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Introduction by James McAllister, Williams College

Why do some national movements succeed at creating their own states while others fail? This fundamental question lies at the heart of Peter Krause's important new book. While recognizing the excellence of much of the existing theoretical and empirical research on social movements and violence, Krause argues that this scholarship has not fully appreciated "the competitive internal dynamics that are at the foundation of the success of groups and the movements of which they are a part" (8). He advances a structural theory in *Rebel Power*, which he calls "Movement Structure Theory (MST)," to account for how the distribution of power among individual groups contributes to the success or failure of national movements. The national movements most likely to successfully gain their own states, Krause argues, are those that feature a hegemonic actor vastly stronger than other rival actors. Free from concerns about losing their position of leadership within the movement, hegemonic actors can concentrate their efforts and resources against their external adversary. In his view, "In a hegemonic movement, there is more pursuit of victory and less counterproductive violence, making such movements far more successful. A hegemonic movement—with one dominant group—incentivizes the pursuit of victory and reduces counterproductive violent mechanisms because the hegemon has no challengers to outbid, fight, or spoil" (11).

Krause's parsimonious theoretical framework is tested by an ambitious, multi-method research design. In addition to constructing an original data set to test his arguments, Krause conducted extensive archival research and personal interviews with participants for each of his four case studies of national movements: Algeria, Ireland, Zionism, and the Palestinians. As Wendy Pearlman notes, all of the historical case studies in *Rebel Power* are based on a "staggering amount of research."

The four reviewers, all important contributors to the scholarly literature on national movements, find much to praise about Krause's book. Kristin Bakke appreciates how *Rebel Power* "conceptualizes and theorizes the power hierarchy within groups." Adria Lawrence believes that Krause "successfully demonstrates the importance of power considerations across this diverse set of cases, and his theory may well travel to many other settings, including rebellions that are not nationalist in nature." The reviews by both Pearlman and William Quandt are also largely positive.

Nevertheless, all the reviewers also have some important concerns about Krause's theoretical framework or one or more of his case studies. Lawrence raises both of these concerns in her review. While the struggle for power among nationalist groups is undeniably important, Lawrence argues that MST slights the importance of political and ideological disagreements among these groups. As an example, she cites the ideological conflicts within the Algerian movement for independence, which she argues cannot be reduced to the struggle for power. In addition, and in this point she is supported by Pearlman and Quandt as well, Lawrence argues that Krause fails to consider the nature and strength of the adversary that is faced by various national movements. Would the Palestinians have a state if their movement consistently had one hegemonic group rather than a number of fragmented groups constantly vying for leadership? Lawrence is skeptical that the ultimate outcome for the Palestinians would have been any different regardless of the internal balance of power. As Pearlman argues, "in consistently upholding the primacy of internal drivers of movement success or failure, the analysis at times seems to disregard the interests and capacity of movement's adversary."

H-Diplo/ISSF thanks Professor Krause and all of the reviewers for contributing to an important debate that will be of continuing interest to both political scientists and historians.

Participants:

Peter Krause is Assistant Professor of Political Science at Boston College and Research Affiliate with the MIT Security Studies Program. In addition to *Rebel Power: Why National Movements Compete, Fight, and Win* (Cornell University Press, 2017) he recently published a co-edited volume *Coercion: The Power to Hurt in International Politics* (Oxford University Press, 2018). He has published articles on Middle East politics, political violence, and national movements in *Comparative Politics*, *International Security*, *Journal of Experimental Psychology: General*, *Security Studies*, *Studies in Conflict and Terrorism*, and *Terrorism and Political Violence*.

James McAllister is the Fred Greene Third Century Professor of Political Science at Williams College. The author of *No Exit: America and the German Problem, 1943-1954* (Cornell University Press, 2001), McAllister is currently writing a book examining Harry Garfield and the Institute of Politics that he created at Williams in the aftermath of the First World War.

Kristin M. Bakke is Professor in Political Science and International Relations at University College London, and Associate Research Professor at the Peace Research Institute in Oslo. Focusing on political violence, her research has explored the relationship between political institutions and intrastate struggles, fragmentation within self-determination movements, the impact of foreign fighters on domestic insurgencies, and the dynamics of post-war state-building and stability. She is the author of *Decentralization and Intrastate Struggles: Chechnya, Punjab and Québec* (Cambridge University Press, 2015), and her work has appeared in journals such as *Annals of American Association of Geographers*, *International Security*, *International Studies Quarterly*, *Journal of Conflict Resolution*, *Journal of Peace Research*, *Nations and Nationalism*, *Perspectives on Politics*, *Political Geography*, *Regional and Federal Studies*, and *World Politics*.

Adria Lawrence is the Aronson Distinguished Associate Professor of Political Science and International Studies at Johns Hopkins University, and the author of *Imperial Rule and the Politics of Nationalism: Anti-Colonial Movements in the French Empire* (Cambridge University Press, 2013). Her work on the use of violent and nonviolent nationalist movements has appeared in *International Security* and *Rethinking Violence: States and Non-State Actors in Conflict* (co-edited with Erica Chenoweth, MIT Press, 2010). She has also written on protest during the 2011 Arab Spring. She is currently writing a book on colonial state formation, which examines how indirect and direct methods of colonial administration were altered and modified in North Africa and sub-Saharan Africa.

Wendy Pearlman is the Martin and Patricia Koldyke Outstanding Teaching Associate Professor of Political Science at Northwestern University, where she specializes in Middle East politics. She is the author of *We Crossed a Bridge and it Trembled: Syrian Chronicles* (New York: HarperCollins, 2017), *Violence, Nonviolence, and the Palestinian National Movement* (New York: Cambridge University Press, 2011), and *Occupied Voices: Stories of Everyday Life from the Second Intifada* (New York: Nation Books, 2003). Her current research examines questions of identity, integration, and socio-economic class among Syrian refugees in the Middle East and Europe.

From 1994 to 2013, **William B. Quandt** held the Edward R. Stettinius chair in the Department of Politics at the University of Virginia. He taught courses on the Middle East and American Foreign Policy. In 2012, he received the University's Thomas Jefferson Award. He received his PhD. From MIT in 1968 and his BA from Stanford in 1963. From 1979 to 1994, he was a Senior Fellow in the Foreign Policy Studies Program at

the Brookings Institution, where he conducted research on the Middle East, American policy toward the Arab-Israeli conflict, and energy policy. Before going to Brookings in 1979, Dr. Quandt served as a staff member on the National Security Council (1972-1974, 1977-1979). He was actively involved in the negotiations that led to the Camp David Accords and the Egyptian-Israeli Peace Treaty. Dr. Quandt was also an Associate Professor of Political Science at the University of Pennsylvania, worked at the Rand Corporation in the Department of Social Science from 1968-1972, and taught at UCLA and MIT William Quandt has written numerous books, and his articles have appeared in a wide variety of publications. His books include: *Peace Process: American Diplomacy and the Arab-Israeli Conflict Since 1967* (Brookings, 2005, 3rd ed.); *Between Ballots and Bullets: Algeria's Transition from Authoritarianism* (Brookings, 1998); *Camp David: Peacemaking and Politics* (Brookings, 1986); and *Revolution and Political Leadership: Algeria, 1954-1968* (MIT Press, 1969). Most recently, he co-authored *Peace Puzzle: America's Quest for Arab-Israeli Peace, 1989-2011* (Cornell University Press, 2013).

Review by Kristin M. Bakke, University College London

A common source of both conflict and change in the international system is national movements' pursuit of political autonomy, often independence, for the groups they represent. Yet there is notable variation in their success. While the Algerians, the Eritreans, and Zionists today have states of their own, others, like the Basques, Chechens, Kurds, and Palestinians, do not. And, while some national movements do not resort to violence, many do engage in violence at some point in their struggle, sometimes even among the different groups that make up the movement. Why? These are the questions at the heart of Peter Krause's *Rebel Power: Why National Movements Compete, Fight, and Win.* Drawing on archival data, interviews, and fieldwork observations, Krause takes the reader on a tour across four prominent national movements over time: the Algerian, Irish, Palestinian, and Zionist movements. He argues that the key to explaining variation in these movements' ability to achieve independence, and to do so with or without accompanying violence, is the balance of power within the movements.

Akin to how realist international relations scholars think of power in the international system,[1]

Krause posits that the most crucial aspect of a movement's structure is the internal balance of power—"and the most powerful groups play the dominant role in campaign dynamics and outcomes" (17). The central distinction is between, on one hand, hegemonic movements, in which one group dominates the others by virtue of its membership size, wealth, or popular support, and, on the other hand, fragmented and united (allied) movements, in which there is no dominant group. Hegemonic movements, Krause argues, are more likely to achieve their strategic goals because they can focus their attention on fighting the external enemy, the state, rather than on internal challengers. Hegemonic movements are also less likely to turn to violence, both because the dominant group has no internal challengers to fight *and* because it is more likely to shun escalation in its interactions with the state, given that the dominant group would be likely to bear the brunt of the state response. That is not to say that hegemonic movements never resort to violence, but that "their use of it more generally aligns with external, strategic imperatives rather than internal competition" (31). The "movement structure theory" also provides predictions for how individual groups within a movement will act, based on their position in the power hierarchy, which, drawing on prospect theory,[2] shapes how they assess the benefits of victory and risks of violence.

Both the argument and evidence in *Rebel Power* are compelling. The book builds on a growing body of work, including works by the reviewers in this forum, that puts front and center the dual struggle in which groups, or factions, within movements find themselves: the struggle against their shared external enemy, over autonomy or independence, and the struggle against one another, over positions of dominance within the movement.[3] What is new here is the emphasis, both theoretically and empirically, on the internal distribution

[1] Kenneth Waltz, *Theory of International Politics* (New York: McGraw-Hill, 1979).

[2] For example, Daniel Kahneman and Amos Tversky, "Prospect Theory: An Analysis of Decision under Risk," *Econometrica* 47:2 (1979): 263-291.

[3] Adria Lawrence, "Triggering Nationalist Violence: Competition and Conflict in Uprisings against Colonial Rule," *International Security* 35:2 (2010): 88-122; Kathleen Gallagher Cunningham, Kristin M. Bakke, and Lee J. M.

of power as *the* determining factor for movement tactics and outcomes. While, for example, both Wendy Pearlman and my co-authors and I have argued that the distribution of power within movements has implications for spoiler behavior or infighting,[4] we have suggested that coordinating institutions, such as alliances, can also help overcome a movement's internal competition. In contrast, Krause argues that fragmented movements and those united through alliances are rather similar: "(A)lliances between nonstate actors are generally so weak and wrecked by commitment problems and struggles over relative power that united movements are often not much different in their actions or outcomes than fragmented movements" (18). Thus, what matters for explaining movement tactics and outcomes, he argues, is the power hierarchy among the 'significant' groups in a movement, and, in particular, whether there is one group sufficiently strong to dominate any rival in the movement.

In the case studies, the distinction between a movement dominated by one group and an alliance in some instances seems blurred, for example, the merger between Sinn Féin and the Irish Volunteers in October 1917 (137-138)—and Krause recognizes that mergers are a particular form of alliance that "can change the game" (185). Indeed, capturing power relationships within a movement is tricky, and *Rebel Power* does a great service by helping us think about how to do this—and provides detailed empirical data on the power hierarchy among the groups within the four movements under study, both in the book itself and in online appendices. Conceptually, the book distinguishes between whether a group is a 'hegemon', 'leader', 'challenger', or 'subordinate'. A hegemon, for example, "is a dominant group that is more than three times stronger than any rival in the movement" (9). Strength comes in different forms and, in the book, is assessed through a movement's membership, wealth, and popular support. Weapons is not an explicit criterion for assessing a group's strength, as not all groups in a movement use arms, though whether groups are armed matters in the empirical discussion. For example, "The fact that the SDLP [Social Democratic and Labour Party] did not form an armed wing and Provisional Sinn Féin did not run for elections at the time makes it difficult to compare the strength of these competing groups directly" (151). Indeed, I would have wanted to know more about how different forms of power shape the reasoning of the different groups in the movement's power hierarchy.

I appreciate the importance of focusing on the distribution of power within movements, and I welcome how the book conceptualizes and theorizes the power hierarchy within groups. Yet claims such as "(a)dding ten groups of ten people each to a national movement is likely to have no impact, whereas adding one group of 10,000 people is likely to significantly alter the behavior of other groups and the outcome of their movement" (17) sit somewhat uneasily. Indeed, though recognizing that the number of groups is not the only aspect of intra-movement dynamics that matter, there are several examples of works demonstrating that such a measure provides explanatory power over outcomes we care about, such as infighting, side-switching, and concessions.[5]

Seymour, "Shirts Today, Skins Tomorrow: Dual Contests and the Effects of Fragmentation in Self-Determination Disputes," *Journal of Conflict Research* 56:1 (2012): 67-93.

[4] Wendy Pearlman, "Spoiling Inside and Out: Internal Political Contestation and the Middle East Peace Process," *International Security* 33:3 (2008/2009): 79-109; Kristin M. Bakke, Kathleen Gallagher Cunningham, and Lee J. M. Seymour, "A Plague of Initials: Fragmentation, Cohesion, and Infighting in Civil Wars," *Perspectives on Politics* 10:2 (2012): 265-284.

[5] Jesse Driscoll, "Commitment Problems or Bidding Wars? Rebel Fragmentation as Peace Building," *Journal of Conflict Resolution* 56:1 (2012): 118-149; Hanne Fjelde and Desirée Nilsson, "Rebels against Rebels Explaining Violence

That is, the question is perhaps not an either/or version of whether considering the hierarchy among significant groups only or counting the number of groups best captures intra-movement dynamics, but, rather, how the distribution of power conditions the relationship among the groups in the movement.

Krause notes that an inconvenient implication of the argument in *Rebel Power*, which complicates policy recommendations, is that, "the movements that are more likely to win are less likely to become stable democracies, given that hegemonic movements are more successful but an effective democracy requires multiple significant parties crafting representative institutions and competing for support" (11). The Algerian case is an example in this respect, where the hegemony of the *Front de Libération Nationale* (the FLN) was key to achieving independence but did not bode well for democracy afterwards.[6] This is a trajectory yet to be explored more systematically, and, indeed, an important avenue for further research growing out of this book—and the broader research program on which it builds—is the relationship between war-time intra-movement dynamics and post-war stability.

between Rebel Groups," *Journal of Conflict Resolution* 56:4 (2012): 604-628; Lee J. M. Seymour, "Why Factions Switch Sides in Civil Wars: Rivalry, Patronage and Realignment in Sudan," *International Security* 39:2 (2014): 92-131; Kathleen Gallagher Cunningham, *Inside the Politics of Self-Determination* (Oxford: Oxford University Press, 2014).

[6] See also Terrence Lyons, "From Victorious Rebels to Strong Authoritarian Parties: Prospects for Post-war Democratization," *Democratization* 23:6 (2016): 1026-1041.

Review by Adria Lawrence, Johns Hopkins University

Nationalist challenges to states and empires account for a substantial amount of the conflict that has occurred in the world over the last century. Nationalists have engaged in peaceful tactics, such as competing in elections, carrying out strikes, and organizing mass protests. They have also employed violence, including targeted assassinations, bombings, and insurgency. Why do nationalist movements differ in their use of violent and non-violent tactics? Why do tactics change over time? And why do some nationalist movements succeed in securing their own state, while others fail? Peter Krause's ambitious new book *Rebel Power* takes on these important questions, offering a clear, parsimonious theory to explain variation within and across nationalist struggles for independence.

Borrowing from the realist literature in international relations, Krause applies a balance-of-power logic to groups within a larger nationalist movement. Groups, he suggests, are not only interested in the ultimate goal of independence, but also in maximizing their own power within the larger nationalist movement. Where there are multiple nationalist organizations, intra-nationalist competition affects both the tactics nationalists employ and whether they are successful in achieving a state. While IR scholars have focused on how the balance of power affects state behavior, Krause shows that struggles for power are also ubiquitous among those trying to form states (11).

Krause identifies three types of nationalist movements. Hegemonic movements contain one dominant group with few rivals; the National Liberation Front (FLN) in the last years of the Algerian War of Independence is one example. Since hegemonic movements face little internal competition, they are able to focus their energies and resources on opposing the regime. They are thus more likely to achieve victory. Any use of violence will be strategically directed against the opponent; outbidding, spoiling, and infighting do not occur where there is a single nationalist group (30-31).

United movements are the second type. They contain more than one significant nationalist organization, grouped in an alliance against the opponent. Such alliances occur rarely in the four cases included in Krause's study. There are no documented periods of unity in the Zionist and Irish cases, and nationalist groups were only briefly united in Palestine in 1974 and in Algeria from 1944-1945 and in 1951 (see Table 7.1, 176). United movements are less likely than hegemonic movements to succeed. Within these movements, groups may be leaders, challengers, or weak and subordinate. Leaders pursue victory, but they are dogged in their attempts to win by challenges from lesser groups who employ violence to spoil negotiations, outbid the leader, or attack rivals.

Fragmented movements are the third and most common category in these cases. The Palestinian movement since 2001 exemplifies the type. They are the least likely to succeed and most likely to be plagued with violent infighting. In fragmented movements, challenger groups pursue organizational goals rather than victory and leading groups have difficulty restraining violence (34).

This balance of power theory is written with precision and clarity. By laying out a typology and generating predictions, Krause provides a simple logic to address both the behavior of individual groups within a movement and the overall success of the movement. The theory also helps to explain an ongoing puzzle for scholars and observers of nationalist conflicts: why alliances among groups ostensibly with the same goal are often unstable and short-lived. Even when they manage to form an alliance, they have difficulty sustaining agreement and working together to extract concessions from the other side. Struggles for power within the

movement help explain why fragmentation is so common, even though nationalists themselves recognize the importance of unity.

A parsimonious, deductive theory such as this one has many virtues: clarity, simplicity, and the ability to explain a lot of variation while making a small number of assumptions about human behavior. Parsimony is valuable to social scientists; the goal of theorizing is to cut through the messiness of history in order to identify isolate key causal factors. Yet how much parsimony is too much? A move toward greater parsimony necessarily involves some trade-offs. In the remainder of this review, I focus on three consequences of Krause's decision to provide "a parsimonious and powerful theory" (7): inattention to politics among nationalist groups, the omission of important causes of nationalist victory, and a tendency to privilege theory over history. In my discussion of these issues, I draw primarily on examples from Algeria, the case I know best.

Krause explains his focus on nationalist movements by emphasizing their importance in history. "Nationalism," he writes, "has arguably been the greatest political force in the world over the past two centuries" (5). Yet nationalism plays virtually no role in the argument. In Krause's understanding, these movements are only nominally nationalist. Their nationalist goals are subordinate to their desire for power, power vis-à-vis the state certainly, but power vis-à-vis other nationalist groups first and foremost. Drawing from the realist playbook, he writes that within nationalist movements "groups seek to ensure their survival and maximize their power above all else" (9). He offers the rivalry between the Kurdistan Democratic Party (KDP) and the Patriotic Union of Kurdistan (PUK) as an example of the primacy of power over nationalism, quoting a member of the nationalist movement: "There are some people within the PUK who are against an independent state of Kurdistan if it is announced at the hands of [KDP leader Masoud] Barzani" (sic, 10). The implication is that competition between nationalist organizations is driven by the desire to maximize power, even at the expense of shared nationalist goals.

Yet such an understanding misses the very real and salient political disagreements that explain why there are different opposition groups to begin with. Nationalist group rivalries are not apolitical quests for power, but arise from meaningful differences among nationalist organizations. Rival organizations exist because people disagree about strategy, tactics, and even goals – whether to seek autonomy or independence, for example, what kind of relationship should be forged with the opposing state, or how the state should be governed if in fact independence is achieved. People in organized opposition groups are not solely seeking power for its own sake; if they were, we might expect to observe people switching group allegiances more frequently, as groups' relative power fluctuates. Instead, people often stay committed to their organization, in part because it best represents their views. There are politics to nationalist rivalries that a focus on power maximization overlooks.

A discussion of the Algerian case illustrates this point. Krause's approach is to effectively control for nationalism by studying rivalries within nationalist movements. If groups all share nationalist objectives, their inability to cooperate is puzzling, and power-seeking accounts for their behavior. He thus characterizes all Algerian political groups opposing France as part of one, larger nationalist movement. Algerian organizations, however, did not all share the same objectives. Ferhat Abbas, the leader first of the *Amis du Manifest de la Liberté* (AML) and then of the *Union Démocratique du Manifest Algérien* (UDMA) in the postwar period, held very different views on Algeria's relationship to France than Messali Hadj, the leader of the *Parti du Peuple Algérien* (PPA) did. Abbas was a leader in the movement to extend French citizenship to Algerians; this movement's goal was not independence but further incorporation with France. His status as a nationalist for much of the period before the war of independence is dubious at best. In the early years, he famously claimed

that he had looked for an Algerian nation, but not found it.[1] Messali Hadj, in contrast, was the first to organize an explicitly pro-independence movement. His early organization had ties to the Algerian Communist Party. He founded the PPA at the height of the movement for increasing rights for Algerians within the structure of French rule, but unlike Abbas, he opposed the goal of citizenship. A comparison of these leaders points to the existence of diverse political commitments; the rivalry between Messali and Abbas is not reducible to struggles for power.

In his analysis of Algeria, Krause acknowledges the different origins of these organizations, but in a way that exemplifies a tendency to subordinate history to the requirements of the theory. He minimizes political differences among Algerian actors, calling the early efforts to gain French citizenship "proto-nationalist," whereas Abbas and others did not have nationalist aims at all at this time. His teleological language implies that the movement to incorporate Algerians as French citizens was but a short-lived phase along a path toward demanding independence. For instance, he states that in 1937, "the emergence of a broad-based Algerian nationalist movement was just around the corner" (103), and he argues that Abbas was "finally" on board for Algerian Independence in 1943 (104), as if Abbas's earlier commitments were destined to be ephemeral. In fact, calls to extend rights to Algerians within the framework of French colonial rule were not short-lived, but dated from World War I all the way to the outbreak of the war in 1954, as I have argued elsewhere.[2]

Krause is mainly concerned with the period from 1944 to 1962, when nationalism was, he suggests, the dominant mode of opposition and the "proto-nationalist" period was finished. But this characterization neglects aspects of history that point to the importance of political differences for group rivalries, not just power differentials. Krause leans heavily on the fact that Abbas called for independence in his 1943 *Manifesto of the Algerian People* to justify his characterization of the movement as united from 1944-1945. Abbas did indeed call for statehood, but only in a supplement to the manifesto, not the main text, which demanded the participation of Algerian Muslims in the government. Further, after French authorities placed him under house arrest, he recanted and reaffirmed his loyalty to France. The new organization he founded in 1944 aimed to create an Algerian republic federated with France.[3] This goal was at odds with Messali's desire for outright independence, calling into question the claim that there was unity in this period.[4] Krause implies that Messali's disagreements with Abbas stemed from a desire for his group to lead the movement (104), but this goal reflects his opposing views on Algeria's relationship with France, not just a desire for power and position. Disagreements about whether to seek reform or independence continued into the post-war period,

[1] See Alistair Horne, *A Savage War of Peace: Algeria 1954-1962* (New York: The Viking Press, 1977), 40-41.

[2] See Adria Lawrence, *Imperial Rule and the Politics of Nationalism: Anti-Colonial Protest in the French Empire* (New York: Cambridge University Press, 2013), chapter 2 in particular. Diversity in the demands made by empire's opponents are not limited to Algeria; Frederick Cooper's work on Africa shows that Africans opposed colonialism in multiple ways that are not reducible to nationalist goals for statehood. Cooper, Frederick. *Colonialism in Question. Theory, Knowledge, History* (Berkeley: University of California Press, 2005).

[3] Krause claims on page 106 that Abbas's UDMA sought independence through electoral means, but this is inaccurate. In footnote 21, he offers a partial correction, stating that Abbas often "shied away from stressing the goal of independence."

[4] For a discussion of this history, see Lawrence *Imperial Rule*, 82; Horne, *A Savage War of Peace*, 43 and Abun-Nasr, Jamil M, *A History of the Maghrib* (Cambridge: Cambridge University Press, 1975), 323.

but these politics are missing from Krause's account. It was only when the war broke out in 1954, and the FLN began its quest for hegemony, that Abbas and his followers fully switched to supporting outright independence.

The point of my critique is not that these actors do not care about power, leadership, or survival. I share Krause's sense that they valued their leadership roles, and vied with one another for dominance. I disagree, however, that power maximization is the main lens through which nationalist movements can be understood. A theory is too parsimonious under two conditions: when it leaves out important factors that affect behavior, and when it sacrifices historical accuracy in order to increase the compatibility between evidence and theory. Because there is no place in the theory to discuss differences among the Algerian groups, including different aims (some of which may not be nationalist at all), Krause is forced to downplay the political reasons for competition and rivalry. When politics are considered, it is difficult to maintain that power maximization was the primary objective of rival leaders.

My discussion thus far has focused on the assumptions that undergird Krause's explanations for nationalist movement behavior. I turn now to considering one of the main outcomes he seeks to explain: the success or failure of nationalist movements. Krause argues that hegemonic movements have a clear advantage over either united or fragmented movements. Hegemonic movements are able to focus their energies on victory; they do not have to make compromises with allies and they do not face threats from challengers. All their efforts are directed at the external enemy, not internal enemies.

Krause's claim that hegemonic groups are better able to pursue victory is plausible and intuitive. But does the single-minded pursuit of victory actually produce victory? Is hegemony necessary for success? It makes sense that a fragmented movement may have difficulty succeeding, if there are no leading figures with whom the state can bargain, or if there are actors engaged in spoiling negotiations.[5] The structure of the nationalist movement itself is, however, only one factor that affects the outcome. It is not only the relative balance of power among nationalists that matters, but also the balance of power between the nationalists and the state. Krause writes that "Palestinian groups knew they needed hegemony to succeed, but their desire for power kept them largely fragmented" (15). This claim invites consideration of counterfactuals: if one Palestinian group had consolidated hegemony at some point, would Palestine have a state today? When the Israeli position is taken into account, it does not seem likely that fragmentation is the reason for Palestinian statelessness. The state itself can also cause fragmentation, through co-optation and repression, making fragmentation a potential consequence, rather than cause, of failure.

The FLN in Algeria post 1958 is a prime example of a hegemonic movement that succeeded in gaining independence. There are other important explanations for its victory, however. The FLN in the late years of the war had eliminated its rivals or enfolded them within the FLN structure; it succeeded in becoming the sole voice of Algerian nationalism. Militarily, however, it faced a formidable foe in the French army, against whom it suffered multiple defeats until the FLN's effective force was isolated in Tunisia. The war was not

[5] For examples of work on spoiling, see Kathleen Gallagher Cunningham, "Divide and Conquer or Divide and Concede: How Do States Respond to Internally Divided Separatists?" *American Political Science Review* 105:2 (May 2011): 275-297. Andrew Kydd, and Barbara F. Walter, "Sabotaging the Peace: The Politics of Extremist Violence," *International Organization* 56:2 (2002): 263-296.

won through military means. It was a political victory not only between the FLN and France, but within France itself. The end of the Algerian war has been subjected to lengthy historical debates, but it is clear that an important part of the story is the battle between political actors and the public in France over whether to continue supporting the settlers in Algeria against the native Algerian population.[6] French President Charles de Gaulle was nearly overthrown in a coup for granting Algerian self-determination. He ultimately ended the war despite the objections of a colonial army that considered itself on the verge of crushing the FLN. This victory might have only been possible under FLN hegemony, but it is difficult to know. France might also have withdrawn from Algeria even if the battles between the FLN and its rivals had continued throughout the war. It is reductive to attribute the victory to FLN dominance, and the account here would have been strengthened by taking into consideration not just the attributes of the nationalist movement, but also what was happening on the opponent's side of the conflict.

Krause's claim that FLN hegemony was causal in producing victory rests on a comparison of how well Algerian groups fared over time – his empirical strategy is to compare campaigns from 1944 through independence in 1962. Krause writes "the internal dominance of the FLN achieved the victory that a smaller total number of united groups backed by a more mobilized movement operating within the same norm of decolonization had failed to secure a decade earlier" (101) The causal importance of the movement's structure is not easy to isolate and his task it thus tough. But since the goals of the groups that preceded the FLN were not uniformly aimed at independence, it is possible that they did not fail because there was no hegemon, but because they were still experimenting with other solutions besides independence. If they were not seeking independence, it makes sense that they did not achieve it.[7]

Movement hegemony may be neither necessary nor sufficient for victory. Algeria's neighbor, Morocco, achieved independence from France precisely when the nationalist movement was most fragmented. In 1952, France decapitated the leading nationalist movement and in 1953, it exiled the sultan of Morocco. These actions led to movement fragmentation, and a rural insurgency and urban guerrilla campaign broke out. In response, France brought back the sultan and granted independence.[8] France granted victory not because the movement was fragmented or hegemonic, but largely because of its own political imperatives: France prioritized retaining control of Algeria over Morocco, and as the Algerian War began, it made sense to withdraw from Morocco and Tunisia to focus on fighting for Algeria. This example, which falls outside the cases Krause address, illustrates the need to consider the calculations of states when trying to explain why nationalist movements succeed or fail.

In reviewing this book, my aim has not been to argue that Krause's argument is wrong. Krause is to be commended for drawing our attention to the very real struggles for power and position that occur among

[6] Scholarly work on the denouement of the Algerian war includes: Matthew A. Connelly, *A Diplomatic Revolution: Algeria's Fight for Independence and the Origins of the Post-Cold War Era*. Illustrated edition (New York: Oxford University Press, 2003); John P. Entelis, *Comparative Politics of North Africa: Algeria, Morocco, and Tunisia* (Syracuse: Syracuse University Press, 1980); Horne, *A Savage War of Peace*; Todd Shepard, *The Invention of Decolonization: The Algerian War and the Remaking of France* (Ithaca: Cornell University Press, 2006).

[7] Further, it is disputable that the movement was more mobilized before the war than it was during the war.

[8] Adria Lawrence, "Triggering Nationalist Violence: Competition and Conflict in Uprisings against Colonial Rule," *International Security* 35:2 (October 2010): 88-122.

state-makers in a wide variety of settings. My aim is not to call this claim into question, but to suggest that nationalist struggles are not just a clash of egos: political issues matter too, for both intra-movement competition and the achievement of victory.

Rebel Power opens up a very welcome debate about the value of parsimony and the relationship between theory and history. This is a debate well worth having, not just in reference to Krause's work, but within the larger literature of historically-oriented scholarship in the discipline. Scholars can reasonably disagree over whether to prioritize theory or history, and no theory can be expected to satisfactorily explain every aspect of complex political interactions. Further, Krause's empirical approach allows him to make an important contribution to the literature on rebel group fragmentation: by process tracing four very complex cases, he is able to capture changes in the capabilities of different organizations over time. These changes in the relative power of different actors is missed in large-N treatments that mechanistically count the number of rebel groups without reference to their size or popularity.

Krause's approach also offers an advantage over single case studies, providing a fruitful middle ground by taking on four important and lengthy nationalist struggles. This approach is ambitious, since the historiography on each of these cases is vast. It is much easier to discuss one case, as I have here. Perhaps experts on his other cases may have similar quarrels with aspects of his historical analysis, but it is worth keeping in mind that by addressing four cases, Krause offers a breadth that regional and single-case treatments lack. He successfully demonstrates the importance of power considerations across this diverse set of cases, and his theory may well travel to many other settings, including rebellions that are not nationalist in nature. This is the mark of an important scholarly work. *Rebel Power* is well worth reading and discussing, for its theoretical and empirical claims, as well as for the scholarly debates it inspires.

Review by Wendy Pearlman, Northwestern University

Why do some national movements achieve states while others do not, and why do they exhibit such varied behaviors in the course of these struggles? In *Rebel Power: Why National Movements Compete, Fight, and Win*, Peter Krause argues that the key to explaining these puzzles lies in the balance of power among a movement's constituent groups. Comparative strength determines whether the movement is internally competitive or noncompetitive, which in turn drives groups' strategies in the two-level games that they play with both each other and the state from which they seek independence.

Krause's Movement Structure Theory (MST) distinguishes between three types of movements: hegemonic movements dominated by a single group, united movements in which all of the two or more significant groups form an alliance, and fragmented movements in which two or more significant groups are not are in an alliance. Noting how susceptible alliances are to collapse, Krause argues that they are not nearly as important as the ranking of groups according to their relative power. He crafts a perceptive typology distinguishing between four types of groups. *Hegemons* are the strongest groups in hegemonic movements in which there are no significant challengers. *Leaders* are the strongest groups in united or fragmented movements with at least one *challenger*, which is a group that possesses at least one-third the strength of the leader. *Subordinates* are weak groups that do not possess at least one-third the strength of the leader.

These various groups engage in 'wars of movement' to achieve nationalist objectives and 'wars of position' for dominance within the movement. As all groups seek to maximize power, the key question for any movement is how its structure brings groups' interests in these two wars either to align, encouraging a coherent commitment of resources and action toward achievement of victory, or contradict, such that actions that advance groups' competition with each other are counterproductive vis-à-vis the state.

Krause's answer for the best structure to mobilize the power of the former and avoid the pitfalls of the later is hegemony. Hegemonic movements are most likely to be successful not because hegemons subordinate their parochial interests to the public good of movement victory, but because they know "that the benefits of victory accrue disproportionately to the leading groups of a national movement" (22). In a smart and persuasive application of prospect theory, Krause reasons that hegemons operate in a realm of gain, which leads them to be risk averse and therefore to shun escalatory violence and seek negotiations that aid strategic progress. Inversely, challengers operate in a realm of loss; understanding that they are unlikely to share in the spoils of nationalist victory, they accept risks and "gamble for resurrection," undermining the likelihood that victory is achieved (26-27).

Four well-written, thoroughly-researched empirical chapters use MST to explain group behavior and movement outcomes through qualitative analysis of the cases of the Palestinian, Zionist, Algerian, and Northern Ireland national movements. Focusing on "tight comparisons of most similar and most different case for the same groups over time" (175), Krause cleverly explains why the same group both engaged in escalatory behavior at one point in time (when it was a challenger) and cracked down on others' escalatory behavior at another (after it had consolidated hegemony). For movements that did not achieve hegemony, he shows why groups shifted their behaviors as they rose or fell in the movement hierarchy, without changing the strategic outcome of the movement as a whole. Along the way, Krause insightfully draws out various implications and extensions of MST for other issues of import, such as states' design counter-insurgency strategies.

Rebel Power delivers on Krause's goal to craft a parsimonious theory that offers admirable leverage for explaining puzzling patterns for two important dependent variables: movement outcomes and group behavior. It offers a powerful analytical framework and amply demonstrates its utility in expertly documented case studies that showcase archival research and more than 150 interviews conducted during 23 months of fieldwork in nine locations over eight years. That staggering amount of research yields convincing evidence not only that groups behaved as MST would predict, but also that they did so for the reasons that MST identifies. Supporting this demonstration of causal processes is excellent use of counter-factual thinking which, at various junctures throughout the book, challenges reader to imagine how a different movement structure would have result in different outcomes (for example, on Algeria, see 122-23, 127). The concluding chapter further demonstrates the importance of Krause's work by clearly enumerating its implications for policy. Among them, this research should be credited with hammering a much-welcome final nail in the coffin of discourse that categorizes groups as 'moderates' or 'radicals' rather than attending to their incentives as rational political actors.

With these and other strengths, *Rebel Power* is no doubt a significant contribution to literature on nationalist movements, with broad applications for social movements, political violence, and intra-state conflict, in general. Still, like any ambitious work, it invites debate. In this spirit, I suggest four general nodes of possible discussion.

First, though the work is empirically rich, that empirical richness is uneven. In some sections, such as those on the Zionist movement from 1940-1949 (75-99) and Algeria from 1954-1956 (107-123), Krause skillfully offers an abundance of fascinating and convincing detail to make a formidable exhibition of causal mechanisms. In other sections, a much broader historical sweep offers breadth with less depth. In particular, the chapter on the Palestinian national movement, the book's shortest case study, leaves many questions unanswered. It is understandable that space limitations prohibited examination of the movement's entire history. Yet the choice to discuss only campaigns from 1965-1970 and 1986-1993, while including a summary table indicating movement structure and outcomes during six additional campaigns (44, 176-79) might leave some readers eager for more explanation. For example, how does MST explain what is coded as Fatah's simultaneous use of escalating and restraining violence from 1971-1976, 1985, and 2001-2016? Why is 1994 deemed a "failure" while the periods 1986-1993 and 1995-2000 are judged "moderate successes"? Beyond this, Krause's categorization of the first Intifada as a time of Fatah hegemony (55) will seem odd to those of us who believe that the first Intifada's 'moderate success' was most attributable to it popular unity across and beyond factions.

Second, all measurement choices entail trade-offs, and those in this book are no exception. Krause's measure of the independent variable is tremendously impressive in its conceptualization, operationalization, and the heroic amount of work required to implement it through data collection and coding. The measurement of the dependent variable is, however, invites questions. Krause adopts a scaled assessment of victory or failure at the end of each campaign or the point at which the movement structure or hierarchy changes significantly. This innovative periodization offers a creative, useful alternative to conventional post-hoc identification of 'tuning points' (36). Yet, in breaking a continuous history into bounded chunks of victory or failure, it risks underestimating the degree to which movement developments in one period crucially determine that movement's struggle in a subsequent period. Krause acknowledges this in the book's final pages when he notes, as a possible question for future research, the possibility that short-term failures lay the foundation for future success (193).

This might be insufficient for addressing what can be read as a deeper problem related to the role of time in evaluating the dependent variable. For example, the Zionist movement is judged to have failed from 1920-1939, even though the movement's political, economic, demographic, institutional, territorial, and military developments during that era were essential for its subsequent success. Similarly, Ireland's Easter Rising is judged a failure, even though its aftermath "contributed to the emergence of the only hegemonic national movement for the next one hundred years" (133). And to return to the Palestinians, Krause argues that the 1967 war "cannot be understood as anything but a strategic failure" (48), though many instead see it as critical in the subsequent meteoric rise of an independent national movement embodied in the PLO.[1] These alternative readings of Krause's cases present a question: if movement action in one stage fails to achieve nationalist goals, but is fundamental in growing the nationalist movement that succeeds in a subsequent stage, is it really a 'failure,' or is it instead a critical step in a long struggle?

Third, Krause can be read as sometimes overstating the power of his theory. Late in the conclusion, he grants that "hegemony alone is certainly not sufficient for movement success nor are unity and fragmentation sufficient for movement failure" (191). The empirical chapters, however, often employ less qualified language in claiming that it was hegemony or lack thereof that "drove" (40) or "was the key" to (95) or "explains" (171) outcomes. Indeed, the language used to describe the theory's explanatory power is no less bold in the conclusion's brief look at the cases of the American Revolution, Vietnamese national movement, and Eritrean national movement (193-194).

In this respect, Krause's resoluteness in making the case for MST brings admirable focus and cohesion to research that spans centuries and continents. Yet it also misses opportunities for nuance. First, in consistently upholding the primacy of internal drivers of movement success or failure, the analysis at times seems to disregard the interests and capacity of a movement's adversary. It would not have undermined the significance of MST to have devoted some space to the ways that strategic interaction with the state might matter in ways not exclusively filtered through movement structure. Second, at some points the books appears to sustain MST even when it might not be the best lens through which to view a particular historical event. For example, Krause argues that "the fragmentation of the Zionist movement and its subsequent divided strategy contributed to the British issuing a damaging White Paper in 1939" (75). This assertion seems to overstate the role of movement structure in driving a decision instead typically attributed to Great Britain's recognition of the unviability of alternative resolutions in the wake of three years of Palestinian rebellion, its interest in not further alienating Arab and Muslim opinion, and its hope for stability in the Middle East given the Second World War brewing on the horizon.[2] Third, in insisting that change in a group's behavior is strictly derivative of change in its rank in movement hierarchy (i.e. on Northern Ireland, see 132), he might grant too little credence to other causes of preference change, including learning, fatigue, rethinking, attentiveness to shifts in public opinion, response to changing geopolitical considerations, etc. Change is to be expected. It

[1] See the tellingly titled, Yezid Sayigh, "Turning Defeat into Opportunity: The Guerillas after the June 1967 War." *Middle East Journal* 46:2 (Spring 1992): 244-265.

[2] These explanations come to the fore in mainstream histories of the Israeli-Palestinian conflict, such as Ian J. Bickerton and Carla L. Klausner, *A History of the Arab-Israeli Conflict*, 6th ed. (Boston: Prentice Hall, 2010), 54; Mark Tessler, A *History of the Israeli-Palestinian Conflict*, 2nd ed. (Bloomington and Indianapolis: Indiana University Press, 2009), 245; Charles Smith, *Palestine and the Arab-Israeli Conflict*, Seventh Edition (Boston and New York: Bedford/St. Martin's, 2010), 142.

would be odd if a nationalist organization remained uniformly militant over time; movement structure is a powerful engine of such change, but it is it always the dominant one?

Finally, the book might have benefited from a differently tailored engagement with relevant literature. Krause proposes that major alternative explanations for group behavior are its ideology, age, and leadership. These points are presented with little elaboration in the introduction (7), and then are so quickly disproven when they appear in the case studies, that one wonders if they were ever truly viable counter claims.

As alternative ideas with which to engage throughout the book, it might have been more useful to say more about how it relates to the existing research program on fractionalization in insurgent groups and civil wars. Krause motivates his study in part with the argument that "most scholars who have analyzed the success of movements and the use of political violence have employed a framework of a unitary non-state actor" (7). Years of research, however, has advanced the same critique.[3] It is thus no longer necessary, or sufficient, to argue that non-state actors are not unitary. Instead, researchers need to lay out what prior work does and does not explain about non-unitary dynamics, and identify how they build upon it.

Rebel Power duly cites many of these works. However, it might have gone further in using them as an anchor for framing its own contribution. Where it engages scholarship expressly focused on movement fractionalization, it levies two critiques: that this scholarship discounts intra-movement balance of power, and that it fails to measure movement unity/fragmentation systematically or does so simply by counting the total number of groups in a movement (8). These critiques struck me as unnecessarily dismissive. The latter point could have been supported by more discussion of the varied ways that qualitative and quantitative scholars have assessed intra-movement divisions, what they offer, and where they fall short. The former critique risks making scholarly rivals out of potential allies. I doubt that students of movement structure (and I am one of them) would contest the importance of the relative power of a movement's groups. Yet in focusing on other puzzles or implications of movement structure (because there are many), they have simply not put balance of power at the center of their investigations.

Krause fills that gap. He has made an outstanding contribution by dissecting movement hierarchy and showing us how it matters. But this does not seem to me to invalidate prior scholarship on fractionalization as much as enrich it with a novel layer of analytical and empirical refinement. A more detailed, nuanced exploration of how others have conceptualized, theorized, and operationalized movement fractionalization might have demonstrated precisely in what ways MST affirms existing models, is distinct from but compatible with them, or leads to contradictory understandings or predictions. This would have been more generous toward existing research without taking away from the genuine importance of Krause's innovation.

Without a doubt, *Rebel Power* advances existing work. More than any other work before it, Peter Krause has shown in this book that in national movements, as in other realms of politics, economics, and social life, "where you stand depends on where you sit." Students of national movements will not, and should not, look at national movements in the same way again.

[3] For overviews, see the special issue, "Nonstate Actors, Fragmentation, and Conflict Processes," *Journal of Conflict Resolution* 56:1 (February 2012); Kathleen Gallagher Cunningham, "Fractionalization and Civil War," *Oxford Research Encyclopedia* (July 2017), DOI: https://doi.org/10.1093/acrefore/9780190228637.013.337.

Review by William B. Quandt, University of Virginia, Emeritus

There is much to like to Peter Krause's book, *Rebel Power*. But that does not include the rather misleading title. Not one of the four movements that he studies can really be considered a rebellion, at least as I understand the term. They were, as Krause says in the book, broad-based movements that sought national independence. 'Rebel' is usually the dismissive word used by the authorities against which such movements develop. A small point, perhaps, but titles, I believe, should tell us what the book is about. The subtitle is a more accurate guide to the book's content.

With that minor point out of the way, let me say that I generally agree with Krause that it is important to look at the often shifting balance of power within these movements if we want to understand their tactics and, eventually, their prospects for success. He usefully distinguishes among hegemons, challengers, and minor players within the movements, and notes that it is usually the up-and-coming challengers to the dominant player in the nationalist movement game that seek to escalate the level of violence that most of these movements eventually resort to. The reason is twofold: the challengers have more to gain and less to lose by seeming to be more militant in asserting nationalist claims; and when the repression comes, as it will, it is the larger group that will be disproportionately blamed by the governing authorities.

Apart from the propensity to resort to violence, the other dependent variable that Krause examines is success or failure of the movement in achieving its national goals. Simply put, he argues that success is much more likely when a movement is under the control of a hegemon. By contrast, coalitions that allow the subgroups to maintain their own identity, which he calls united movements, rarely succeed, and deeply divided movements have little chance of achieving their goals. In general, this is probably accurate, but between success and failure of a nationalist movement there exist many 'campaigns' where an ostensible setback sets the stage for a subsequent step forward. When reading the book I often felt that the success/failure dichotomy was too sharply drawn, as I will try to show in looking more closely at specific cases.

Krause is a structuralist and I think he makes a good case that movement structure can account for a great deal of what we want to understand. But it cannot explain everything, and other factors, such as individual leadership and the nature of the opponent, have to be taken into account.

The four cases that Krause studies in depth are Algeria, Ireland, Zionism and the Palestinian nationalist movement. Algeria and the Zionists succeeded under hegemonic movements and against formidable adversaries. The Irish nationalists were partially successful, and the Palestinians have mostly failed. Two of these movements, Algeria and the Palestinians, I have studied quite carefully, and I would like to use them to test the validity of Krause's model.

Let me start with the Algerian case, which incidentally became something of a model for the Palestinians in the 1960s. Algeria saw the emergence of two distinct nationalist tendencies in the 1930-1940s. By the early 1950s, a younger generation was turning away from both of the established parties, in frustration over their lack of success and in opposition to the highly personalized and authoritarian leadership of one of the groups. The *Front de Libération Nationale* (FLN) was meant to be an alternative to both of the legal parties. It was also wedded to the notion of armed struggle and to a collective leadership. Members of the old parties were recruited into the FLN, but only if they foreswore their prior allegiances. In fact, all but one of the pre-FLN groups was eventually dissolved. So the FLN became, in Krause's words, a hegemonic movement. And, eventually, the FLN was able to achieve full independence.

I have a few quibbles with the historical record as put forward by Krause, although his research shows a very good command of the existing sources. One important moment in the development of Algerian nationalism came at the very end of World War II. At the time there were two significant groups that claimed to represent the demands of Algeria's Muslim population. The more assertively nationalist one was led by Messali Hadj; the more legalist, moderate one was led by Ferhat Abbas. Krause sees Messali as a challenger, willing to provoke violence, whereas Abbas, as the front runner, was opposed to violence, knowing that it would be followed by harsh repression. My own sense from talking to Algerians about this period is that the balance between the two factions was not so obviously in Abbas's favor. Messali was, however, by all accounts the more radical of the two leaders. But does that explain the bloody events that began to unfold on May 8, 1945? Was this really an example of Messali telling his partisans to rise up in revolt, or was it more a case of Algerians from many backgrounds expecting that the end of the war in Europe would lead to some real changes in Algeria itself, and the French determined to show who was in charge?

However the violence began, it is clear that France cracked down in a brutal way. For a whole generation of younger Algerians, this was the moment that they concluded that neither Messali nor Abbas could bring about independence by playing the electoral game or working within the framework of French legality. Only armed struggle would succeed, as the Viet Minh were eventually to show in another French colony.

Krause is right that many followers of Abbas believed that Messali deliberately instigated the violence that broke out in Setif and Guelma and elsewhere in May 1945. But there is still a great deal that is not known about these events and about Messali's role. It is not so clear to me that this is a good case of a challenger resorting to violence to unseat the leading position of a rival.

Another seminal moment in the Algerian struggle came in 1956-57, with the Battle of Algiers, as it is popularly known. This was the FLN's attempt to bring the revolution into the capital city and to recruit a network of activists there who could carry out operations against the French authorities and, eventually, against French civilians as well. In military terms it ended in failure for the FLN. Most of the leaders were killed or arrested. In Krause's terms, one of the reasons for failure was that the FLN was not yet a fully hegemonic movement. Yet this so-called failure was precisely the moment when many Algerian bystanders realized that they had to choose sides and they gave their active, or at least tacit, support to the FLN. The visible success of this campaign was the week-long strike called by the FLN in January 1957. The entire Casbah was shut down in solidarity. This was a turning point for the FLN on the national and international stage, even if the FLN was eventually forced from the capital.

One final point on the Algerian case. France was already in the process of disengaging from its colonies by 1956. Morocco and Tunisia had been given independence. True, many French believed that Algeria was fundamentally different from these other two colonies, but French opinion was deeply divided by 1957-1958. This was the moment when General Charles de Gaulle arrived on the scene as France's president. And de Gaulle certainly understood from early on in his presidency that Algeria could not remain an integral part of France. He tried to find alternatives to negotiating with the FLN, he toyed with the idea of partition, but in the end he accepted the logic of ceding Algeria in order to protect France itself. And this is important. The French, like the British, could lose a colony and remain a major world power with a long-standing identity of its own. The million or so Europeans in Algeria had a homeland that they could return to if they were forced to leave Algeria. As painful as decolonization no doubt seemed to many British and French citizens, it did not place their own national existence at risk. When we turn to the Palestinian movement, we will see why this mattered. Their opponents, the Zionists/Israelis, literally felt that their national existence would be threatened

if the Palestinians were able to achieve their national goals. The conflict was viewed in zero-sum terms, in a way that was not so obviously the case in other cases of anti-colonial movements.

The Palestinian case offers many interesting comparisons with the Algerian one. For one thing, many Palestinians tried to model their movement on the FLN. But the Palestine Liberation Organization never became a hegemonic movement like the FLN. Krause is correct in noting this, but I think the reasons for the PLO remaining, to this day, a coalition of distinct groups, needs further examination. Unlike Algeria, the Palestinians, by the time the most important phase of their national movement had begun in the 1950s-60s, were a dispersed people, with sizable parts of the community living under Israeli control, in Jordan, under Egyptian control in Gaza, in Lebanon, or in Syria. This made anything like a unified movement very difficult to achieve. Indeed, some Palestinians accepted the idea that they should merge their identity into a larger Arab identity in the hope that the Arab states could lead the campaign to restore Palestinian rights over their ancestral lands. On their own, many Palestinians believed after 1948 that they were too dispersed and fragmented to succeed against the Zionist state.

The Arabist narrative was discredited by the outcome of the 1967 war. Within a short period, several distinct Palestinian groups were competing to lead the movement of liberation. By contrast with the FLN, which received some support from other Arab countries, but never became heavily dependent upon any one of them, different Palestinian groups counted on support from Syria, Iraq, Saudi Arabia, Egypt and Jordan. Eventually Fatah emerged as the largest group, but its leader, Yasir Arafat, never was able to eliminate all of the smaller groups that enjoyed the support of Arab governments—and on occasions acted as their agents.

On at least three occasions, these smaller groups, as Krause would predict, led the PLO into violent confrontations that left it weaker. In September 1970, it was the leftist groups aligned with Syria that pressed for a confrontation with the Jordanian regime. The result was the expulsion of most of the armed Palestinian factions to Lebanon.

Then in 1982, it was a very marginal Palestinian faction, the so-called Abu Nidal group backed by Iraq, that tried to assassinate the Israeli ambassador in London, giving the Israelis the pretext they were looking for to invade southern Lebanon and to oust the PLO from the country. The Iraqi motivation at the time was to try to entangle the Syrians in a war with Israel in Lebanon, and the Palestinians were caught up in this larger inter-Arab conflict with little way of avoiding defeat. This time the PLO was forced to leave Lebanon and to regroup far away in Tunisia. Iraq's reason for unleashing Abu Nidal had much more to do with the ongoing war with Iran, backed by Syria, than anything having to do with Palestinian national goals.

The third time that a small Palestinian group aligned with an ambitious Arab state was able to affect the PLO was in mid-1990. An Iraqi-backed group led by Abul Abbas carried out an ineffectual raid on Israel. The problem for PLO leader Arafat was that Abul Abbas's group was a member of the PLO and the PLO had recently adopted a stance of renouncing terrorism and had entered into a dialogue with the United States. Iraqi President Saddam Hussein, as he was planning his invasion of Kuwait, apparently wanted to be able to play the Palestinian card to win over Arab nationalist opinion, and thus he wanted Arafat's dialogue with the United States to come to an end -- which it did after the Abul Abbas raid. When the invasion of Kuwait took place a few months later, Arafat was seen by the Bush administration, and by many Arabs, as in the Iraqi camp, and it took some time for him to recover from that setback.

These three examples are provided to show how difficult it was for the PLO leadership to achieve the kind of hegemonic control that the FLN had. The broader regional structure was stacked against them. And they confronted an adversary, Israel, that was determinedly opposed to the maximum claims of the Palestinians, and was even reluctant to consider the more modest outcome of a divided Palestine—the so-called 'two-state solution'—that might have been considered at least a partial success for the PLO. In addition, Israel enjoyed the unqualified backing of the most powerful country in the world, the United States. Even if the PLO had managed to get its act together, to discipline its ranks, to eliminate its minor rivals, it is hard to imagine that an Algerian-style outcome was within its reach.

In sum, Krause has certainly advanced the comparative study of nationalist movements. He has focused on key variables that help to explain tactics and outcomes. But at times the narratives become a bit too deterministic; at times they seem to place too little emphasis on the nature of the opponent; and at times one would like to hear more about leadership and the role that it can play, even in a highly structural explanation of movement dynamics. With these few reservations, I nonetheless feel that Krause has made a significant contribution and that future scholarship will be enriched by the framework that he offers.

Author's Response by Peter Krause, Boston College

Thank you to H-Diplo, Tom Maddux, and James McAllister for this wonderful opportunity to present a discussion of my book in this great public forum. I hope that all who are interested in nationalism, social movements, Middle East politics, insurgency, and political violence can benefit from this healthy exchange on the arguments, methods, and evidence in *Rebel Power*. H-Diplo assembled an outstanding group of reviewers who could not be more qualified to assess the theory and empirics of my book. Thank you to all of them for taking the time to read it—a true honor—and to comment in such detail.

I am further honored to receive their praise and positive reviews, especially from such prominent scholars whose work I both build on and challenge. Adria Lawrence states that my Movement Structure Theory (MST) "is written with precision and clarity," while Wendy Pearlman agrees that it "offers admirable leverage for explaining puzzling patterns for two important dependent variables: movement outcomes and group behavior." Kristin Bakke claims that "both the argument and evidence in *Rebel Power* are compelling," and Pearlman concludes that my "staggering amount of research yields convincing evidence not only that groups behaved as MST would predict, but also that they did so for the reasons that MST identifies." As for the broader field, William Quandt agrees that *Rebel Power* is a "significant contribution" that "has certainly advanced the comparative study of nationalist movements," and that "future scholarship will be enriched by the framework that [I offer]."

I am also grateful to receive the reviewers' careful critiques and have the opportunity to respond to them here, which helps us to more clearly define both the boundaries of debate and the frontiers for future research. I grappled with most of the challenges raised by the reviewers while researching and writing the book, and this exchange allows me to further explain my choices and the logic behind them. I agree with a number of the points raised by the reviewers. Some are the result of purposeful choices of emphasis, shortcomings that are unavoidable when analyzing the internal politics of four national movements in significant depth. Even for those critiques on which we continue to disagree, the process of thinking through and returning to my fieldwork notes, archival documents, and the reviewers' own work has improved my understanding and presented new lines of inquiry and debate for future projects. I will not go point by point through every challenge, but instead will focus on the most significant in order to highlight areas of unrecognized agreement, push back on what I consider to be misconceptions, and in some areas, analyze clear differences of opinion and the logic and evidence upon which they are based.

Rebels, Nationalism, and Importance of Definitions

Rebel Power asks two big questions: Why do some national movements achieve independent states, while others do not? And why do some nationalist organizations within those movements opt for violence or nonviolence, and for negotiation or spoiling in the course of their struggles? Before answering these questions, it is important to define what a national movement is—and who counts as a member—in order to delineate the scope of the study and lay the groundwork for my theory of group behavior and movement effectiveness, which I do at the outset of Chapter 1.

Quandt is the first to challenge me here, suggesting that the title for my book is misleading because none of the participants in my cases count as "rebels," which he claims is a "dismissive word used by the authorities." On the contrary, the word "rebel" is defined as an individual who refuses allegiance to, resists, or rises in arms in opposition to the established government. Every organization and individual I include in my study

qualifies: all practiced civil or armed resistance against their existing governments as they struggled for new states. Perhaps as happens with other epithets turned badges of honor, my interviewees happily considered themselves rebels and supported the book's title in their discussions with me. Furthermore, the term 'rebel' has been widely used for years by scholars to discuss the exact type of struggles I analyze here, such as in the growing subfield of "rebel governance."[1] Nonetheless, I strongly agree with Quandt that accurately defining one's terms is essential for engaging with existing scholarship and determining external validity, which is why I open my first chapter with a Venn diagram and discussion of the similarities and differences between national movements, self-determination movements, social movements, insurgencies, and revolutions.

While Quandt suggests that my nationalists cannot be rebels, Lawrence questions whether it matters if my rebels are nationalists, writing, "nationalism plays virtually no role in the argument." As I state, "National movements are distinct in that their social solidarity is based on national identity and their common purpose is political autonomy" (3). First, my entire argument is based on the central goal of national movements: the achievement of an independent state. The fact that this objective carries with it lumpy, private goods of office, wealth, and status drives both how nationalist groups behave and when national movements succeed. If these were instead non-national movements aiming for gradual increases in legal protections for the environment, their objective would carry neither the significant political benefits for member groups to capture nor the lumpy, winner-take-most distribution of these benefits to incentivize group dominance at the moment of victory.

Second, nationalism is essential to the competition among groups at the core of my argument and analysis. Nationalist groups may fight with one another, but (and because) they agree that the others are members of their nation and future desired state. Therefore, fellow nationalist groups are marked as peer competitors for leadership in ways that non-national competitors are not. As I detail in a chapter of a new co-edited volume, *Coercion: The Power to Hurt in International Politics*, from the mid-1970s through the early 1990s, robust insurgencies ravaged Ethiopia as numerous groups fought and sought to overthrow the ruling Derg regime.[2] Among these insurgent groups, the Eritrean People's Liberation Front (EPLF) viewed the Eritrean Liberation Front (ELF) as a peer competitor from within its nation of Eritrea, which had been annexed by Ethiopia in 1962. The EPLF perceived the Tigrayan People's Liberation Front (TPLF) differently, because the TPLF sought to overthrow the Ethiopian (Derg) government but was not nationally Eritrean. The EPLF ultimately eliminated the ELF and allied with the TPLF. This led to the EPLF achieving an independent Eritrea in 1993 after it became the movement hegemon, while the TPLF was able to lead the way in toppling the Derg and then ruling Ethiopia. Had the TPLF been an Eritrean nationalist group or the EPLF a non-nationalist group simply seeking to overthrow the Derg, the perceptions of potential rivals and the outcome of the conflict would have been significantly different. Nationalism therefore plays the central role in determining which

[1] For a few great examples of scholars using the term 'rebel' to describe similar phenomena, see Reyko Huang, *The Wartime Origins of Democratization: Civil War, Rebel Governance, and Political Regimes* (Cambridge: Cambridge University Press, 2016); Zachariah Mampilly, *Rebel Rulers: Insurgent Governance and Civilian Life during War* (Ithaca: Cornell University Press, 2015); Paul Staniland, *Networks of Rebellion: Explaining Insurgent Cohesion and Collapse* (Ithaca: Cornell University Press, 2014).

[2] Peter Krause, "Coercion by Movement: How Power Drove the Success of the Eritrean Insurgency, 1960-1993," in Kelly Greenhill and Peter Krause, eds., *Coercion: The Power to Hurt in International Politics*, (Oxford: Oxford University Press, 2018), 138-159.

movements and groups are included in my analysis, their perceived rivalries among each other, the nature of movement and group objectives, the dynamics of their behavior, and the definition and likelihood of success.

Measuring and Pursuing Power and Success

I argue that the balance of power is key to explaining both group behavior—where you stand (on violence and victory) depends on where you sit (in the movement hierarchy)—and movement outcomes. The reviewers are largely supportive of the theory. Quandt writes, "I generally agree with Krause that it is important to look at the often shifting balance of power within these movements if we want to understand their tactics and, eventually, their prospects for success," while Pearlman calls it "a smart and persuasive application of prospect theory." Nonetheless, the reviewers are right to question how I measure power and its distribution within a movement.

As detailed in Chapter 2, I measure the strength of a group by the size of its membership, the amount of its funding, and the extent of its popular support (captured through public opinion polls and/or seats in common political institutions). Bakke raises an important issue: Why are weapons absent from my operationalization, and how might armed and unarmed power lead to different dynamics? It would be easier to compare only armed groups or only political parties, but national movements are generally composed of both. Furthermore, some groups have both political and armed wings, while others transition from one to the other at various points in their struggles.[3] Finally, the groups themselves are constantly evaluating the strength of other groups regardless of their type, leading me to select proxies for strength that the vast majority of groups, armed or unarmed, would aim to maximize. Members can be used to fight or to canvass, money can be used to buy weapons or campaign ads, and popular support can be used to generate intelligence or votes.

The measures I provide in the book and appendices match well with the perceptions of groups at the time, as I discovered in my interviews. For instance, the Social Democratic and Labour Party (SDLP) saw the Irish Republican Army (IRA)/Sinn Féin as a growing rival long before they competed in elections. I do think that group type can make a difference, as Bakke suggests, although I also found that groups often moved between types when push came to shove: The Jordanian Communist Party formed an armed wing after claiming it never would; Fatah and various IRA/Sinn Féin iterations ran for office and negotiated after attacking others for years for doing the same. Furthermore, even though the SDLP did not turn to violence, it did escalate its tactics by turning to absentionism once Sinn Féin started to directly compete for votes. It seems likely that the possession of arms can be a positive or a negative depending on the context, such as the extent of state repression and popular support for violence. Group type may also have an impact on their abilities to capture the spoils after victory—armed groups may perform better in subsequent civil wars and political parties in elections—an issue I am examining in my next book.

The reviewers are thus relatively supportive of the clarity and logic of the theory, but they take greater issue with its parsimony. At one point or another in the book, the reviewers argue that some factor matters more than I suggest it does, which is to be expected given that I analyze 40 groups within 44 campaigns of four national movements over 140 years of struggle. They point to factors like ideology and leadership to explain

[3] Aila Matanock and Paul Staniland, "How and Why Armed Groups Participate in Elections," *Perspectives on Politics*, forthcoming; Benjamin Acosta, "From Bombs to Ballots: When Militant Organizations Transition to Political Parties," *Journal of Politics* 76:3 (July 2014): 666-683.

group behavior, or the enemy state's strength, strategy, and commitment to the territory to explain movement success. I agree that in certain cases, these factors are significant or even primary causes of actions and outcomes. Indeed, this is why I wrote in the conclusion that "by attempting to explain so much with so little, MST also makes a number of incorrect predictions," which I then highlighted in subsequent sections (182). That chapter contains summary tables that show, for example, that 45% of the challenger groups in my study restrained violence and 24% of leading groups escalated it, and that 33% of united movements achieved some degree of success. These represent a number of cases that my theory cannot explain, but that the other factors suggested by the reviewers likely do. Nonetheless, my predicted trends hold by a large margin—91% of leading groups restrained violence and 52% of challengers escalated it—and would do so even if a few cases were miscoded (which I am confident is not the case).

It can be difficult to isolate and weigh the impact of competing factors, which is why I stated that the best tests of my theory and its competitors are tight comparisons of most-similar and most-different cases for the same groups over time. So, for instance, when Lawrence argues that ideology may trump power position as an explanation of group behavior, I can point not only to the clear trends across the 122 group-campaigns summarized in Chapter 7, but also how Fatah and the Arab Nationalists Movement/Popular Front for the Liberation of Palestine (ANM/PFLP) dramatically swapped approaches to escalating and restraining violence as their relative power changed from 1965-1970, but their ideology and leadership remained the same (43-54). Or I detail how Cumann na nGaedheal (later Fine Gael), Fianna Fáil, the Official IRA/Sinn Féin, and the Provisional IRA/Sinn Féin, "escalated violence, shunned elections, and denounced negotiated compromise while challengers only to then shun violence, participate in elections, and negotiate compromises after they became the leader or hegemon" (131). As I demonstrate with extensive archival evidence and interviews with key group leaders, what changed was not what these groups ideologically had proclaimed was necessary (e.g. a withdrawal of British forces) but rather their position in the movement hierarchy and their expected organizational benefits.

At the same time, I agree with Lawrence that ideology is important in a few key ways that I detail in the book. In Chapter 6, I present and analyze the balance of power within the Irish national movement not simply as a cohesive whole, but also within its republican and nationalist wings, which are divided based on ideological differences. Although I find clear evidence that the leaders of both wings (the SDLP and IRA/Sinn Féin) treated each other as rivals, I also conclude that greater variation in group behavior and movement outcome can be explained if we analyze the hierarchy and rivalries inside each wing, as if each wing is itself an ideological movement inside a shared national movement. A similar approach that examined the balance of power within and across religious and secular wings of other national movements would provide fertile ground for ideology and power to complement each other in a more comprehensive fashion. I continue to grapple with these issues of causality and historical interpretation in the Palestinian and Algerian cases below.

Analyzing the Palestinian National Movement

Hegemony and Hamas

The Palestinian national movement (Chapter 3) is the first of the four I analyze in *Rebel Power*, and it received a great deal of feedback from the reviewers. In a general sense, Pearlman claims that the book, "offers a powerful analytical framework and amply demonstrates its utility in expertly documented case studies that showcase archival research and more than 150 interviews conducted during 23 months of fieldwork in nine locations over eight years." Quandt agrees with much of my interpretation of the Palestinian case, stating that

"On at least three occasions, these smaller groups, as Krause would predict, led the PLO into violent confrontations that left it weaker." As Bakke explains, "capturing power relationships within a movement is tricky, and *Rebel Power* does a great service by helping us think about how to do this—and provides detailed empirical data on the power hierarchy among the groups within the four movements under study, both in the book itself and in online appendices."

Given the difficulties of determining the strength of clandestine groups by year across multiple decades, I fully expected and continue to welcome discussions about my codings. This is why I sent the book appendices to three experts of each movement before publication, and they agreed with over 90% of the codings. I was honestly happy that the number was not 100%—as long as I was not missing key evidence—because it meant that my data would challenge the conventional historical wisdom in a few key periods. Both Pearlman and Jonah Schulhofer-Wohl (who wrote an *H-Diplo* review of an earlier article of mine related to *Rebel Power*) seize on one of these challenges: my argument that the Palestinian national movement was hegemonic from 1987-1993 and 1995-2000.[4]

On the issue of movement hegemony, the key question is whether Hamas was a challenger during these periods, as the reviewers seem to agree with my conclusion that former challengers Saiqa, the PFLP, and other groups had faded from serious contention with Fatah. It is undeniable that Hamas was formed at the beginning of the First Intifada in 1987-1988, and that the group aimed to lead the Palestinian national movement and challenge Fatah from its early days. Nonetheless, when I analyzed the available data from a variety of sources on group size, funding, and popular support among Palestinians, it was clear that although Hamas was one of the fastest growing groups at the time, it did not come within one-third of Fatah—and so represent a challenger as I define it—until much later. The Israeli military, which had every reason to hype Hamas funding as it labeled it a terrorist group, stated that foreign funding to Hamas in the West Bank and Gaza emanating from Saudi Arabia, the Gulf, and Iran reached $1 million annually in 1992.[5] Around the same time, Fatah faced a revenue crisis due to its support of Iraqi President Saddam Hussein during the 1991 Gulf War. Still, its yearly revenue was over $100 million and it had cash reserves in the billions (55-58). As I detail in the book, Hamas briefly came within one-third of Fatah's support in public opinion polls conducted in 1994, after the deadly attack on Palestinians by an Israeli settler in Hebron. However, although Hamas continued to have the second-largest amount of support among Palestinian groups, Hamas always registered less than one-third of Fatah's support from 1995-2000.

One could consider alternative measures of relative power, and I sought some out in response to this challenge from Pearlman and Schulhofer-Wohl. Because the use of violent tactics against the state enemy is one of my dependent variables—and I predict and regularly demonstrate that weaker groups will escalate violence more—I do not think that such data is helpful for assessing group strength. Evidence for more Hamas attacks

[4] Pearlman also asks "Why is 1994 deemed a 'failure' while the periods 1986-1993 and 1995-2000 are judged 'moderate successes'?" However, her premise here is mistaken, as I code 1994 a "moderate success," as demonstrated in the summary tables in Chapter 3 and Chapter 7 (42, 176); Jonah Schulhofer-Wohl, "Article Review 30 on Peter Krause, 'The Structure of Success: How the Internal Distribution of Power Drives Armed Group Behavior and National Movement Success'," *H-Diplo/ISSF*, 14 January 2015; https://issforum.org/articlereviews/30-the-structure-of-success.

[5] IDF Spokesman, "Hamas: The Islamic Resistance Movement," January 1993.

during the first intifada would therefore support my theory, not weaken it. However, one could argue that certain types of internal violence might serve as a proxy for capability, such as the number of accused Palestinian collaborators with Israel that each group executed. From 1987-1993 in Gaza, one scholar concludes that Fatah killed 211 collaborators and Hamas killed 75.[6] In this measure, Hamas is just barely within a third of Fatah, but this is also in Gaza where Hamas had its main operations, meaning that the total number including the West Bank likely increases the gap significantly.

This points to one alternative that I do raise in the book that could account for the disagreement: regional balances of power. As a first cut, I calculated the strength of each group in total. However, it is clear today that while Fatah and Hamas may be neck and neck for leadership of their movement, Fatah has been the leader or hegemon in the West Bank and Hamas the leader or hegemon in Gaza for some time. Given that many of Fatah's members and leaders were outside of the West Bank and Gaza during the First Intifada, a regional calculation of group size (if not popular support, which was already calculated regionally) may shift the coding of movement hierarchy. The coding could also shift if we count the strength of the local Muslim Brotherhood within Hamas's total, given that it was the foundational wellspring for the group. Furthermore, while Fatah dominated the external Palestine Liberation Organization (PLO), formal power was divided more equally within coordinating institutions in the West Bank and Gaza during the first intifada, as Pearlman expertly details in her book.[7]

In any case, I am happy to assess and incorporate any evidence that Hamas was stronger (or weaker) than I suggest from Pearlman, Schulhofer-Wohl, or anyone else. Facing the same question from two scholars whom I hold in such high esteem gives me pause and at the very least places a greater degree of uncertainty on this coding than on others. As Kathleen Cunningham notes in another review of *Rebel Power*, "It is the first book to fully address the dynamics of power relations among factions within these movements."[8] I know it will not be the last, and I hope that these discussions reveal the importance of both clear definitions and evidence for judging the relative power of rebel groups.

The Role of States in Palestinian Success and Failure

Multiple reviewers touch on what I agree is the biggest missing piece of the book: the role of states (especially the enemy state) in movement dynamics and outcomes. To be fair, I do incorporate the enemy state at

[6] Brynjar Lia, *A Police Force Without a State: A History of the Palestinian Security Forces in the West Bank and Gaza* (Ithaca: Ithaca Press, 2006), 68.

[7] This also helps to resolve a seeming point of contention between Pearlman and myself, which is whether hegemony or unity drove success for the Palestinians in this period. There was not "unity" according to my definition because there were no other large factions to challenge Fatah. However, it is also correct that there was unity among Fatah and a number of other factions (which I label subordinates), which fits Pearlman's definition of unity in Wendy Pearlman, *Violence, Nonviolence, and the Palestinian National Movement* (Cambridge: Cambridge University Press, 2011). I would simply add the suggestion that it was precisely the internal imbalance of power that made this type of unity more likely, as there was a lack of such unity in earlier eras when these groups challenged Fatah for leadership (and in subsequent eras when Hamas did so).

[8] Kathleen Cunningham, "Review of *Rebel Power*," *Perspectives on Politics* 16:1 (March 2018): 255-257.

numerous points in my argument and empirical analysis: who it represses in response to violence, how it reads signals from the movement, and which groups it negotiates with. One of Quandt's challenges on this score is actually a point of agreement between us. He claims that supportive states have goals that clash with those of their proxies, and that therefore state support can actually hinder movement success (as it did with Iraq, the Abu Nidal organization, and the Palestinians). I not only agree, I also identified in *Rebel Power* the conditions under which state support helps (when a hegemonic movement is a seller's market for influence) or hurts (when a fragmented movement is a buyer's market for influence). Nonetheless, my discussions of states are usually in reaction to or funneled through nationalist groups who play the starring role, not the other way around.

This was somewhat intentional, because many of the foundational works on the topic of nationalist outcomes are state-centric and largely omit the internal politics of movements.[9] Rather than search for a grand theory of everything, I encourage readers to digest *Rebel Power* alongside the excellent books by these scholars and the reviewers for a more complete picture. I plan to do the same for my next book project, which will more deeply integrate the role of the state in explaining which groups take power after regime change.

It is not always a difference in emphasis, of course; there are clear differences of opinion regarding causal weight.[10] I definitely believe that movements have more agency and influence than those state-centric authors—and perhaps some of the reviewers—suggest. Lawrence poses the question of whether the Palestinians would have a state if the movement were ever hegemonic, and then, like Quandt, suggests that I should consider the importance of Israeli state strength and commitment. A hypothetical is not needed, as the Palestinians were indeed hegemonic from 1986-1993 and 1995-2000, and it was—as predicted—the period in which they were the most successful. Does Israel's strength and relatively strong commitment to the territory help explain why the Palestinians achieved only moderate success and not statehood during this time? Of course, and I never claim in *Rebel Power* that hegemony is sufficient for national movement success—no single factor is.[11] My data shows this—hegemonic movements only achieved statehood 40% of

[9] Bridget Coggins, *Power Politics and State Formation in the Twentieth Century: The Dynamics of Recognition* (Cambridge: Cambridge University Press, 2014); Ian Lustick, *Unsettled States, Disputed Lands: Britain and Ireland, France and Algeria, Israel and the West Bank-Gaza* (Ithaca: Cornell University Press, 1993); Hendrik Spruyt, *Ending Empire: Contested Sovereignty and Territorial Partition* (Ithaca: Cornell University Press, 2005).

[10] Sometimes the differences in emphasis may be (mis)perceived as differences in causal weight. For example, Pearlman seems to interpret my more in-depth discussion of Zionist fragmentation as a claim that it was the main cause of the 1939 British White Paper. I write that those actions "contributed to" the White Paper, whereas I claim two sentences later, "The specter of World War II and the British desire to placate the Palestinians also played a key role" (75).

[11] This is relevant for Quandt's suggestion that the Algerians had an easier road than the Palestinians because the French could simply retreat to Paris whereas the Israelis have no piece of territory that the Palestinians do not claim. I agree to an extent, although it is important to note that the *colons* certainly saw the Algerian national movement as an existential threat. Furthermore, the concept of a two-state solution, despite its bleak prospects at the moment, does involve the Israelis pulling back from some territory to allow for the creation of the state of Palestine while maintaining their own state, thus making "total success" of some form possible for both movements. On the related issue of

the time—but it also shows that united and fragmented movements never achieved statehood in many more total campaigns. Furthermore, Israel has always been a strong state committed to the territory, and yet there has been significant variation in success and failure for the Palestinians over time (as for all four movements in the book), which these state-centric factors cannot explain but Movement Structure Theory can and does.

As for the broader challenge that state behavior via the decline of colonialism better explains these outcomes, I not only detailed my response in the conclusion chapter, but I also subsequently conducted an analysis of the first successful, post-colonial national movement in Africa to probe external validity (187-188). The actions and outcomes of the Eritrean national movement were well-explained by Movement Structure Theory: infighting and failure when fragmented or united, success and independence when hegemonic, despite facing one of Africa's strongest and best-equipped armies (Ethiopia's) that was supported by the U.S., U.S.S.R., *and* Israel.[12]

Analyzing the Algerian National Movement

I could not agree more with Lawrence about the importance of historical accuracy, as demonstrated by the many years I spent in nine archives, in conducting numerous interviews with movement participants, and in meticulously creating yearly appendices with clear citations for movement structures over time. Although Quandt notes "a few quibbles" on the Algeria case, he concludes that "[my] research shows a very good command of the existing sources." Lawrence and Quandt's challenges to a few of my historical interpretations are understandable given the scope of my study and warranted given that they both wrote excellent books on the Algerian case.[13] I spent significant time returning to relevant primary and secondary sources to analyze their queries with an open mind.

The first and most significant challenge is Lawrence's suggestion that my analysis of the Algerian national movement from 1944-1954 is problematic because Algerian leader Ferhat Abbas supported assimilation and was not a nationalist during that period. This is an important determination because, as I note in the book, "I begin collecting data on a national movement once a majority of its groups support political autonomy" (36). This is why I do not start analyzing Algerian nationalism in earnest in the 1920s and 1930s. Even though Messali Hadj's Étoile Nord-Africaine (ENA) pushed for Algerian autonomy at that time, it was in the minority due to Abbas's group and others pushing for assimilation within France.

The change for Abbas from assimilation to autonomy occurred during World War II in response to frustration over years of French intransigence, the 1940 revocation of French citizenship for Algerian Jews—and what that precedent meant for Algerian Muslims—and the opportunities revealed by the Allies and their

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determining borders for national movements (with a focus on the Zionist case) see Nadav Shelef, *Evolving Nationalism: Homeland, Identity, and Religion in Israel, 1925–2005* (Ithaca: Cornell University Press, 2010).

[12] Krause, "Coercion by Movement."

[13] William Quandt, *Revolution and Political Leadership: Algeria, 1954-1968* (Cambridge: MIT Press, 1969); Adria Lawrence, *Imperial Rule and the Politics of Nationalism: Anti-Colonial Protest in the French Empire* (Cambridge: Cambridge University Press, 2013).

rhetorical support for self-determination. This culminated in the "Manifesto of the Algerian People" which rejected assimilation and called for self-determination and an Algerian republic, as well as an addition that included the related objective of an independent state with its own constitution. The Manifesto, authored and presented by Abbas, thus aimed at an autonomy that carried with it the associated private benefits of office, wealth, and status for Algerian nationalist groups. One need not take my word for it. Quandt himself interviewed Abbas in 1967, and, in the summary of their discussion about the World War II period, noted that Abbas, "believed independence would come at the end of a long period of negotiations."[14] Alistair Horne wrote of how Abbas moved beyond assimilation in the aftermath of the failed Blum-Viollette bill in 1937, while Jean-Pierre Peyroulou detailed how Abbas hoped the Americans would emancipate Algeria after their arrival (which was why Abbas sent the Manifesto to President Franklin D. Roosevelt before French resistance leader and subsequent President Charles de Gaulle).[15] Jennifer Johnson confirms how the WWII experience "hardened previously amenable leaders, such as a Ferhat Abbas, to the point where they embraced national independence."[16]

It is unclear to me how a "recantation" while under arrest could represent an individual's true feelings, and not the words contained in a major manifesto he authored, presented to world leaders, used as the platform for the entire national movement, and struggled publicly for months to realize before being detained by the French in September 1943. In any case, Abbas not only pushed for autonomy before his arrest, but also immediately after it. Offered the assimilation by De Gaulle in a 1944 ordinance that he had struggled for in earlier eras, he rejected it as inadequate and formed a political party one week later—not coincidentally named "The Friends of the Manifesto and Liberty" (AML)—in which "all the principal components of nationalism joined hands," in a push for autonomy and Algerian, not French, citizenship.[17] After the AML was broken up by the French and Abbas was arrested again in 1945, he again formed a new political party that referenced the Manifesto and continued to push for an autonomous Algerian republic. This party and its objectives endured until Abbas disbanded it and himself joined the *Front de libération nationale* (FLN) in the final push for independent Algeria.

In light of all of the evidence presented here and in the book, do I feel confident saying that a majority of movement groups supported autonomy after 1944? Absolutely. Is it also true that despite their shared goal of autonomy, in the late 1940s Abbas was more moderate in both the pacing of ends and the utilization of means than Messali Hadj? Yes, and I analyze why in *Rebel Power*. I think the saying that 'Abbas was an evolutionary and Messali was a revolutionary' has much truth to it. But remember that my unit of analysis is

[14] Ferhat Abbas, interviewed by William Quandt, 17 February 1967, William Quandt Field Notes, CEMA.

[15] Alistair Horne, *A Savage War of Peace: Algeria 1954-1962* (New York: New York Review of Books, 2006), 42; Jean-Pierre Peyroulou, *Guelma, 1945: Une subversion française dans l'Algérie coloniale* (Paris: Découverte, 2009), 26.

[16] Jennifer Johnson, *The Battle for Algeria: Sovereignty, Health Care, and Humanitarianism* (Philadelphia: University of Pennsylvania Press, 2016), 21. Jeremy Lane agrees that "it was the experience of the War that had finally convinced [Abbas] that an independent Algeria was the necessary precursor to any genuine improvement in the political status and socio-economic condition of Algeria's Muslim population." Jeremy F. Lane, "Ferhat Abbas, Vichy's National Revolution, and the Memory of the Royaume arabe," *L'Esprit créateur* 47:1 (Spring 2007): 19-31.

[17] Horne, *A Savage War of Peace*, 43.

organizations, not simply individuals. When push came to shove, I had a hard time finding examples of Abbas, Messali, or FLN leaders acting in ways that purposely made their groups weaker. However, I found examples for every single one of them acting in ways that violated previous ideological stances, whether it was Abbas joining the FLN and legitimizing their violence in the mid-1950s, Messali supporting the outbreak of violence in 1945 but not in 1954, or the FLN leaders killing and spoiling in the early periods of the revolution, then negotiating with France and restraining violence once their expected benefits changed.

In any case, Lawrence's challenge implicitly suggests two helpful expansions. First, I could have subdivided the Algerian national movement into ideological wings as I did in the subsequent chapter on the Irish national movement. The relationship between the *Union démocratique du manifeste algérien* (UDMA) and *Mouvement pour le triomphe des libertés démocratiques* (MTLD) is not unlike that between the SDLP and Sinn Féin/IRA, in that they pursued different paths towards their shared goal of autonomy even as they offered different versions of what their future state would look like.[18] This would help ensure that ideology and power would be analyzed not simply as competitors but also as complements. Still, I share with Pearlman the desire to move away from the often misleading labels of "moderates" and "radicals," and so I need to think further about when and how to utilize this approach for future projects.

Second, this discussion ironically has pushed me to consider expanding the full application of Movement Structure Theory to movements before a majority of their groups support political autonomy. For example, if the "assimilationist" Abbas had succeeded in the 1920s and 1930s, what would equal rights have meant in an Algeria where 85% of the population was Muslim? As Quandt suggested in his interview with Abbas, it would have meant far more than simple individual equality: the dominance of the Algerian parties and the empowerment of leaders like Abbas and Messali would have become a reality. Even though the size of the prize would have been slightly smaller, the general benefits—and so the competition—would have been similar. I therefore plan to analyze the applicability of my theory at earlier points in self-determination movements.

On the May 1945 violence in Sétif, Guelma, and elsewhere, I agree with Quandt that the record is uncertain as to what extent Messali and the *Parti du peuple algérien* (PPA) initiated the outbreak. Stora calls it "a spontaneous uprising, supported by the PPA militants of Constantinois" while Quandt himself claims that "Messali Hadj and some members of his party were clearly preparing for armed insurrection in the spring of 1945."[19] In any case, as predicted by my theory, Abbas did not initiate the violence, did not support it, and yet he and his party suffered extensively in the subsequent crackdown. Messali may have ordered the uprising, but even if he did not he was preparing for it, and he was more supportive of the escalation than Abbas.

Finally, both Lawrence and Quandt suggest that French state behavior was crucial to the success of the Algerian national movement. I strongly agree with Lawrence that the FLN's victory was as much political as military, as on pages 120-128 in which I discuss the evolution and effectiveness of the FLN's diplomatic

[18] Quandt himself provides a clear guide for how to group these Algerian organizations. Quandt, *Revolution and Political Leadership*.

[19] Benjamin Stora, *Algeria 1830-2000: A Short History* (Ithaca: Cornell University Press, 2001), 21; Quandt, *Revolution and Political Leadership*, 51.

efforts and the impact it had on international recognition and French public opinion.[20] I also agree with Quandt that indiscriminate repression by the state can rally supporters to the movement and ultimately aid its success. My theory adds that that repression often falls disproportionately on the stronger groups, helping weaker ones to rise—which is one reason that they did not always dislike the repression and violence that led to it. As I detail in the book, this is the exact dynamic that occurred in 1954 (and to some extent in 1945).

Thank you again to *H-Diplo* and the reviewers for participating in this invigorating exchange. I hope that this discussion of how to frame questions, define and measure key concepts, construct and present theory, and conduct historical analysis will help not only in interpreting and utilizing *Rebel Power*, but also to improve the quality of projects by all who work on national movements, civil war, Middle East politics, and political violence in the future.

[20] I see this section in particular as a good complement to Coggins and Lustick, as it demonstrates not simply that other factors matter, but also how a movement's internal politics interacts with them.