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Author-critic forum: popular protest and regime change in Central Asia

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Author–critic forum: popular protest and regime change in Central Asia

Weapons of the wealthy: predatory regimes and elite-led protests in Central Asia, by Scott Radnitz, Ithaca, New York, Cornell University Press, 2010, 231pp., US\$49.95 (hardback), US\$24.95 (paperback), ISBN 9780801449536

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Introduction: Nick Megoran

The ‘Author–Critic Forum’ consists of a standard review of a new book plus a number of shorter appraisals of it, and finally a response by the author to all these contributions. The choice of book is agreed upon by the editorship of the journal, the Editorial Board and the International Advisory Board. Books are selected because they engage pressing and contested theoretical and/or empirical issues within the broad field of Central Asian Studies. Explanations of regime formation and change in Central Asia are one of the most debated areas of political-science research into the region, and we thus considered it timely to bring an experienced and varied group of scholars together to consider Scott Radnitz’s important contribution to this discussion. The purpose of the format is to provide a lively forum that will acquaint the readership of the journal with the range of arguments, debates and issues within a particular field. The Book Review Editor invites rejoinders to the debate begun by this forum.

Review: John Heathershaw

Kyrgyzstan’s Tulip Revolution is one of the most important moments of post-Soviet Central Asian history. Many of us privileged to witness at first hand the events of March 2005, which set in motion a period of political crisis that is yet to be definitively concluded, found that it changed our view of the politics of the region. Most importantly, the Tulip Revolution was an exceptional event which required that our theories and preconceptions about what is and isn’t possible in Central Asia be reassessed. Many early analyses which attributed this act of regime change to a Western-supported and broadly liberal civil society, and those that saw it as the final module in a course of coloured revolutions, have been shown to be faulty. Building on his extensive doctoral research, Scott Radnitz’s *Weapons of the wealthy* is the book on the Tulip Revolution which has reset the debate on how it might be explained.

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Radnitz's book is in fact much more than a deep analysis of the Tulip Revolution. It contributes to both theory development and methodology for the study of mass unrest. The book considers two 'successful' and one 'unsuccessful' case of revolt against the state: Kyrgyzstan's Aksy rebellion of 2002, the Tulip Revolution of 2005 and the fateful Andijon uprising in Uzbekistan later that year. The first half of the book develops the literature on contentious politics towards a theory of elite-led protests in terms of the author's new concept of subversive clientelism. Radnitz argues that hybrid regimes which have undergone partial economic and political liberalization provide environments where non-state elites (patrons) can build independent fortunes and use these to develop their local political bases (clients) through their agents or 'brokers' (vertical ties) and their place in national politics (horizontal ties). These clientelistic ties can be used by opposition elites to compete with the patrimonial power of the state. Local politics in such circumstances is not about competition between different agendas and interest groups, or about 'bottom up' self organization, but about 'top-down' collective action. Thus, national rebellion takes the form of multiple local revolts which combine into a fairly incoherent and emergent revolution only *if* horizontal ties are present between patrons.

Radnitz applies this approach to contemporary Kyrgyzstan and, secondarily, some comparable cases. 'In the final analysis,' he notes, 'regime change was inadvertent – a result of tentative adaptations to political and economic insecurity and hastily improvised responses to the regime's attempt to steal an election' (6). This is as good a one-sentence summary of the elite politics of regime change in Kyrgyzstan as one will find. The book then goes on to demonstrate the importance of elite leadership, as opposed to local grievances, which were widespread, in explaining why certain communities rebelled and others did not in Aksy in 2002 (chapter 5) and across Kyrgyzstan in 2005 (chapter 6). Finally, in chapter 7, Radnitz considers some comparable and contrasting cases including Andijon, rural China, early-modern England and Mexico's early nineteenth-century rebellion to test the author's claims that what matters is the extent of both horizontal and vertical ties. Where one of these ties of clientelism is absent rebellion will remain stillborn (Andijon) or localized (Mexico, China).

There are at least three areas where Radnitz makes agenda-setting contributions to the study of protests in Central Asia and further afield. Firstly, the relationship he establishes between protests and economic transition allows him to make telling distinctions between the political trajectories of Central Asian states. The author argues that weak formal institutions, economic opportunities for non-state actors and a deficit of public goods in society (5, 64) – in short, rapid post-Soviet market 'reform' – generates inter-elite conflict and instability. As Kyrgyzstan was the most advanced case of such reform, where the regime did not quickly consolidate central control, it is to be expected that it would suffer the most instability. In short, it is not simply the inequalities created by reform which generate conflict, as a relative-deprivation or moral-economy approach might argue, but the institutional environment which it creates.

Secondly, *Weapons of the wealthy* offers an original conceptualization of this institutional environment: subversive clientelism, the capacity of opposition or peripheral elites to mobilise citizens against state elites (21). The two key elements of subversive clientelism are the development of a local client base and the trigger of new attempts to exclude and control by a weakened regime. The implication of this argument is, crudely put, that presidents Putin and Karimov may have a point about democratization, instability and the need for 'vertical power': this book would certainly be well received in most capitals of the region. The other side of Radnitz's argument is a theory of hegemonic authoritarianism under limited market reform. This is a more convincing theory of the nature of authoritarianism than that offered by most studies of 'hybrid regimes' and is set to make a significant contribution to the literature beyond Central Asia.

Thirdly, Radnitz provides an excellent example of how political ethnography (as an orientation, if not a structured method) can be deployed to provide better answers to some political puzzles than those offered by more orthodox methodologies. This is a mixed-methods study involving almost two years in the field, which combines a large survey of protestors with observation of protests and semi-structured interviewing. Radnitz encouraged participants to describe what they did, as opposed to their opinions and analyses. It is clear that it is the ethnographic examples of practice that provide the evidential base upon which the argument rests. Along with Schatz's recent and excellent edited 2009 book, *Political ethnography*, Radnitz's methodological appendix will be helpful to doctoral students embarking on similar research. Whilst Radnitz's generalizations about 'communities' acting in unison according to the direction of a patron may be distasteful to anthropologists, for Political Science the adoption of ethnography represents a giant leap forward, and it is one where Central Asianists are at the forefront.

However, and inevitably, there are weaknesses to Radnitz's book which will be engaged critically and in more depth by the other contributors to the forum. Firstly, in superseding the coloured-revolutions literature, Radnitz disregards the singular contribution which it made: to raise the question of cross-case influence and the 'international diffusion' of mass unrest. Given that we now see another spate of rebellion by emulation – this time across the Arab World – this is unfortunate. Whilst it is clear that there is not a simple model being transferred across all cases, it is nevertheless the case that precedent does matter in the realm of symbolic politics. It provides inspiration if not example. However, political scientists have yet to come up with adequate explanations for this phenomenon.

Secondly, and more importantly, Radnitz's claim that protesting communities are entirely subservient to a particular patron divests them of their agency and obscures local politics. *Weapons of the wealthy* argues that grievance was widespread in Kyrgyzstan in 2005 – present amongst those that rebelled and those that did not – and can thus be disregarded as an explanation of why protests endure in one place and not another. It also suggests that clients simply followed their patrons without any bargaining or extraction of concessions. In one telling paragraph in the methodological appendix (220), Radnitz acknowledges that he sees reference to 'justice' and 'democracy' as a subjective rhetoric which did not represent genuine motivation or causation. However, doctoral research conducted after 2005, including by Elmira Satybaldieva (2010) and Aksana Ismailbekova (2011), seems to suggest that these relations have changed. In short, grievances are important and are a part of social mobilization against both local and national elites, including one's own.

Finally, there are major and perhaps unavoidable limitations on the author's study of the case of Andijon. Radnitz claims that the 'bottom-up' and 'spontaneous' nature of the protests were their weaknesses as, contrary to the Government of Uzbekistan's claims, the rebellion lacked vertical ties to political patrons. 'There was no credible evidence,' he asserts, that the businessmen behind Akramiya 'had political interests' (172). This is a contentious claim that needs more evidential support than can be provided in a four-page case study (172–176). Surely the point is not that the businessmen did not have political interests and ties, but that these were largely confined to the *local* rather than the *national* scale. Given that Radnitz spent six months of his fieldwork in Uzbekistan, and that Andijon is his primary contrast case, this is disappointing – but most likely reflects the difficulty of conducting credible academic research in Andijon after the uprising.

Nevertheless, this is an outstanding book. Radnitz has pushed against the boundaries of current Political Science by adopting an ethnographic sensibility in his research, however shackled he ultimately is by the elitist and materialist preconceptions of his discipline. As an account of why Kyrgyzstan's 'Tulip Revolution' was successful, *Weapons of the wealthy* is

unsurpassed. As a theory of post-Soviet protests which is neglectful of the complexity of localism and the significance of transnational symbolic politics, it is not wholly convincing.

Commentary 1: Asel Doolotkeldieva

Weapons of the wealthy offers a new explanation of the phenomenon of coloured revolutions, which is more consistent with the clientelistic nature of Central Asian politics than the earlier liberal civil-society thesis. His concept of ‘subversive clientelism’ helps us to understand how opposition elites manipulated communities, that is, their regional bases, to topple the Askar Akaev regime in 2005. Moreover, his analysis of vertical and horizontal networks alludes to an important theme of regional politics and thus brings about, though unintentionally, a crucial but rarely explored phenomenon of localism in Central Asian Studies. This focus on regionalism is unintentional as it is dictated by the author’s desire to explain merely elites’ rationales rather than regional politics *per se*. This results from Radnitz’s problematic conceptualization of ‘communities’ as simply structures, and not as agents in their own right. This commentary will explore the shortcomings of the book’s account of structure and agency, and its subsequent omission of important bottom-up movements as an alternative explanation of social unrest. It is based on research into rural social mobilization carried out in the framework of a PhD thesis during 2010–12.

The conceptualization of communities as homogenous blocks solely dependent upon the charitable sponsorship of individual politicians is an over-simplification if not an under-estimation of politics in Central Asia and elsewhere. Put in this perspective, Radnitz’s thesis of ‘subversive clientelism’ is no different from earlier readings of agency through the clan/tribe lens. The failure to capture communities as sites of intense local dynamics doesn’t help us to understand that Kyrgyz unrest, whether it is ‘successful’ in toppling the regime or not, is above all about social conflicts. These conflicts have emerged as reactions to issues of bad governance. Access to land, water and farming; stolen elections; low wages; and the exploitation of extraction sites are a few examples of the causes of recurrent protests. These issues constitute core motivations for social mobilizations across the country. Local movements might appear as lacking any ideology, but in fact local activists frequently have sophisticated political understandings of their own participation. Their ideational incentives are not always visible to scholars who only look in their discourses and actions for clichéd ‘traditional’ patterns of allegiance.

As elsewhere, social conflicts in Central Asia are thus both about a broken social contract and the private interests of elites. In fact, ignorance of local politics, which are shaped by complex urban–rural and centre–periphery dynamics and bad governance, leads to false and abstract appreciations or an over-estimation of the merits of elites. For a better account of unrest, one should therefore combine elite greed and popular grievances theories. Yet existence of grievances on its own does not explain why certain areas are more prone to unrest than others. Radnitz claims that this can be accounted for by the extent of horizontal and vertical ties between elites and communities. Undoubtedly there are links between elites and constituencies: it would be strange if there weren’t. But the presence of ties (be they subversive or not) is not a unique variable responsible for protest occurrence. If we are to consider elite-led mobilizations, my argument is that by co-opting elites, local politicians and activists pursue interests and strategies on their own. Communities are not simple resources that are serviceable to elites; they are heterogeneous and provide a highly contingent base for collective action. Moreover, competition for scarce resources makes people opt for what can at first appearance be considered as highly unpredictable and unstable allegiances to patrons. However, under closer scrutiny, what appear to be unpredictable allegiances look more explicable: people learn to navigate between individual patrons and various political factions, based on their diversified loyalty and the desire to

maximize their interests. Competition for scarce resources has made communities extremely entrepreneurial. These dynamics constitute local politics and dictate the nature of ‘contracts’ passing between local activists and central elites. Appreciation of the fact that local activists are agents and not mere puppets is the key to understanding the complexities of social mobilizations. In this light, we see that elite rationales become less important for the analysis of regional and local unrest.

Since resources appear in abundance specifically during elections, as each candidate or party seeks to gain communities’ support via financial feeding, elections represent a particularly critical focal point of instability for incumbent regimes. This is not only because independent elites are more likely to converge in efforts to bring down a weak incumbent regime, as Hale observes in his helpful explanation of coloured revolutions as regime cycles (Hale 2005). It is also because local politicians and middlemen are more inclined to take greater risks for the chance to tap the rich opportunities presented by capturing power. During elections, individual opportunities are up for grabs and opposition elites are more likely to mobilize large communities in their support. Finally, because elections are a focal point of open opportunities in an otherwise resource-scarce environment, even inadvertent actions by elites or failure to recognize mutual interests make the chances of regime change pretty high.

Radnitz’s book is helpful in understanding that a ‘successful’ regime overthrow is not only about the regime’s weakness but is also about the relationships of elites to constituencies. It’s a skilful analysis of the patronage relations existing in Kyrgyz society. But a further nuanced analysis is needed to investigate the nature of these patronage relations.

Commentary 2: Madeleine Reeves

Radnitz has crafted an elegant analysis of elite-led protest in Kyrgyzstan, drawing on detailed empirical research over several years. Students new to the region will learn much from the forensic analysis of two significant moments of political mobilization in Kyrgyzstan: the Aksy events of 2002 and the ‘Tulip Revolution’, which saw President Akaev flee the country in 2005. For specialists in comparative politics the book offers a detailed and theoretically sophisticated discussion of the conditions under which ‘subversive clientelism’ can arise in hybrid regimes. And the analysis of the reasons why elites were able to mobilise communities to subvert the state in Kyrgyzstan, but not in Uzbekistan, is instructive for those interested in comparing the divergent dynamics of post-Soviet ‘transitions’.

I approach Radnitz’s book as a student of Kyrgyzstan and an anthropologist of politics. From this perspective three contributions of the book are particularly worth highlighting. The first is that, through empirical analysis of the dynamics of mobilization at a village level, Radnitz reveals the limitations of explanatory models grounded in terms of either ‘clan politics’ or the mobilizing capacity of ‘North versus South’. It is not that these logics of identification are irrelevant, Radnitz argues, but they do little to explain why, at times of political polarization, some people go out to support a given elite patron and others do not. ‘Clan’ or ‘locality’ alone are insufficient to explain political loyalty or political mobilization: what matters are the political economies of patronage. Secondly, Radnitz’s analysis cautions against a celebratory assumption that bottom-up political change can act as a panacea for the deficiencies of post-Communist rule. Mass mobilization may successfully chase away a corrupt president if there is a large enough cohort of elites with little to lose from that president’s departure, but this is far from a guarantee of a democratic future or a solution to years of structural inequalities. Thirdly, and following from this, Radnitz highlights what he calls the political economy of political contention. Without substantive policies to address the grievances of the poor, neither formal elections

nor political mobilization in the shape of street protests and demonstrations are necessarily going to result in democratic outcomes.

Radnitz is engaged in theory building through comparative analysis: centrally, in this case, why elites were successfully able to mobilize outside (and against) the state in Kyrgyzstan, whereas they weren't in Uzbekistan. Such a project necessarily entails developing rather schematic distinctions for the purpose of analytical clarity. But one consequence is that complex differences in social organization, historical legacies or situated political imaginaries concerning the characteristics of 'right rule' are denied explanatory weight. Thus, Radnitz argues, Kyrgyzstan and Uzbekistan 'were culturally similar, shared a common legacy of Russian and Soviet rule, and began the 1990s with comparable prospects' (167). Much recent historical and anthropological work on these two countries suggests a rather more complex story: one that situates patronage relationships within enduring commitments of kinship and loyalty and alternative local practices of representation (Petric 2002; Ismailbekova 2011); which highlights the local autonomy of situated models of political order, quite different in Uzbekistan and Kyrgyzstan (Liu 2005; Rasanayagam 2002); and which shows how state authority is 'customized' in rural Central Asia (Beyer 2007). Engaging such perspectives would allow for a more complex portrait of local political life and political motivation. As it is, the 'subversive clientelism' that Radnitz identifies is explained ultimately in terms of economic rationality: for rural communities, having a protective patron is an economic benefit in contexts of failing state provision, whilst for elites positioned outside the state, the cultivation of relations with a local community represents an 'investment portfolio' that can bring big returns at election time.

A second significant consequence of a comparative model organized in terms of elite–community mobilization is that the 'state' remains something of a spectral presence, categorically apart from 'society' but not really theorised in its own terms. In Radnitz's schematic representation of the mass-mobilization infrastructure the state floats above the elite networks (but disconnected from them), which in turn are connected to, but spatially above communities. This topography of power structures Radnitz's whole narrative: just as there is a 'below' (the community) and an 'above' (elites); so there is an unambiguous divide between those people and institutions that are 'inside' the state, and those that are 'outside'. But the situation in rural Kyrgyzstan (as elsewhere) is rarely so clear. Is a local businessman inside or outside the state? Is a mosque? Is an elder (who in Kyrgyzstan might well be receiving a state salary as an employee of an *aksakal* court?) As ethnographic work has shown, what is important is often who comes to take on the person of the state (which may include those very 'autonomous elites' whom Radnitz characterises as outside), and can they successfully maintain a claim to its authority?

One consequence of Radnitz's framing is that whilst the differences in models of mobilization between Kyrgyzstan and Uzbekistan are explored in illuminating detail, differences in the form, organization and capacity of these two states – or of how 'statehood' itself is imagined and conceived, both locally and in halls of government power – receives relatively little discussion. And yet this difference, by the early 2000s, is critical for understanding the dynamics of political mobilization in Kyrgyzstan: in 2002 parliamentary deputy Azimbek Beknazarov was able to mobilize considerable support beyond his immediate community in part because he was successfully able to present Akaev as having undermined the state's territorial integrity. State capacity in Kyrgyzstan had been dramatically undermined by 2005 in part because of a configuration of international dependencies shaped by the 'war on terror'. And in Uzbekistan Islom Karimov was able to respond to the Andijan uprising with overwhelming force by pointing to the 'chaos' across the border and framing the uprising as fed by 'Islamic extremism'. The instances of mobilization that Radnitz describes, in other words, need to be situated not just within the internal logics of elite-community dependencies, but within what David Lewis

(2008, 236) has called the ‘gepoliticisation of democracy’ in the West’s relations with Central Asia: an external context that is referenced only fleetingly in the text.

Weapons of the wealthy is an important and timely contribution to the comparative politics of hybrid regimes. It is a vivid illustration of the ways that empirical fieldwork can be used to inform ‘broad brush’ theoretical arguments. But to this reader, at least, the story that Radnitz tells about the dynamics of political mobilization in Central Asia would be strengthened by engaging approaches and theoretical perspectives that have questioned the topography of power on which his analysis rests.

Commentary 3: Sally N. Cummings

Scott Radnitz’s 2006 article ‘What *Really Happened* in Kyrgyzstan?’ has become an important interpretation of the so-called ‘Tulip Revolution’ of 2005. His analysis then was already ‘more intricate than a simple narrative based on “people power”’ (16). This book places that event and others in a longer trajectory, particularly of Kyrgyz and Uzbek political transformation. It reinforces how early liberalization in the Kyrgyz Republic created a different regime to that of its Central Asian counterparts, and also advances the argument that a set of elites who were autonomous from the governing elites was created. Where other Central Asian states saw a continued inter-relationship, if not overlap of economic and political interests, the Kyrgyz case saw an emergent fragmentation of political and economic power and an accompanying autonomous elite. This autonomous elite was able to engage in its own patron–clientelist relations, leading to what Radnitz terms ‘subversive clientelism’. And the book looks deftly at the implications of this bifurcation for power maintenance and regime longevity, arguing that: ‘[T]he trigger that brings about mass mobilization is set off when the regime challenges embedded elites’ (33). He also astutely observes that in this framework for mobilization to work ‘elites must occupy a middle position between power and powerlessness’ (34).

I do not question the central premise that early liberalization led to a path dependency of regime structure. I think this is a critical point for explaining the Kyrgyz Republic’s future trajectory. I also commend Scott Radnitz for the clarity with which he has conveyed his argument and the manner in which he has combined political theory with empirical evidence. The three main suggestions outlined hereafter concern instead how the argument’s complexity might be strengthened, possibly in a follow-up piece.

First, the book suggests an uncomplicated linear mobilization process that denies the actors, perhaps paradoxically, of some of their agency. Even if people have certain incentives for mobilization, such as material reward, these may not in and of themselves be sufficient. The argument discounts that individuals or communities were driven by loftier goals, civil-society activism or simple frustration and desperation of their own lives, or a combination of the material and ideal. While it is hard to call the Tulip Revolution a revolution with democratic intent, it is also not possible to discount, if not *the* popular will, then *some* popular will – for whatever end – entirely. For example, support for deputy Azimbek Beknazarov in 2002 was in good part due to popular support for him as a person and the sense that his imprisonment was unjust. These emotions might well have been expressed by people irrespective of whether they were tied vertically to an embedded elite member who had the resources to mobilize. They also convey the possibility that different triggers in the Kyrgyz case may have been responsible for mobilizations in 2002, 2005 and – although outside the scope of this book – 2010.

This first point might be partly addressed by including more of the material from the author’s unstructured interviewing; it might give the reader some insight into how individuals framed their motivations. Inclusion of some direct citations might have been helpful here, for example.

Second, while attractive in its parsimony (like all models), the model depicted (for example, on 32) of the relationship between 'state', 'independent elites' and 'communities' seems unable to differentiate between varying organizing principles. The mass-mobilization infrastructure depicts horizontal and vertical elite networks that work together to encourage mass mobilization but it is unclear how these mechanisms work in practice. While Radnitz rightly argues for a shift in the debate in Central Asia away from clans and clan politics, communal belonging as a factor behind grouping, allegiance and mobilization cannot be entirely discounted. The account suggests strong fluidity between networks. Thus, for example, he says on the 2002 Aksy events 'tribal affiliation had no discernable impact in social, economic, or political life. Although people were aware of their tribal identity, their affiliation did not prevent all manner of cross-tribal interaction, from business to marriage' (110). Although Kyrgyz clans may be more fluid in their membership patterns than those encountered elsewhere in the world and have undergone significant transformation under modernization, they remain a source of genealogical, reputational and symbolic identities. Clientelism is indeed different from clan allegiance but it would be interesting to explore how these two types of social networks reinforce or compete with each other.

Third, more detail on what the author understands by 'the state' would be helpful. The book suggests that it is equated with the governing regime but this is not always clear and such equation needs justification. While the state is failing its citizens by not providing basic services, simultaneously it provides the framework against and within which the embedded and autonomous elites act. These elites require the state and the state requires the elite. The state trundles on. Discussion of the state would also have brought in to a greater degree the role of the international community in its transformation, particularly the detail of the decentralization, privatization and price-liberalization programmes promoted by the Washington consensus in the early 1990s.

These concerns notwithstanding, *Weapons of the wealthy* is a welcome addition to the literature on Central Asia and on revolutions more widely. In closing it, Radnitz's words (198) ring true: 'The autocrat's dilemma is that good governance, which facilitates economic growth and increases his legitimacy, can also hasten his downfall if opponents of authoritarianism are empowered in the process.'

Author response

I thank the contributors to this forum for their thorough reading of my book and their thoughtful comments. The field of Central Asian Studies has grown significantly since I first arrived in the region in 2002. At the time, young scholars struggled with a paucity of high-quality research to rely on; much of what guided us stemmed from assumptions imported from other regions and applied uncritically to Central Asia. In the past decade, however, scholars, including all of the contributors to this forum, have made major advances in elucidating some of the critical processes taking place in Central Asia and investigated them with depth and sophistication, paving the way for others.

I began with this preface in order to situate my own work. A decade ago – and arguably today as well – most Political Science dealing with the post-Soviet world looked at the region through the master frame of democratization. As the contributors would probably agree, this was not a helpful approach to guide inquiry into Central Asian politics. So it was with the coloured revolutions, which, following several years of disappointment with post-Soviet political development, were reflexively viewed in the West as democratic breakthroughs.

It was my intention in writing *Weapons of the wealthy* to interrogate this narrative by examining one of these events in all its messy complexity. My jumping-off point was regime change,

but my charge was to dig beneath the surface to reveal the social and political structures, processes and behaviours that underlay it.

Thus, while not pre-determining where my inquiry would lead, I trained my sights on answering the question driving my research: What caused these mass-mobilization events? My narrow focus on this outcome – what political scientists call the dependent variable – necessarily diverted my attention away from highly interesting, but for my immediate purposes, extraneous, issues. In developing an explanation and telling a theoretically informed story, I deliberately reduced the complexity of the social world, a trade-off that enabled me to explain obscure events with parsimony and (I hope) clarity.

At the risk of being evasive, I believe some of the commentary in this forum is the dialogue (and disagreement) that inevitably occurs between disciplines – in particular Anthropology and Political Science. Unlike many political scientists, I look beyond elites and capital cities and I see ethnography as an essential methodology for making sense of the world from ordinary people's point of view. But I ask questions with an eye toward comparative politics, which inevitably leads me down different paths than from that of an anthropologist.

As such, I do not disagree with the sentiments behind many of the critiques, but I defend the choices I made for the book. For example, the commenters saw my characterization of elite–mass ties, which I argue were the primary vehicles of mobilization, as rigid and one-dimensional. It is a fair point that I view the role of patrons as important and I highlight material redistribution. However, I do not claim that elites always get their way, that people follow them automatically, or that economic interests always trump others. In chapter 2, I describe the multiple and intricate ways that people seek to mitigate everyday problems without turning to elites. The table on p. 85 illustrates the non-material sorts of appeals that elites make. I also note that many people reject the appeals of patrons, whether because they see the rich as undeserving or discern the insincerity of election-time largesse (88–89). But in the theory and case-study chapters, I highlight only those factors that explain how clientelism produces mobilization because that outcome is what animates my narrative.

This leads to the question of agency, another common point of critique. In my interviews, I found that people, especially community leaders, were highly sophisticated in their understanding of politics, power and corruption. Some of them – at least in interviews months after the event – articulated a desire for justice or democracy, and described their protests as spontaneous. I take their words at face value. Yet even while granting people's complex motivations, grievances against the regime, yearning for justice, and ambivalent attitudes toward power, the fact remains that some people participated in protests and (many more) others did not. What is more, the protests were spatially concentrated. As such, on the narrow question of why the Tulip Revolution or Aksy protests occurred, none of those sentiments is sufficient to explain this striking variation. Something else must have distinguished participants from non-participants. My argument, which encompasses the initial push from elites and the activation of ties within communities, is a way to explain this. Those who protested may indeed have gone out for justice, however they understood it – and it is certainly in short supply in Kyrgyzstan – but my methodology allowed me to trace exactly how those people arrived at their regions' central squares, while the majority of their compatriots who sought the same goals decided to pursue them in other ways.

I entered the field with a number of hypotheses about what collectives and social identities enabled protest, among them religion, clan and tribe. I found none of them to be a factor either in the connections among people who joined, nor in their articulation of why they protested. I found communities to be a useful concept, even though, like clientelism, it necessarily simplifies reality. I treat communities as structures rather than actors because inherent within them are the social connections, mutual understandings, and trust that enable collective action. I do not

depict them as homogeneous; indeed, their role in facilitating collective action is highly contingent. Chapters five and six show how networks *within* communities were or were not activated for protest.

Several contributors make excellent points about the ‘state’ in my diagram (32). I agree that the boundary between state and society is blurred and ambiguous in Kyrgyzstan, as across much of post-Soviet space. Throughout the text, I try to distinguish between the Akaev *regime* and the state that it nominally controlled. The elites that figure into my narrative are, for the most part, not part of the regime (even though some of them, such as parliamentary incumbents, are certainly part of the state). The critical point in the diagrams is that the state is ineffective as a service provider, having severed the social contract that existed in Soviet times, and is in effect disembedded from society in ways that would be recognizable in many parts of the world. The specific placement of the state in the diagram, ‘hovering above’ the rest, is arbitrary, but to my mind worked well for aesthetic and conceptual reasons.

Finally, I agree with some that I downplayed international influences in my explanation. Partly, this comes down once again to my intention to explain variation in mobilization processes. The mechanisms leading from regime challenges to disgruntled elites to protesters were sufficiently accounted for by domestic political contestation, rendering international factors were superfluous for theoretical and explanatory purposes. This is not to gainsay the potential effects of ‘war on terror’ on regime responses or the previous ‘revolutions’ on the form or character of protest. As I have written elsewhere, in later stages of the revolution in 2005, once protests had unified and reached Bishkek, the protagonists belatedly adopted the coloured-revolution template, including slogans, signs and tactics. With the recent example of the ‘Arab Spring’, we can see the role played by diffusion across borders, but even in the Middle East and North Africa region, I would argue, scholars must look at the roots of mobilization from the inside, which can reveal a very different story than we might imagine from the outside.

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