

From Civil War to Political Violence

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Word Count: 12,437

Abstract: Research on civil war has grown dramatically over the last 25 years. This review essay uses five recent books to take stock of the most productive path forward in the wake of this surge of work. The books show the limits of viewing “civil war” as a self-contained category: new work is pushing beyond the traditional boundaries of the subfield. The essay argues for creating a *political violence* field that encompasses civil war, electoral violence, authoritarianism, policing, and other forms of politics that involve the threat or use of violence. This approach can generate new theory, comparisons, and data across areas of research that have previously existed in self-contained silos. While ongoing research on conventional civil conflict topics remains essential, the biggest gains in coming years will come from cross-pollination between research programs tackling diverse aspects of order, conflict, and violence. The essay highlights four particularly promising agendas to pursue and their relevance to pressing contemporary policy challenges.

- Balcells, Laia. 2017. *Rivalry and Revenge: The Politics of Violence during Civil War*.
- Blaydes, Lisa. 2018. *State of Repression: Iraq under Saddam Hussein*. Princeton: Princeton University Press.
- Debos, Marielle. 2016. *Living by the Gun in Chad: Combatants, Impunity and State Formation*. Zed Books.
- Fazal, Tanisha. 2018. *Wars of Law: Unintended Consequences in the Regulation of Armed Conflict*. Ithaca: Cornell University Press.
- Finkel, Evgeny. 2017. *Ordinary Jews: Choice and Survival during the Holocaust*. Princeton: Princeton University Press.

Introduction

The study of civil war has expanded remarkably since the early 1990s. This research has been driven by the uncertainties and opportunities of the post-Cold War world, in which interstate war is rare but internal wars remain unfortunately common. Topics that had largely been left to Vietnam War-era debates received a jolt of new energy and attention as book contracts, prestigious jobs, and journal slots flowed toward a newly defined and increasingly robust civil war field.

Examining five recent books, and surveying the literature more broadly, this essay explores where we go next.ⁱ I argue that “civil war” may have outlived its usefulness as a self-contained area of study.ⁱⁱ The books under review show the possibilities for moving beyond classic research on civil war into an integrative “political violence” field that can also speak to topics like state repression, policing, authoritarianism, state-building, political economy, crime, and international security. The greatest intellectual success of the civil war subfield would thus be its disappearance into the broader study of the use and threat of violence, and their consequences.

I use the books to identify a set of research agendas for building out this field. First, the books under review makes clear just how tightly interwoven wartime dynamics are with pre-war politics, how porous the boundary between war and post-war can be, and the varieties of

relationships that exist even within war zones. Second, echoes of the processes identified in civil war research can be found in a far wider range of phenomena, including policing and state repression, non-violent civil resistance, electoral violence, and authoritarianism, that are generally siloed off into separate scholarly communities. Third, it is clear that we can do a better job of merging levels of analysis in political violence research, especially in weaving together international dynamics with domestic processes. Finally, the books show fascinating variation in the “stickiness” of political cleavages and organizational alignments: in some conflict environments, there are tight and sticky lines of division, while in others there is remarkable fluidity and haziness in alignment and identification.

The books under review draw on diverse intellectual traditions but all touch on core questions and approaches in the civil war field. This positions them nicely to identify new directions moving beyond it. They are also methodologically eclectic, including deep fieldwork in Debos, global cross-national data in Fazal, and qualitative and quantitative micro-data in Balcells, Blaydes, and Finkel. They form a valuable sample of research from which to take stock of where the field has gone and where it can go next.ⁱⁱⁱ

This argument proceeds in three parts. The first section summarizes the key arguments of each book. I focus on contributions relevant to the agenda of building a political violence field. Second, I use these contributions to offer a forward-looking vision built around four potential research agendas. The study of political violence will inevitably have blurry boundaries, but can stretch from policing to authoritarian rule to electoral violence to full-scale civil war, while explicitly considering when and why we see movement across this spectrum. The essay concludes by pointing to contemporary political questions – from techno-authoritarianism to the

diffusion of “lone wolf” terror attacks – that would benefit from a new, integrative approach to order, conflict, and violence.

Politics and Violence in the Spanish Civil War

Balcells seeks to explain the dynamics of violence against unarmed civilians in conventional civil wars, drawing primarily on the Spanish Civil War, as well as the conflict in Cote D’Ivoire. She argues that most research equates civil war with guerrilla conflict, does not explain why civilians might be killed in territories where one armed group already has high levels of control, and struggles to explain variation over time within a conflict in these patterns.

Balcells’ theory aims to overcome a previous “neglect of political variables” (pp. 8-9) and instead “put politics at the center of an explanation of wartime violence” (p. 182). *Rivalry and Revenge* argues that the distinction commonly drawn between ethnic and ideological civil wars is not meaningful in contexts with high levels of pre-war political mobilization: this generates ideological identities that can be identified at the local and individual levels (p. 29).^{iv} Locally, past voting patterns provide valuable information to combatants about the landscape of political loyalty. Individually, mobilization and competition put individuals in situations in which their ideologically allegiances become known in their communities. Mobilized civilians provide an essential resource for armed groups whom they support and a deep threat to groups they oppose (pp. 26-27).

This sets up a wartime context in which “civilian collaboration with armed groups are rooted in factors that are exogenous to the war, namely local prewar political configurations” (p. 7). Where there is parity in political loyalties, the partisans of an armed group that controls the territory will collaborate with the armed group to target the members of the other bloc in order to shift the long-term political balance of power: “their political ideas will be threatening in the

future social and political order” (p. 27). This is collective political targeting.^v By contrast, non-mobilized individuals are more “malleable” (p. 27) and thus less likely to be targeted. Parity is where “direct” violence against ideological enemies is most likely. In areas of lopsided loyalty, there is little chance of decisively tilting the future balance one way or another. In areas dominated by loyalists of an armed group, the future is secure; in areas dominated by ideologically suspect individuals, the costs of killing all of them becomes too high to be worth it (p. 34).^{vi}

Local political elites are the crucial hinge actor because they have symbolic and coercive powers that allow them to more easily and effectively work with armed groups to target enemies (pp. 30-33). Balcells uncovers a grim world of “identifying and assassinating individuals” (p. 36) based on prewar political identities. This helps us understand sweeps behind the frontlines against alleged “fifth-columnists” and “subversives” that are disproportionate to the actual level of military threat they pose. Individuals are expected to act in future according to their past political mobilization, rather than having their behavior dominated by an apolitical logic of survival.^{vii}

Over time, these initial variables are joined by other dynamics. Emotions – especially revenge – can accelerate cycles of violence that a straightforward prewar politics explanation cannot account for. If a territory changes hands between armed groups, past violence can spur vengeance once the new group takes over – “revenge motives add to strategic motives for revenge, so violence in a locality is likely to escalate and to be greater in each round” (p. 39). In protracted or highly fluid wars, they can overcome past pre-war dynamics.

Balcells also considers “indirect” violence, in which military actors use bombardments of some sort to unilaterally strike at particular territories (p. 230). She argues that indirect violence

can be somewhat discriminating, at least at the level of communities. In the early stages of war, armed groups use prewar information about politics to target areas seen as hotbeds of resistance – in Spain, she shows that the Nationalists systematically directed greater airstrikes against areas with a high leftist vote share in the 1936 election and trade union representation, even when controlling for various military variables (pp. 139-140). As with direct violence, however, as the war advanced, there was an additional revenge dynamic in which bombings were used against areas in which compatriots had been targeted the adversary (p. 142).

This is a hugely impressive book, showing deep historical knowledge, extensive new data collection, and creative theorizing. What are the limits of Balcells' account? The book has a tendency to scope out numerous topics and disclaim any effort to deal with them – numerous interesting and seemingly relevant topics are banished from discussion when they do not deal with the core puzzle at hand.^{viii} This is admirable from the perspective of clarity, but creates a somewhat unnecessarily lean sparseness in an already-short book.

Theoretically, the claim that local political elites act in the same way when they are in an overwhelming majority as when they are in an overwhelming minority requires more justification. Even if killing huge numbers of the majority political cleavage may be too costly (though the costs of killing, and what drives variation in them, are left fairly vague), violence or coercion intended to trigger flight or generate new behavior seem quite plausible in a context in which elites are outnumbered but their favored armed group has control. Local political elites are the crucial actors in Balcells' theory, but their capacities and goals seem flat and under-theorized relative to their importance.

Regardless, *Rivalry and Revenge* offers essential contributions. I highlight three for the purposes of this essay. First, it shows that historical political processes have a deep effect on key

dynamics of civil war, often at a highly local level and with very contingent origins. This is especially true in the early days of a war, which structure much of what is to come. While Balcells is clear that endogenous dynamics are very important (p. 183), the deep structure of conflicts are established by prewar politics. Too much emphasis on “micro-dynamics” can, at least in some conflicts, make us miss the forest for the trees. Not only are macro-politics very important, but Balcells highlights the role of “meso-level” local political elites who exist in between the armed forces of each side and the local civilians on the ground. Individual-level agency can recede in the face of structural forces and elite calculations.

Second, her book argues that “those who had been mobilized by leftist political forces before the war were not easily turned into right-wingers afterwards, and vice versa” (p. 184). Elections, trade unionization, and mass mobilization, by both left and right, created and diffused ideological visions and political identities that stuck. Balcells correctly argues that she helps to reveal “a ‘dark side’ of both political mobilization and political competition” (p. 190): elections and “normal” politics were intertwined with the dynamics of warfare.

Third, Balcells notes that a variety of cleavages that existed in pre-war Spain “collapsed in the civil war cleavage to the point that people sometimes fell on one side or the other without having taken a deliberate decision about it” (p. 191). The politics before the war were clarified and transformed during the war, leading to a related but distinct set of political cleavages after the bloody end of the conflict. As she concludes, “cleavage dynamics during armed conflict have interesting implications for the study of the dimensionality of political spaces, and they can have implications on political dynamics in the postwar period” (p. 192)

Constrained by History: Jews in the Face of the Holocaust

Finkel's fascinating book, like Balcells', draws heavily on historical evidence from the grim world of 1930s-40s Europe: both reach back into a seemingly-familiar past to uncover novel data and insights for the present. *Ordinary Jews* is a bleak book, as befits its subject matter – the individual choices and community-level trajectories of Jews during the Holocaust. If Balcells outlines in stark detail the dark side of political mobilization and competition in the bloodbath of 1936-39 Spain, Finkel brings us face to face with the impossible choices and profoundly constrained options of Jews facing the Nazi death machine.

Finkel identifies a set of strategies Jews could pursue: cooperation and collaboration (subtly distinct from one another), coping and compliance, evasion, and resistance (p. 7). He then explores a three-case comparison of Minsk, Krakow, and Bialystok, all lying within the “zone of extermination” in which no Jew was to be spared (p. 13). He uses election data, oral histories, data on ghettos and their experiences under the war, data on Zionist organization elections, and numerous archives, along with extensive secondary sources, to offer a remarkably detailed reconstruction of the experiences of both individual and communities.

At the individual level, *Ordinary Jews* argues that politically active Jews prior to World War II were more likely to pursue cooperation, public collaboration, or resistance during the war – they viewed themselves as defenders of the community, were more visible, and possessed political and organizational skills and connections (p. 9). Such individuals found themselves either trying to protect their community through working with the Germans or fighting them. At the community level, Jews who were more integrated into non-Jewish society were better equipped to pursue evasion, while those lacking that integration pursued coping within their community (pp. 9-10).

Why did the overall distribution of strategies vary across communities? Finkel's core claim is that "the variation in Jewish behavior was a direct outcome of one key variable: pre-Holocaust political regimes" (p. 10). This macro-structural context affected both the integration of Jewish communities with non-Jewish communities around them and the occurrence of independent political activism within Jewish communities. Rather than picking up the story after the German tanks rolled in, we instead "need to begin by looking at the sociopolitical realities of their earlier lives" (p. 36).

These historical legacies fundamentally shaped the options available once thrown into the lethal maelstrom of the Holocaust. Evasion was most likely where there had been state strategies of integration – Jews knew non-Jews and had access to information and cultural cues that would let them escape; in these same places, however, coping tended to collapse into collaboration in the absence of strong, if enclosed, communal organization. With low levels of integration, Jewish communities "successfully coped as long as the Nazi authorities allowed ghettos to exist and levels of private collaboration were low" (p. 11). But there was nowhere else to run, and these isolated communities were entirely at the mercy of the Nazis.

Where did we see armed resistance? Finkel points to pre-war state repression as a key variable driving variation in "skills" of resistance (pp. 162-3). Where states selectively repressed before World War II, Jews acquired expertise in responding to state violence by going underground and organizing resistance. When repression was either indiscriminate or nonexistent, there was little reason to acquire the necessary skills – indiscriminate violence made it irrelevant whether one was knowledgeable about underground resistance, while the lack of repression obviously did not generate much resistance expertise.

Though the book contains many more specific and nuanced arguments than this essay can accommodate – about expectations from past experiences with Germany, interactions between Polish and German Jews, the role of gender, and many others – we can see the broad outlines of this theory in the three case studies, as well as broader data Finkel gathered on distributions of behavior across ghettos (somewhat unfortunately buried in Appendix 3).

In Minsk, the Soviets had targeted Jewish communal life in the 1920s-30s; as a consequence, “old religious and political institutions and practices gradually vanished as the city’s Jews became increasingly Soviet not only in citizenship, but also in political outlook and behavior” (p. 38). Integration occurred to a greater extent than in Krakow or, especially, Bialystok. During the Great Terror, this integration occurred with even greater force as a “mandatory state policy” (p. 39). As a consequence, Minsk would see very high levels of evasion – Jews could more easily “pass” and navigate non-Jewish life outside the ghetto en route to the “partisans’ zone” (p. 156). Yet for the same reasons, communal life within the ghetto was more disorderly and coping suffered in the face of collaboration. Moreover, Minsk had experienced the Stalinist indiscriminate repression of the Terror and its Jews had not developed skills in surviving selective repression. The underground in the Minsk ghetto did not take up arms against the Nazis, focusing more on evasion and assisting fellow communist partisans in the forest (pp. 168-170).

In Krakow, we see an intermediate case. There was some assimilation prior to World War I (p. 40), while its aftermath saw both the existence of communal organizations (especially Zionist) and of assimilated Jews who “did not belong to any Jewish bodies and organization” (p. 41). Once the war hit, there would be less evasion than in Minsk, but more than in Bialystok, and mixed success at coping. Bialystok in turn occupies the far extreme from Minsk: “coping was a

reasonably successful and attractive strategy” (p. 34). Assimilation had not occurred prior to the German arrival, and the “radical” (p. 43) segregation of the city would make evasion essentially impossible. Instead, a robust Jewish communal life existed that, along with the impossibility of escape, created a highly organized system of coping until the ghetto was finally wiped out.

Finkel’s book is impressive in many ways, but its most important and unusual quality is viscerally bringing alive the hopes and fears of Eastern Europe’s Jews. For Finkel, these were not passive victims, but agents and actors caught in an unimaginably difficult and tragic situation. His ruminations on guilt, culpability, and choice are haunting, and avoid easy retrospective judgment.

Yet Finkel’s book, perhaps because of its extraordinary range and scope, falls into something of the opposite trap of Balcells’. *Rivalry and Revenge* is unwilling to stray from an insistent, if at times repetitive, focus on its core hypotheses. *Ordinary Jews*, by contrast, is rich and layered, but sometimes undisciplined – mechanisms emerge and disappear, the historical depth can be difficult to follow, and the individual and community-level arguments can be hard to fit together. The book’s embrace of complexity and historical specificity are laudable but can also make it difficult to crisply summarize the core argument and its attendant mechanism.

The biggest theoretical challenge that arises from this framework is merging individual and community behavior. As the book unfolds, the community level is most consistently and systemically linked back to the overarching theory. The individual-level arguments come and go, and it is hard to know how to weigh them against alternative explanations. Most of the historical evidence on trajectories is similarly focused on community dynamics, whether of prior communal organization or state strategies of assimilation, repression, or isolation. Studying

individual-level behavior thus becomes something of a distraction compared to the bigger historical argument.

Nonetheless, it is easy to draw important claims from *Ordinary Jews*. Finkel makes clear that resistance was just one possible choice among many, and that we need to explicitly theorize why it was chosen over other alternatives. Like Roger Petersen, Finkel shows that the choice of rebellion is not a simple dichotomy of quiescence vs revolt.^{ix} Individuals that rebelled could have coped, collaborated, tried to flee, or cooperated; the determinants of revolt must be directly integrated with the determinants of other choices. Finkel wisely does not frame this as a book about Jewish insurgency – that was only one, relatively rare, outcome among others, all of which need to be considered together.

Second, there is no escape from history. For Finkel, “if they [Jews] appear in an analysis only after their confinement to ghettos and murder by the Nazis, our ability to understand their behavior is limited” (p. 12). The story instead starts decades prior, with complex processes of mobilization, repression, and state integration policy.^x This generated structural constraints that severely limited political agency – in Bialystok, identity cleavages were rigid and Jews had fewer options than in Krakow or Minsk. As we see with Debos below, the fluidity of cleavages and alignments across conflicts varies hugely: some see remarkably, easy side-switching, passing, and identity change, while in others lines of cleavage are tragically locked in place.

Repression, Resistance, and Identity in Saddam’s Iraq

Lisa Blaydes’ *State of Repression* tackles a set of fundamental questions: what affects regimes’ views of their societies, the consequences of varying state policies for citizens’ identification and behavior, the sources of variation in ethnic identity over time, and the determinants of resistance and revolt. Her core claim is that “states create the political behaviors

that they face as a result of their policies toward their citizenries” (p. 5). Violent revolt is only one possible outcome of state policy and its outbreak is intimately tied to a set of other political processes, from rumor-spreading to coup attempts.

This makes civil war an inextricable part of the politics of state policy and identity construction under Saddam’s rule, rather than an aberrant outcome apart from “normal” politics. Blaydes focuses her explanation on the “intensity and precision of expected rewards and punishments administered by the state” (p. 35). These are conceptualized as “distribution and punishment regimes” (p. 39) that citizens interact with and act in anticipation of. High intensity, highly individualized rewards lead to active cooperation and investment in the regime, while collective goods provision that is low intensity leads to acquiescence or depoliticization. On the cost side, the imposition of low-intensity collective costs generate disaffection, while individualized and high intensity costs drive grievance and disloyalty (p. 43) On the side of non-compliance, individualized and low-intensity punishments create public non-cooperation, group/communal targeting at middling intensity creates “communal” underground transgressions, and collective punishment at high intensity generates open nationalist rebellion (p. 52).

Where do these policies come from? Autocracies are faced with substantial resource constraints and limits on their ability to render society legible (p. 44). Language, geography, and cultural distance can make some populations more difficult to penetrate, and thus more likely to be treated as crude collectives. Resource shocks can undermine the ability to autocratic regimes to distribute material goods. These interact to create differing distribution/punishment regimes (pp. 50-54). These can vary within a society: “the politically relevant cleavage structure may differ across communities within an autocratic regime” (p. 49).

Blaydes then examines the evolution of regime policy over time and the responses of different kinds of social groupings. She argues that 1970s Iraq had the ability to distribute resources quite broadly, generating reasonably high levels of public compliance. The growing costs of the Iran-Iraq war and drop in oil prices in the 1980s reduced the ability of the regime to provide material goods to the population. Iraqi Shi'i bore a disproportionate burden, while Kurdish armed groups' collaboration with Iran led to state crackdowns that were collective because of Baghdad's difficulty in pursuing selective repression in Kurdish areas.

The collective repression of the Kurds in the late 1980s fueled a surging nationalist rebellion that was able, with American support, to carve out an area of self-rule in northern Iraq. The regime shattered the Shi'i uprising of 1991 and moved toward a regime of intense, "middling" collective punishment aimed at tribes and religious groups. This generated ongoing underground resistance (p. 57). The Sunni population, outside of Saddam's Tikriti core, became less invested in the regime, though not to the same extent as either the Shia or Kurds. There were coup efforts that the regime prevented through its deep penetration of potential Sunni dissident networks. The divergent trajectories across ethnic groups show how "the way that punishment is implemented influences forms of political identity that become salient within the population" (p. 308).

Like other books under review, *State of Repression* represents an iterative mix of theorizing with empirical work. The data is remarkable: for instance, to measure the relative losses across the country during the Iran-Iraq war, Blaydes draws on a school census from 2001-2 (asking students if they had brother or father killed in that war or the Gulf War), an archival list of "distinguished families" from 1989, and extrapolations from a set of Baath party memos from

1984 (pp. 93-100). There is no silver bullet regression or decisive case study; instead, the book deploys a slow but eventually overwhelming accumulation of different forms of evidence.

Several claims are particularly relevant for conflict scholars. Blaydes argues that the Kurdish insurgency escalated into a full-blown civil war because Baghdad pursued “suboptimal policies” (p. 134) in periods of economic and political crisis. This collective punishment was a cause, not a consequence: “broad-based cohesion as a nationalist group did not emerge until the end of the Iran-Iraq war” (p. 134). She challenges “the conventional characterization” of this relationship as “ill-fated from the start” (p. 135). Instead, Blaydes argues that it was the murderous Anfal campaign that generated much higher levels of nationalist sentiment and mobilization (pp. 148-160).

Blaydes’ argument about the Shi’i is particularly important because it illustrates how compliance and resistance can take *non-insurgent* pathways that can still be understood through the same theory as explanations of open insurgency. Blaydes shows that rumors were heavily concentrated in Shi’i populations (pp. 197-198), and followed systematic patterns of circulation. The cause of this pattern was the greater degree of state penetration of the Shia community compared to the Kurds: “the increasing intensity of punishment meted out at Shi’i populations after the 1991 Uprisings encouraged individuals with grievances to engage in private rather than public forms of non-compliance” (p. 212).

Similarly, the Shi’i community became a site of network-based mobilization because of the “middling” ability of the Ba’ath apparatus to penetrate the community: the state could render the Shi’i legible enough to prevent recurrent mass revolts, but struggled with religious groups and networks around particular clerics as these organizations “posed a particular legibility challenge for the Ba’ath regime” (p. 238). It engaged in collective targeting of such groups, and

“religious group members continued to engage in transgressive behavior with implications for the types of religious identity groups to emerge in Iraq after the US invasion” (p. 238).

Finally, Blaydes’ account unpacks intra-Sunni dynamics in which coup attempts were the primary form of resistance, rather than rural insurgency or clandestine religious mobilization. The relative under-representation of non-Tikriti Sunnis within the military created intra-ethnic imbalances in access to the highest rungs of state power: as the sanctions regime reduced Saddam’s material resources, these population “saw significant declines in quality of life and . . . did not enjoy the same levels of access, employment, and privilege” (p. 294). Revolts were thwarted by “the complete security sector penetration” (p. 304) of Sunni networks, leading to very high levels of public compliance even among the relatively marginalized portions of the Sunni population.

The book is of course not perfect. The theory sometimes alternates between a fairly simple, standard cost-benefit framework that does not break new ground, and an intricate set of highly specific mechanisms spread out across multiple charts and driven by a large number of moving parts. There is little systematic discussion of where the argument is wrong or could use further improvement. The extraordinary evidence sometimes flows without being systematically linked back to the theory. One issue that raises its head is what we can know about compliance from a state’s own records: the question of bias, missingness, and interpretation in using regime archives is brought up, but despite these repeated and clear caveats, the book sometimes slips into treating Ba’athist records as true representations of reality.

These are inevitable issues for such an ambitious and sweeping piece of research, however. *State of Repression* is a fascinating, creative book that forces us to expand how we think about the sources of ethnic and nationalist identity, the variants that resistance can take,

and the ways that state policies can drive remarkable shifts in these outcomes. It makes clear how theoretically interlinked open rebellion is with clandestine resistance, tactics like spreading rumors, and coup efforts. It shows that we need to theorize rebellion directly alongside its alternatives and offer arguments that can explain a wider spectrum of activities.

Second, *State of Repression* does not view internal warfare as a politics apart from what came before or after. Blaydes' account explores the Kurdish and Shi'i insurgencies, as well as other forms of resistance, but they are part of a broader historical story. Revolt is caused by patterns of state repression, which then drive long-lasting feedback loops that can reshape identities.^{xi} "Civil war" was not a distinct period or type of Iraqi political history: it was inextricably part of it. The numerous pieces of quantitative data provide the empirical backbone of a deeply historicized narrative. *State of Repression* identifies a common set of mechanisms running across contemporary Iraqi politics, whether or not in a context of open revolt; indeed, the conclusion of the book works through the consequences of Ba'athist policies for the structure of political forces after the American invasion.

Between War and Peace in Chad

If the previous three books rely heavily on historical quantitative data and case studies, Debos' fascinating book draws instead on extensive, on-the-ground field research in a challenging research environment. Debos uses this fieldwork to make ambitious claims: "the start of a conflict does not represent a dive into the Hobbesian war of all against all; the practices and representations of a pre-war period do not disappear overnight" (p. 4). She focuses on the experiences of fighters and civilians in what she calls the "inter-war" in Chad: "spaces and times that are affected by violence even if there is no direct fighting between rebel and governmental forces" (p. 8). We see clear echoes of the books above: "what is at stake here, theoretically

speaking, is an attempt to think in terms of non-linear, interwoven processes that lie outside the framework of the transition between war and peace” (p. 9).

Debos argues that “armed violence is governed by rules and structured by boundaries” (p. 15). The first part of the book is devoted to a reconstruction of Chad’s political trajectory, embedding it in a broader colonial and post-colonial history that created a set of structures and practices that generated deep “lines of continuity” (p. 36). In particular, auxiliary fighters and specialists in violence without fixed loyalties or institutions existed well before independence. Debos argues that independent “Chad is in the grip not of chaos but of a political field that has never excluded war” (p. 42).

The extraordinarily complex history of rebellion in Chad, with labyrinthine splits and recurrent revolts, provides the raw material for Debos’s account in combination with local field research. She focuses in particular on a floating set of specialists in violence and their trajectories across anti-regime revolt, regime co-optation, and in-between spaces of local activity de-linked from the “master cleavages”^{xii} of Chad’s various internal wars. These trajectories are made possible by fluidity across lines of conflict: “no player, internal or external, is ultimately unacceptable as a coalition partner” (p. 78). Social networks constitute organizational actors (pp. 94-96), but given these networks, “it is all a matter of negotiations and improvisations in an unstable environment” (p. 96). As rebel groups are bought off and co-opted, the upper ranks of the formal state swell with former insurgents, but meaningful benefits are unevenly distributed (p. 117).

Many former rebel soldiers end up “integrated” into Chad’s security apparatus, a shambolic institution with uncertain numbers, numerous “ghost” soldiers, and a brutal and arbitrary mode of action. Yet for Debos, this is not a case of a “weak state”: “incomplete control

overly personnel, flexible chains of command, an absence of *esprits de corps* and of discipline are not necessarily weaknesses” (p. 120). The concept of the “unnumbered decree” anchors the later chapters of the book: “the ‘decree’ indicates state power, while the absence of a number reflects that such practices are illegal and unofficial” (p. 120). The formal security services have a remarkably similar “mode of operation” as the “politico-military” armed groups that get incorporated into them after deals (p. 128). The regime does not push too hard to monopolize violence: instead, “it leaves things alone when it would be too expensive to intervene, and it intervenes directly or indirectly when the preservation of the balance of forces is at stake” (p. 130): factional coalitions emerge and shift within the governing coalition as they do on the opposing side. Indeed, the regime’s defense is a bespoke affair, as “the army’s profile was adapted, on a case-by-base basis, to the profile of its enemies” (p. 130).

This fluidity leaves long stretches of “inter-war” in which powerful armed actors are able to gain resources and political clout. Hierarchies persist, even if they do not neatly map onto formal organizational charts of state power (pp. 147-148). This interweaving of forms of power matters for how we think about state power. In Debos’ analysis of Chad, “the state has been informalized, but it is neither weak nor absent, even in regions far from the capital” and “the strengthening of one [the official state] does not necessarily imply the weakening of the other [informal state]” (p. 168). Indeed, “disorder is a way of governing the army and the state” (p. 175). Yet this is not chaos: “Overt violence is the exception rather than the rule” (p. 176). Debos concludes with an “invitation to go beyond analyses that conclude, rather hastily, that what is not war is, by default, peace and stability” (p. 179).

Living by the Gun in Chad is a fascinating, though at times uneven, book. It can be somewhat hazy in its argument, finding its firmest ground in either describing in rich detail how

politics works in Chad or sweepingly critiquing more conventional accounts without always providing an analytically clear alternative. The empirical work moves from granular field research to biographical studies of commanders' trajectories to macro-historical narratives of Chadian history, but figuring out how these pieces fit together is at times a bit challenging for the non-specialist.

Debos' insistence that the politics of disorder are in fact ordering is persuasive, but runs into questions: if everyone knows the rules of the game, why have so many revolts broken out and regimes been replaced at such high human costs? How precisely would we distinguish ordering-disorder from its blander alternative, of a state institutionally unable to penetrate society? What explains variation in group and individual trajectories, and do these trajectories matter? The book asks many hugely important questions, but does not always fully answer them.

That said, the fluidity that Debos highlights raises the broader question of why conflicts vary in the "stickiness" of their core cleavages. In Chad, Debos argues that "The role of ideology is marginal" (p. 176), a finding with resonance in some – but not all – other conflict research.^{xiii} Debos shows in detail just how dynamic the choices and political alignments of individuals and groups can be, forging a radically different political arena than if the salient cleavages allowed less movement.

Debos' account also forces us to think more carefully about state power. Blaydes offers a more typical understanding of state strength, with legibility and resource endowments driving variation across space and time. Debos suggests instead that precisely the lack of formal state authority may actually provide opportunities for regimes to entrench themselves. The very haziness of the state makes it possible to accommodate an extraordinary variety of factions within its loose and porous boundaries. The legal authority that the government possesses can be

used when convenient, while informal and illicit resources can co-exist with it. While similar to this work in some ways – above all a focus in the fluidity of coalitions – Debos also challenges the conceptions of the state in the “weak state paradigm”^{xiv} of civil war. That paradigm posits a Hobbesian realm in which ethnic groups and strongman factions maneuver within the broken shell of a hollow state. For Debos, elements of this conceptualization are important, but mischaracterize how strength can actually be summoned.

International Law and Violence

Fazal’s book marks a different approach, turning to macro-historical shifts over time in international politics and their importance for how wars are waged and end. Much of her book examines interstate relations: specifically declarations of war, compliance with international law during war, and peace agreements ending these wars. In this domain, she argues that the rise of codified international humanitarian law and laws of war have had a set of “perverse” (p. 5) consequences: peace treaties and declarations of war have declined because states want to avoid legal liability and accountability for lack of compliance. Largely forged by NGOs and legal practitioners, international law has led states and militaries to opt out of their provisions by refusing to formally declare or end wars. These arguments and findings alone would make the book a major work for IR scholarship.

Yet Fazal also grapples with issues central to this essay: when and how rebel group declare independence, engage in targeting of civilians, and end their wars in peace treaties. Fazal argues that rebel groups take cues from the international landscape in deciding how to act and focuses on secessionist groups because their ability to generate recognition of statehood hinges on persuading a critical mass of states to support them. Fazal identifies several patterns: “in contrast to interstate war, civil wars are increasingly likely to conclude with formal peace treaties

today” (p. 2), secessionists have become less likely to unilaterally declare independence, and they are less likely to target civilians than other types of insurgents (p. 6).

These patterns have emerged because secessionists “are highly attuned to the preferences of the international community and the demands of international law” (p. 162). Secessionists are trapped in an international system whose rules and norms they have to try to comply with in order to achieve their war aims. This system is stacked against them: due to the “state centric nature of IHL [international humanitarian law]. . . it pays to be a state” (p. 5). Following the rise of the UN system, there has been a “decline in the use of unilateral declarations of independence since 1945” (p. 164) despite the actual amount of secessionism itself increasing (p. 169). Put simply, “despite the stated support for self-determination, the principle of supporting state sovereignty and territorial integrity was elevated over the principle of self-determination” (p. 171). Secessionists therefore have ended up locked in long, protracted wars without availing themselves of the possibility of a unilateral declaration, in hopes of winning the favor of powerful states.

The same basic principle applies to civilian targeting: “secessionists have political incentives not to target civilians for many of the same reasons that restrain them from issuing unilateral declarations of independence” (p. 194) though there are exceptions.^{xv} Peace treaties have similarly increased as a civil war termination measure because “of the expressed preferences of the international community in favor of such agreements” (p. 219). The proliferation of laws of war “has affected rebels engaged in civil war very differently from states engaged in interstate war” (p. 215). Yet Fazal notes that these consequences have had complicated, often counterproductive, effects. Peace treaties may get signed – but many of these deals fail and often do not actually build meaningful peace (p. 242). Secessionists who try to play

by the rules often receive little reward in terms of actual recognition by the international community: “behaving well does not necessarily end well for secessionists” (p. 249). Over time, the mismatch between what the international system prescribes for secessionists and the paltry rewards they receive for following the prescription seems hard to sustain.

Where does *Wars of Law* leave the reader wanting more? Given that it considers both inter- and intra-state violence, it can feel like two quite separate books in spite of Fazal’s compelling efforts to draw them together. Given the nature of her data, controlling for the many factors that can influence secessionists’ choices about civilian targeting other work identifies is extremely difficult; the kind of micro-level data and close study of specific conflicts we see in Finkel and Balcells is not compatible with Fazal’s much broader empirical scope. There is a rich literature on civilian victimization in civil wars that can only, at present, be tested using sub-national comparisons and data, and so it can be hard to tell how Fazal’s international relations-based theory truly fares against the full range of existing approaches.

Fazal’s emphasis on secessionism makes sense given her interest in the incentives of armed groups to receive recognition. She acknowledges that center-seeking groups also take cues from the international system, but ultimately limits her discussions to secessionists (pp. 60-64). This seems like a missed opportunity. The fates of non-secessionists can hinge on international responses as well, and the theorizing Fazal offers about the differences between the two is underdeveloped. That said, this is less a failing of an already-ambitious book than an open question.

Fazal’s book only deals in part with the central themes of this essay, but it highlights important implications for a broader understanding of conflict. Much of the work in civil war research emphasizes micro-level variation in violence and conflict, but this risks missing a bigger picture: armed groups do not just respond to variables located within their country of operation,

but are also trying to “read” the international system. We need to take the systemic context seriously even when doing fine-grained studies: this may account for differences across micro-studies.

Another takeaway from Fazal’s book, in line with several of the others, is the importance of unintended consequences in understanding politics. While scholars like to seek strategy and intentionality behind choices, Fazal’s account of the rise of international humanitarian law and norms around conflict shows how well-meaning choices helped to generate totally unexpected results. There is not a straightforward “strategic” account of IHL’s origins available, beyond the banal statement that actors tried to achieve preferences: the goals that IHL’s crafters had simply were not what resulted. This is a thread that runs through all of the books under study here, providing more support for the claim that we need to take a much longer-run view of political violence, instead of starting analysis once the guns have started firing.

Toward A Political Violence Field

The books under review identify a set of important questions for a new, integrative *political violence* field that builds on but moves beyond the classic questions of the last quarter-century. We should think of civil war as one of a wide range of political processes involving the threat and use of violence to achieve political goals by individuals, non-state armed groups, and states. The claim here is *not* that we have learned everything we need to about civil wars – basic questions continue to deserve sustained attention - but instead that learning more about both civil war and other forms of political violence will be most fruitfully advanced in dialogue.

This means that the political violence field can bridge everything from high-intensity civil wars all the way to day-to-day policing, including along the way phenomena as diverse as riots, international interventions in domestic conflicts, authoritarian and democratic state

repression, low-level civil wars, electoral violence, coups and intra-elite purges, and lynchings and vigilantism under its aegis. This intellectual organization would also bring together research traditionally separated into international relations and comparative politics subfields, while also seeking to embed the American case in broader comparisons. The boundary on one end is clear: intense civil wars, genocide, and other forms of highly visible, violent, and open political contestation. But the boundaries on the other extreme are admittedly fuzzy: policing and state surveillance, for instance, represent a blending into “normal” politics that can be difficult to cleanly separate from social control. To some extent, all politics involves a struggle over power and authority; questions of coercion and control lie in the background of even the most seemingly anodyne topics.

It may be possible, however, to establish boundaries that take seriously how direct the threat or use of violence is: policing in racial divided cities is far closer to the study of political violence than assignment to congressional sub-committees. Regardless of where precisely we draw boundaries, the books under review make clear their porousness and the potential to make progress by exploring these areas of overlap and complementarity. This integrative approach can enrich literatures that are not commonly thought of as part of research on political violence. In the rest of this section, I identify four agendas as starting points for carving out a political violence field. They are far from exhaustive, and some may prove to be dead-ends, but they can provide initial directions for a forward-looking integrative project.

Cleavages, Fluidity, and Violence

It is striking how much variation there is in the fluidity of alignments, both individual and organizational, across contexts of political violence. DeBos present compelling evidence of dramatic swings in allegiance in Chad. But the wars that Balcells and Finkel study are far

“stickier” – being Jewish under the Nazis tended to only end in one, very grim, way, while the Republican/Nationalist cleavage in Spain was identifiable and rigid. Blaydes’ book shows variation over time, but variation that eventually crystallized into a set of increasingly hard ethnic cleavages (as well as intra-ethnic divisions) in Iraqi politics, during periods of both civil war and comparative peace.

This raises the question of when and why permeability across cleavages occurs, and when instead armed groups, governments actors, and civilians remain confined to the side of the “master cleavage”^{xvi} around which they first mobilized. What factors lock some wars into a pitiless clash of arms while allowing other to be free-flowing, highly permeable “wars” of ongoing deal-making?^{xvii} We can find echoes of similar questions in non-civil war contexts of political violence: when do governments gain informers from within ethnically distinct populations? When is policing biased along the lines of politicized social cleavages, and when does it transcend them? When do we see mass protest or underground mobilization based on class, as compared to other cleavages? What determines how party-linked specialists in violence direct their coercion, and what explains variation in their allegiances? How often do security forces factionalize along lines of social identity or ideological belief, and when instead do they remain cohesive even in the face of social unrest?

Understanding these questions requires studying dynamics far prior to conflict onset, examining how the legacies of conflict shape the cleavages of post-war politics, and grappling with both international and domestic variables to understand how particular political constellations emerge and change. There is no way to isolate this question into the temporal period of an actual civil war^{xviii}, especially since the “hardness” of identity cleavages is

obviously relevant well beyond traditional civil war contexts, affecting everything from state surveillance and repression to electoral politics.

Several possibilities suggest themselves in the books under review. Fazal notes that macro-historical changes in the international system may create varying opportunities for different kinds of actors to harden or soften politically salient cleavages: secessionist groups, for instance, must navigate treacherous geopolitical waters, and adopt their alignment choices accordingly. Blaydes puts her focus on both social factors and state resources as driving longitudinal shifts in ethnic boundaries, in line with a strand of historical constructivism in the study of ethnic politics.^{xix} These point to at least some relatively slow-moving, structural conditions, as well as more fluid variables like resource endowments. For Finkel and Balcells, the ideological visions of contending actors play a central role in determining ethnic, religious, and political boundaries: Debos' claim that "the role of ideology is marginal" could not be plausibly said of the Nazis' rampage across Europe or the bitter dynamics of the Spanish Civil War. As a wave of other recent work suggests, ideology in general has been under-studied in political violence and offers one especially intriguing area of research for better understanding where and when cleavages are fluid as opposed to locked into place.^{xx}

Such a move also means expanding how we think about politics in the aftermath of conflicts. Debos shows how even "interwar" periods in Chad have been deeply shaped by prior histories of violence, while Finkel's analysis of Jewish resistance to the Nazis focuses in part of the legacies of past repression. These insights find echoes in other emerging literature. Blattman, Gilligan et al., and Blattman et al. show that exposure to conflict may have prosocial effects, suggesting a very different conclusion at the community-level than assumptions that war breaks down ties and increases social isolation.^{xxi} Autesserre and Lake explore the limits of "post-

conflict” as a meaningful term in Congo, using detailed fieldwork to identify the ways in which conflicts march on even when the guns are mostly silent.^{xxii} Legacies of conflict are likely to fundamentally shape numerous aspects of politics, society, and economy, and to also imprint themselves deeply on the individuals who experienced the conflict.

Expanding the Meaning of “Conflict”

Conflict, like violence, is a word used in a huge variety of ways. The books under review make clear that a broad approach to what “counts” as conflict behavior, and as the nature of conflict itself, is the right course for future research.^{xxiii} At the level of individuals, we need to consider rumor-spreading, underground mobilization, evasion, and collusion as all being integral tools in the struggles for power against the use or threat of violence. As Petersen argued nearly two decades ago, the question of what counts as “recruitment” into insurgency or counterinsurgency – or any number of other forms of mobilization – is incredible difficult to answer beyond the extreme case of formally joining an armed organization.^{xxiv} Resistance can take many forms, as can repression - and both are possible (and common) without open violence being observed.

This is why I included the threat of violence and use of coercion in the definition of a political violence field above: equating conflict with observed violence ignores all of the ways that conflict plays out in its absence. Rather than seeking to separate them we need to consider their relationship to one another.^{xxv} Blaydes’ book, for instance, shows how underground mobilization and rumors can be integrated into the same theoretical framework as open revolt; Finkel explores how compliance, revolt, collaboration, and coping can all be responses to repression. Carving such behaviors off into a different category of politics artificially limits our understanding of what conflict can be and how power is built and contested.

The same is true at the level of organizations. In the books under review, and beyond, we see tenuous ceasefires or live-and-let-live deals between states and armed groups (as in Iraqi Kurdistan), the loose integration of armed factions into state structures without meaningful demobilization (above all in Chad), groups making more or less open political demands for secession (as in Fazal), armed political parties, de facto criminal actors, and local bands of armed individuals desperately trying to survive the grim tides of high politics far beyond their control, in addition to more traditional insurgents and pro-state militias.^{xxvi} There has been a growing literature exploring when civil resistance is chosen as opposed to violence, but here we see an even broader array of possible options for political actors.^{xxvii} The landscape of organizational political roles and behaviors is remarkably diverse; even more strikingly, groups can shift between these roles, or combine them in a variety of sophisticated ways. For instance, many groups need to generate economic resources to survive – when should we categorize them as primarily criminal, as opposed to political, and under what conditions should we dispense with these categorizations altogether?^{xxviii}

We see similar heterogeneity in the nature of the state structures tasked with repression, social control, and counterinsurgency.^{xxix} Police and especially national interior ministries, for instance, deserve vastly more attention: work on policing and race in the United States makes clear the importance of politics to policing choices, rather than viewing security forces as straightforward instruments of law and order.^{xxx} Understanding counterinsurgency requires a much richer appreciation of the ways that political ideologies, identities, and coalitions can become embedded within security forces, rather than focusing too much on tactics, organizational processes, and micro-variables. Formal security forces can be further joined by local law enforcement agencies, loose networks of hard-liners or reformers, specialists in

violence contracted out to by the state as provocateurs or deniable repressors, free floating militias, posses, agents provocateurs, private armies, and gangs, and external advisers and militias (as in Syria).

Lurking in the background of these specific topics is the broader question of how to think about state power that finds such radically different answers in the Debo and Blaydes books. There is no need to make a hard choice between them, however. Instead, further research needs to unpack the different ways it is possible to be a “strong” state and/or regime and what these may imply about the onset and nature of violence. For instance, Reno and Day note that many African regimes hold onto power despite pursuing strategies wildly at odds with counterinsurgency best practices.^{xxxix} What can this tell us about the diverse pathways to political survival – and loss of power – that governments are able to deploy? And how can we think about the full array of options facing prospective challengers, from accepting placid co-optation to trying to infiltrate the state apparatus to secretly preparing for mass protests?^{xxxix}

Civil war is therefore only one manifestation of a set of broader struggles for power among individuals and organizations. Reframing the field in this way identifies a promising set of research agendas that do not abandon the insights and evidence gathered thus far, but that push for new comparisons, data sources, and theories. We need to devote as much attention to the many contexts in which open battle has not erupted but conflict, violence, and its threat fundamentally structure the political arena as the open, straightforward cases of civil war.

Normal Politics and Political Violence

Normal politics is the stuff of most political science: processes that we generally associate with the functioning of political systems in periods without a major rupture in political order or the direct threat of open violence. In democracies, this most centrally includes elections

and the functioning of independent institutions; in autocracies, the operations of a ruling party or other autocratic apparatus distinct from the direct use or threat of violence. In both types of regimes, it can include the distribution of resources (whether public, private, or club goods), the implementation of policy directives by local bureaucrats, and efforts to socialize the mass public.

Political science tends to study these topics as categorically distinct from political violence. But coercion can be uncomfortably present in “mainstream” political life. A rich literature has shown how electoral competition can trigger a wide variety of forms of violence and its threat: tight elections, highly politicized state institutions, and floating networks of specialists in violence linked to parties and security forces can all increase the risk of violence around elections.^{xxxiii}

But the linkages can go even deeper in democracies. As in Balcells’ book, election results can actually provide valuable information to civil war combatants.^{xxxiv} Armed groups can intervene in elections, both backing and opposing politicians and parties to try to tilt the playing field in their favored direction: democratic contestation provides a variety of potential opportunities for armed actors to shift resources, supporters’ attitudes, and violence toward or away from mainstream contestants.^{xxxv} Patronage can flow through networks controlled by armed political parties and elected warlords,. Club and private goods provision may be run by armed actors, while political parties – like a number of Shia parties in contemporary Iraq – can fuse social service provision with military capacity.

Violence and its threat are obviously central to understanding authoritarianism and scholarship focuses on either the threat of violence among authoritarian elites or the use of violence/coercion to keep mobilization from below at bay.^{xxxvi} But the ways in which these linkages actually operate are often conceptualized too narrowly. Important studies of

authoritarian rule and breakdown have embedded within them theories of revolution and/or coup-making.^{xxxvii} But these theories are often far less sophisticated and compelling than that of scholars of insurgency and high-risk collective action: for instance, the “masses” or “lower classes” almost never rebel as a cohesive force, class identity frequently does not map onto actual mobilization, and ethnicity, ideology, and political/social networks are often central to who actually rebels or can credibly threaten to. This may help explain substantial misfires in this literature.^{xxxviii} Similarly, work on authoritarian regimes could use a richer appreciation of the interconnections between coups, insurgencies, mass protest, and underground plots.^{xxxix}

Merging Levels of Analysis

A final agenda is better integrating international with domestic dynamics in the study of political violence. Fazal’s book is obviously centered on precisely this issue: she shows how huge structural shifts in the laws and norms governing international politics have influenced the behaviors of both states and rebels. This joins a larger literature on international dynamics on civil wars, ranging from external backing for rebels to post-war reconstruction and peacebuilding.^{xl} While important, this work is often not very well matched to domestic-focused work on political violence: the methods, data, and theories that IR-focused scholars emphasize are often fundamentally different than those deployed by comparativists. Fazal moves in an exciting, if at times uneven, integrative direction by showing how the same international set of norms can have radically different effects at the interstate level compared to intrastate domains: she notes that “it is unusual to treat both types of war [interstate and civil] in a single book.”^{xli}

Fazal is not the only one in the books considered who points to the international dimension as crucial. Balcells argues that “further research might explore the international and domestic sources of prewar political mobilization and how they are intertwined”^{xlii}: the

extraordinarily ideological polarization of 1930s Europe, for instance, cannot be understood as a primarily domestic phenomenon, but instead diffused across borders. In DeBos's account, "International actors. . . present themselves – and are often presented – as actors who might contribute by providing 'answers' and not as playing an active role in those very 'problems.'"^{xliii}

Substantively, we see the spread of forms of surveillance, monitoring, policing, and counterinsurgency, from the School of the Americas to Iran's efforts to export the revolution to Chinese technology for mass social monitoring.^{xliv} The spread of both ISIS and white nationalist terrorism has been deeply transnational, fueled by social media and demonstration effects. Modular organizational models have been adopted by aspirational Marxist-Leninist, Maoist, and Islamist armed groups: even when not wholly successful at adopting these, the example of these models has had an effect on visions of both strategy and organization. There are fascinating transnational flows of security forces, insurgents, private contractors, and training that have effects both within and across borders.^{xlv}

This all suggests the opportunities for research on how international-domestic interactions influence a wide variety of forms of political violence. Domestic-focused researchers cannot ignore the transnational and international dynamics of state repression, revolt, dissident mobilization, diffusion and demonstration effects, and the actual involvement of various kinds of foreign actors. Explicitly theorizing, or at least empirically controlling for, cross-border dynamics will be essential for making plausible claims about intrastate political violence: Fazal, for instance, argues that we need to know much more about when secessionists decide to turn to violence and what factors influence that choice over the lifecycle of secessionist movements.^{xlvi}

In turn, internationally-focused scholars are faced with the task of matching their data and theories, which often operate at the level of whole countries, or even the entire international

system, to the often very fine grained micro- and meso-level processes that many conflict scholars use. Autesserre and Lake show one way to deal with this challenge, using extensive fieldwork in Democratic Republic of Congo to see what happens when international peacebuilding projects run into the realities of on-the-ground politics, while a stream of research on counterinsurgency has sought to understand how foreign intervention strategies interacted with local conditions.^{xlvii}

Conclusion

The study of civil war has been enormously productive over the last quarter-century. And yet precisely this progress now signals the time for a shift in how we think about key questions, problems, and approach. The five books reviewed here capture, in very different ways, possibilities for future research that build on, but move beyond, the now-traditional canon of research on civil conflict. There are core questions that remain unanswered even about classic civil war questions: I am not making a case for abandoning bread-and-butter civil conflict research. Nevertheless, there are exciting opportunities to move beyond the core questions that have animated conventional research, and to more aggressively integrate this research with work on authoritarianism, “violent democracies,”^{xlviii} and policing and state repression, among others. Rather than continuing to operate in siloes with limited overlap, a more self-consciously ambitious political violence field could pull these topics together. Scholars can continue to work on their favored areas of focus, but all would benefit from broader engagement across related research terrain.

This is not merely an exercise in scholarly semantics. There has, unfortunately, never been a more pressing time to study political violence and coercion in all of its forms. While the return of great power competition demands renewed attention to big questions on international

security, questions of political order, repression, and rebellion remain relevant across much of the globe, from criminal conflict in Latin America to ongoing civil war in the Middle East to new forms of authoritarianism and illiberal democracy finding purchase from Hungary to China.^{xlix} The spread of elections has not removed the possibility of violence – looming in the background of our contemporary crisis of democracy is the possibility of a resort to violence by states, organized non-state actors, and lone wolves. The end of “big footprint” counterinsurgency has not ended foreign interventions in civil wars; rather, low-level proxy wars and counterinsurgency campaigns mixing special forces and airpower, from Yemen to Mali to the southern Philippines, characterize much of the international security environment.^l As the post-Cold War international order faces growing strain, the norms around peacekeeping and conflict mediation are likely to change in the coming years.^{li} These real-world politics make a better understanding the use and threat of violence crucial. Rising to this challenge requires breaking new ground in how we conceptualize, theorize, and study political violence.

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- ⁱ See also Bulutgil 2019 and Kalyvas 2019 for excellent recent assessments of the state of the field.
- ⁱⁱ For canonical definitions of civil war see, Sambanis 2004 and Melander et al. 2015.
- ⁱⁱⁱ I do not claim that this selection is fully representative of the field, but throughout I draw on other work to flesh out a broad range of approaches to the study of internal conflict.
- ^{iv} Compare with Kaufmann 1996
- ^v See also Steele 2017.
- ^{vi} This assumes that local elites are not genocidal and do not actively want to purge the entirety of the opposing bloc in their area.
- ^{vii} Kalyvas 2006.
- ^{viii} For instance, discussion of why we see variation in pre-war political mobilization is relegated to a future research agenda at the end of the book (p. 192).
- ^{ix} Petersen 2001.
- ^x Bulutgil 2019 also highlights the importance of prewar variables.
- ^{xi} The broader literature on civil war onset includes Fearon and Laitin 2003 and Cederman et al. 2012.
- ^{xii} Kalyvas 2006.
- ^{xiii} Christia 2012, Seymour 2014.
- ^{xiv} Roessler 2016.
- ^{xv} Such as targeting ethnic others in the area that a group wishes to carve out as its own state. Fazal 2018, 196.
- ^{xvi} Kalyvas 2006.
- ^{xvii} On alignment in civil wars, see Kalyvas 2008, Christia 2012, Seymour 2014, Otto 2018.
- ^{xviii} Lewis 2017; Staniland 2017.
- ^{xix} Laitin 1986.
- ^{xx} Lyall et al. 2013; Straus 2015, Gutierrez-Sanin and Wood, Hoover Green 2018, Maynard Leader 2019. For a somewhat skeptical view, see Bulutgil 2019, 9-11.
- ^{xxi} Blattman 2009, Gilligan et al. 2014., Bauer et al. 2016. For other work on the political *consequences* of violence, see Slater 2010, Davenport et al. 2019.
- ^{xxii} Autesserre 2010, Lake 2017.
- ^{xxiii} Davenport et al. 2018 makes a related argument about "peace."
- ^{xxiv} Petersen 2001.
- ^{xxv} Cf. Scott 1985.
- ^{xxvi} Even just among rebels, we see strikingly difference approaches to governance. Arjona et al 2015.
- ^{xxvii} Chenoweth and Stephan 2011; Cunningham et al. 2017.
- ^{xxviii} Barnes 2017, Lessing 2017, Yashar 2018.
- ^{xxix} For a recent overview and analysis of the state repression literature, see Jones and Lupu 2018.
- ^{xxx} Davenport et al. 2011; Greitens 2016; Soss and Weaver 2017; Obert 2018; Mummolo 2018; McCall 2019.
- ^{xxxi} Reno and Day 2014.
- ^{xxxii} Parkinson 2013.
- ^{xxxiii} Levitsky and Way 2010, Hyde and Marinov 2012, Condra et al. 2018, Smith 2019.
- ^{xxxiv} See also Schubiger and Steele 2018.
- ^{xxxv} Matanock and Staniland 2018.
- ^{xxxvi} For instance, Svolik 2012, Greitens 2016, Slater 2010, Levitsky and Way 2010.
- ^{xxxvii} Among the most influential are Boix 2003 and Acemoglu and Robinson 2006.
- ^{xxxviii} Haggard and Kaufman 2012; Slater et al. 2014; Albertus 2016.
- ^{xxxix} Roessler 2016; Brooks 2019.
- ^{xl} 2002, Gleditsch and Salehyan 2006, Fortna 2008, Matanock 2017.
- ^{xli} Fazal 2018, 161.
- ^{xlii} Balcells 2017, 192.

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- xlⁱⁱⁱ Debos 2016, 178.
- xl^{iv} On counterrevolutionary diffusion, see Weyland 2016.
- xl^v Avant 2005, Hegghammer 2011.
- xl^{vi} Fazal 2018, 255.
- xl^{vii} Autesserre 2010, Lake 2017, Berman et al. 2018,
- xl^{viii} Arias and Goldstein 2011.
- xl^{ix} On social media and violence, Mitts 2019, Gohdes forthcoming.
- ¹ Mir 2018.
- ^{li} Howard and Ward 2018.