Book Discussion

Jesse Driscoll, *Warlords and Coalition Politics in Post-Soviet States* (New York: Cambridge University Press, 2015)

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Alexander Kupatadze
King's College London
akupatad@gmail.com

Jesse Driscoll's excellent book, *Warlords and Coalition Politics in Post-Soviet States*, is one of the best scholarly texts available about the politics of state formation in post-Soviet Eurasia. Driscoll skillfully combines analytical rigor with impressive ethnographic work that is rare in contemporary political science research. The strong sides of the book are too numerous, so I will just focus on a few weaknesses and the issues I thought the author should problematize more.

The key argument Driscoll puts forward is that warlords colluded to establish the post-communist state, hoping to secure a share of foreign aid flowing into the country. But the assumption going into it, that post-Soviet warlords are believed to anticipate the amounts of forthcoming international aid and then would compare those profits with the domestic rents, sounds problematic mainly because the text does not make it clear whether these warlords really had a good sense of how much foreign cash would flow to the country (129).

Driscoll admits that his account of events gives "tremendous agency to war- lords" (45). This is probably fair initially, in the process of warlords installing the civilian president, but does not it change after the presidents take office, are "elected" in public balloting, and get access to formal power? The power foundations of Georgian President Eduard Shevardnadze and Tajikistan President Imomali Rakhmonov (now Rakhmon) were still shaky but didn't they have more bargaining power vis-à-vis the warlords? Also, is it fair to treat Shevardnadze, an experienced politician and former Soviet foreign minister, on par with Rakhmonov, "a completely unknown figure" (89)? One can argue that Shevardnadze's personal connections facilitated his access to decisionmakers in Europe and the United States, and therefore to international aid, and if the rents that accompanied this aid was so important to the warlords, would not that also make Shevardnadze's involvement more crucial compared with his counterpart in Tajikistan?

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Much of the book's descriptions of post-Soviet militias are extremely interesting and useful.[1] I was left with the question, does it make a difference that most militias in Georgia are urban in nature compared with more rural Tajik militia (Table 3.2, on p. 58)? More important, the militia recruitment process and the rationale for violence are mainly explained as rational actors making profit-oriented calculations. Driscoll argues that the members were recruited into militias by either the lure of short-term benefits or the hope of long-terms jobs (98). In fact, many of the Tbilisi-based militias were just neighborhood gangs established by childhood friends (e.g., *kulinarielebi* in Saburtalo district or *Borotebi* in Vera district of Tbilisi). They also functioned as "status communities." Even though most of the fighting among them could have initially been triggered by competition over resources, some of the ensuing violence can be better explained as blood feuds rather than fighting over the spoils. Souleimanov and Aliyev, for example, (2015) have discussed the role of blood feuds in Chechnya context.[2]

Another issue that I thought should have been problematized more is the differing backgrounds of these warlords. Gia Gulua in Georgia, mentioned on page 108, was a police general with conventional police training and had no prior experience in the illicit economy or organized crime. He and other individuals with similar background are lumped up together in the same category of warlords. But are not they different from the warlords with criminal background? There must be different implications for state-building and regime consolidation depending on the previous role of the warlords and their related objectives and calculations regarding the political regime and public policy, especially in criminal justice system.

Bernard I. Finel
National War College, Washington, dc
bernard.finel@ndv.edu

This is a fascinating book that makes important contributions in several areas. Most unique, perhaps, is that this is a rare academic book that combines theoretic rigor, rich empirical evidence, and significant policy relevance. Taken

1 On a more formal note, the text is not free of errors. For example, on page 168 Driscoll mentions that 60,000 traffic police have been fired in Georgia (instead of 16,000) and on page 116 he misspells the name of Kakha Targamadze (Tamunindze instead of Targamadze).

2 Emil Aslan Souleimanov and Huseyn Aliyev, "Blood Revenge and Violent Mobilization: Evidence from the Chechen Wars," *International Security*, 40, no. 2 (2015): 158–180.

together, this combination makes this study a very important addition to the literature on conflict resolution, post-conflict stabilization, and state-building. The book addresses a significant puzzle, namely, why the transition to post- Soviet states was as peaceful as it was. Why, in particular, did the former Central Asian republics not devolve into an endless cycle of instability, conflict, and insurgency? How, as Driscoll asks in the introduction, "did order consolidate so quickly in the post-Soviet space?" (1)

This outcome might be associated with a clean handing-off of strong state institutions from Soviet to post-Soviet authorities. But Driscoll effectively notes that the strong institutions—the Communist party and the security services, among others—were not actually handed down intact (48, 50) but subject to a great deal of competition. Though even in the worst cases, this competition was constrained by essentially a shared strategic commitment to authoritarian control over the distribution of resources.

Intuitively, it seems possible that third-party intervention might help shape a peaceful transition. Indeed, it is widely accepted, following Walter,3 that third parties can help solve sub-state disputes by serving as a neutral arbiter and stabilizer of last resort, by helping solve commitment and coordination dilemmas in accordance with norms of inclusiveness and pluralism, and by providing security guarantees that mute security dilemmas. In other words, we might as- sociate the successful post-Soviet transition to an outside power implementing "state of the art" post-conflict resolution and mediation techniques (12).

The problem is that the outside intervener in this case was Russia (85), whose behavior was far from "state of the art." Russia was often aloof, biased, uninterested in reconciliation and inclusiveness, and generally solely interested in minimal stability (90–92). It was backed by international aid that was largely impossible to control once disbursed, which might have been expected to feed corruption. So how did Russia, in a sense, do better than the United States has in places like Iraq and Afghanistan? And better than the United Nations and other international organizations in their various post- Cold War interventions?

The answer Driscoll proposes is bargaining among local actors, with a significant caveat. The Russians were interested in minimal stability and were willing to provide arms and money to regimes able to provide this outcome. The inter- national community was nominally more interested in norm-driven outcomes, but ultimately incapable of imposing these. The local actors were thus put into a position of needing to form coalitions to secure these payoffs, and in doing so they established stable post-Soviet orders. This is, in a sense, counter-intuitive because establishing a prize for control of the state—analogically similar to

3 Barbara F Walter *Committing to peace (*Princeton: Princeton University Press, 2002).

the dynamic that occurs in resource-rich countries—might encourage endless competition and conflict. The richness of Driscoll's book is to refute this intuition both conceptually and empirically.

Others will be better able to assess Driscoll's modeling, but the core argument is compellingly simple. In structuring the post-Soviet dynamic as a Stag Hunt game (33), the situation encouraged cooperation. Why? Because in a traditional Stag Hunt, the problem isn't just commitment, it is coordination. So this is a Stag Hunt with communication, and the commitment problem is "solved" through disincentives to defect since the Stag is not a one-time future payoff, but rather an iterated series of smaller payments. Over time, this created incentives to cooperation, and also allowed emergent regimes to "coup- proof." (34–35, 123–124)

Others will also be better able to assess Driscoll's empirical work, particular- ly that focused on Georgia and Tajikistan. That said, he deserves a tremendous amount of credit for being willing to develop new empirical evidence through extensive field work and interviews with local actors in Central Asia. This is rare, particularly among scholars also working with formal models. Driscoll's work should be held up as a model of what a multi-methods approach can deliver when implemented by a scholar with a rigorous approach and extraordinary commitment (213–219).

One of the most important contributions of this study is the emphasis on bargaining between local actors. Particularly from a policy perspective, this is significant. In the past decade or so, us policy elites have repeatedly been drawn to analytical frames that minimize the importance of local actors in sub-state disputes. Whether in the development community or the military, proposed solutions have tended to be structural and externally imposed. Development efforts have increasingly centered on conditional aid, focused around various process-oriented development goals. The Millennium Challenge Corporation is a prime example of a sort of one-size-fits-all model of "good governance."

Similarly, the us military has been repeatedly bewitched by models of stabilization activities that essentially deny agency to local actors. Whether in the form of us Counterinsurgency doctrine as written in Joint Publication 3–24 (revised 2013), which essentially ignores local actors except as responding to simple Maslow-style hierarchies of need, or the even more pervasive tendency to reduce local actors to "human terrain"—that is, passive factors that need to be "navigated," there has been little effort to conceptualize actors in development, stabilization, or counter-insurgency scenarios as agents pursuing their own interests strategically.

Here is where Driscoll's work really shines. He doesn't just replace this passive scheme with an active conceptualization, his field work strips away

the surface and unambiguously demonstrates the depth and sophistication of the strategic calculus among "warlords" and their followers. Rather than simplistic caricatures, they emerge as fully realized agents engaged in sophisticated (and hardnosed) calculation.

Now, the interesting thing is that Driscoll's finding fits into an older intellectual tradition. Tony Smith's *The Pattern of Imperialism*, published in 1981, provided an answer to Marxist interpretations of imperialism by framing the process of imperialism as a dialectic between core and periphery in which the actors in the periphery for strategic reasons often embraced the outsiders and allowed them to gain a foothold in new territories.

Smith's work was controversial because it seemed to "blame the victims." Yet, it explained the key puzzle of imperialism, which is how a small number of outsiders were able to dominate much larger populations often at the far edges of their capacity to project even minimal power, particular in the early, pre-institutionalization phase of the process. "Blame the victim" is the wrong frame. A better frame is to note that Smith's work restored agency to the local actors. British imperialism in India is not fundamentally a story of British power, it is story of Indian discord. Similarly, what Driscoll does in his study is lodge an explanation of outcomes as a consequence of strategic decisions and actions of Central Asian actors in pursuit of incentives provided, in part, from outside, but still fundamentally about interests and calculation of what others have chosen to see as passive "human terrain."

In short, Driscoll work is a model of relevant, methodologically sophisticated, and policy-relevant social science. It may be underappreciated by formal modelers uninterested in empirical work, by Central Asian experts uninterested in theory, and by policy practitioners wedded to their own implicit and unsophisticated intuitions. But all three would benefit from engaging it deeply. For practitioners, particularly those focused on post-conflict stabilization and counter-insurgency, this book, along with work by Stathis Kalyvas and Jeremy Weinstein, should prompt a major rethinking of their approaches and models.

Finally, a small criticism in closing. It is possible that Driscoll's conclusions are less generalizable than suggested thus far. Although the structural arguments are compelling and perhaps more widely applicable, there are various normative sources of order in this study that are underexplored. In particular, one might note the importance of the position of "president" (86–90) as the institution around which Driscoll describes consolidation and bargaining. That, in itself speaks to a set of understandings and expectations that go beyond the immediate logic of distribution of rents. The same also applies with the general acceptance of physical boundaries, the lack of unbridgeable cleavages

among rival actors, relatively muted legacies of traditional divide-and-conquer colonialism as imprinted on status relationships, and so on.

Driscoll invites generalization explicitly by assessing outcomes in the post-Soviet states against other cases of decolonization and post-conflict stabilization, but it is probably worth remaining skeptical simply because of what seem be to at least some unique elements. That said, this is a major work and should be widely read in the field.

Kimberly Marten
Barnard College, NY
km2225@columbia.edu

Jesse Driscoll's new book, *Warlords and Coalition Politics in Post-Soviet States*, fills a gap in the existing literature on warlords by providing a rich, ground- level view of warlords' beliefs about their own behavior.

What makes this book remarkable is the brave (and sometimes dangerous) field research that went into it, and the 173 interviews with warlords and their patrons and followers in post-Soviet Georgia and Tajikistan that Driscoll originally conducted for his doctoral dissertation at Stanford University. Driscoll's choice to live long-term among the warlords and militias he studied makes him a true anthropologist of warlordism. In this sense, he is following in the footsteps of sociologist Sudhir Ventakesh, who turned his own University of Chicago dissertation on the political economy of violent gangs on Chicago's South Side into the bestselling book, *Gang Leader for a Day*.4 Driscoll also follows directly in the tracks of Georgi Derluguian's groundbreaking observational work on violence in the post-Soviet Caucasus, particularly Derluguian's findings that supposedly "ethnic" conflict is often really centered on how private militias divide up the spoils.5

Driscoll's book is complex and confusing at times. For example, the brief synopsis in the front matter implies that the book will test and challenge the prevailing argument that Russia played a key role in settling the post-independence

4 Sudhir Venkatesh, *Gang Leader for a Day: A Rogue Sociologist Takes to the Streets* (New York: Penguin, 2008).

5 Georgi Derluguian, *Bourdieu's Secret Admirer in the Caucasus: A Biography in World-Systems Perspective* (Chicago: University of Chicago Press, 2005); Derluguian, "The Tale of Two Resorts: Abkhazia and Ajaria before and since the Soviet Collapse," in Beverly Crawford and Ronnie D. Lipshutz, eds, *The Myth of "Ethnic Conflict": Politics, Economics, and 'Cultural' Vio- lence* (University of California International and Area Studies Digital Collection, 1998, http:/ repositories/cdlib.org/uciaspubs/research/98/8/).

conflicts in Georgia and Tajikistan, stating that outcomes in these cases "had nothing to do with security guarantees by Russia … and everything to do with local innovation by ruthless warlords." Yet Driscoll states:

> It is not surprising that Russian military units intervened to settle almost every single decisive military battle fought on the former territory of the USSR. It was only with military assistance from Russia that South Ossetian and Adjaran militias held back Georgian paramilitaries. Tacit aid from nearby Russian bases eventually became explicit when Abkhaz secessionists were aided by Russian airpower and paramilitary fighters. … Artillery from the Russian Black Sea Fleet played a crucial role in the defeat of the Zviadist insurgency [in Migrelia] (81)

He continues, "In Tajikistan … Russia lent its armed forces and intelligence to support the [President Emomoli] Rakhmonov regime against domestic challengers." (127)

Driscoll's argument about Russian involvement seems to be more subtle: that Russia did not appoint or control the warlords and militias that were involved in the fighting, and that the local bargains reached between those militias were really what drove civil-war settlement (82). Even then, though, he does not provide deep evidence about the absence of Russian political involvement in the bargains, taking at face value what could be seen as self-serving statements by some participants that Russia played no role in their choices.

Rather than being an argument about Russian involvement, the key purpose of the book, as highlighted on p. 23, seems instead to be to outline and explain the comparative order-providing bargains that warlords reached with each other and with early state leaders in the two newly independent countries. Driscoll accompanies his anthropological insights with rational choice mathematical modeling. His basic hypothesis is that warlords will form, join, and remain in coalitions with each other when they perceive it to be in their interests to do so, and will together support a titular head of state when they perceive that the benefits of doing so outweigh the costs. He argues that they will command their private armies so as to maximize the political and economic payoffs they earn.

The idea that successful warlords are rational cost-benefit maximizers with keen political instincts about the state is not surprising to anyone who has researched warlord politics. One weakness of Driscoll's book is that he does not spend much time situating his own arguments within the broader comparative literature on warlords. For example, Driscoll borrows and builds upon many of the analytical insights of William Reno, the scholar who essentially

invented the modern study of the political economy of warlords in his study of Africa.6 While Driscoll includes Reno's book in his bibliography, in the text itself Driscoll cites only factual findings from a chapter Reno wrote on the post- Soviet space in a later edited volume,7 rather than Reno's path-breaking and generalizable arguments about warlords and states. This includes, for example, the idea that warlords create the appearance of a functioning state in order to attract foreign aid, which they then proceed to loot (argued by Driscoll on p. 24, but without reference to Reno's 1998 arguments on just this point).

Driscoll does not reference at all the work of one major scholar on the relationship between warlords and the state in Afghanistan, Dipali Mukhopadhyay,8 and mentions another, Antonio Giustozzi,9 only once in passing (15, fn. 28), and then without citing Giustozzi's book-length monograph on the political economy of warlords and the Afghan state. I was dismayed to find that one of his citations to my own book10 misstates my argument. Driscoll insists that I wrongly "define … warlords as always and already distinct from the state" (52, fn. 12), ignoring the fact that my book contains chapter-long case-studies of very complex interrelationships between warlords and the states for whom they are sometimes official employees in four countries, including a detailed section that Driscoll later cites (142, fn. 36) on former Adjara governor and Communist Party leader Aslan Abashidze of Georgia. Driscoll also fails to mention the extensive evidence my chapter on Georgian warlords provides on the relationship between Abashidze—as well as Upper Kodori warlord Emzar Kvitsiani—and the Russian state, and how this relationship figured into the calculations of Georgian presidents Eduard Shevardnadze and Mikheil Saakashvili—arguments that challenge his own book's emphasis on the local agency of warlordism.

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6 William Reno, *Warlord Politics and African States* (Boulder, co: Lynne Rienner, 1998).

7 Reno, "Mafiya Troubles, Warlord Crises," in Mark R. Beissinger and Crawford Young (eds.), *Beyond State Crisis? Post-Colonial Africa and Post-Soviet Eurasia in Comparative Perspective* (Baltimore, md: Johns Hopkins University Press, 2002).

8 Dipali Mukhopadhay, *Warlords, Strongman Governors, and the State in Afghanistan* (New York: Cambridge, 2014). While her book may have been published too recently to be cited by Driscoll, her earlier articles are also directly relevant to his arguments, including "Warlords as Bureaucrats: The Afghan Experience," Carnegie Papers 101 (Washington: Carnegie Endowment for International Peace, August 2009).

9 Antonio Giustozzi, *Empires of Mud: Wars and Warlords in Afghanistan* (New York: Columbia University Press, 2009).

10 Kimberly Marten, *Warlords: Strong-Arm Brokers in Weak States* (Ithaca, NY: Cornell University Press, 2012).

Driscoll also does not situate his broader rational choice arguments within the broader comparative literature. His hypotheses about dynamic coalition formation and the reactive "coup-proofing" by the leaders they install seem to reflect the arguments of Bruce Bueno de Mesquita's selectorate theory.[11] Driscoll does not mention Bueno de Mesquita's work, even though warlords are clearly the "selectorate" in Driscoll's cases—in other words, the influencers whose support is maintained by authoritarian leaders who craftily distribute private goods to prevent a rival coalition from succeeding. Driscoll's arguments about side-switching and alliance-building by warlords closely parallel those made by Fotini Christia in her work on civil war and local power-brokers in Afghanistan and Bosnia-Herzegovina,[12] again a work that Driscoll does not cite even though it won several prizes and was published by Cambridge, his own publisher.

In places Driscoll over-claims what the evidence he provides shows. For example, he argues that what was necessary to overcome warlord dominance in the capitals of both Georgia and Tajikistan was "an exogenous shock" (118). Yet in each case the evidence he provides shows that the real path to state independence from the warlords came from savvy decisions made by the central leadership in the face of warlord mistakes. In Georgia, Shevardnadze used a failed assassination attempt against him to bolster sympathy for his government and to sideline one of the warlords who had originally enthroned him, turning the warlords' own men inside the Interior Ministry against their former patron (118). In Tajikistan, Rakhmonov used the classic tactic of gerrymandered redistricting to hang on in a hotly contested election (120). Nothing was exogenous about either of these shocks; instead they demonstrated that the warlords lacked the resources to outmaneuver sophisticated national leaders who awaited an opportunity to outflank them.

Despite these flaws, Driscoll's unique and major contribution really shines through in the fascinating histories he relates about why people joined warlord militias, how inter-militia violence and bargaining worked, and how Shevardnadze and Rakhmonov managed and eroded warlord power. It is these gems that make this book indispensable reading for anyone who is interested in the histories of post-Soviet Georgia and Tajikistan, and how their trajectories exemplify warlord bargains and battles over state sovereignty.

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11 Bruce Bueno de Mesquita and Alastair Smith, *The Logic of Political Survival* (Cambridge, MA: MIT Press, 2004); Bueno de Mesquita, *The Dictator's Handbook: Why Bad Behavior Is Almost Always Good Politics* (New York: Public Affairs, 2011).

12 Fotini Christia, *Alliance Formation in Civil Wars* (New York: Cambridge University Press, 2013).

Jesse Driscoll
UC San Diego, California
jdriscoll@ucsd.edu

I am honored that my three interlocutors accepted the invitation to this *Central Asian Affairs* roundtable and offered such thoughtful responses to my work. My comments herein are devoted mostly to providing clarifications on points where the four of us may not see eye-to-eye. Kupatadze's essay seems to be addressed directly to me; he asks clarifying questions. The questions in Finel's and Marten's essays seem more rhetorical and more calibrated to an imagined third-party audience. I will respond reciprocally.

My book presents a bottom-up account of the settlement of the post- Soviet wars. For several years I lived in Georgia and Tajikistan, both of which experienced violent state failure in the early 1990s. Both states shared many institutional, geopolitical, and structural characteristics. Both countries' wars came to a relatively quick end despite the fact that none of the fundamental grievances that motivated fighters to take up arms in the first place were resolved. I provide an account in which Georgians and Tajiks, rather than external (Russian) military professionals, were the heroes and villains. I use an analytic model that gives local actors substantial agency—most importantly, the ability to switch sides between incumbent and insurgent (42–43, 154, 160, 163). Finel's contribution to this volume summarizes my argument efficiently and eloquently.

In my theoretical apparatus, the president is installed (or not) by a coalition of warlords. The civilian government, once installed, is an agent of these warlords. But the civilian-operated regulatory and security apparatus of the state also becomes a focal point for international aid efforts ("capacity building") and commercial investment. Warlords are well-positioned to siphon off these rents, and they never disarm. Instead, they retain the ability to coordinate together to punish the president if he misbehaves by instigating a coup. In essence, militia captains literally shaved their beards, put on suits, and became ministers, and then merged the rank-and-file of their memberships with the state security forces. The civilian government, flush with foreign aid and investment, was gradually able to bribe additional warlords into joining the state. These new warlords changed the governing coalition and made divide-and-rule easier.

In this account, warlords are functionally interchangeable: A figurehead president needs many former militia leaders in his coalition to create basic or- der, which is necessary to open the airport and to secure downtown property

so that foreign governments can open embassies, to allow for foreign aid and assistance. He needs *some* of the former militia leaders in the coalition in the long term, so he bribes them with ministry appointments and de facto autonomy over their fiefdoms. But the president does not (usually) *need* any *particular* one of them. Warlords, knowing this, engage in what amounts to a violent game of musical chairs. In an urban setting, where divide-and-rule politics are fluid and a security state saturated with criminals is obviously emergent, no external actor can credibly commit to enforcing liberal outcomes, providing the kinds of security guarantees that militia leaders would have needed to dis- arm their private armies, or even keeping track of the kinds of property rights that are the real stuff most people are actually fighting over (11). A practical observable implication of my theory is that the laws and institutions of the state, including property rights enforcement and import-export-banking laws, would have to be set up to the benefit of warlords in order to prevent a violentpalace coup.

Kupatadze interrogates the fit between some of the model's key assumptions and reality. Were warlords *really* sophisticated enough to gauge the foreign aid flows (v^*) and anticipate that they would be able to access payoffs via extortion down the road? Were Georgian and Tajik warlords *actually* interchangeable, as the simplified model suggests, or would further investigation into variables like professionalism or urban- vs. rural- social bases produce a more textured picture? What about factors outside the ken of rational choice modeling—like blood feuds? And is it *really* appropriate to think of the president as an agent of the warlords, given the overwhelming structural power of the office of the president (especially when the seat was filled with a larger-than-life characterlike Shevardnadze)?

Importantly, it is not only indirect taxes on the aid and foreign investment (v^*) that the warlords expect to acquire if they win in the long term. Foreign money is like icing on the cake; the cake itself is regulatory control over import and export of pretty much *everything* in the polity (the parameter in the model that I call v) (128–129). Whether or not they anticipated that indirect taxes on the salaries foreign embassy workers, consultants, and NGO workers would come to provide state liquidity in a manner analogous to the budget subsidies from the Soviet center is irrelevant. In the most violent stages of the war, which I describe in the third and fourth chapter of my book, the *kto kogo* ("who gets what"), question referred to the division of casinos, banks, car bazaars, aluminum factories, lucrative drug routes (153–157, 165–166), resorts (179), and downtown real estate (116). In my account, foreign aid has perverse effects, pro- vided a (slightly) larger set of spoils to fight over (181). It is valuable to consider

the possibility that foreign aid programs are divisible spoils that accrue to the winners of an extremely violent urban tournament, and the formal theoretical exercise was meant to make this point explicit.13

Could all this have been anticipated ex ante by the warlords, though? How savvy were the warlords about international affairs? Based on my interactions with them, the most prominent Georgian and Tajik warlords were extremely sophisticated about geopolitics and the political economy of aid. Tengiz Kitovani and Jabba Ioseliani both understood that one disadvantage of bringing back Shevardnadze was that he was a peerless coalition-manipulator, likely to run the table on them. They took a calculated gamble anyway, inviting him back because of his unique ability to attract foreign aid and investment (88). Rural Kulobi Tajiks understood that if their country ended up aligned with Iran or Afghanistan, Tajikistan would become isolated from the global economy (77). Were warlords really interchangeable? In the moment, and from the viewpoint of recruits deciding to join up, no—there's only one Shah Massoud, only one Tengiz Kitovani. Statistically speaking, however, the answer is less clear (159–166, 213–218). I would not want to have had the job of sorting the "good apples" from the sociopaths in either the Georgian or Tajik Ministry of the Interior in the mid-1990s (190). A variety of anecdotal evidence scattered through the book suggests to me that militia members sometimes followed their commanders because of, not in spite of, their lack of professionalism. And while I do not think liberal humanitarians should discontinue providing assistance
(175–176), exporting military and police professionalism is hard.

What about blood ties? Some, but not all, *avlod* and ethnic bonds predict longevity for warlords in the Tajik state. I probably do not devote enough attention to the specifics of either blood feuds or cross-clan predatory coalitions. I could not document these dynamics without also documenting specific kinship geologies, and doing so would violate the letter and spirit of the anonymity promises that anchored my relationship with my human subjects (20–23, 219).14

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13 Explicit, that is, to the relatively small sub-community of political scientists who happen to find mathematical models aesthetically attractive or intrinsically valuable. See Robert Powell, "The Modeling Enterprise and Security Studies," *International Security*, 24, no. 2 (Fall 1999): 97–106. Reprinted in *Rational Choice and Security Studies: Stephen Walt and His Critics*, an *International Security* Reader, ed. Michael E. Brown, Owen R. Cote, Jr., Sean M. Lynn-Jones, and Steven E. Miller (Cambridge, MA: The MIT Press, 2000), 86–95.

14 Without getting into specifics, I can affirm what every anthropology undergraduate al- ready knows about blood feuds (108): A murder, rape, or vicious maiming can practically ensure retaliation from a family member. If these norms are functioning, taking a retaliation-provoking action anyway to intentionally escalate matters and to demonstrate a

Kupatadze also asks whether the president is really an agent of the warlords (44). I say yes. Warlords can, and in both Tajikistan and Georgia did, force the head of state to resign at gunpoint (69, 75, 80, 121–123). In Chapter 5 I explain how Rakhmon and Shevardnadze used a weak set of incentives to *construct* the power that Kupatadze attributes to them. Hobbes would have called this power "awe." I show how Shevardnadze and Rakhmon used different tactics, but similar strategies, to make themselves indispensable. Both men rehabilitated certain Soviet institutions and placed themselves at the center of a very opaque favor-based economy. The basic problem from the point of view of warlords is that they would have had to work together to replace the president, but he could distribute ministry positions ("divide the pie") to split the warlord coalition, selectively purge warlords one at a time, and assemble different overlapping support coalitions, to minimize (but never fully remove) the threat of a coup (129–131, 210). Shevardnadze excelled with these tactics. I agree that Shevardnadze never seems like much of a puppet, especially not compared to Rakhmonov in his early years in office. But he too learned how to divide and rule the warlord coalitions. By 1996—many years before Emomali Rakhmonov transitioned to "Rakhmon"—even the most brazen kinds of extortion did not include demands for him to step down from power (155). In both cases, executive power is Hobbesian, based on recognition that the alternative to personalist rule is anarchy, and that the presidents are placeholders (agent) for violence specialists behind the throne.

With that said, it would be completely valid to contemplate the reverse and set up a model in which the head of state is the principal and the warlords are the agents. After all, is it not somewhat plausible that shadowy political actors in Washington dc or Moscow ought to be considered the terminal principal, and various warlords who reside in the developing world their agents?15 For some warlords, this may raise a problem of multiple potential principals— perhaps a principal in Tbilisi and a principal abroad (creating dynamics that are analogous, in some ways, to the role that would be played by a very high reservation value (*r*) in my model (39, 178, 210–211)). A theoretical framework

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group's serious intent and ability to absorb the retaliating punishment, may, actually, be the point. None of this is captured in the model, of course, which assumes fully rational actors responding to material incentives.

15 See Kimberly Marten, *Warlords: Strong-Arm Brokers in Weak States* (Ithaca: Cornell University Press, 2012), 30. I enjoyed this text and referred to it often: See 43, fn 11, 52, fn12, 178, fn 7. Based on my reading of Marten's *Enforcing the Peace*, I suspect that the two us are dedicated to subtly different intellectual projects that reside in different quadrants of Figure 1.1 of my book (3). If I am correct about this, then a degree of cordial aesthetic disagreement is healthy and inevitable.

in which the great powers are principals and warlords are agents illuminates certain facets of political life in poorly governed parts of the world that are sometimes uncomfortable. But other dynamics are obscured. The point is not that one model is right and the other is wrong—the point is simply that all social scientific observation is theory laden (2, fn2). There is considerable potential complementarity in considering politics in weak states from both points of view. If warlords are bad agents, it is puzzling why states do not disarm war- lords more often. If warlords are principals and presidents are agents, it is puzzling how post-conflict states develop laws and institutions that are *not* riggedto the warlords' benefit.

I genuinely appreciate that Marten used this roundtable to position my book in the political science canon. It is true that my bibliography skews toward the monographs of scholars who dedicated significant portions of their lives getting the facts right in Georgia and Tajikistan. I could never have written what I did if not for the virtuous labor of area experts like Kathleen Collins, Shirin Akiner, Stina Torjensen, Monica Whitlock, Kirill Nourzhanov, Georgi Derluguian, Anna Matveeva, Larry Markowitz, Steven Jones, Pauline Jones- Luong, Sergei Gretsky, David Darchiashvili, Olivier Roy, Stephane Dudoignon, and Muriel Atkin. I am of course indebted to Venkatesh and de Mesquita, and could have done more to acknowledge them (and I hope to improve my index in future reprinting).16 Ultimately I hope that this is a book will be valued by an audience of area specialists—and, if I am lucky, citizens of Central Asian states. It would be a high honor indeed to be remembered by future generations of Tajiks and Georgians as "a true anthropologist of warlordism."

The disadvantage, as Finel wryly points out in the conclusion of his generous essay, is that an argument calibrated to an area studies audience may be locally valid but not travel very well beyond the post-Soviet space. I agree. As Kupatadze points out, there really is something about the weight of pre-Soviet history and Soviet institution-building that stacks the deck in favor of centralized executive power. These states were "born strong" in many important respects (4, 23, 125, especially fn4). The fact that the wars of the 1990s took place within Russia's uncontested sphere of influence meant that proxy-war dynamics were muted (8–10). How is this relevant to the contemporary Middle East, for example, where proxy-war dynamics are pretty much the entire story? One answer, which I hint at in the conclusion (178) and in the "coda for *v**" subsection (166–172), is that *partial incorporation* may be the new normal in some places. Georgia is probably not the last place we will see *de facto* secessionist states that survive for decades without *de jure* recognition. When the great

16 As for Reno's *Warlord Politics and African States*, see page 173–174, footnote 3.

powers, especially the P-5 of the United Nations Security Council, agree on the desired outcome, like in Tajikistan, *full incorporation* is easier to engineer. But the process of getting to peace is not pretty.

I am afraid that there is also a somewhat discouraging bottom-line for area specialists who seek validation for their labor in the form of policy influence. The Russian experience in both Georgia and Tajikistan suggests, at least to me, is that attempts to pick sides an ongoing war of attrition, when coalitions are changing all the time, can easily backfire (18 fn31, 194–196). This can be the case even if the intervener has not only staying power but also the kind of extremely good humint that you can only acquire over a century of imperial rule. Commonwealth of Independent States interveners spoke the languages, knew the relevant local history, and, in general, were well-equipped to distill something actionable from the vapor of cultural nuance. But in the end all of this just made many of them really cynical (83–84, 194). Sometimes the best of many bad options for a retreating empire may be to just sit on the sidelines and let ugly local politics play out. After all, as Fanon observed, decolonization is a violent psychological process.17 This does not mean that providing public goods to civilians in war zones is necessarily doomed to fail; indeed, it is sometimes hard to see what else the intervener ought to be doing. But based on my research, Russian military professionals resigned themselves early to the sobering conclusion that they would be the villains in the story told by future nationalist historians pretty much no matter what they did, no matter which coalition emerged in possession of the symbols of state power in Tbilisi and Dushanbe. As United States policymakers consider humanitarian interventions in complex civil wars in places like Syria, Iraq, and Yemen, it would beprudent to keep Russia's experience in mind.

17 Franz Fanon, *The Wretched of the Earth*, trans. Constance Farrington (New York: Grove Press. 1963).