely noted that it is dangerous to fetishize the “decade as a default chronological unit of analysis.” Furthermore, there have been important challenges to the notion of 1991 as a “great break” in the historiography of the Soviet period, as well as valuable analyses of continuity in conceptualization and paradigms across the 1991 divide. Some have also acknowledged that the aftermath of the collapse happened to coincide with the “growing interest in cultural and social theory that in the 1990s pulled the historical profession away from the social sciences and towards the humanities”—and toward, in particular, the new cultural history that drew upon theorists such as “Foucault, Derrida, Habermas, and Bourdieu … as cultural authorities,” as well as the surge in postcolonial studies. Yet tremendous causal weight is placed on the collapse itself, which is represented as the essential catalyst, as Mark von Hagen has put it, for “relocating the history of the USSR] in the broader intellectual contexts of a changing academic culture of historical writing.” In granting the Soviet collapse itself immense causal power, we have told ourselves that it not only motivated but made reflexive and indeed forced upon us the stock-taking and the “search for new modes of understanding” that have swept the field since 1991. However, other than our assertions, no evidence exists that the collapse acted as such a catalyst. It is also limiting, for reasons I elaborate below, to tell ourselves it was the most important factor. There has been a shift in the Soviet field. But the complexity of that change has itself been collapsed into the Soviet collapse.

While there is consensus about the “collapse as catalyst” thesis, we have not agreed upon, let alone always specified, what the collapse was, when it began and ended, and what, specifically, about it was causal. For some historians the “collapse” refers to the formal end of the Soviet Union itself; for others it seems to mean the collapse of the “system,” whose definition has also been contested by historians and political scientists. For some, the collapse is a circumscribed moment in time, such as the dissolution of the Soviet Union in December 1991; for others, it is a process or a series of transformative events by which not only the Soviet Union but also Soviet Communism and its ideocratic party-state—in other words, not only the political system but also the civilization that for seventy-four years presented itself as the superior alternative to Western capitalism—unraveled. Even when historians implicitly convey that they understand the collapse as a process, they tend not to specify when that process began, and why: in 1917, or even before? (For Martin Malia, the ultimate origins of the collapse lay in Bolshevik ideology, even if he restricts it to 1989–91.) 1956? The Brezhnev Era? 1985? 1986? 1989?

2"Post-Post Historiography, or the Trends of the 'Naughts,'" Kritika 5 (Fall 2004): 645.
August 1991. Nor is it always clear, let alone explained, when the collapse ended and why. There has also been disagreement *cum* lack of specificity concerning what it is about the collapse that “forced” historians to engage in a wider dialogue with other fields and non-Russian historiographies: the insights gleaned from the opening of the archives? An “identity crisis” that was “provoked” by the disintegration of the geopolitical entity and political system that most Western academics and Soviet citizens alike assumed would last, in one form or another, forever? What the collapse as process revealed about the nature of the Soviet system—to, as Stephen Kotkin has put it in his discussion of the conceptual breakthrough that Jan Gross gleaned through the context of Poland’s Solidarity, “insights via dismantling”? Some un- or ill-defined combination thereof? This essay is, in part, devoted to answering these questions, and unpacking these different ways of imagining the causal relationship between the collapse and historiographical shifts.

If we are going to reassess the “collapse as catalyst” thesis, we need a definitive concept of what constituted the collapse and why this was the case. As for what collapsed, it is useful to keep in mind that the Soviet system included, arguably, six “functioning elements”: “the official and obligatory ideology; the especially authoritarian nature of the ruling Communist party; the party’s dictatorship over everything related to politics, buttressed by the political police; the nationwide pyramid of pseudodemocratic soviets; the state’s monopolistic control of the economy and all substantive property; and the multinational federation, or Union, of republics that was really a unitary state dominated by Moscow.” But did this Soviet system, in fact, “collapse”? Collapse, to be sure, is the noun of choice for historians and political scientists of a variety of political leanings and scholarly approaches. Yet one reason to be hesitant about using the word collapse is that it is so politically loaded (and analytically consequential); it assumes that both the Soviet state and the Soviet system were doomed by deadly flaws. But the term raises other

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12The components of the Soviet system have been contested, of course. Here I draw on those given in Stephen Cohen, “Was the Soviet System Reformable?” *Slavic Review* 63 (Fall 2004): 468–69.


concerns. "Collapse" suggests a structure that falls down without much, if any, human agency, and a process in which the previously existing structure is turned into unrecognizable rubble. Both of these assumptions are incorrect. Rather, it has been apparent from the outset that it took quite a bit of human agency, especially that of existing Soviet elites (party, Komsomol, and other nomenklatura officials), whether in the Center or in the republics and periphery more generally, to dismantle the Soviet system. Despite the media’s trumpeting of the rapid disappearance of the Communist party, for example, this institution simply resurfaced in different form, returning as the Communist Party of the Russian Federation. To emphasize that such decisive human interventions resulted not in unrecognizable rubble but rather, in some cases, in recognizable (if significantly regrouped) elements of a previously integrated system, I prefer the term “Soviet dismantlement” to the shorthand of Soviet collapse.

In the parts of the essay in which I interrogate the “collapse as catalyst” thesis, I understand dismantlement as the process of the intertwined unraveling of Soviet communism and the USSR, a process whose beginning I date to 1988. After reaching a milestone in 1991, the process lasted until 1993, when the last “functioning component” of the Soviet system, the “nationwide pyramid of pseudodemocratic soviets,” which had engaged in a power struggle about “constitutional primacy” with the presidential administration, was finally dissolved. 1988 saw major, objectively discernible challenges to the Soviet system: national movements challenged the integrity of the USSR, Mikhail Gorbachev’s perestroika began to contribute to the disintegration of the Soviet economy, and rightist challenges to Gorbachev’s reforms led him to seek a political base outside the party, in the soviets, thereby launching the process of breaking with the party’s formal monopoly of political power. It was, in fact, in the summer of 1988, at the Nineteenth Party Conference, that Gorbachev saw himself as inaugurating a “peaceful, smooth transition from one political system to another”—a transition he regarded as nothing less than a “revolution.” Perhaps as a result, 1987–88 was also the moment during which many Soviet citizens, especially of the “last” Soviet generation, experienced, as a result
Fetishizing the Soviet Collapse 99

of these simultaneous challenges, a “break in consciousness” (perelom soznaniia) in which they came to believe the Soviet Union would not, in fact, last forever.\(^{20}\) The caesura of 1988 was not between stasis and reform, as in other reform cycles in Soviet history, but between reform and fundamental transformation of the functioning components of the system.\(^{21}\)

As for the principles of selection, to reexamine the relationship between the Soviet dismantlement and the historiography of the Soviet Union is a huge undertaking. I have thus chosen to begin at the beginning, focusing only on the historiography of the early Soviet period (approximately 1917–39). Even given this restricted focus, I do not intend the essay to be an exhaustive survey of the relationship between the dismantlement itself and the state of the scholarship regarding major issues and themes. This is because the organizing categories are not the standard ones of periodization and historiographical debate, but those we have regarded as the cornerstones of the shifts in perspective tied to the Soviet collapse: new paradigms generated by our supposedly post-collapse, prompter-collapse dialogue with the “broader intellectual contexts of a changing academic culture of historical writing.”\(^{22}\) This essay does not, it should be noted, examine the relationship between the collapse and those other shifts in perspective not fundamentally derived from such dialogue, such as our heightened post-collapse perception that there were fundamental continuities between Tsarist and Soviet Russia. In particular, I focus on two examples of the new perspectives or paradigms said to have emerged because of the jolt provided by the collapse: that of scholarship on Soviet subjectivity (attention to language and related practices in order to investigate how ordinary Soviet citizens experienced the Soviet system), and on what Mark von Hagen has recently called the “Eurasia anti-paradigm.” The latter is a corpus of scholarship in Russian history extending from the pre-Petrine through the Soviet period, said to have emerged after and largely because of the collapse, that steers a course between and beyond the hegemony exercised by the Russia/Orient paradigm, especially dominant in the late 1940s and early 1950s, which assumed Russia’s essential “otherness” from the West, and, concurrently, the grip exercised by the Russia–Soviet Union modernization paradigm, especially dominant from the 1970s through the 1980s, which privileged social change as the source of Russia’s semi-convergence with the West.\(^{23}\) In an era of increasing scholarly globalization, encompassing an expanding dialogue between scholars in the amorphous “West” and elsewhere, the decision to focus only on Western scholarship in these two paradigms might be questioned. It is not, for example, that Russian scholars have failed to produce discourses that have reflexively emphasized the importance of the collapse. Rather, a significant degree of this exaggeration has been produced, and reproduced, in the way non-Russian scholars have written about what the collapse has meant for them and for their scholarship. Our most urgent task,

\(^{20}\) Yurchak, Everything Was Forever, esp. 1–3.

\(^{21}\) My thinking on these important issues—the distinction between the Soviet system and the Soviet state (the USSR), and that between “reform of the system” and “systemic transformation”—is informed by Kramer, “The Reform of the Soviet System,” 505–12; and Brown, “The Soviet Union: Reform of the System?” 489–504.


\(^{23}\) Ibid. Discussion of these paradigms can be found, inter alia, in ibid., 449–51 and 451–54. The latter paradigm should not be equated with Marxist-inflected social history of the 1970s and 1980s.
then, is to examine critically the stories that non-Russian scholars have told themselves about the relationship between the collapse and the history they have written. Treating the other side of the story, so to speak, must be the subject for another essay. As for the side of the story told here, I draw upon evidence gleaned not only from a reading of the relevant historiography itself but also from conducting interviews with some of the scholars whose work has made important contributions to the paradigms under focus.

This essay has three parts. The first discusses how we have portrayed the relationship between the Soviet collapse and the development of the historiography of the early Soviet period. I then turn to other factors whose significance for the field’s development we have, arguably, minimized while at the same time privileging the causal role of the dismantlement: the contribution played by certain shifts in historical writing that had nothing to do with the caesura (for example, the linguistic turn), the influence of historians and historiography of Imperial Russia on historians of the early Soviet period, the challenge to area studies, and the role of personal experience. Analyzing the contribution of these underappreciated factors to historiography of the early Soviet Union—factors that began to influence the field prior to the dismantlement—requires some counterfactual analysis, in which I explore the changes in perspective that might have occurred in the field had the caesura not occurred, or had not occurred in the way it did. Offering a revisionist history of the historiography, I also reconstruct shifts in perspective within the field of the history of the USSR in the 1970s and 1980s. In the last section, I suggest how and why overemphasizing the dismantlement has been limiting for historians of early Soviet socialism, and how they could benefit by moving beyond that stance.

THE SOVIET COLLAPSE AND HISTORIOGRAPHICAL RUPTURE: A CONTESTED AND MURKY CONSTRUCT

So much has changed since the collapse of the Soviet Union, and we are, it seems, dizzy from success. At first glance it might seem that so much has changed, not just in the decade of the 1990s but in the “naughts,” that we are talking about a qualitative, not quantitative, shift in the field—and, indeed, a qualitative leap of epic and seismic proportions. For many, the most prominent of these shifts has been the archival bonanza. In fact, some of the scholars I interviewed, such as Yuri Slezkine and Jochen Hellbeck, indicated that this was the chief, and perhaps only, way in which the collapse influenced the design and/or revision of their work.24 True, the bonanza may not have been as rich as we once thought or expected. The archives have been opened only partially; their riches have not yielded, nor are they likely to yield, answers to burning questions; and we have had to be reminded that no matter what the documents say, it is the kinds of “questions asked and the reasons for asking them” that matter most.25 Archives, in other words,

24For example, interview with Yuri Slezkine, 2006, and with Jochen Hellbeck, 2006. All interviews, except with Alton S. Donnelly, were conducted by email and are in my possession.
have allowed scholars to tweak old claims and/or provide more extensive documentation for those claims, but they have not brought new interpretations or conceptual frameworks. Nonetheless, because we have gained access to formerly verboten archival documents, we have been able, as Sheila Fitzpatrick has noted, to engage in certain kinds of historical inquiry, such as that into high politics, the study of which had languished. We have seen a long overdue and very welcome surge of collaboration between Western and Russian scholars, as well as scholars in the successor states of the Soviet Union. The kind of scholarly exchange with indigenous counterparts in the former Soviet Union that historians of the Soviet Union who reside in the West now take for granted is something that ought occasionally to stun those of us who began our study of the Soviet Union during the Cold War. Then it was difficult for Westerners to have contacts with all the Soviet scholars they wished, or to have frank discussions with those scholars with whom they did meet. In the days of closely censored, ultra-snail mail, exchanging ideas from afar was virtually impossible. Today, historians of the Soviet Union, both inside the geopolitical space it once occupied as well as outside it, have integrated the field into the mainstream of historical writing to an unprecedented degree. After 1991 we published scholarship placing the history of the Soviet Union in dialogue with the linguistic turn, subaltern studies, postcolonial studies, gender analysis, and on and on. In a similar way, and in a self-congratulatory tone (with exceptions granted), we have celebrated, as Anna Krylova has wisely reminded us, all that is “new” in the 1990s in the historiography of Stalinism and in that of the Soviet Union more generally.

We have even exaggerated the import of the Soviet collapse for periods of Russian history other than the early Soviet period. To be sure, historians of Imperial Russia have been less prone to overemphasize the impact of the collapse itself on the historiography of Tsarist Russia. In the introduction to a recent volume on the peoples of Russia’s imperial borderlands, the editors note that the “‘revisioning [of] imperial Russia’” has come not from “events alone,” namely the “collapse of the USSR,” but that new perspectives, such as those drawn from disciplines besides history, were “emerging long before the demise of the Soviet Union and ... establishing a foundation for what could be done under the changing circumstances of the past decade.” Still, in the historiography of Imperial Russia, the Soviet collapse is said to account for the rethinking of certain themes. One scholar has written, for example, that it is “due largely to rethinking precipitated by the


One example of dialogue with subaltern studies is Sheila Fitzpatrick, Stalin’s Peasants: Resistance and Survival in the Russian Village After Collectivization (New York, 1994).

For an elegant survey of some of the ways in which historians of the Soviet Union have been placing the field in dialogue with a “changing academic culture of historical writing” see von Hagen, “Empires, Borderlands, and Diasporas,” 445.

Krylova, “Tenacious Liberal Subject,” 119 ff.

collapse of the USSR” that the “supposedly tidy distinction” between “Russia’s past” and the “empire’s history” has been seen as an “academic oversimplification”—or, we might add, between the “Soviet past” and the “Soviet empire’s history.” Laura Engelstein notes that if it is the “imperial dimension of the tsarist regime that has focused scholarly attention in the wake of 1991,” then this “new orientation resonates not only with the issues raised by the geographical breakup of the Soviet Union but also with the current importance of postcolonial themes worldwide.” If she here allows some room for the influence of the general culture of academic writing in the humanities and social sciences, elsewhere she provides examples of the convergence of the professional and the political, seemingly making professional themes a product of the political. Not only did the “rebelliousness of the 1960s express itself in scholarly terms in a sympathy for revolutionary movements or at least for their ideals,” but, from 1985 onward, the study of Imperial Russia by Western scholars matched developments in the Soviet Union: scholarly study of Orthodoxy and the church’s rebirth in post-Communist Russia coincided chronologically, while studies of judicial reform and legal institutions in Imperial Russia dovetailed with efforts to create a rule of law in the “transition” to democracy and a market economy.

What we have most held in awe is the import of the collapse on the historiography of the (early) Soviet period. After, with, and because of the collapse, not only the new paradigms that are the focus of this essay but also specific themes of historiographical investigation are said to have come into focus. Ethnic conflict is one of these themes. Both specialists and nonspecialists alike attribute the surge of research, and the “new assumptions” that have guided that research (for example, those taken from postcolonial theory), to the role played by ethnic conflict in the Soviet collapse itself. Lest it seem that I am scorning self-perceptions, I want to clarify that I do not discount the fact that because we have sought to explain a collapse that took so many of us by surprise, we have focused on questions of national identity, nation-building, ethnic conflict and mobilization, and similar issues. The interest of the general public in these issues, and in the mystery of the collapse itself, has not only fed the production of such studies but also allowed them to reach a mainstream audience. (Compare the fate of Gerhard Simon’s superb study, published in 1986, to that of Andreas Kappeler’s 1993 The Russian Empire: A Multiethnic History.) But the collapse is also said to have brought into the scholar’s gaze certain

32See, for example, von Hagen, “Empires, Borderlands, Diasporas.”
33Engelstein, “New Thinking About Old Empire” (quotes are from pp. 493 and 488; see also pp. 491 and 493).
35Gerhard Simon, Nationalismus und Nationalitätenpolitik in der Sowjetunion: Von der totalitären Diktatur zur nachstalinischen Gesellschaft (Baden-Baden, 1986); Kappeler, Russland als Vielvölkerreich.
Fetishizing the Soviet Collapse

103

less obvious themes or foci, such as denunciation. Old topics, so the story goes, are, because of the collapse, treated in new ways: the study of “terroristic regimes of the past” is said to have shifted because of “these monumental historical changes” from an emphasis on a top-down approach to a focus on “everyday or routine terror in society at large,” and the placing of “greater emphasis on the terror as seen from the bottom up.”

Moreover, there is a tendency to assume that scholarly developments are a function of, or response to, more fundamental historical, social, political, and economic developments. Even scholars who have, in analyses of the early Soviet period, privileged the autonomy of culture nonetheless tend to treat the cultural realm (that is, scholarship) as a product of more fundamental social and economic developments: “fresh conceptualizations” are “occasioned only, if at all, by shifts in the context larger than the historian—such as the Secret Speech, or the Vietnam War as well as countercultural upheavals, or the Soviet collapse (reforms) as well as its continuation after 1991.”

This is not to say that the autonomy of developments within the historiography itself—such as the amassing of empirical findings that do not match what a paradigm is predicted to yield—is completely eclipsed. But whatever causal role is accorded those developments—in what Robert Gellately and Fitzpatrick call, for example, the “beginning” of the “accumulation” of “anomalies” in 1985–90—is assumed, rather than shown, to be secondary to historical developments themselves, or to a “particular historiographical conjuncture.”

What is striking here is this: in thinking about the relationship between the Soviet collapse and the historiography of the Soviet period, some have used a default mode of analysis taken from the very paradigm—“modernization”—that exercised such a powerful influence on the historiography of Russia and the Soviet Union, especially in the 1970s and 1980s. In the modernization paradigm, just as in so much recent analysis of the relationship between the Soviet collapse and historiographical interpretation, culture is held to be both “dynamic, changing, and fractured” as well as of secondary importance to, and a product of, “macro-systemic” changes in society, politics, and technology.

Even those among us, such as Krylova, who have so helpfully insisted on “conceptual continuities” across the 1991 divide have not questioned the causal relationship between 1991 and the turn to “innovative interpretive frameworks,” let alone less dramatic if no less important shifts in historiographical interpretation. It is not only that the subcategories she uses to structure her argument (for example, “Triumph of the Resisting Subject: The 1990s”) treat the 1990s as a discrete unit of analysis. More significant is her reduction of

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37Ibid., 750.

38Note, for example, the analytical distance between Kotkin’s analysis of the terror in *Magnetic Mountain: Stalinism as a Civilization* (Berkeley, 1995), which privileges the autonomy of culture and the political, and his analysis of Jan Gross’s *Revolution from Abroad*, in which the latter’s “context (Solidarity) helped make possible his extraordinary insights into the operation of Soviet-style regimes as a kind of strategy for their dismantling” and “altered the parameters of what the field has been calling ‘resistance’” (“The State—Is It Us?” 35–51, quotes from p. 44).


41Von Hagen, “Empires, Borderlands, and Diasporas,” 453.
historiographical trends to historical developments under way in the former Soviet Union, seeing the “neglect of revisionist historicism as well as justification for the complete marginalization of the totalitarian notion of indoctrination” as the product of “changes in Soviet society from 1985 onward.” Even more significant, the “initiation of perestroika and glasnost’” are said to have generated the very categories—Krylova’s “triumph of the resisting human spirit”—through which American scholars of the Soviet Union were able to construct the Soviet experience. Moreover, the discontinuities of American scholarship of the 1990s vis-à-vis “totalitarian and dissident traditions” are still seen as a reflection of political and social change during the Gorbachev period and beyond.42 Others, including Malia, who have also insisted on conceptual continuities across the 1991 divide, nonetheless perpetuate the assumption of a causal relationship between the collapse and shifts in historiography, but in a negative sense—mourning the major historiographical shifts that the collapse should have, yet did not, produce.43

This brings us to a different kind of exaggeration of the collapse’s importance, one that invokes the collapse to prove that, after all, one’s interpretive framework(s), namely totalitarianism, were right all along. In other words, historians in this vein argue that those revisionists who had critiqued advocates of the totalitarian paradigm ought to concede defeat, and acknowledge the infinite wisdom of the totalitarian interpretation. In his Soviet Tragedy, published in 1994 but the product of decades of teaching and lecturing on Soviet history in Berkeley and Paris, Malia was optimistic that the collapse could provide proper perspective to the field: only with the closure of the Soviet tragedy, when the scholar’s perspective could no longer be clouded by “expectations as to how the experiment might turn out,” could the “real process of assessing the Soviet adventure at last begin.”44 If, as noted above, shortly before his death in 2004 he registered his dissatisfaction with what had happened to the field since the collapse, in his 1994 magnum opus he still believed that the collapse would convince those who had seen the USSR through the blinders of modernization theory, and hence denied its sui generis status, of the correctness of the totalitarian paradigm. For Richard Pipes too, the collapse is seen as having the power to bring correct perspective to those whose perspective was formerly amiss.45 For Malia and Pipes, the collapse, in essence, turns champions of the totalitarian perspective into a vanguard that had true consciousness long before benighted revisionists would be able, because of the demise, to attain it.

The more distant in time the collapse becomes, the more some of us have made it the core explanation for the decisive shifts in perspective that are said to have brought historiographical rupture to the field. With regard to the study of Soviet “subjectivity,” its influential practitioners did not, in the mid-1990s, tend to attribute their new perspective to the collapse itself. Rather, in explaining the changed perspective on the strength of

42Krylova, “Tenacious Liberal Subject,” 141.
43Malia, “Archives of Evil.”
44Malia, Soviet Tragedy, esp. 4–5 (quotes are from p. 4).
Stalinism, they focused on perceived shortcomings in the explanatory power of the totalitarian and revisionist paradigms. A case in point is Kotkin’s 1995 introduction to *Magnetic Mountain: Stalinism as a Civilization*, whose intellectual genesis in the first half of the 1980s will be discussed below. But, the inevitable exceptions granted, beginning in the late 1990s, some of these same scholars, as well as other analysts of historiography, tied the study of Soviet subjectivity to the influence of the Soviet collapse. Kotkin, in fact, emphasizes the importance of the Soviet collapse for generating “perspective” more in his 1998 and 2002 articles than in the introduction to *Magnetic Mountain*. Writing in 2000, 2004, and 2006, respectively, Fitzpatrick, von Hagen, and Hellbeck also portrayed the study of Soviet subjectivity as a response to the “collapse.” Probably no single reason explains this shift. Certainly much of the emphasis of the collapse’s importance seemed to be expressed at least ten years after the Soviet Union and late Soviet socialism began to unravel, as we took stock of the field’s development over the preceding decade. One could argue that the intervening decade or so provided us with the perspective to appreciate how decisive the collapse was for generating the original approaches that have since swept the field. To sum up, in place of attributing our insistence on the importance of the collapse for providing new perspectives to the wisdom brought by the passing of time, I would suggest that we should also consider that this stance may come from other sources, such as the need to make historiographic advance seem unassailable in the context of disappointing yields from archives, diminished lay interest in the field, and the absence of any truly innovative theoretical perspective (along the lines of “subaltern studies” in the South Asian field) to emerge from the empirical research that we have conducted since the archives opened.

**UNCoupling the Soviet Dismantlement and Historiographical RUpture: THE “SUBJECTIVITY” PARADIGM**

In thinking about whatever kind of shift *did* occur in the historiography, we have minimized the import of certain shifts in historical writing, such as the linguistic or cultural turn, that had nothing to do with the dismantlement—and indeed, in contrast to received wisdom, whose entry into the Soviet field actually *predated* it. (To provide an example of how I, too, have invested the collapse with immense perspective-generating powers see my writing, in another context, on the way historians of the Soviet period have engaged with the concept of “political culture.” From the vantage point of the “naughts,” I assumed that it was only after 1991 that I had first read Lynn Hunt’s *Politics, Culture, and Class in the French Revolution*. To my surprise I found that I had in fact cited her study in my dissertation, which I finished well prior to 1991. Rereading the preface, I was reminded that it was awareness of work on the political culture of revolutionary France that led to

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47See Kotkin’s “1991 and the Russian Revolution” and “The State—Is It Us?”

the formulation of my dissertation topic in 1985. Because of space limitations, I illustrate this assertion through a single, yet important, case study: Stephen Kotkin’s use of the “new cultural history” to reconceptualize the “strength” of Stalinism. Although there is arguably no precise, generally agreed upon definition of the “linguistic or cultural turn,” I have in mind the rejection of the “social” as the basis for historical explanation; sensitivity to the cultural contexts of human behavior, a sensitivity exhibited in the interpretation of meaning; and the privileging of analysis of cultural forms such as symbols, rituals, and discourse; and the awareness, produced by the poststructuralism of the 1970s, of how language produces knowledge, and inflects what we understand as real.

In 1988, Kotkin filed a Berkeley dissertation (the basis for his 1995 Magnetic Mountain), written in 1987–88, that incorporated what he had learned from Michel Foucault and Lynn Hunt in the early-to-mid-1980s before “defecting” to the Soviet field. The dissertation—a study of Stalinist city-building through the example of Magnitogorsk, launched in 1929 as the model “socialist city” and eventual home of what was then the largest steel plant in the world—does not directly cite the theory informing the work. But as Kotkin noted in the introduction to his book, the “idea of pursuing a study of power at the micro-level on the subject of Stalinism crystallized in conversations with the late Michael Foucault, whom I met at Berkeley while he taught there in 1982 and 1983.” Kotkin’s most lasting conceptual innovation may well be his application of Foucault’s concept of power “not localized in the central state apparatus” but deployed in such areas as “mutual surveillance and self-identification” in order to reconceive Stalinism as a “civilization” with its own subjectivities, language, and customs. But Kotkin also saw himself as applying Hunt’s concept of political culture, and, more broadly, the linguistic or cultural turn; and those influences can be seen in his analysis of the terror’s process in Magnitogorsk in 1936–38. The terror’s path, form, and scope, he insisted, could not be understood without attention to a previously ignored category of analysis: the “proper” political “attitude[s]” and actions that Magnitogorsk party members themselves constructed from the meaning of the party’s revolutionary mission. It was, he argued, a mission made pertinent to specific political events (for example, demotion within the party) by “signals” provided by the political language of the official press, in a “grand crusade” to build socialism during a darkening international context. Kotkin’s “primacy of the political” was, therefore, not the organized political will of a totalitarian state, but a broader, autonomous “political” that represents a translation of Hunt’s “rules for political behavior” or “common expectations of behavior” defining “revolutionary political culture.”

50My discussion of the cultural turn is taken from the “Introduction” to Victoria E. Bonnell and Lynn Hunt, eds., Beyond the Cultural Turn: New Directions in the Study of Society and Culture (Berkeley, 1999), esp. 5–9.
52Magnetic Mountain, xviii, 23. Martin Malia’s insistence on the autonomy of the political, a theme he developed in Soviet Tragedy, was also a major influence on Kotkin (Malia was a member of his doctoral committee).
Fetishizing the Soviet Collapse

sure, Kotkin’s book used Hunt’s notion of a nonreductive political sphere to produce a more full-blown analysis of the terror. But this does not detract from the significance of his dissertation, one of the first attempts to apply Hunt’s approach to political culture to the Soviet field. My point here is this: Kotkin’s 1987–88 dissertation work, building on work with Foucault and Hunt that he had done in the first half of the 1980s, suggests that it was not, as Engelstein claims, communism’s collapse that allowed Kotkin to ask “questions [that he] could not have asked before.” The history of Kotkin’s work also calls into question Ron Suny’s 1994 assertion that the “insights from literary theory and the fallout from the Foucauldian revolution have only begun to make their way into historical writing on Russia.”55 As Kotkin himself put it, he began his “Ph.D. dissertation in 1985 ... and completed it in 1988 (well before the deluge was imaginable).”56

At this juncture, it is instructive to think about what, absent the dismantlement, the substance and form of Kotkin’s *Magnetic Mountain* would have been, in order to explore a question that cannot be answered: What would the field of early Soviet history look like now without such historical rupture? Arguably, had the dismantlement not occurred, or had it not occurred in the way it did, we would still have the interpretive core and insight of *Magnetic Mountain*. And Kotkin did assemble an innovative conceptual core (drawn from Foucault, Hunt, Malia) in the early-mid 1980s. To be sure, his book is framed much more broadly than the dissertation, as a reconceptualization of the very nature of Stalinism. And it articulates, much more clearly than the dissertation, some of the core assumptions of the emerging Eurasia anti-paradigm (for example, its attempt to recast the way we think about similarity and difference, and the focus on the “dark aspects” of the Enlightenment and modernity more generally). But these very assumptions, including that of Stalinism as a “civilization,” can be found in the dissertation *en nuce*.57 Arguably, the distance between the 1988 dissertation and *Magnetic Mountain* reflects not just the typical broadening that happens to dissertations as they become books, but Kotkin’s engagement with general changes in the culture of academic writing (such as the stylized invocation of theoretical masters) that almost certainly would have made their way into the historiography of the early Soviet period even if not in connection with the dismantlement.

The historiography of Imperial Russia is another underappreciated source of the new perspective historians of the early Soviet period have brought to the field. Some historians whose research focuses on the Soviet period nonetheless have considerable training in the history of Imperial Russia—and, it is hoped, perhaps against all odds, in that of pre-Petrine Russia. Nonetheless, analyses of “post-Soviet” historiography tend to omit this fact, placing historians of the Soviet period only in dialogue with its prior historiography, with changing practices of historical writing in the academy, with the often amorphously defined Soviet collapse, and, occasionally, with current events in the

57For emphasis on power, à la Foucault, as productive rather than repressive see Kotkin, “Magnetic Mountain,” 1–2 (“Abstract”).
United States, as well as in the world (for example, “globalization”)—with, in other words, “shifts in the context larger than the historian.” There has been the occasional acknowledgment that shifts in perspective hit the Imperial field before the Soviet field, but we have not considered the import thereof. Historians of the Soviet Union might have drawn some of their new “perspectives” neither from the collapse, nor even directly from the linguistic or cultural turn, from postcolonial studies, or from anthropology, nor even indirectly from the way these theoretical perspectives were applied by historians of Western Europe and elsewhere, but rather from the way historians of the Imperial period had, in the 1980s and perhaps even before, applied the theoretical insights of these cultural authorities. Not only was “subjectivity,” for example, examined by established scholars of late Imperial Russia in publications that began to appear in the 1970s and early 1980s. Some of these pioneers trained those who would eventually make major contributions to the study of Soviet subjectivity. For instance, Hellbeck, in the acknowledgments to his just published Revolution of the Mind, writes that Leopold Haimson, whose own work emphasized the subjective meaning that historical actors gave their experience, and whose teaching stressed close attention to the language deployed by those actors in specific social and political contexts, “immediately saw the significance of the documents I showed him, and he guided my research with analytical acuity.” Haimson, Hellbeck says, taught him to “understand language as a historically situated, evolving entity.” Even if Haimson’s methodology allowed for a “degree of instrumentality” not present in the “study of the genealogy of the subject” assumed to be completely constituted historically by language, Hellbeck still would stress the “similarities more so than the differences.” As for the Eurasia anti-paradigm, when von Hagen does cite scholarship published prior to 1991, and even prior to 1988, which exemplifies assumptions and the research foci thereof, it is invariably scholarship on Imperial Russia. Even as historians of the Soviet Union tended to assume a primordial nationhood, historians of Tsarist Russia were, before (and sometimes significantly before) the Soviet dismantlement, turning to “different kinds of analysis” to explore Russian expansion, the interactions between Russians and non-Russians, borderlands, and frontiers. If, as I am going to suggest, a persuasive case can be

59See, for example, Kotkin, “The State—Is it Us?” 43; and Russia’s Orient, xi.
61Hellbeck, Revolution on My Mind, 422.
62Interview with Hellbeck, 2006.
63Von Hagen, “Empires, Borderlands, Diasporas,” 465, where he mentions, among many examples of works in the emerging Eurasia anti-paradigm, Edward Thaden, Russia’s Western Borderlands, 1770–1870 (Princeton, 1984), and John Doyle Klier, Russia Gathers Her Jews: The Origins of the “Jewish Question” in Russia, 1772–1825 (DeKalb, IL, 1986).
64Russia’s Orient, xi.
made that the assumptions of the Eurasia anti-paradigm first made their debut in the
histiography of Imperial Russia, then their appearance in the historiography of the
Soviet Union in the 1980s, and their aggregation in the 1990s, can, arguably, be just as
easily attributed to the lag time needed for historians of the latter to assimilate the former,
as to any shift in “perspective” magically produced by the Soviet dismantlement.

This chronology raises a fundamental question, one suppressed by our overemphasis
of a relationship between the Soviet dismantlement and the shift in the historiography of
the former Soviet Union: How, if at all, did the challenge to area studies, which exploded
in the second half of the 1980s and continued through the 1990s and beyond, affect the
histiography of the Soviet Union?65 We have not put this question on the scholarly
agenda, let alone addressed it seriously. Kotkin, for example, omits the challenge to area
studies in his elaboration of examples of “shifts in the context larger than the historian.”66

By a challenge to area studies, I mean not only the attack on “detailed description of
a nation or region” but also the “contention that area studies must use knowledge about
particular regions to make more broadly applicable generalizations.”67 That the victory
of that contention came at the end of the 1970s and the early 1980s had significant
implications for the study of Soviet history at that time, when, as we will see below,
historians in both the late Imperial and Soviet fields began to assume that generalizable
theory (modernization, poststructuralism) and concepts drawn from the study of other
regions (such as Fitzpatrick’s “Cultural Revolution”) were fundamental prerequisites for
framing, and for yielding deeper understanding of a Russia/USSR that was no longer
seen as eternally exceptional, as sui generis. While we should resist the tendency to make
the challenge itself another “agentless abstraction” to which we give inordinate
“explanatory weight,”68 we might consider the following hypothesis regarding the
relationship between the challenge to area studies and the development of historiography
of the Soviet Union. Specifically, might the challenge be linked to an essential element of
the Eurasia anti-paradigm, an effort to recast the way we “think about similarity and
difference in historical comparisons” by sidestepping the issue of whether Russia is part
of Europe or Asia—and, more broadly, how “other” Russia is?69 To be sure, it is difficult
to establish causality between the challenge to area studies and the emergence of new
perspectives and paradigms in the early Soviet field. What is incontrovertible is that
training outside the field of Soviet and Russian history had an impact. Whether or not it
was undertaken directly in response to the challenge to area studies, it had a significant
conceptual influence on the work of scholars who embarked, from the mid-1980s forward,
on dissertations, monographs, and other projects in early Soviet history. Examples include
Slezkine, who, when he began the dissertation that in 1994 was published as Arctic Mirrors,

65I am grateful to Sarah Stein for suggesting this point to me.
67Peter A. Hall and Sidney Tarrow, “Globalization and Area Studies: When Is Too Broad Too Narrow?” Chronicle
68Frederick Cooper, Colonialism in Question: Theory, Knowledge, History (Berkeley, 2005), 17.
Glennys Young

was influenced, as he designed his study, by his study of African history, as well as by visits to Indian reservations in New Mexico. His intention was to apply insights thereby gleaned to Russian history. 70 Another example is Kotkin, who not only trained with Foucault but also initially studied German and Central European history. Moreover, in my view, it is helpful to consider the indirect influence that the challenge to area studies may have had on the Russian field. Because political scientists’ challenges to area studies had persuaded many granting agencies and university administrators that the days of area studies were numbered, some institutions sought to hire Russianists who had the capacity to connect with scholars besides other Russianists.

The challenge to area studies in the 1980s overlapped chronologically with a third factor whose contribution to a shift in perspective has been minimized and/or misunderstood: postcolonial studies. In much of the extant historiography, one finds the assumption that postcolonial studies made its debut in the Russian/Soviet field after, and largely because of, the collapse. 71 This is in contrast to other disciplines (literary studies, anthropology) and fields of history (for example, Africa and China) where the 1980s are said to mark the beginning of postcolonial studies, with Edward Said’s Orientalism (1978) acting as catalyst. What is presumed to account for the lag time of the spread of postcolonial studies to the Russian/Soviet field is the grip of the Orient and modernization paradigms in this area; as well, there is an assumed parallel between the USSR’s perceived “otherness,” its lack of integration into global economic and political structures, and the field’s scholarly insularity, its impermeability to challenges from the general “culture” of academic writing. What propagators of these assumptions miss is that in other fields (such as Africa), postcolonial studies emerged out of empirical anomalies produced by research conducted under the modernization paradigm. 72 Although a thorough investigation of whether this has been the case in the Soviet field must be the subject for another paper, there is some evidence that Soviet history, too, fits that model. 73 As a result, one can find what might be called glimmers of postcolonial studies “lite” in the 1980s, even the early 1980s, in both the Imperial and early Soviet fields. Examples include Kappeler’s work in the early 1980s, and Alton S. Donnelly’s The Russian Conquest of Bashkiria, 1552–1740: A Case Study in Imperialism (1968). Kappeler’s work, which shares with postcolonial studies an interest in how the “experience of colonization affect[ed] those who were colonized while also influencing the colonizers,” to list just one of its amorphous and contested themes, was in part a product of his reading in anthropology. 74 Donnelly, while obviously not using the term Russian “Orientalism” in the way that Edward Said would have meant it, calls it to mind (at least for the reader in the know about postcolonial studies) when he

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70 Interview with Slezkine, 2006.
71 See especially von Hagen, “Empires, Borderlands, Diasporas.”
72 Cooper, Colonialism in Question, 45–46.
73 Even though Kotkin does not explicitly say so, the dissertation that became Magnetic Mountain is an example of embrace of a new paradigm (subjectivity) because of the explanatory limitations of the modernization paradigm.
regrets the documents’ “bias” that the “conquest in the long run had progressive results, because the cultural level of the Russians was higher than that of the conquered.”

**MORE UNCOUPLING: THE EURASIA ANTI-PARADIGM**

Collapse or no collapse, there is evidence to suggest that we would have had the intellectual components to create one of the central paradigms said to have emerged following, and as a scholarly response to, the Soviet collapse: in other words, the Eurasia “anti-paradigm.” This is an “anti-paradigm” in the sense that it has been constructed in opposition to the Russia/Orient and the Soviet Union–modernization paradigms. It is both a set of thematic foci and a cluster of interrelated intellectual assumptions. Since the late 1980s and especially in the 1990s, several themes are said to have emerged in the “new scholarship.” These include empires in central Eurasia, including their interactions with one another; a focus on the edges of and boundaries between empires, manifest in a preoccupation with borderlands and the renewed interest in regional history; and, in the context of disdain for traditional narratives of the nation, an emphasis on the “importance of diasporas in the history of the region,” understood as the peripatetic movement of those groups, not necessarily of Russian nationality, who journeyed throughout the empire and its borderlands.

As for its assumptions, in contrast to the “monolithic and static cultural determinism” of the Russia/Orient paradigm, the Eurasia anti-paradigm rejects the notion of an essentially culturally inferior and static Russia or Asia pitted against an enlightened and progressive West, and attempts to account for long-term processes of demographic, cultural, ecological, and political change. Eschewing the modernization paradigm’s insistence on political, economic, and social convergence as a result of social change, the Eurasia anti-paradigm has, as noted above, attempted to recast the “way we think about similarity and difference in historical comparisons” by “deflect[ing]” the “question of whether Russia belongs to Europe or Asia.” Jettisoning the fundamental assumptions of both the Russia/Orient and modernization paradigms, practitioners of the Eurasia anti-paradigm developed a set of somewhat interrelated historical practices: an interest, said to be “unprecedented” in the field, in geopolitical imaginaries; “chronological promiscuity,” or the tendency to cross established historical divides as research questions dictate; keen sensitivity to the part that scholars themselves play in the “making of empires and nations”; the violence, dominance, and economic inequality present from the very beginning of European projects of exploration and discovery; disenchantment with the construct of the “nation”; a focus on “comparative and interactive histories of peoples, ideas, and goods”; and the goal of writing a global history for a world that is “increasingly integrating and simultaneously proliferating differences.” The paradigm’s content is to some extent region-specific (especially in that it has been developed in dialogue with “classical Eurasianism”).

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76 Von Hagen, “Empires, Borderlands, Diasporas,” 448.
77 In the writings of such classical Eurasianists as Trubetskoi, Savitskii, and Suvchinskii, the Asian influence upon Russia is given a positive valence, and the virtues of Russian civilization are contrasted to a European civilization.
important elements of its content can be found in the historical analysis of almost all other regions of the world. Their emergence elsewhere, linked to the cultural or linguistic turn and post-colonial studies, was not said to be prompted by new outlooks produced by a rupture equivalent to the Soviet dismantlement.\textsuperscript{78} It is not surprising, then, that some studies in the emerging Eurasia anti-paradigm were launched not only prior to 1991, but even before 1988.

We can begin to uncouple the Eurasia anti-paradigm from the dismantlement by examining “literature exploring the complex and often contradictory relations among empire, nation, and state power in Eurasia.”\textsuperscript{79} Among those works that have sought to place empire at the center rather than the periphery of analysis, and to decenter the narrative away from imperial capitals of Moscow and St. Petersburg, Kappeler’s \textit{The Russian Empire: A Multiethnic History}, which von Hagen justifiably lauds as the “manifesto” of the Eurasia anti-paradigm, is a case in point. The book—the “first survey to place at the center of its attention the ‘imperial’ character of the Russian Empire, above all its multinational subject populations”\textsuperscript{80}—was published in 1993, and was written during “1990 and the summer 1991 ... in a new time, when nationalities’ problems had become prominent.” But, as the author puts it, its “pre-history goes back to the 1970s.” Kappeler began working on the topic of “multiethnic Russia during the 1970s” as a result of travels in the USSR that caused him to recognize that the “USSR was not Russia, but a conglomerate of many different peoples and languages.” In 1982 he published his second dissertation, on the “relations between Russia and the peoples of the Middle-Volga-region (Russia’s first ‘nationalities’) from the sixteenth through the nineteenth centuries.”\textsuperscript{81} As early as 1982 he conducted research on “other peoples of Russia,” focusing on nationalities in the census of 1897, and organizing conferences on Russia’s Germans (1985), Muslims (1987), Russians (1989), Ukrainians (1991), and “later” on the Caucasus and the Russian Federation.\textsuperscript{82} As early as 1982 he “began to teach a course on multinational Russia which became the core of the future book,” publishing also in 1982 a programmatic article on the topic that articulated many assumptions that made up what von Hagen would later call the “Eurasia anti-paradigm,” and set forth the same premises that motivated

\textsuperscript{78}Von Hagen, “Empires, Borderlands, Diasporas,” 454–55 (quotes are from pp. 454, 458, 457, 460, 458, 459, and 460, respectively).
\textsuperscript{79}Ibid., 463.
\textsuperscript{80}Ibid., 460.
The Russian Empire. Although, as von Hagen notes, Kappeler does not “find the Eurasia anti-paradigm entirely acceptable to characterize his own work,” both the survey and the 1982 article resonate with the paradigm’s interest in “geopolitical imaginaries,” with historians’ “disenchantment with the nation,” the paradigm’s reluctance to privilege the “nation-state or the influence of nationalism in history” as chief causal factors, and its “chronological promiscuity.” If, in Kappeler’s own perception, the dismantlement had nothing to do with generating the “perspective” brought forth in The Russian Empire, the USSR’s dissolution was hardly irrelevant to the book’s influence. Because the book was published after the Soviet Union had unraveled, it was, as Kappeler put it, “ready for a broader public whose interest in the nationalities of Russia and the Soviet Union was growing.” Widely reviewed in newspapers and journals, and translated into French, Russian, English, and Ukrainian, the book was made a “standard work” by the “dissolution of the USSR.” He surmises that “if I had published it ten years earlier, it would have been noticed only by a few specialists.”

Moving beyond Kappeler to other themes that have been central to the Eurasia anti-paradigm, we find several examples of scholarship whose origins and conceptual framing predated the collapse. Because of space limitations, I focus only on the second thematic component of the emerging Eurasia anti-paradigm, namely scholarship on borderlands and diasporas. Within a subdivision within this category, work on Russia’s borderlands in “scholarship about Russia’s ‘Orient,’” examples are Slezkine’s Arctic Mirrors, published in 1994, but begun as a doctoral dissertation at the University of Texas at Austin in 1986. Among scholarship on the Western borderlands there is Theodore Weeks’s Nation and State in Late Imperial Russia (1996), begun as a UC Berkeley doctoral dissertation in the late 1980s, around “1987 or so”; in conceiving the study, Weeks, who in the early-to-mid-1980s planned to explore nationalities issues in the context of the Hapsburg Empire, was influenced by the work of Edward Thaden and John Klier. Nor is recent scholarship on diaspora an exception; one of the finest recent examples of scholarship on the Jewish diaspora, Benjamin Nathans’s Beyond the Pale, had its origins in a 1987 graduate seminar, during the author’s first year in graduate school. One of Klier’s books, Russia Gathers Her Jews, was published in 1986; his Imperial Russia’s Jewish Question, which also speaks to the borderlands component in the Eurasia anti-paradigm, although published

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84 Von Hagen, “Empires, Borderlands, Diasporas,” 459, 460. That Kappeler’s 1982 article draws upon the work of Karl Deutsch in positing an understanding of nations and nationalism as modern constructions calls into question the claim by Suny and Martin that the “shift in the theoretical literature on nationalism and nations,” associated with the work of Ellie Kedourie, Ernest Gellner, and Karl Deutsch in the 1950s and 1960s, had a resonance among Soviet historians that was “next to none.” So taking Sovietologists and Soviet historians to task, they in fact omit Kappeler from their historiographical survey (A State of Nations, 7).
85 Interview with Kappeler, 2006.
87 Theodore Weeks, Nation and State in Late Imperial Russia: Nationalism and Russification on the Western Frontier, 1863–1914 (DeKalb, IL, 1996); Interview with Weeks, 2006.
88 Interview with Nathans, 2006.
in 1995, was the “product of twenty years of research and writing.”89 I do not, to be sure, have the evidence to disprove the hypothesis that the dismantlement influenced these studies’ intellectual evolution. But their intellectual origins and fundamental design were conceived in a political, cultural, and intellectual context separate from that caesura.

It is also possible to demonstrate that specific assumptions of the Eurasia anti-paradigm predated the Soviet dismantlement and, at times, even with time to spare, Gorbachev’s inauguration of perestroika. Here I examine the genealogy of only three of the “features” said to constitute the Eurasia anti-paradigm. The first—sensitivity to the dark sides of European expansion—can be found in a number of works published before, and sometimes well before, the Soviet collapse began.90 Underappreciated in this regard is Donnelly’s 1968 Russian Conquest of Bashkiria. This is not to say that there is no difference between Donnelly’s work and more recent work on Russian colonialism. In contrast to this recent work that sees colonialism as a complex political, cultural, and social form in which power did not emanate only from the colonizers against the colonized, and in which the colonizers are themselves transformed by the process of colonization, Donnelly makes Russian aggressors the chief historical actors against which the Bashkirs and other non-Russians responded; it is only Russian strategies of colonization, rather than their self-understanding and/or culture, that are transformed through interaction with the colonized. Nonetheless, anticipating some of the assumptions of the Eurasia anti-paradigm, particularly its sensitivity to the “ethical dimension” of “inequalities in power and wealth,” the author sees the Russian conquest of Bashkiria, which is compared to cases of American and British colonization, as a case of “aggressive, imperialist, subjugation of an alien people.”91 In fact, many of Donnelly’s working assumptions adumbrate the statement of “new” directions in the study of Russia’s “Orient” as discussed by Brower and Lazzzerini. For example, like the contributors to Russia’s Orient, Donnelly seeks to emphasize the “importance of colonial experience,” the “need to explore points of contact or ‘encounters,’ imagined or lived, among the empire’s peoples and between the tsarist regime and its subject communities,” and how these peoples’ “accommodation with and resistance to the empire constitute in many respects the real imperial history of Russia.”92

The second “feature”—disenchantment with or skepticism toward the nation as an organizing principle—was clearly articulated by Kappeler in 1982.93 The third feature, something that von Hagen calls a “chronological promiscuity,” in that, rather than an “earlier generation’s segregation into Soviet and imperial specialists,” we now have a growing body of historiography in which “research questions—increasingly ones with

89John Doyle Klier, Imperial Russia’s Jewish Question, 1855–1881 (New York, 1995), ix. For references to the “borderlands,” a term used in the text itself, see, for example, ibid., xvi, xviii, 452–53.
90Examples include Donnelly, Russian Conquest of Bashkiria; and Kappeler, “Historische Vorraussetzungen.”
91Von Hagen, “Empires, Borderlands, Diasporas,” 458; Donnelly, Russian Conquest of Bashkiria, vii (for examples of Russians changing strategies in response to Bashkir resistance see pp. 18, 23).
92Russia’s Orient, xvi. For the subjective meaning of colonial experience to the Bashkirs see Donnelly, Russian Conquest of Bashkiria, 19, 118 (on “encounters” see pp. 118, 138, 153, and 170, and for examples of Bashkir accommodation and resistance to Russian colonization see pp. 15, 17, 19, 20, 21, 33, 48, 49, 62, 63, 75, 99, 101, 106, 107, 108, 140, 153, and 154).
comparativist ambitions—are allowed to frame the historical period that the scholar
demarcates,” can actually be found in the mid-1980s, if not before, and because of, not
despite, the modernization paradigm. This, by the way, is a chronological and causal
pattern seen in other fields as well, such as Chinese history. Whenever this chronological
promiscuity began, it would be more apt to call it chronological promiscuity redux. This
is because the strict identification of scholars with particular fields of Russian history
occurred in the 1970s and 1980s. Before the 1960s, when the bifurcation into modern
Russianists and medievalists or pre-Petrine specialists began to occur, Russian historians
published on various chronological periods, and the chronological parameters of specific
studies could transcend perceived “ruptures” or “divides” in Russian history if the subject
required it. In the Russian field, pre-collapse chronological promiscuity can be found in
work exemplary of the modernization paradigm and modernization theory, such as
Theodore von Laue’s Why Lenin, Why Stalin? (1964) and Moshe Lewin’s Making of the
Soviet System (1985, though individual essays had been published in the 1960s and 1970s).
For Lewin, the complex historical phenomenon of Stalinism can only be explained and
understood by leaping backward across the 1917 divide and peering into the “medieval”
religious and cultural cosmology of Russian peasants. One might object that these
examples say nothing new, in that von Hagen himself acknowledges “voices of dissent”
within generations and their paradigms. Nor can it be contested that it was only in the
“post-Soviet era” that these “voices of dissent” became a somewhat unified and coordinated
chorus. But given the foundation that had been laid for the paradigm in the 1980s,
might we not have heard that chorus in the 1990s even if the dismantlement had not
occurred?

An affirmative answer to this question is further suggested by the following: in those
cases in which a nominally socialist polity had or has not collapsed (China, North Korea),
we see historiographical developments analogous to the Eurasia anti-paradigm. The
same intellectual stuff out of which the Eurasia anti-paradigm has been crafted has been
integrated into these fields. Even before significant reforms in Communist China, scholars
there and elsewhere engaged with the linguistic turn and with the seminal texts of
postcolonial studies. In recent scholarship on the history of North Korea, one can find

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94 On the crumbling “solidity” of 1949 as a “dividing line” in Chinese history see Jeffrey N. Wasserstrom, ed.,
Twentieth-Century China: New Approaches (New York, 2003), 4. See also, for example, Paul A. Cohen, “The
Post-Mao Reforms in Historical Perspective,” Journal of Asian Studies 47 (August 1988). Quotes are from von
Hagen, “Empires, Borderlands, Diasporas,” 460.

95 As Donnelly’s book, published in 1968, attests, even work published in the 1960s crossed periods. Donnelly’s
original Ph.D. supervisor at UC Berkeley was George Lantzeff. But, it should be noted, he was also trained by
Nicholas V. Riasanovsky, whose work has also crossed what eventually became clearly demarcated periods of
Russian history (interview with Donnelly of 2006).


97 Von Hagen, “Empires, Borderlands, Diasporas,” 448.

98 One such seminal text, Edward Said’s Orientalism (New York, 1978), had, by the early 1980s, influenced the
historiography of nineteenth- and twentieth-century China. See Paul A. Cohen, Discovering History in China:
draws upon Orientalism in formulating his recommendations for a “China-Centered History of China” (discussion
of Said’s Orientalism can be found on p. 150). Certainly the linguistic “turn” significantly influenced the Chinese
revisionist ways, à la the Eurasia anti-paradigm, of thinking about similarity and difference, and, though they are not necessarily articulated as such, modes of historical writing that fit with the Eurasian anti-paradigm’s goal of writing a “new world history for a global age.”

ANOMALIES, PERSONAL EXPERIENCE, AND HISTORIOGRAPHICAL RUPTURE (OR AT LEAST CHANGE)

Another important causal factor that we have minimized as we have thought about the source of dialogue with other fields and non-Russian historiographies is what Thomas Kuhn once called the “accumulation of anomalies.” What we have told ourselves about the history of the historiography of Stalinism is a case in point. Certainly that is how, for example, Kotkin has explained his rejection of both the totalitarian and revisionist paradigms, as well as why the totalitarian paradigm “came under attack from a subsequent generation of self-proclaimed revisionists.” (At the same time, his analysis includes other factors, such as generational identity and rivalry, as well as the implicit advantages of perspective that an “outsider” such as Fitzpatrick could provide, and the “domestic convulsions” of the Vietnam War.) And, as noted above, postrevisionists such as Hellbeck and Igal Halfin do not invoke the collapse when they critique Kotkin’s work. Rather, they complain of its inability to explain what it purports to explain—the strength of Stalinism, or political outcomes writ large.

Yet another causal factor that we have minimized has been the role of personal experience. To be sure, we have not completely ignored the role of personal experience in narratives of the historiography’s development that attribute the development of new “perspectives” to our assimilation of the Soviet collapse. Rather than treat these new perspectives as a result of dispassionate intellectual analysis, many have seen them as the result of a passionate attempt to come to terms with the emotional disequilibrium created by the collapse. But at the same time we have emphasized the personal stake we have in these new perspectives, the category of personal experience remains largely an abstraction. Moving beyond this cookie-cutter approach, as the use of interviews in this essay has sought to suggest, has the potential to yield important insights and fascinating stories.

99See, for example, Bruce Cumings, North Korea: Another Country (New York, 2004). Quote is from von Hagen, “Empires, Borderlands, Diasporas,” 460.

100See Thomas Kuhn, The Structure of Scientific Revolutions, 2d ed. (Chicago, 1970), 67–68. For an example, drawn from African history, of paradigm shift as a result of “accumulation of anomalies,” see Cooper, Colonialism in Question, 45. The very research conducted under the auspices of the modernization paradigm revealed that “change” was “a much more convoluted process” than the paradigm itself had scripted it to be (p. 46).

101Kotkin, Magnetic Mountain, 3.
about how our complex lives help to shape our scholarly agendas (and vice versa), and in a larger context even propel us to engage with other fields and non-Russian historiographies.¹⁰²

INSIGHTS VIA UNCOUPLING:  
WHY DOES FETISHIZING THE SOVIET DEMISE MATTER?

In any case, it is time to take stock of what the consequences of overexaggerating the collapse have been, and are likely to be, for those who study early Soviet socialism. In this I hope to avoid overexaggerating the import for the historiography of the collapse as catalyst thesis. But this stance has had historiographical consequences, some of which I explore below. By “historiographical consequences,” I do not mean a specific program for future research in terms of particular topics to be studied and how to study them. Rather, what seems even more fundamental to me, and what I undertake below, is to explore the implications, in terms of our self-understanding and our practices of engaging in dialogue with other fields and non-Russian historiographies, of our triumphalist belief in major historiographical rupture after, and because of, the dismantlement. Others, depending on their own research interests and/or backgrounds, will come up with other consequences, and I hope my thoughts will produce discussion and debate.

By overemphasizing the dismantlement we have, among other consequences, underplayed all that is not new and overplayed all that is said to be new in the field, perhaps implicitly invoking a parallel between the magnitude of the collapse and the magnitude of historiographical change. This is not to say that we have completely ignored elements of continuity across the 1991 divide. Uncoupling theoretical transitions from the Soviet collapse, Krylova has traced an interpretive narrative, developed in the discourse of the early Cold War, that assumed the Stalinist subject to be the inverse of the “liberal subject,” a narrative that persisted through historiographical schools—totalitarian, revisionist, and postrevisionist—commonly assumed to be at loggerheads. But I have in mind not the tenacity of the “liberal subject” but the tenacity of “old” paradigms, even among those who claim to have cast them out and to have embraced the new. Elements of the modernization paradigm, as I have suggested elsewhere, can be found even among those scholars whose work, and professional activities (for example, editing *Kritika*), have done so much to construct and advance the Eurasia anti-paradigm.¹⁰³ Von Hagen’s own explanation of the emergence of the Eurasia anti-paradigm privileges social causation, the motor of historical development so prominent in the modernization paradigm. If the modernization paradigm continues to have resonance even for those scholars who have made essential contributions to the “anti-paradigm” said to supplant it, then surely its persistence in the work of those who do not claim to have rejected the modernization paradigm must be substantial. We have yet to appreciate how elements of change and

¹⁰² See the example of above of Yuri Slezkine (interview with Slezkine, 2006).
¹⁰³ Krylova, “Tenacious Liberal Subject, 120 (quote is from p. 121). On the persistence of the modernization paradigm see Glennys Young, “A Middle Ground: Soviet Historians, The Russian Revolution, and the Political Culture Concept” (manuscript in preparation).
continuity can coexist in the work of individual scholars, who themselves have identities that are less one-dimensional and more “hybrid” than commonly thought.104 Because we have exaggerated the new and underplayed our own attachments to paradigms supposedly discarded, we have not understood, let alone thought about, the consequences of the “old” in our historical narratives. Doing so could give us greater control over, or at least greater capacity to intervene in, the narratives we construct.

Another way we have downplayed the old and overplayed the new is in our exaggeration of the degree of historiographical rupture that has resulted from the very way we have made the study of the Soviet Union more interdisciplinary. For example, elsewhere I discuss the fact that recent engagement with the symbolic-linguistic political culture concept—a concept of political culture that, drawing upon symbolic anthropology and Saussurean linguistics in conceiving of culture as an analytically symbolic system, has generated historical analyses of the internal logic of political language, images, and action—has not brought as many original interventions in the major historiographical debates of the early Soviet period as we may have hoped.105 New theoretical perspectives have been used to buttress claims that scholars had advanced before they took the linguistic turn, drew upon subaltern studies, or discovered postcolonial studies. Compare, for example, Suny’s “Revision and Retreat” (1994) to the historiographic review on the October Revolution that he published in 1983. Suny’s agenda in the 1994 article is to counsel use of the linguistic turn to advance the same take on October that he had advanced in 1983, namely that it was a worker’s revolution.106 Nor have new theoretical perspectives necessarily generated fundamentally new questions, whether in the historiography of 1917, of NEP, or of Stalinism. Recent scholarship on 1917, such as that by Orlando Figes and Boris Kolonitskii, which draws upon the cultural or linguistic turn, still addresses the long-standing question of why the Bolsheviks were able to come to power. Recent scholarship on NEP, such as that by Eric Naiman, Anne Gorsuch, and this author, has still attempted to answer the question of why NEP was abandoned. Recent scholarship on Stalinism, such as that by Kotkin, Hellbeck, and Halfin, still addresses the enduring question of how to account for the strength of Stalinism.107 By making these assertions, I do not intend to discount the smaller elements of “newness,” whether or not somehow derived from the dismantlement, in recent historiography of early Soviet socialism. “New,” of course, can mean, as we revisit established questions in the ongoing process of

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historiographical interpretation, introducing concepts (for example, gender) not previously adduced to study certain perennial longstanding questions, or basing analysis on previously overlooked or inaccessible sources (such as diaries of ordinary Soviet citizens). The question is whether these smaller (and certainly valuable and satisfying) kinds of historiographical changes qualify as the historiographical caesura or rupture we have proclaimed, or whether they constitute the kind of “organic” historiographical development that likely would have occurred without the dismantlement.

If we have, at times, exaggerated the degree of historiographical rupture said to have been caused by the collapse, and if this assertion is hard to swallow, I suggest that, viewed with some perspective, this should be cause for hope, not despair. For swallowing it might help us realize that if a certain degree of historiographical rupture is our goal, we might, to quote Frederick Cooper, achieve it through “a more thorough and critical engagement with other fields, a more rigorous and wider reading of social theory that both reconfigures and deepens methodological understandings.” But we are less likely to read widely and rigorously if we are satisfied that the “collapse” has brought the all the new perspective we need. At the very least, if we let go of the assumption that the collapse produced dramatic historiographical rupture, we might be inclined to have an explicit and overdue conversation about what does constitute historiographical rupture and why, and how much of it we want and need. Such open discussion of an important issue would greatly benefit the field.

It may be that, in crediting the collapse for the advent of new perspectives and paradigms, we have celebrated their arrival as the destination, thereby obscuring that our practices of interdisciplinary engagement have sometimes been wanting. In surveying the interdisciplinary study of the Soviet Union, some of which is part of a larger body of interdisciplinary postcolonial studies, we have not avoided, let alone had enough awareness about, the pitfalls of interdisciplinary work. As Cooper has recently reminded us, they include tendencies to accept uncritically the “conventional wisdom in another discipline,” to ignore “internal debates,” and to harvest “tidbits without exploring their relationship.” As we have engaged in dialogue with other disciplines, we have rarely subjected to rigorous critique what we have borrowed and assimilated. We have also accepted, with very few exceptions granted, the conventional wisdom about other disciplines. With respect to the discipline of political science, for example, the conventional wisdom seems to be that theories employed by political scientists, such as those of collective action and the new institutionalism, hold little value for studying early Soviet socialism. As for ignoring

108Cooper, Colonialism in Question, 6.
109Ibid., 6.
110There are, of course, exceptions, if unfortunately too few. See, for example, Kotkin’s critical engagement with Foucault, especially the nature of his attention to “resistances” (Magnetic Mountain, 22).
111In a recent meeting of my graduate seminar on Soviet history, a graduate student in political science pointed out that it was “surprising” that scholars of the revolutions of 1917 had not engaged with theoretical studies of collective action (most basically, the classical text of Mancur Olson, The Logic of Collective Action [Cambridge, MA, 1971]), to explain worker radicalization and support for the Bolsheviks. One scholar who has engaged with recent work in political science, if not on collective action, is Donald Raleigh, Experiencing Russia’s Civil War: Politics, Society, and Revolutionary Culture in Saratov, 1917–1922 (Princeton, 2002). On the new institutionalism see Walter W. Powell and Paul J. DiMaggio, The New Institutionalism in Organizational Analysis (Chicago, 1991); and Andre Lecours, ed., New Institutionalism: Theory and Analysis (Toronto, 2005).
internal debates, to give but one example, historians of the Soviet Union have recently
drawn on the work of anthropologist Clifford Geertz, without realizing that such
interdisciplinarity, however welcome, now seems dated since anthropologists have critiqued
and moved beyond Geertz. As for harvesting “tidbits without exploring their
relationship,” historians eager to explore the political culture of early Soviet socialism
have drawn upon Hunt’s work, but with few exceptions have not considered the relationship
of her concept of political culture to the different concepts of political culture constructed
by Keith Baker and François Furet, let alone even more recent “revisionist” formulations
of the political culture concept by French historians.

Overemphasizing the dismantlement has negative consequences for how we imagine
the study of early Soviet socialism. If we tell ourselves that the collapse itself was the core
explanation for our dialogue with other fields and non-Russian historiographies, we
implicitly make our field more “other” than those, such as the history of China, Korea,
and Africa, in which no historical rupture is said to have been needed to prompt the
integration of new perspectives. At the very moment when we are celebrating the field’s
supposed convergence with the mainstream culture of historical writing, we persist,
ironically, in perpetuating the exceptionalism of the Russian/Soviet field. In our persistence
in believing that historical rupture was needed to generate new perspectives, it could well
be that we have seen our field’s recent development through the prism of a “rupture”
paradigm first advanced by Vasilii O. Kliuchevskii and recently revived, in somewhat
different form, in the influential writings of Boris Uspenskii and Yuri Lotman. In any
case, reinforcing the exceptionalism of the Russian field limits our sense of the field’s
possibilities. For example, telling ourselves that we needed the dismantlement to produce
historiographical rupture might emanate from, and reinforce, doubt in our capacity for
certain kinds of originality, doubt that might limit our historiographical achievements.

There are yet other implications of the fact that significant paradigm shifts, discussed
above and derived from dialogue with other fields and non-Russian historiographies, were
underway before the collapse. One clear implication is that the pre-collapse study of
early Soviet socialism was not as isolated from the mainstream culture of historical writing

112Some recent monographs and articles that have drawn upon Geertz to examine early Soviet socialism include
Boris Kolonitskii, Simvol i vlast’ i bor’ba za vlast’ (St. Petersburg, 2001); Orlando Figes and Boris Kolonitskii,
Interpreting the Russian Revolution (New Haven, 1999); Daniel Peris, Storming the Heavens: The Soviet League
of the Godless (Ithaca, 1998); and J. Arch Getty, “Samonikritika Rituals in the Stalinist Central Committee,”
Russian Review 58 (January 1999): 49–70. Von Hagen does acknowledge these internal debates among anthropologists in
his “Empires, Borderlands, Diasporas,” 458. A political scientist who has used a post-Geertzian anthropological
framework to explain political outcomes is Lisa Wedeen, “Conceptualizing Culture: Possibilities for Political

113For something of an exception see Frederick C. Conney, Telling October: Memory and the Making of the
Bolshevik Revolution (Ithaca, 2004). See, in addition to Hunt, Politics, Culture, and Class, Keith Michael Baker,
Inventing the French Revolution: Essays on French Political Culture in the Eighteenth Century (New York,
critical assessment of this work on political culture, and revisionist conceptualizations thereof, see Suzanne Desan,
Historical Review 108 (February 2003): 119–47.

114Boris Uspenskii and Jurii Lotman, The Semiotics of Russian Culture (Ann Arbor, 1984).
as we have assumed. We need to revise, at least somewhat, the old story about a field mired in Cold War isolation. This, too, contributes to giving the field a history that is less exceptional, less other, than we have assumed it to be. Realizing this should be liberating because, by removing some of the “otherness” we have ascribed to the history of the field, our self-understanding as historians changes. We might understand that we are historians of a field that, historically, is less backward and more integrated into the mainstream culture of academic writing than we have assumed. As our self-understanding as historians changes, this can alter our understanding of what we can accomplish as historians of Russia/USSR. We might begin to think of ourselves as historians who not only assimilate insights and theoretical approaches from other disciplines and non-Russian historiographies, but as *generators* of insights and theoretical approaches that other disciplines and historians in other fields will themselves want to utilize.

Finally, the “collapse as catalyst” thesis is highly problematic as a mode of historical analysis. Most fundamentally and most obviously, it is an incorrect, overly simplified explanation of a complex historical phenomenon, namely the intellectual evolution of our field. And it brings other problems with it. For one thing, by making the evolution of scholarship a product of the “collapse,” as well as, more broadly, the end of the Cold War, globalization, and geopolitical rearrangements, we reflexively and inadvertently, à la the modernization paradigm, privilege social/political change as the motor of cultural change (for example, evolution of scholarship) at a time when many of us say we want to adopt a less reductionist and more open-ended, “messy” approach to historical analysis. If this kind of explanation is a marker, in terms of theoretical perspectives, of where many of us are, recognizing it as such gives us the chance to intervene and shift course. Third, by attributing shifts in historiographical perspective to an ill-defined, agentless abstraction such as the Soviet collapse, we avoid confronting the theoretical issues that are posed by the challenge of explaining the recent evolution of our field. One issue we can therefore avoid is deciding which theoretical approaches to use, and why: The cultural or linguistic turn? Rational choice? (This is not, I realize, a choice likely to be popular among historians!) Something else? Some combination thereof? For those inclined toward the linguistic turn, to explain the recent evolution of the study of early Soviet socialism is to find oneself in its analytical cul-de-sacs. On the one hand, by conceiving of culture as a system of symbols whose meaning is produced by their interrelationship, those taking the linguistic turn can excavate the “logic” or patterns of cultural systems, such as Stalinist political discourse or even historiography itself. But we (and by “we” I mean not only historians of early Soviet socialism but also scholars working within the linguistic turn more generally) are left stymied when individuals act at variance with what these logics or patterns would dictate. (One example of our inability to “fathom individual actions” can be seen in the historiography of the “Great Purges,” where insistence on what has been called the terror’s ubiquitous “logic,” while helpful in dissolving the equation between Stalin (or even “Stalinism”) and the Terror, nonetheless renders us analytically helpless in the face of exceptions to that logic, such as Kotkin’s “thirty-four year old [metal shop chief] Golubitskii,” who refused to scapegoat his subordinates to save himself.115) How

do we “fathom individual actions” and “explain how certain discourses, ideas, or texts become especially meaningful to certain individuals”\textsuperscript{116}—whether those of Soviet citizens in the 1930s or historians of early Soviet socialism in the last several decades of the twentieth century—\textit{without} abandoning the power that cultural analysis gives us to delineate a cultural logic that is much more than the result of the agency or intent of any one historical actor? It is beyond the scope of the essay, and perhaps even the capacity of this author, to offer a solution. But we can only find one if we pose the question. The collapse as catalyst thesis, which avoids this paradox by assigning agency only to an “agentless abstraction,” stops us from formulating this 64,000-ruble question. If we accept the premise that our understanding of both the evolution of our field and early Soviet socialism are constrained by the same conceptual limits, then we can gain greater understanding of both by defining those limits in order to transcend them.

Only if the dismantlement is assumed to date from the Brezhnev era is there chronological overlap between the Soviet demise and Western scholars’ creation of new paradigms through dialogue with other fields and non-Russian historiographies. As for a causal relationship between the dismantlement and new perspectives, it would be scorning self-perceptions too much to claim that the dismantlement had absolutely no perspective-generating powers. But it was but one factor among many, not the chief catalyst, for why historians of early Soviet socialism turned for “perspective” to other fields and non-Russian historiographies. A complete intellectual history of that shift remains to be written. What is clear, however, is that in fetishizing the dismantlement, we have spent our agency making ourselves too satisfied with the historiographical status quo. It is time we reassigned that agency to the challenging conceptual issues that remain before us as we continue the process of placing the study of early Soviet socialism in dialogue with other fields and non-Russian historiographies.

\textsuperscript{116}Spang, “Paradigms and Paranoia,” 147, 120.