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**Author's Response to Response by Max Abrahms to Peter Krause, "The Political Effectiveness of Non-State Violence: A Two-Level Framework to Transform a Deceptive Debate,"** *Security Studies* 22: 259–294 (2013).

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**Author's Response by Peter Krause**, Boston College

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**T**hank you to H-Diplo for publishing this exchange and to Max Abrahms for taking the time to read and respond to my article. My main regret with Abrahms's response has to do less with any of our potential disagreements that he outlines, but rather the fact that he does not engage with many of the main arguments and almost all of evidence in the original article, on which I am sure he has some valuable opinions. I will therefore first briefly summarize these main points for those who have not read the article and would benefit from understanding its context and key takeaways. I will then engage with each of Abrahms's points directly, noting some suggested steps forward along the way.

The core of my article's thesis is noted at the outset: "Once the debate over the political effectiveness of non-state violence is disaggregated into its constituent parts, this article argues that what scholars suggest is different is actually the same (their empirical analysis), what they suggest is the same is actually different (their methods of measurement), and what they truly have in common should be different, or at least diversified (their framework of political effectiveness)."<sup>1</sup> First and foremost, the essence of effectiveness is whether non-state violence causes intended outcomes. If scholars are disagreeing on whether violence "works," then one

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<sup>1</sup> Peter Krause, "The Political Effectiveness of Non-State Violence: A Two-Level Framework to Transform a Deceptive Debate," *Security Studies* 22, no. 2 (2013): 264.

would naturally presume that they disagree on the outcomes of violent campaigns and/or whether violence caused those outcomes. The first surprise is that this is not in fact the case. The studies by Abrahms and others that are cited in the article “agree on the historical outcome of almost every violent campaign.”<sup>2</sup> Although I note that they could still debate whether violence was responsible for causing those outcomes or not, this is not the focus of their debate either.

Are there many (any?) cases where Abrahms thinks a concession was not made but others say it was, or vice versa? If so, it would be enormously helpful to see them laid out to define the spectrum of historical disagreement. As far as I know, the subfield is generating an increasing number of lists of campaigns with dichotomous codings of ‘success’ or ‘failure,’ but not detailed disputes over any coding discrepancies. If there are not a large number of historical disagreements, then what is really driving the differences in many of the findings on the percentages of success and failure is how each scholar codes “success” and his/her case selection, neither of which are constant across studies.

As I demonstrate in the article, much of the discrepancy between the percentage of “successful” campaigns in Abrahms’s and Robert Pape’s work, among other prominent studies, is a result of their different standards for what constitutes success.<sup>3</sup> Those with a high bar for success, like Abrahms, find a much smaller percentage of successful campaigns than others with a lower bar, like Pape, would find using the same cases. The same holds true for discrepancies like those between Timothy Wickham-Crowley, who uses the high bar of “social revolution” for success and finds an 8% success rate for insurgencies, and Jason Lyall and Isaiah Wilson, who employ a low bar that interprets draws as insurgent victories and find a 57% success rate.<sup>4</sup> The common use of terms like “success” or “effectiveness” across studies suggests a commonly defined and measured dependent variable where one rarely exists. Standardizing the dependent variable makes much of the discrepancy in successes and failures fall away. The other part of the discrepancy stems from differences in case selection, which Abrahms discusses in some depth and which I will address below.

To capture the political effectiveness of non-state violence from the perspective of those who use it, and to identify theories that explain the greatest variation in both the use and political effectiveness of non-state violence, my article suggests that scholars should employ a two-level framework that accounts for organizational objectives and effects (such as increasing membership or funding) as well as strategic ones (such as the removal of a foreign regime). Although this may seem like a revolutionary suggestion in the subfield of the effectiveness of non-state violence, it is approaching old-hat status in related subfields on the causes and dynamics of non-state violence,

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<sup>2</sup> Ibid., 260.

<sup>3</sup> Max Abrahms, “Why Terrorism Does Not Work,” *International Security* 31, no. 2 (2006); Robert Pape, *Dying to Win: The Strategic Logic of Suicide Terrorism* (New York: Random House, 2005).

<sup>4</sup> Timothy Wickham-Crowley, *Guerrillas and Revolution in Latin America: A Comparative Study of Insurgents and Regimes since 1956* (Princeton, NJ: Princeton University Press, 1992); Jason Lyall and Isaiah Wilson, “Rage Against the Machines: Explaining Outcomes in Counterinsurgency Wars,” *International Organization* 63, no. 1 (2009). Lyall and Wilson analyze insurgencies from 1800 onwards, but I use their figures from 1900 onwards to create a more equitable comparison with Wickham-Crowley and other studies.

as I noted in the article.<sup>5</sup> I will not rehash the framework or my use of it for historical analysis here, but I encourage interested readers to read the article and send me any responses.<sup>6</sup> From my reading of existing scholarship, as well as my own fieldwork and analysis, I believe that this framework better reflects how armed groups think, better explains when they use or shun violence, negotiate or spoil peace treaties, and better reflects the multidimensional political effectiveness of violence that often varies across groups as well as movements.

The main issue Abrahms takes with my article revolves around the strength of armed groups and movements and their impact on the outcome of violent campaigns. Before I address our agreements and disagreements, two clarifications are in order, both of which stem from the fact that Abrahms quoted a single word from my article rather than the broader sentence. First, Abrahms writes that “When membership size is taken into account, [Krause] asserts, the debate ‘disappears’.”<sup>7</sup> Actually, the full quotation is as follows: “Upon further examination, the gap between [Abrahms’s and Pape’s studies] disappears when differences in their standards of measurement and case selection are considered.”<sup>8</sup> In other words, membership size is not put forth as the only or even the most important factor, but rather I argue that definitions of “success” (discussed above) and case selection—which includes membership size—explain the discrepancy. We can separately debate the importance of group and movement strength for the outcomes of campaigns, but I want to make sure my argument is clear.

Second, Abrahms states, “[Krause claims] that empirical studies have ‘summarily’ dismissed the possibility that terrorist groups are hampered by their weak capability.”<sup>9</sup> I believe no such thing, and I wrote no such thing. The only use of the word “summarily” in my article appears in a footnote where I write, “In a more recent article, Abrahms acknowledges the group strength

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<sup>5</sup> “The enduring dominance of this [single-level strategic] framework in studies of the effectiveness of non-state violence is puzzling given that scholars have increasingly demonstrated the power of two-level analysis of non-unitary social movements and insurgencies for explaining when, why, and how competing armed groups use violence.” A very small sample includes: Mia Bloom, *Dying to Kill: The Allure of Suicide Terrorism* (New York: Columbia University Press, 2005); Stathis Kalyvas, *The Logic of Violence in Civil War* (Cambridge: Cambridge University Press, 2006); David Cunningham, “Veto Players and Civil War Duration,” *American Journal of Political Science* 50, no. 4 (October 2006); Jeremy Weinstein, *Inside Rebellion: The Politics of Insurgent Violence* (Cambridge: Cambridge University Press, 2007); Ethan Bueno De Mesquita, “Terrorist Factions,” *Quarterly Journal of Political Science* 3, no. 4 (2008); Wendy Pearlman, “Spoiling inside and Out: Internal Political Contestation and the Middle East Peace Process,” *International Security* 33, no. 3 (Winter 2008/09); Adria Lawrence, “Triggering Nationalist Violence: Competition and Conflict in Uprisings Against Colonial Rule,” *International Security* 35, no. 2 (Fall 2010); Paul Staniland, “Between a Rock and a Hard Place: Insurgent Fratricide, Ethnic Defection, and the Rise of Pro-State Paramilitaries,” *Journal of Conflict Resolution* 56, no. 1 (February 2012).

<sup>6</sup> See Peter Krause, “The Political Effectiveness of Non-State Violence: A Two-Level Framework to Transform a Deceptive Debate,” 270-294.

<sup>7</sup> “Response by Max Abrahms,” *H-Diplo*, <http://www.h-net.org/~diplo/ISSF/PDF/ISSF-Krause-Abrahms-response.pdf>, 3.

<sup>8</sup> Peter Krause, “The Political Effectiveness of Non-State Violence: A Two-Level Framework to Transform a Deceptive Debate,” 265.

<sup>9</sup> “Response by Max Abrahms,” *H-Diplo*, 2-3.

critique but summarily dismisses it.”<sup>10</sup> What I said is that Abrahms himself dismisses the idea that group strength could impact the success or failure of campaigns, not that all empirical studies have done so. I agree that studies other than those of Abrahms and Pape (which I was comparing at the time) have been conducted that attempt to control for group or movement strength, but that only means that those studies try to isolate the effect of group strength from that of violence, not that group strength does not matter for movement outcome.

In fact, I argue that group and movement strength do have a significant impact on both the use and effectiveness of violence. Can a single three-person armed cell realistically compel regime change? Is a movement with 10,000,000 members more or less likely to meet success than one with 100? A central finding of one of the projects that Abrahms cites as an example (Chenoweth and Stephan 2011), is that movements with more members are more likely to succeed strategically.<sup>11</sup> Chenoweth and Stephan also suggest that violence may drive down movement membership, which is a reason why they conclude that violent campaigns are less successful, but nonetheless the argument holds that movement size matters, at least in this study. I would also suggest that other aspects of group strength have not been adequately analyzed and can help explain individual group action and collective movement success, such as the relative power position of a group in its movement hierarchy and the distribution of power in the movement itself.<sup>12</sup>

Abrahms then writes, “Studies which employ public opinion rather than government concessions as the dependent variable offer additional empirical evidence that capability alone is not responsible for the poor coercion rate.”<sup>13</sup> I could not agree more, but Abrahms is arguing against a straw man that capability alone drives the rate of successful coercion. Who argues that “capability alone is responsible for the poor coercion rate”? I did not put forth such an argument nor did I identify anyone who did. Saying that group or movement strength does not explain all of the variation in campaign outcome does not mean that it does not explain some of it, however.

I cited the Berrebi and Klor article as an excellent analysis of the impact of terrorism, but as I noted in my article, it was not exactly a study of effectiveness because the intentions of the attackers were not examined in any depth. Abrahms and/or others might assume that the attackers would prefer to drive popular support within their enemy state to left-wing political parties (the opposite of what generally occurred in Israel), but is that necessarily the case? What if the attackers wanted to spoil a deal and so would want hard-liners in power (in fact, some did)? What if they benefitted from a hawk-hawk coalition that increased their strength in their own movement, until they could become the strongest group and themselves sit at the negotiating table (they likely did, as did their predecessors)? What if they sought the overreaction of their

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<sup>10</sup> Peter Krause, “The Political Effectiveness of Non-State Violence: A Two-Level Framework to Transform a Deceptive Debate,” 266. The article is Max Abrahms, “The Political Effectiveness of Terrorism Revisited,” *Comparative Political Studies* 45, no. 3 (March 2012).

<sup>11</sup> Erica Chenoweth and Maria Stephan, *Why Civil Resistance Works: The Strategic Logic of Nonviolent Conflict* (New York: Columbia University Press, 2011).

<sup>12</sup> Peter Krause, “The Structure of Success: How the Internal Distribution of Power Drives Armed Group Behavior and National Movement Effectiveness,” *International Security*, forthcoming.

<sup>13</sup> “Response by Max Abrahms,” *H-Diplo*, 3.

enemy state—which many scholars have identified as the key pathway to the success of armed groups and their movements—and right-wing politicians were more likely to deliver just such a response? Abrahms seems to argue that driving support to right-wing politicians is neither desired by the attacking group nor beneficial to it or its movement, but that remains an open question, to say the least, and one that bears directly on whether we are discussing the general impact of violence or the specific achievement of intended goals.

Indeed, Berrebi and Klor themselves seem to argue against Abrahms in this case. They actually anticipate the fact that “Some scholars may interpret this as further evidence that terror attacks against civilians do not help terror organizations achieve their stated goals (Abrahms 2006).”<sup>14</sup> They then go on to note that the picture may be more complex: “these organizations face trade-offs between their main objectives, and a chosen strategy in pursuit of some of them may undermine the likelihood of achieving others.”<sup>15</sup> They even suggest that their own prior studies could accommodate the conclusion that such attacks could in fact help the movement gain concessions. If in a key study that Abrahms cites as conclusive evidence for the ineffectiveness of terrorism, the authors themselves suggest something quite a bit more nuanced and amenable to a multi-level framework with variation in mechanisms and outcomes, it seems that things are far less settled and well understood than Abrahms’s response would suggest.

The fact that Abrahms discusses organizational effectiveness only at the very end of his response is unfortunate. Not only is it the focus of my article, but we also agree on much of it, or at least agree on what we disagree with. Abrahms’s previous articles demonstrate that we agree that strategic motivations are not the only or even the dominant motivation for non-state violence. Where we potentially disagree is that he places the main motivation at the level of individuals seeking to maximize social solidarity, whereas I place it at the level of organizations seeking to maximize power.<sup>16</sup> Both matter. I would argue that organizational concerns are more significant drivers of non-state violence than individual ones, but this is an excellent issue for debate that can and should be subjected to further empirical scrutiny.

Abrahms concludes by claiming that “the empirical record offers scant evidence that attacking civilians with terrorism assists non-state challengers with either objective [strategic or organizational].”<sup>17</sup> I can only point Abrahms and readers to the last third of my article (in addition to the numerous other pieces cited throughout), which is an analysis of eight paradigmatic cases of the empirical record that include numerous organizational and strategic successes and failures. This would seem to be more than scant variation in only eight cases, which deserves analysis and explanation, not dismissal.

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<sup>14</sup> Claude Berrebi and Esteban Klor, “Are Voters Sensitive to Terrorism? Direct Evidence from the Israeli Electorate,” *American Political Science Review* 102, no. 3 (2008): 299.

<sup>15</sup> *Ibid.*

<sup>16</sup> Max Abrahms, “What Terrorists Really Want,” *International Security* 32, no. 4 (Spring 2008).

<sup>17</sup> “Response by Max Abrahms,” *H-Diplo*, 4.

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