

## Response to Comments on *State-Building as Lawfare*

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**State Building as Lawfare: Custom, Sharia, and State Law in Postwar Chechnya**, by Egor Lazarev.  
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I would like to thank the editors of Nationalities Papers for providing a forum for the discussion of my book and for facilitating the symposium. I would also like to express my deep gratitude to Lisa Blaydes, Anastasia Shesterinina, and Dan Slater for taking the time to read the book and for offering their insightful comments and questions. Their scholarship was an inspiration and a source of wisdom for my research in almost all its aspects, from the art of theory-building to their methodological input on survey research and qualitative fieldwork (Belge and Blaydes 2014; Blaydes 2018; Shesterinina 2019, 2021; Slater 2010). It was a wonderful feeling to read their engagement with my work. I believe that the ways the authors of the symposium presented some of the central ideas of my book were often better than the ways I did it myself. In this response, I'll address some of the central questions and comments raised by the reviewers.

### States Make Laws and Laws Make States: Or on Causal vs. Constitutive Explanations

Slater has elegantly formulated the key argument of my book as “Wars Make Laws and Laws Make States.” This formulation highlights the continuity with the research tradition inspired by Charles Tilly – the bellicist account of state-building. I agree with this formulation but want to warn against reading it as a causal sequence: from 1) wars to 2) laws to 3) states. My analysis of the relationship between laws (legal pluralism) and states (state-building) is constitutive rather than causal. States make laws and laws make states. State law and non-state legal systems based on custom and Sharia profoundly shape each other's functioning; they are in a co-constitutive relationship, to the point that they form a hybrid legal order (as I describe in Chapters 2 and 6). At the same time, all these alternative legal systems retain some autonomy and are constituted by distinct normative systems, the systems of enforcement of these norms, and agents in charge of regulation of the norms. All these elements need to be described in order to explain the relationship between law and the state.

The constitutive approach explains the properties of a system by appealing to their parts and their organization. At the core of this approach is the study of the logic of action through description of “the patterned ways in which legal actors use legal rules and the recurrent ways in which people are affected by them” and “analyses of the multiple overlapping constraints and layers of uncertainty under which people act” (Kohler-Hausmann 2018, p. 14). My task was thus to unpack what constitutes state-society relations in the imperial periphery and what structures create these patterned ways of behavior and belief. I outlined four principal structures: legal pluralism, nested sovereignty, gender, and armed conflict, and reconstructed their interrelationship. I believe Slater correctly recognized my analysis as constitutive, as he observed that in the relationship I was studying, “the mechanisms have been multiple and the causal pathways circuitous” and that at the center of my analysis was a “particular descriptive approach” rather than “specific causal arguments.”

Constitutive analysis presents an alternative to the positivist model of causation (Cramer 2016; McCann 1996). However, the most important characteristic of this approach is not epistemological commitments, but the particular types of questions it aims to address, such as “how are things in the world put together?” (Wendt 1998). My central questions were the following: How does the state manage to impose the rules that regulate everyday life? How does a society function when there are multiple alternative rules of the game? I aimed to understand the functioning of legal pluralism and use it as a point of entry to unpack the political and social orders in Chechnya. The study included many other smaller questions, some of which were causal: for example, how does experience of collective violence affect individual choices between state and non-state legal systems? What are the mechanisms that link conflict with the mobilization of state law by women? Other sub-questions were descriptive: for example, who are the authorities in charge of alternative legal systems; how does enforcement work in non-state legal systems? how do men and women differ in their legal preferences and behavior? Descriptions and inferences made in response to these questions served as the foundation for a theoretical account of state-building. Thus, the book is titled “State-Building as Lawfare”, meaning state-building through the analytical lens of legal pluralism, not “How Legal Pluralism Affects State-Building.”

### Gendered State-Building

Blaydes has written an insightful account of my study that sharpens the focus on the principal role of gender in state-building. I fully agree with the centrality of this role; perhaps this should have been reflected in the title of the book. The salience of gender cleavage in legal preferences and behaviors is one of the most consistent empirical findings of the book. The gendered account of the war and the legacies of conflict for women’s mobilization of state law is key to understanding lawfare from below. At the same time, the strategies of politicians regarding engagement with non-state legal systems also have strong gendered implications. This is why, as I document in the book, Ramzan Kadyrov intervenes in seemingly personal disputes related to marriages, divorces, and domestic violence. Thus, I present a gendered account of both social and political orders and claim that gender is the key link between them.

In the book, I emphasize that there is a great degree of heterogeneity in women’s legal preferences and behavior. I highlighted differences related to age, marital status, social class, and whether one has sons. Blaydes suggests that this heterogeneity might be also influenced by women’s social connectedness. Blaydes’ work with Ceren Belge has shown the importance of this factor for making women more likely to rely on state institutions in Cairo and Istanbul (Belge and Blaydes 2014). Social networks that include state officials help women to “navigate politicized, potentially even hazardous, legal processes.” The importance of connections for access to the legal system is also true in Chechnya. For instance, I have recorded cases where female plaintiffs managed to get their disputes to state court because their female relative was a lawyer. However, connections to state officials might not necessarily lead to higher likelihood of using state law, because as I show in the book, state officials in Chechnya are less supportive of state law than the average survey respondent. I also document the systematic efforts of state officials to subvert state law. This paradox is likely the result of these officials’ wartime socialization and their concern for status in the community.

Slater noted that my gendered account of state-building focuses primarily on women. I understand gender as a structure, a set of rules, norms, and practices, and with my emphasis on women I did not want to limit my analysis and reduce gender to a personal attribute. In family disputes, women almost always face men. Male authorities in charge of dispute resolution are also not genderless, of course. In the statistical analysis, my focus is on the gap between women’s and men’s attitudes and behaviors, and in the analysis of war, I show how women’s social, economic, and political roles changed vis-à-vis men. Men are central in this story as well, as I emphasize that war and violence led to proliferation of the norms of hypermasculinity in Chechnya. Kadyrov’s regime also deliberately promotes these norms in its attempts to establish cultural hegemony. My

own positionality as a man who does not comply with the local norms of hypermasculinity showed the importance of analyzing gender from the perspective of men. My long hair, glasses, and non-athletic build distinguished me not only from the law enforcement cadres and government officials I was interviewing, but also from most local academics of my age. My non-compliance with gender norms may have allowed me more space to talk to both men (as I was non-threatening in terms of status) and some women (as I was visibly an outsider). What is missing in my book is an explicit analysis of gendered social order beyond women and men. The wave of violence against queer people in Chechnya happened after I finished my fieldwork, but it provides powerful evidence in support of my argument that management of the gender order lies at the foundation of the political order in Chechnya.

Slater's point that my analysis emphasizes women's war-induced disruption of gender hierarchies, their empowerment and their mobilization of state law – "a cultural revolution" at the expense of "the counterrevolution" from above – is well-taken. I do document the systematic backlash against women's empowerment in Chechnya, which manifests itself in government regulation of female clothes, de facto reversal of the wave of divorces, and lenient prosecution of men who commit "honor killings." I agree with Slater that this backlash from above is likely much more powerful than the gains in protection of women's rights obtained through the state legal system. However, the sheer intensity of that backlash itself testifies to the scale of the fundamental shift in gender relations that provoked it. I like Slater's idea of the "slingshot effect" – "even a slight tug in a liberating direction prompts a far more powerful, disproportionate overreaction against the claims of the newly assertive." However, in order to understand how a slingshot works we need to study both the "slight tug" and the "reaction." The reaction in the form of the government's conservative backlash is well-documented thanks to the magnificent work of women's rights activists and journalists such as Elena Milashina. However, the "slight tug" in the form of disruption of gender hierarchies and women's mobilization of law was less apparent and needed to be documented.

### Legacies of Conflict

Slater outlined the three causal logics that link conflict with state-building outcomes under legal pluralism: 1) attitudinal; 2) structural; and 3) coalitional, and argued for the need for a more systematic exploration of the coalitional politics of legal pluralism. In the book, I divided the theoretical outline into political order and social order and presented the potential impact of conflict on political actors and lay individuals separately. Attitudinal and structural logics were attached to the analysis of social order, and coalitional logic was one of the dimensions of political order. In my analysis of social order, I emphasized the difference between attitudinal and structural logics for understanding the legacies of conflict. The dominant strand of literature on the legacies of conflict focuses on attitudes: how exposure to violence changes individuals' identities and their political worldviews, which subsequently affect their political behavior. I call this *the logic of alienation*: individuals who experience violence become alienated from the perpetrators of violence. This logic makes sense, and we can find its manifestations in many places, including to some extent in Chechnya. My aim was to contrast this established attitudinal logic with an alternative mechanism – *the logic of disruption of social hierarchies*: how experiences of conflict change cohesiveness of communities, authority structures at the local level, and locally-relevant cleavages such as inter-generational relations and gender relations. I argued that the structural effects of conflict can override attitudinal ones and discussed how they can be conducive to state penetration into society.

When I turn to the analysis of the effects of conflict for political actors, I do analyze coalition logic, following Slater's own work (Slater 2010; Eibl et al. 2021). I focus on the communal authorities and former rebel fighters, who got amnestied and became state officials after the Second Chechen War. I argue that promotion of legal pluralism can be interpreted as a concession to these powerful constituencies. Slater's proposal for the study of coalitional politics covers the actors beyond the communal authorities and armed actors. He writes, for example, about "the feminist coalition" that

included members of civil society and pushed for the abolition of the custom of bride kidnapping, and about the anti-feminist coalition that included the government of Ramzan Kadyrov, communal elites, and men. One can also potentially extend the coalitional logic and incorporate into it the actors beyond the local level: for example, the coalition of federal officials who work to allow Ramzan Kadyrov to promote legal pluralism, and another coalition of federal and local officials who work on restricting it and ensuring the primacy of state law (admittedly with limited success). I focus on the communal authorities and fighters who turned into state officials and analyzed how promotion of legal pluralism facilitates the inclusion of these actors into the local ruler's coalition. I did so because it was a safe assumption that for both communal authorities in charge of non-state legal systems and fighters who used to fight under the banners of nationalism and religion, promotion of customary law and Sharia is the desired course of action. Extending this analysis to other actors would have been more challenging. More generally, however, the focus on coalition legacies of conflict is an important move, as one can argue that it is the coalitions of actors that set in motion and reshape the attitudinal and structural effects of conflict and give them meaning and political form. Thus, all three logics should be studied simultaneously.

Shesterinina asked an important question about the conceptualization of conflict as both rupture and social process. I fully agree with the anthropological insight that conflict is not an interruption of the social process (Lubkemann 2008) and is marked by historical continuities (Wood 2008). However, I argue that treating conflict both in terms of a shock and in terms of a process is not in itself a contradiction, especially when one adopts a more longitudinal perspective. I analyzed transformations of political and social orders during a conflict that lasted for more than a decade. For example, I explored the functioning of the institution of blood revenge during the conflict. I also emphasized surprising continuities during the war: for example, that judges at the Sharia courts of independent Chechnya (Ichkeria) largely followed the Soviet criminal code in their practice. At the same time, armed conflict is of course a rupture and a shock for any social system. The changes that conflict brings give me analytical leverage to study the deeply embedded structures behind state-society relations.

### On Ethnography

Shesterinina invited me to reflect more on the ethnography that lay at the foundation of the study. How did my study bridge positivist and interpretivist approaches, considering their different ontological, epistemological, and methodological underpinnings? What did the focus on sense-making entail? What ethnographic narratives were included and how were these interpreted?

My training in political science was strictly positivist. At the same time, during my graduate school years, I was exposed to non-positivist ways of doing research through classes in anthropology, sociology, and history. I came to the field with a positivist research design project based on quantitative analysis of legal preferences through a survey. However, before doing the survey, I wanted to understand the functioning of legal pluralism on the ground. In order to do so, I observed some actual dispute resolution practices and paid attention to everyday social interactions. I wrote fieldnotes that included reflections on what I was observing and listening to. For example, while observing one Sharia arbitration, I noticed how the female plaintiff in a divorce case was modestly dressed and how she looked down when the religious authorities were talking to her. She was accompanied by older male relatives, who spoke on her behalf. All these elements were meant to show that she was a modest "moral woman," who could be trusted with custody over her children. Observations and interpretations of everyday routine social practices were especially important for uncovering the structural gendered conditions of state-society relations in Chechnya. For example, the observation that two young male guards would always check if an older female professor was complying with the government-imposed dress code when we entered the university campus was more telling of the state production of the gender order than any survey results that I could obtain.

My attempt to reconstruct the meanings that people in Chechnya attach to customary and religious law and how they relate to the state legal system was an interpretive exercise. Through the narratives of my interlocutors, I was able to make the link between customary law, the Chechen national identity and the prominence of Sharia in their religious identity. Ethnography also allowed me to understand where the social tensions lie. The reactions of my interlocutors, often nonverbal “metadata,” showed the limits of their comfort zones; for example, one young man got visibly nervous when a friend mentioned to him that I was studying Sharia law, because he thought that I was studying the Sharia courts of the independent Ichkeria in the 1990s – a sensitive topic, discussions of which are made taboo by the current authorities. When I explained that I was studying contemporary informal religious arbitration, he calmed down.

Thus, my study had elements derived from both positivist and interpretivist approaches. Importantly, I did not mix or synthesize different epistemological traditions – I used them for different purposes. This approach within a single study provided me with “the possibilities of posing and exploring more complex sets of intellectual problems that more closely approximate the actual problems faced by actors in the social world” (Sil 2004, p. 324). It also allowed me to expand “the scope of research problems beyond that of each of the competing research communities” (ibid). However, overall, my study was neither positivist nor interpretivist, which shows the problem with this strict binary. I agree with the sociologist George Steinmetz that social scientists need to “break open the two caricatured positions of relativist discursivism versus hard-headed positivism” (1998, p. 170). Within a more pluralist menu of approaches, my study is perhaps closest to critical realism (Gorski 2013, Shapiro and Wendt 1992, Steinmetz 1998). In line with realist social science, I was not only interested in “how people think about and constitute their world” but also in “the material conditions and social structures within which they make their choices” (Allina-Pisano 2009, p. 54). The meanings attached to Chechen history and to customary and religious law, the social construction of clans and custom, and the language used in the courtrooms were all important themes in my study, but equally important were facts about the participation of women in the workforce, the number of criminal court cases per district, and the social background of a judge or an elder. The “subjective” and “objective” elements of the study were not in opposition but organically interrelated.

### What’s in a Name? Reflections on the Ethics of (Not) Naming Research Participants

Shesterinina’s comments also allow me to further elaborate on issues of ethics beyond what has already been said in the second chapter of the book. The most challenging ethical issue that Shesterinina highlighted was related to the naming of research participants in the context of potentially changing political conditions. In the book, I used pseudonyms except when I wrote about or quoted respondents in official positions or those whom I recognized as “historical figures.” I also only attributed quotes when the named interlocutors did not express any politically sensitive information or opinions. Importantly, I have been in regular close contact with my key interlocutors since leaving the field, shared translated parts of my writing with them, and was able to confirm their wish to appear in the book under their real names.

There is a default norm in social science research to anonymize all research participants in order to minimize the risks. However, many research participants do want to be named. In fact, almost everyone I talked to in Chechnya, Dagestan, and Ingushetia wanted to be named in my book. These people insisted they understood the risks, as well as the probability of change in political circumstances (as analyzed in Knott 2019).

The issues at stake here are not only of safety and confidentiality, but also of respect and autonomy, which are central to ethical research. Recently there have been serious critical reflections on the problem of naming/anonymization in anthropology and sociology that are in line with my thinking about this issue. For example, sociologists Colin Jerolmack and Alexandra Murphy (2019) argue against the default norm of masking, the practice of hiding or distorting identifying

information about people, places, and organizations in ethnography. First, they stress that masking/giving pseudonyms might be potentially problematic for the research participants. For some people, seeing their real name in print may be the main benefit from participating in the study. In addition, providing real names neutralizes “the researcher’s gatekeeping power” and enables a “research subject” to share their story, to critically engage with the study. Most problematically, masking may give research subjects a false sense of privacy and security because it implies a promise of confidentiality that it often cannot guarantee. Research participants often unmask themselves and others. In the rare cases when a study attracts attention from the media, journalists can also relatively easily find locations and people. The advent of digital technologies and social media has made masking even more difficult, if not impossible. Second, masking is also problematic for the academic community. According to Jerolmack and Murphy, it leads to “pseudo-generalizability,” implying that research participants are interchangeable and thus that “the case is representative of other, unobserved cases” (p. 12). Masking also complicates redescription and ethnographic revisits, as scholars cannot conduct research in the same locations and interview the same actors. With caveats about vulnerable groups, the authors advocate that naming people should be a default in social science research.

In the powerful words of anthropologists Erica Weiss and Carole McGranahan, “Our ethical discourse can infantilize research participants, while simultaneously reducing our responsibilities to them to a series of bureaucratic precautions. As a result, our performance of these tasks of conforming to ‘ethics’ often becomes perfunctory and impassive, distanced from our actual fieldwork relationships.” Angela Lederach (2020) extended this line of argument in the context of the study of violent political conflict in Colombia and stressed the need to avoid reducing ethics to bureaucratic protocols and recognize and honor the agency of research participants who want to be named. Obviously, the conclusion from these critical reflections is not that researchers should always name their interlocutors, especially under conditions of authoritarianism and violent political conflict. The conclusion is that social scientists need a “more rigorous, responsible, and inclusive decision-making process for naming or not naming our research subjects” (Weiss and McGranahan 2021).

My tendency was to err on the side of caution and use pseudonyms for the vast majority of interlocutors. However, I provided the real names of officials and those whom I thought of as historical figures – for example, people who occupied important positions in the past.

In addition to the considerations outlined in the anthropological and sociological contributions mentioned above, my thinking about naming/anonymization included the factor of surveillance. Following data protection protocols is necessary and I described my approach to it in the book, but if a state with high surveillance capacity wants to know what social science researchers are investigating, it will. At the same time, state officials and other powerful actors rarely care about academic articles and books. They care much more about media reports and the work of human rights NGOs. That is why we see attacks such as the one on the journalist Elena Milashina, highlighted in Slater’s commentary. Of course, the state is not the only actor to consider when protecting research participants. When an interlocutor shares their opinion or a story or some other information, this might put them at odds with their family, friends, colleagues, and/or bosses. For my part, I did not report some stories and narratives that I found to be simultaneously too specific and too sensitive and therefore traceable to particular respondents with potential repercussions for them. Here the key issue is not anonymization, but my decision not to include this information in my writing, even though it would have been important for my descriptive account of legal pluralism and my arguments.

My decision to name some key interlocutors follows the idea that a one-size-fits-all approach to naming/anonymization does not accommodate the range of relationships we have in fieldwork, “from deep, intimate connections to one-time meetings” (Weiss and McGranahan 2021). In my writing, I tried to show respect to all those who participated in my study, even if, out of caution, I named only very few.

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