

policy makers. This book is a must-read for all students and scholars of political violence.

Response to Ioana Emy Matesan's Review of *Ordering Violence: Explaining Armed Group-State Relations from Conflict to Cooperation*

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— Paul Staniland 

I am grateful to Ioana Emy Matesan for her thoughtful, probing, and generous review of *Ordering Violence*. She has identified two important issues in my project: how ideology and tactical imperatives relate to one another, and how to think about armed groups' own agency.

First, Matesan asks for clarity on how ideological variables intersect with more fine-grained tactical imperatives. She is right that the specifics of this relationship vary across cases. This is an issue I wrestled with throughout the project. Ultimately, I put my theoretical emphasis on ideational processes of perceived threat and affinity, with tactical concerns distinctly secondary. In part, this reflects my judgment that so-called strategic or tactical explanations in the literature are quite saturated already, and so meaningful progress at this point requires a shift toward a different set of political questions.

However, there is no doubt in the empirical record that the actual relationship between ideas and local military/political incentives can be far more complex and variable. Matesan is correct that sequencing is often key, which aligns with the theoretical structure of the book: big-picture ideological politics set the general context, and then more immediate tactical incentives can "fill in" variation that is otherwise indeterminate.

That said, matters are not always so clear-cut. There are certainly cases in which tactical considerations are more important than my argument expects. That is fine, because disconfirming evidence reduces concerns about tautology and operationalization. I point to several such examples in the book.

Matesan's concern is most far-reaching when ideology and tactics collapse into one another, with the worldviews of regimes or armed groups seeming to completely determine all their behavior. In retrospect, I wish I had been able to conceptualize more carefully the trade-off between what Matesan in her book calls "principle" and "pragmatism." The reality is of a spectrum or distribution, rather than any single fixed prioritization. Measuring where states and groups lie on this spectrum *ex ante* is obviously enormously difficult, but the book would certainly have benefited from a more extended discussion of this kind of variation.

Second, I agree with Matesan that *Ordering Violence* focuses more on states than on armed groups, and that I tend to see more stability in governments' than armed

groups' goals. Her own book does a much better job of explaining shifts over time in group goals than my state-centric account: *The Violence Pendulum* is where I would point those interested in this question.

In general, however, I view governments' power advantages as putting greater pressure on nonstate actors to adjust their aims than vice versa. There are important exceptions to this generalization, to be sure, but it certainly applies to the bulk of empirical cases in my book. Although some armed groups do maintain an unyielding set of ideological commitments, many others must adapt or risk being destroyed or marginalized. The constraints on armed groups tend to bind more tightly, and thus limit their options, far more than those on governments.

The Violence Pendulum: Tactical Change in Islamist Groups in Egypt and Indonesia. By Ioana Emy Matesan. Oxford: Oxford University Press, 2020. 288p. \$74.00 cloth.

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Ioana Emy Matesan has written an important and wide-ranging book that grapples with a central issue in the study of order and violence. It explores variation in the strategies of Islamist groups in Egypt and Indonesia, seeking to explain movement toward and away from violence. This is a hugely important question, but one that quickly runs into extraordinary complexity and contingency in the empirical record: movements often change their positions over time and, even at a single point in time, can adopt behaviors at odds with stated ideologies, and state repression can trigger armed groups' adoption of violence, as well as its abandonment.

Matesan offers a theory of when and why political movements escalate and de-escalate, arguing that these trajectories hinge on the movements' perceived need for activism, changes in the cost of violent and nonviolent tactics, and pressures they are experiencing. She deploys a set of comparative case studies of movement trajectories, examining the Muslim Brotherhood and Al-Gama'a al-Islamiyya in Egypt, and Darul Islam and Jemaah Islamiyah in Indonesia. This research design leverages both within- and cross-case analysis, and other examples are sometimes used to illustrate key concepts.

Matesan's most novel contribution is a wonderful conceptualization of how political organizations engage with violence, moving beyond a straightforward binary of violent versus nonviolent groups. Instead of trying to jam groups into this blunt distinction, she valuably identifies "eight distinct tactical outlooks that organizations can adopt at any point in time" (p. 7). She carefully parses the literature, showing that concepts like "radicalization"

and “escalation” are used in a wide variety of ways, with little consistency. Crucially, they can be applied to both rhetoric and behavior, which often do not align.

Matesan offers a fundamental scholarly innovation by introducing a new typology of tactical outlooks that let us see variation in (1) the use of violence, (2) the rhetorical justification for violence, and (3) the existence of armed wings. She shows that there are different configurations of these variables, with nonviolence and active violence occupying the extremes and a set of more nuanced tactical outlooks emerging in the intermediate space.

For instance, Matesan identifies outcomes like Inactive Violence, when an armed wing created by a group that rhetorically justifies violence does not actually use it, and Uncoordinated Violence, when a group does not publicly justify violence or implement it through an armed wing but its members nevertheless sometimes use violence. This is an incredibly useful analytical map that needs to be engaged with and used by the field more broadly. Using the framework makes it dramatically easier to describe and compare variation across political movements. This is a signal achievement. The chapter that works through this dependent variable is required reading for anyone interested in political violence.

Matesan offers a theory to explain this variation that is built around the interaction of three causal mechanisms: “the perceived need for activism, the social and political cost of violence, and internal or external pressure to act” (p. 23). This leads Matesan to focus on measuring policy convergence between the group and government, repression of the group, organizational strength and coherence, and public attitudes toward the group and its activities (p. 23).

These underpinnings lead to arguments about the processes of escalation (moving toward violence) and de-escalation (moving away from it). As I discuss later, things can get a bit complicated at this point, but my read is that the pathway to violent escalation starts with a mix of state repression, perceived threats to the community, and deep disagreement with the government, which then can become accelerated by social support for violence (p. 28). This mix generates “ideological escalation” (p. 31). However, the specific turn to violence from this point depends on organizational dynamics: either there can be internal fragmentation/competition within a movement, or external attacks can then shift groups into actual violent behavior.

De-escalation occurs when the costs of violence become unbearably high, whether resulting from state repression or the public’s abandonment (p. 33). There are several distinct trajectories that can lead to reduced violence, as well as the possibility that a move toward de-escalation triggers new rounds of violence by aggrieved factions. Matesan argues that de-escalation is not simply the mirror

image of escalation, nor do shifts in grievances play a central role in this process (pp. 32–33).

I am not an expert on the comparative case studies Matesan uses from Egypt and Indonesia, but I found them clearly structured. The periodization of group tactics in each was especially effective in highlighting the variation and heterogeneity in approach and thus the importance of a sophisticated multicausal framework for making sense of the often labyrinthine histories of these movements.

The Violence Pendulum makes key contributions to thinking about and understanding political violence. It is an important book that deserves wide readership and engagement. Yet such a book inevitably also raises questions that can guide future research.

The main challenge with summarizing and then applying Matesan’s theory is its complexity: there are a substantial number of permutations that can move a group to each of the eight values of the dependent variable, and they can be sequenced in a variety of ways. When Matesan maps out trajectories (for instance, figure 1.2 on p. 33) and explores the interactions of variables, we can see just how complex the theory can quickly become. In table C.1 on p. 179, there are eight combinations of grievances, violent norms, and external/internal escalation pressures, which can then generate different blends of ideological escalation, organizational escalation, and behavioral escalation. Similarly, table C.2 on p. 183 identifies four different starting points (one for each case), thus generating four distinct trajectories toward ultimate Active Violence.

This complexity is both a strength—for reasons I have outlined enthusiastically—and a potential weakness. The number of moving pieces might create difficulty in determining what precisely would support or disconfirm the argument: grievances and ideology, state policy, public opinion, and internal competition can all play a part in the argument, and very little is excluded as potentially causally relevant. Matesan is persuasive in arguing that none of these broad variables is sufficient on its own to explain variation, and I greatly appreciate the book’s careful, thoughtful disaggregation and theorization; there are real costs to forced parsimony. But at times it felt a bit hard to get my hands around the core, “big picture” claim of the book. This is not an uncommon issue with such ambitious projects, of course, and it provides fertile terrain for future research.

Second, there remains some ambiguity about what the category of “Islamist” is and is not doing for the theory. In the introduction (pp. 13–15), Matesan considers the question, “Are Islamist groups distinctive?” but does not come down very hard in a particular direction. At times as the book progresses, it seems that Islamism is mainly acting as a shared comparative context for disciplining the research design, with mechanisms that could equally plausibly apply beyond this subset of cases. Yet as the argument advances, the question of ideology becomes quite important

(i.e., pp. 24–27 in the theory chapter), which begins to mix theory and the research design in ways that leave the scope conditions of the argument a bit ambiguous. The history in the case studies centrally involves debates over and mobilization of “Islamist” rhetoric and symbols.

Thus, at its thinnest, the argument is that grievances—religious, redistributive, linguistic, regionalist, or anything else—emerge that can then generate unfolding processes of radicalization when there is public support, the group sees lower costs to violence than nonviolence, and pressures on the group to act rise (perhaps from internal fracturing or a form of state repression). That ideology just happens to be Islamist in the book is a way for controlling a potentially confounding variable. At its thickest, by contrast, Islam provides an ideological language and array of symbols for legitimation and world-making that deeply structure and inform subsequent mobilization.

These are two quite different approaches, and the space between them is, if anything, even more complicated. Matesan correctly notes that Islamist movements are both “principled and pragmatic” (p. 22), but work remains to be done on identifying more precisely when and how principle interacts with pragmatism and how we might causally prioritize them. Put differently, is a focus on Islamist groups a theoretical choice or a research design strategy?

The Violence Pendulum without question should be read by anyone interested in violence, the evolution of tactical choice by political movements, and the complex relationships between states, nonstate organizations, and societies. It is the kind of book that moves the field forward, especially in its creative and flexible new typology of movements’ tactical outlooks. Although open questions remain, they reflect the richness and breadth of the book’s ambition.

Response to Paul Staniland’s Review of *The Violence Pendulum: Tactical Change in Islamist Groups in Egypt and Indonesia*

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— Ioana Emy Matesan 

I am grateful to Paul Staniland for the generous review of my book. His sharp analysis raises some important questions.

Staniland is right that causal complexity can be both a strength and a weakness. My goal was to challenge the bifurcated study of radicalization and de-radicalization and explain tactical shifts over time. Therefore, it seemed important to show how background conditions may affect outcomes in the long run or alter the impact of repression. The result is an account that can at times become admittedly complicated. To add clarity, I differentiate between the determinants of ideological versus behavioral adjustments. For instance, grievances and repression have a

stronger effect on ideological than on behavioral escalation, whereas fragmentation and organizational weakness have a stronger impact on behavioral changes than ideological shifts. Public opinion and norms of resistance do not directly affect either ideological or behavioral escalation or de-escalation, but they are important background conditions. Evaluating the cost of violent and nonviolent tactics is the most powerful causal mechanism underlying tactical shifts.

The main “big picture” claim that I put forward is that tactical changes are a form of principled and strategic adjustment to intraorganizational developments and the sociopolitical context. In arguing that Islamist groups are simultaneously principled and strategic, I not only suggest that ideology and pragmatic considerations both matter, but also that it is not always possible to neatly untangle their causal effects. Tactical decisions are usually the result of pragmatic considerations and have less to do with religious principles, but it would be a mistake to consider such pragmatism void of ideology. Convictions inform activism. Even when pursuing what may seem to be purely organizational interests, groups believe they are righteous actors. When faith in the group starts to falter or a group experiences ideological decay, we see disillusionment, dissent, or defections. In response, pragmatic leaders may adjust their tactics to save the organization and continue pursuing their ideologically driven mission. Such tactical adjustments can influence ideology. Drawing on Seliger, I differentiate between fundamental principles, which define a group’s doctrine, and operative ideology, which justifies daily actions. Groups may find it necessary to adjust their repertoire of contention, altering their operative ideology. Over time, changes to the operative ideology can lead to changes in the fundamental principles.

Given this argument, it is understandable that Staniland asks whether the focus on Islamist groups was a theoretical choice or a research design strategy. For me, it was a question of research design. Perhaps the ambiguity that emerges throughout the book reflects the changes in my own understanding of the role of ideology. At the outset of the research, I embodied the rationalist bias that pervades both terrorism studies and works on Islamist movements. However, the more interviews I conducted, the more tenuous my attachments to rationalist assumptions became. Although I still do not believe that questions of violence or nonviolence are primarily about ideology, I am now also convinced that we cannot fully understand social actors without taking their ideas seriously, whether they are Islamists, Marxists, or ethnic separatists.

In this regard, Staniland and I may have different understandings of how ideology and tactical incentives interact, but our works complement each other in calling on scholars of political violence to take both seriously and to study both violent and nonviolent mobilization among armed actors.