differences over inter-ethnic divides, a discussion of how data from this period might be biased against finding support for strong ethnic identities seems warranted. While I am confident Bergholz is too good of a historian to have fallen into such a trap, a discussion on how he circumvented these problems would likely have been of interest to others grappling with this problem.

Despite these minor concerns, Bergholz has accomplished something remarkable in writing a book likely to influence debates in history as well as the social sciences. The book shows historians how close engagement with social science theory can deepen archival work. At the same time, *Violence as a Generative Force* raises the bar for empirical research for social scientists. Bergholz demonstrates how fine grained archival work not only allows scholars to create indirect and quantitative measures of conflict, it also enables us to trace the dynamics underlying the production of violence itself. As such, I believe this book will feature prominently on political violence syllabi for years to come. I for one have already added it to mine.

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Violence as a generative force: identity, nationalism, and memory in a Balkan community, by Max Bergholz, Ithaca and London, Cornell University Press, 2016, 464 pp., $35.00 (hardback), ISBN 978-15-017-0492-5

Evidence, explanation, and telling histories of violence: a response to Dragojević, Braun, and Fedorowycz

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In myriad forms, violence remains a crucial and, arguably, an increasingly dominant form of political practice by a host of actors in the contemporary world. It is thus not surprising that during the past two decades research on various aspects of violence has increased significantly. Historians have long been the central chroniclers of the violent past, but others, especially social scientists, have recently moved into the spotlight with a host of compelling analyses about the origins, dynamics, and effects of violence, including those of riots, pogroms, civil war, and genocide, among others. Today, the story of violent human behavior is one that many scholars seek to tell and explain, and in a host of different ways – from research methodology and scale, to narrative style. Yet regardless of who seeks to tell

histories of violence, the question of what drives people to inflict immense pain and large-scale death on others continues to remain a perplexing question in today's world, and thus is one that remains in urgent need of attention from researchers.

In my 2016 book, *Violence as a Generative Force: Identity, Nationalism, and Memory in the Balkan Community* (Cornell University Press), I offered an empirically rich historical account of a single community during 1941 in an attempt to provide answers to two questions of global significance: What causes intercommunal violence? And how does such violence between neighbors affect their identities and relations? As I discuss in the book's introduction, which I elaborated on in a February 2017 blog post for Cornell University Press entitled "Archives in Bosnia in Minutes and Hours," I stumbled into the largely unknown history of this community on a September afternoon in 2006, when I was unexpectedly given access – for 15 minutes – to a basement storage depot in a Bosnian archive. Just over a decade later, after having unearthed thousands of documents from 12 archives across Bosnia–Herzegovina, Croatia, and Serbia; after having conducted scores of interviews with residents in this Balkan community; and after having discovered a number of unpublished manuscripts written by wartime participants, I published this study.

Violence as a Generative Force reflects a number of my main research interests. I have long been drawn to the genre of microhistory, which is a scale of analysis that has informed all of my published work. At the same time, I have always been keen to use the specific geographical focus of my research (i.e. the Balkans, and in particular the Yugoslav lands) as a means through which to shed light on broader questions about the dynamics of violence, nationalism, and historical memory – a set of subjects that animate the study of modern European history in significant ways. I have always focused my attention on the countryside as the place through which to explore these subjects, a site where the vast majority of the population has lived in the part of Europe I study, yet also the place in that region that researchers have devoted the least amount of attention to.

Apart from the specific answers to my main research questions about the causes, dynamics, and effects of violence, my book makes a number of conceptual moves that challenge not only the field of European history, but also that of political violence. For the former, I aimed to dig deeper into the under-researched history of social conflict in the Balkan countryside, while simultaneously endeavoring to use my specific findings to offer answers to questions in ways that could potentially interest all scholars of European history. For the latter, I sought to amass a set of rich empirics that could challenge the social science dominated literature on political violence, which generally relies on using indirect "datasets" to engage in large-scale theorizing. Against that dominant approach, I sought to use my "data," or what historians would call "evidence," to build nuanced and contextually specific theories, which hopefully could still have some degree of explanatory portability across temporal, geographical, and even disciplinary boundaries, especially toward those working in the social sciences. How well I answered my questions, and whether my conceptual challenges are of any use to historians of Europe and scholars of political violence, can only be determined by engaging with those who have chosen to read and critically respond to *Violence as a Generative Force*.

I am, therefore, deeply grateful to have this opportunity to respond to the reflections of three of my colleagues: Mila Dragojević (The University of South, Sewanee), Robert Braun (Northwestern University), and Daniel Fedorowycz (University of Toronto). They, along with Emil Kerenji (U.S. Holocaust Memorial Museum) and Lee Ann Fujii (University of Toronto), were part of a book panel that I organized for the 2017 Convention of the Association for the Study of Nationalities (ASN) in order to discuss *Violence as a Generative Force*, which was awarded the Harriman-Rothschild Book Prize that year by the ASN

and the Harriman Institute at Columbia University. All of these colleagues have given generously of their time in engaging with my book, and I thank them. Here, I will briefly reflect about some of the comments that Dragojević, Braun, and Fedorowycz wrote as part of this book symposium. Their responses are stimulating, rich in insights, and packed with interesting questions, all of which reflect their deep knowledge of violence and nationalism. I have chosen to respond here to two issues that I see cutting across their comments.

The first concerns the evidence that I gathered in order to write my book and, specifically, its use for historians and social scientists of violence and nationalism. All three respondents were complimentary in various ways with regard to the research that I conducted. Dragojević described my book as based on "in-depth archival research" and "impressively documented." Braun noted that I seemed to have travelled "the entire region collecting and reading every possible document about the killings that seems to exist culling both official and newly discovered archives to craft a masterful narrative." And Fedorowycz wrote that my book is based on "meticulous research" and "incredible detail." But what does this level of historical research ultimately add up to? Are the challenges of excavating largely unknown histories of violence in far-off, rural locales really worth the time and effort?

These respondents, who received their PhDs not in history but in the social sciences, seem to think that the type of microhistorical research that I carried out has value beyond simply amassing a huge amount of local sources and reconstructing unknown, poorly understood, and long-silenced instances of violence. Braun, for example, notes that "*Violence as a Generative Force* raises the bar for empirical research for social scientists … [because it] enables us to trace the dynamics underlying the production of violence itself." Fedorowycz observes that

> to systematically analyze the effects of wartime violence on the identities of individuals requires that a researcher record identity preferences prior to and after a war, for which data rarely exist. Bergholz's book is an example of how meticulous research and a micro-comparative research design can begin to address this dearth in our understanding of inter-communal violence and identity formation, dispelling still prevalent assumptions that ethnicity and nationalism are the root of such violence.

The implication in such comments is that detailed historical research can actually provide new analytical leverage and explanatory depth – rather than simply offering a new mountain of evidence, often mined by historians, but to be generally used by social scientists in their often more theoretically-driven analyses. This is refreshing to hear. My general experience in trying to cross disciplinary boundaries between the humanities and the social sciences is that although some social scientists might applaud historians for uncovering new "data," little interest – or even appreciation – exists for how historians might actually go about explaining complex events, such as intercommunal violence. Both Braun and Fedorowycz offer eye-opening criticisms of their own disciplines (i.e. sociology and political science) as to why this may be the case. "A large body of social scientific scholarship on violence," writes Braun, "relies heavily on indirect measurement and strong assumptions rooted in choice theoretic models, often with a rationalist flavor." Fedorowycz states that my book "strays from formulaic (i.e. social scientific) approaches that successively refute alternative explanations offered in existing literature in order to introduce a novel and supposed more accurate explanation." Instead, he notes that I tried to build on "existing theories, using these insights as a guide to understanding the rich empirical evidence amassed through meticulous archival research and in-depth fieldwork."

What I find encouraging in such reflections is the appreciation by these scholars of the productive space within the margins between the social sciences and the humanities that I tried to open up in writing my book. As I conducted my research, my dissatisfaction grew

with much of the historical writing on violence, which seemed to me not to push hard enough to answer challenging questions about the causes, dynamics, and effects of violence. My reading of social science literature provided me with an exciting set of compelling questions that helped me carry out my fieldwork with an eye on trying to solve perplexing puzzles, such as how to explain the simultaneous existence of restraint and escalation of violence in local communities, rather than simply reconstructing events. At the same time, I found much of the social science literature to be empirically impoverished, and analytically reliant on excessively broad theorizing that, in my view, too often flattened out the nuance and complexity of local violence, and often unproductively divorced such violence from its short- and long-term historical context. In my book, I tried to chart a new course by finding a way to write within margins of the social sciences and humanities, drawing on their strengths while trying to avoid their drawbacks, by trying to forge a type of social science-inspired microhistory. It is gratifying to hear that these social scientists appreciated this new approach.

A second area in which all three respondents offered similar comments concerns my explanations – and especially my stress on the role of situational factors and contingency – in how I tried to account for the violence in the region that I wrote about. Dragojević notes that

> the book provides extensive evidence that the progression of violence is not a linear process. In many situations, factors contributing to violence prevailed, while in other cases, factors contributing to rescue, such as the questioning of the legitimacy of authority or pre-war friendships, motivated individuals to risk their lives in order to save their friends and neighbors who did not share their own ethno-religious identities.

Braun observes that

> Bergholz reveals the crucial role of situational factors, such as the momentary decisions of local elites or small-scale events that induced collective rage; violence was more likely to escalate, for instance, after warring parties excavated mutilated corpses or when more peaceful group leaders left the scene.

Fedorowycz concludes that the answer in my book as to what explains the presence or absence of violence "is a host of situational factors, 'momentary circumstances,' a confluence of 'highly contingent and situational' factors." Extending the observations of Dragojević and Braun in a somewhat more critical direction, Fedorowycz wonders about the tension that he discerns in my book with regard to a "historical, context-specific explanation," which is presented "using broad social scientific language." For him, a tension in the book between "the general and the specific" does not seem to be resolved by the stress on "context, nuance, and contingency."

I think that all three respondents have perceptively put their fingers on a major issue that my book engages with, which perhaps can be succinctly stated as the tension between "the general and the specific," to use Fedorowycz's words. What kind of explanatory framework, research methodology, and level of analysis, are most conducive to explaining the presence and absence of intercommunal violence? Historians, along with anthropologists, would tend to go with the specific, or the "thick description" or events and personalities, and would emphasize overarching context – both small and large. Social scientists might be more inclined toward the general, with emphasis on a limited set of broader explanatory factors, which might even take the form of illuminating non-causal correlations. Most would likely express disdain for contingent and situational factors, which might appear to preclude more general theorizing. We are left with two questions: Is one approach more effective than the other? Or can they be combined in some productive way that does not fatally water down either?

The answer, perhaps, lies in asking the scholar who decides to tell a history of violence what his or her overarching purpose really is. Pushing hard to explain the how and the why of local intercommunal violence is probably something on which all sides can agree. But studies of violence usually have other objectives, which are sometimes more implicit. Fedorowycz puts his fingers on what one of my purposes might be when he reflects on the question that I pose at the end: "Could such intimate violence, and the history of it we tell, ultimately be a choice that we all hold in our hands?" He concludes that "after reading the book, I am not convinced that it is." These words bring us back to the subject of contingency, and the tension between the more generalized theorizing of the social sciences, and the more contextual specifics of the discipline of history. "Beyond the choice for all humanity to do good," Fedorowycz writes,

> if the transformation of a context conducive to violence into actual killing depends on a multitude of factors coalescing in a particular moment, it seems that the individual is stripped of his or her agency; there are simply too many variables at play.

And yet, one of my implicit objectives in writing *Violence as a Generative Force* in the way that I did was to restore as much agency as the historical record would allow, and to do so all the way down to the smallest possible units: the village level and its individual residents. Throughout my book, one certainly sees larger, more impersonal historical forces at work, which decisively structured the possibilities and limits for human choice at the micro level. But within the parameters of those important forces, life and death was always determined by local people exercising a form of "constrained agency," to use Lynne Viola's evocative phrase (2017).

In my view, the complex dynamics of their agency continue to remain a subject that historians and social scientists of violence most urgently need to wrestle with explaining. And if discerning the agency of people at the local level remains a crucial challenge in the study of violence, then perhaps *Violence as a Generative Force* can offer inspiration to those seeking to tell histories of violence, regardless of their disciplinary camp. Deep historical research remains vital, but can be enhanced by directing it in a much more focused, problem-driven direction, a path that can be better illuminated by serious engagement by historians with the social sciences. If *Violence as a Generative Force* can contribute to creating a more mutually-beneficial relationship between history and the social sciences in studying past instances of violence, then the perplexing phenomenon of violence in today's world, as well as in the past, will be one that stands to be much better understood.

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