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H-Diplo/ISSF Editors: **James McAllister and Diane Labrosse**
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Commissioned for H-Diplo by **James McAllister**

Introduction by **James McAllister**

Fotini Christia. *Alliance Formation in Civil Wars.* Cambridge: Cambridge University Press, 2012. ISBN: 9781107023024 (hardback, $95.00); 9781107683488 (paperback, $32.99).

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Introduction by James McAllister, Williams College

Over the last decade much of the best work in comparative politics and international relations has focused on explaining the onset and termination of civil wars. In her new book, *Alliance Formation in Civil Wars*, Fotini Christia seeks to explain the constant shifts in alliances that characterize these conflicts. With a combination of theoretical richness, quantitative analysis, and extensive fieldwork in Bosnia and Afghanistan, Christia has produced an important and innovative book that will surely have an important influence on the field of civil war studies and international conflict.

Understanding the frequent changes of alliances in civil wars is often viewed as a complex issue of identity, history, or ideology; as a phenomenon that can only be understood by analysts with a deep understanding of a nation's history and culture. While certainly not denying the importance of these factors, Christia argues that there is a fundamental logic to shifting alliances in civil wars that would not be surprising to Thomas Hobbes or to European balance of power theorists. The crucial factors that determine alliance formation for Christia are rooted in basic power considerations rather than issues of identity, which she argues are "useful for public consumption" but do not really explain why elites make the decisions that they do (7). Drawing on neorealist conceptions of international relations, Fotini's central argument is that "Warring groups in multiparty civil wars are motivated first and foremost by relative power considerations. These groups dwell in an anarchic environment where they seek not only to survive, but also to profit. As a result, each group seeks to form wartime intergroup alliances that constitute minimum winning coalitions: alliances with enough aggregate power to win the conflict, but with as few partners as possible so the group can maximize its share of postwar political control (239-240)."

All the contributors to this roundtable appreciate the importance of Christia's book. Costantino Pischedda argues that *Alliance Formation in Civil Wars* "will represent a central milestone and source of inspiration for scholars struggling to make sense of civil war behavior for years to come." Paul Staniland believes that Christia has produced a book that is "a model of mixing theory, nitty-gritty fieldwork, and broader empirical generalizations." Nevertheless, all of the reviewers have some concerns about her central argument. Zachariah Mampilly suggests that Christia has knocked down a strawman conception of identity politics that hardly any scholars endorse, a claim that Staniland alsovoices. Mampilly believes that Christia has written a top-down conception of elite choices that fails to appreciate the importance of identity narratives for rank and file cadres involved in civil wars. While Hobbes surely has some important things to offer students of civil wars, Staniland argues that Christia should have paid more attention to political processes and the insights of Charles Tilley and Max Weber.

The editors would like to thank all of the participants in this roundtable and we extend our congratulations to Fotini Christia for her impressive new book. It is a very worthy recipient of the American Political Science Association's 2013 Luebbert Best Book Award, which is given to the best book published in the field of comparative politics within the last two years.

Participants:

Fotini Christia is an Associate Professor of Political Science at MIT. Her research interests deal with issues of conflict and cooperation in the Muslim world. Fotini has done extensive ethnographic, survey and experimental research on the effects of development aid in post-conflict, multi-ethnic societies with a focus on Afghanistan and Bosnia. She is the author of *Alliance Formation in Civil Wars*, published by Cambridge University Press and awarded the 2013 Gregory M. Luebbert Award for Best Book in Comparative Politics. Her research has also been published in Science and in the American Political Science Review among other journals and she has written opinion pieces for *Foreign Affairs*, *The New York Times*, and *The Washington Post*. She earned her Ph.D. in Public Policy at Harvard University in 2008.

Zachariah Mampilly is an Assistant Professor of Political Science, International Studies and Africana Studies at Vassar College. His research focuses on the nature of contemporary conflict processes, with an emphasis on Africa and South Asia. Based on field-work behind insurgent lines in D.R. Congo, Sri Lanka and Sudan, his first book, *Rebel Rulers: Insurgent Governance and Civilian Life during War*, was published by Cornell University Press in 2011.

Costantino Pischedda is a Ph.D. candidate in the Political Science Department at Columbia University. He previously worked as a research analyst for the World Bank and the Peterson Institute for International Economics. Costantino holds an MA in Strategic Studies from SAIS, Johns Hopkins University.

Paul Staniland is an Assistant Professor of Political Science at the University of Chicago, where he co-directs the Program on International Security Policy. His research has been published in *Civil Wars*, *Comparative Political Studies*, *Journal of Conflict Resolution*, *India Review*, *International Security*, *Perspectives on Politics*, *Security Studies*, and *The Washington Quarterly*.

Review by Zachariah Mampilly, Vassar College/University of Dar es Salaam

Fotini Christia offers a novel application of neo-realism to internal war based on a comprehensive and truly impressive research process that included both difficult fieldwork and creative and convincing secondary sources. In the book, she puts forward an instrumental perspective of social identity in which culture and ideology reflect power shifts, and are only as durable as the balance of power prevailing at a particular moment. In short, survival trumps identity concerns in matters of alliance formation and fractionalization as power is preeminent in the calculus of warring groups' behavior.

Before I continue, it is important to stress that few authors suggest that identity always trumps power in matters of war. Indeed, the instrumental approach appears dominant. For example, in the *Arthasastra*, the ancient Indian treatise of statecraft and strategy, Kautilya suggests that: "The king who is situated anywhere immediately on the circumference of the conqueror's territory is termed the enemy. The king who is likewise situated close to the enemy, but separated from the conqueror only by the enemy, is termed the friend (of the conqueror)."[1] More succinctly, the proverb, 'the enemy of my enemy is my friend,' is firmly ensconced in the public conscience. However phrased, the basic truth expressed is the same: in war, friends and foes are fluid.

What then to make of the identity straw man constructed by Christia in her stimulating new book? Best to do away with him quickly, methinks. Further slogging the dead notion of identity trumping power in matters of survival can appear almost cruel. Yes, 'journalists,' may still attribute wartime behaviors to some primordial force. Yet, the opening epigraph taken from the renowned *New Yorker* writer Dexter Filkins' book on the Afghan war is just one of many to comment on the byzantine and seemingly ideologically bereft nature of switching sides in civil war.[2]

The key to creating a convincing argument against which to contrast your own is that at least in some cases the story it tells must be credible. The subject of fractionalization and alliance formation is one that necessarily entails non-ideological behavior. After all, if groups could only align with their identity allies, then we would expect splits and alliances to occur far more infrequently, if ever. Such stability would contradict the considerable commentary on civil wars in which the instability of alliances among warring groups is a frequent point of observation, as Christia's opening epigraph demonstrates. As the author acknowledges, "fractionalization and alliance change are so common that a picture of war

[1] Kautilya. *The Arthashastra*. Book VI, "The Source of Sovereign States." 3. http://www.sdstate.edu/projectsouthasia/upload/Book-VI-The-Source-of-Sovereign-States.pdf

[2] Though Christia cuts off the epigraph before Filkins offers his own take on why men switch sides, in the text he refers to the process as a form of "natural selection" in which those who fail to switch when the time was nigh die. He goes on to call those unwilling to make decisions instrumentally "too stubborn, too stupid, or too fanatical," making it clear that their behavior was the exception to what he observed. Filkins, Dexter. *The Forever War*. New York: Vintage. P. 51.

that does not include them is incomplete" (9). By raising the 'Primordial Straw Man,' Christia unfortunately resurrects a figure rightly consigned to the historical dustbin.

Does this mean her position on the role of social identity in war is correct? Unfortunately, her take on identity relies on an approach that misconstrues several of constructivism's key ideas about the nature of identity formation. Christia suggest that identities come into being to justify decisions made in response to power considerations: "elites of the warring parties pick their allies based on power considerations *and then* (emphasis mine) construct justifying narratives…" (7). In other words, identities are mere post-facto justifications. Later on, she expands this claim:

> …notions of shared identity are not causes of alliance behavior but are employed instrumentally to justify the power-driven alliance decisions that are actually made by elites…Notions of shared identity thus prove endogenous to alliance preferences: Elites pick their allies first based on tactical dictates, and then look to their identity repertoire for characteristics they share with their friends—and the same time do not share with their enemies—that would allow for the construction of justifying narratives (46).

Constructivism suggests that identity narratives and relationships between actors are co-constituting; that the constructed identity will affect the kind and quality of relations in which an actor engages with other actors; and that those relations will simultaneously transform the original identity. While embracing the fluidity of identity, Christia's approach goes against many constructivists' rejection of the position "that one's identity is reducible to those relations, or that one could predict aspects of a person's identity simply by extrapolating from those relations."[3] As the philosopher Ann Cahill explains, "such assumptions would deny the dynamism of intersubjectivity."[4]

Christia's decision to situate the agency to define identities solely with a political elite and simply as a response to power (defined territorially or militarily) rejects the inherent dynamism of a constructivist understanding of identity formation. By claiming a specific moment at which identity narratives are constructed—after the political calculation made to form or break a relationship with another actor—a process that is dynamic and fluid becomes stable and fixed, and contingent on power politics.

This relates to the main concern I have, which is the question of the agency of the masses, or in this case, the rank and file cadres. In the book, identity construction during civil war is a top-down project with elites simply picking and choosing identity characteristics from a repertoire of rank-ordered traits and feeding them to their supplicant followers. But such a view does not comport with my own understanding of two important cases (and I would

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[3] Richard Marshall. 2013. "Carnal Ethics: Interview with Ann Cahill." *3:AM Magazine*. http://www.3ammagazine.com/3am/carnal-ethics/

[4] Ibid.

suggest others), where cadres exhibited significantly more agency in their responses to movement elites.[5]

For example, in the case of South Sudan,[6] anti-Dinka sentiment among Southern ethnicities was in constant tension with the project of pro-Southern unity being espoused by the primarily Dinka leadership of the Sudan People's Liberation Army (SPLA). Intra south tensions were not manufactured wholesale by non-Dinka elites; rather they preceded the project of southern unity itself driven by the often tense historical relations between the Dinka and the plethora of other groups living within the region. In fact, it was these tensions and their tendency to undercut resistance to Khartoum that forced the SPLA to attempt a project of identity construction in the first place.

Similarly, in Sri Lanka, the decision by the eastern commander Karuna Amman[7] to break from the Liberation Tigers of Tamil Eelam [LTTE] reflected both incentives put forth by the Colombo government, but also, a general sense of resentment by Tamils from the east of the island. Eastern Tamils had contributed a larger numbers of troops and suffered significantly higher casualties while failing to gain equal representation among the rebel leadership as compared to northern Tamils. Historically, the tendency of Sri Lankan Tamils to venerate sites in the north of the country as the historical homeland further reinforced this sense of ostracization.

Christia is suggesting that the eastern Tamil cadres were prodded by factional leaders to both abandon their sense of Tamil racial solidarity and simultaneously embrace other identity traits, either cultural or ideological, that they shared with the Sinhalese government, even if these traits were diametrically opposed to their previous sense of identity. Similarly in South Sudan, non-Dinka southerners presumably abandoned their 'African' racial identity as a key signifier and instead embraced some other identity characteristic that tied them to Khartoum. As Christia makes clear, "There is therefore no sense of stickiness in the alliance narratives that would prevent any power-dictated alliance from taking place" (48).

Yet, a closer look at the debates within the organizations (and their factions) as well as the perceptions among the cadres show that in both cases factional leaders suffered

[5] The cases below are drawn from my 2011 book, *Rebel Rulers: Insurgent Governance and Civilian Life during War.* New York: Cornell University Press.

[6] Though I have little to offer regarding her cross-national test of the theory, I did note one surprising exemption from the 53 case sample. Specifically, it excludes the twenty-year war between the Sudanese government and rebels in the south of the country discussed above. As has been amply documented, the case is paradigmatic in the depth and frequency of alliances and splits that characterized the southern rebellion. Most of these divisions had little to do with the ideological character of the war and Christia's notion of power as a motivating factor for the division is certainly relevant, so it is unclear to me why it was excluded.

[7] Karuna was the *nom de guerre* of Vinayagamoorthy Muralitharan who prior to the split was the number two military commander of the LTTE.

consequences as a result of their decision to align with opposition forces. Put another way, though elites (possibly) did not have any attachments to their identities, those identities were far more "sticky" among their followers than Christia acknowledges.

In both cases, rank and file cadres fled the incipient faction in substantial numbers to reject the perceived disloyalty of factional leaders to the putative identity category (Tamils or Southern Sudanese) and to register a strong distaste for their association with the 'enemy' (Sri Lankan or Sudanese government), even if they could understand the tactical logic that explained the decision. Importantly, many eastern Tamils remained loyal to the original LTTE while many non-Dinka southerners remained within the SPLA, despite the exhortations of their commanders.[8] I happened to be in eastern Sri Lanka when Karuna broke from the LTTE, and the killings by his followers of fellow eastern Tamils on the opposite side of the split (and vice versa) were extensive and gruesome. These defections are important both strategically in that they undermined the strength of the incipient factions, as well as analytically, as they demonstrate the agency of the cadres in accepting or rejecting Christia's "justifying narratives" (46). Yet this aspect cannot be accounted for due to the author's elite level focus and assumption of omnipotent commanders.

Though she occasionally hints at a more nuanced relationship between commanders and cadres in the case studies, in the end, they too reinforce the notion of a powerful organizational elite and mindless followers. But buried in the well documented case studies are also the seeds of what I think is a more interesting research project, one that Christia is uniquely situated to conduct. Though Christia suggests that the case studies use process tracing to show how identity narratives "get constructed and how they work," we learn little about how they work beyond the deployment of identity rhetoric by elites (48). The interesting question is not whether ethnicity or ideology *determine* alliance formation and fractionalization; rather, it is how they can be stretched to justify certain power considerations, and most importantly, what the limitations are of such identity meddling. To answer this would require a closer understanding of the relationship between elites and followers within violent organizations. Without that, her analysis, while meticulously researched and intellectually stimulating, feels incomplete.

[8] Or on occasion, they simply gave up participating in violent groups at all.

Review by Costantino Pischedda, Columbia University

With this book Fotini Christia makes a significant contribution to the civil war literature by tackling two pervasive, yet poorly understood, civil war dynamics: alliance formation and rebel group fragmentation in multi-party civil wars. The book's importance goes beyond its intelligent and creative contribution to academic debates about civil war processes and the impact of identity on wartime behavior. Alignment switches and group fragmentation tend to affect the balance of power among belligerents and ultimately war outcomes. To offer three of many examples, the 1991 split of the Sudan People's Liberation Front, followed by the realignment of the splinter faction with Khartoum, contributed to significant battlefield successes for the government forces in the following years.[1] Similarly, the Tamil Tigers' split-cum-realignment in 2004 facilitated a subsequent offensive by the Sri Lankan army, culminating in the Tigers' complete defeat in 2009, after almost thirty years of intermittent fighting and negotiations.[2] Finally, the decision by Anbar province's local rebels to side with U.S. and Iraqi security forces against their erstwhile ally – al-Qaeda in Iraq – marked a key turning point in the counterinsurgency campaign, paving the road to a radical reduction in insurgent activity in the province and in the rest of Iraq.[3] A nuanced understanding of the driving forces of civil war alliances is therefore crucial for designing effective policies in the realms of counterinsurgency and intervention in ongoing civil wars.

Due to its immediate relevance to my research interests (as well as space limitations), in this essay I will focus on Christia's explanation of alignment patterns, rather than on her argument about rebel group fragmentation. Drawing inspiration from neo-realist thinking in International Relations, Christia argues that civil war alliances follow minimum winning coalition (MWC) logic; belligerents strive to be part of an alliance large enough to win the war but as small as possible given the requirement of being on the winning side. When the MWC threshold is passed, one or more belligerents will abandon the dominant coalition in search for an optimally-sized one. A combination of greed and security considerations underlie this logic: an oversized alliance entails a smaller share of spoils for individual members and a higher risk of exploitation and victimization of the weak by the strong member. In this view, ethnicity and other forms of identity do not influence alignment choices; decision-makers rather use them to justify to their followers' choices that are actually based on material considerations.

[1] Douglas H. Johnson, Douglas, *The Root Causes of Sudan's Civil Wars*, Bloomington: Indiana University Press, 2006, pp. 99-100.

[2] Ashok Mehta, "Sri Lanka's Ethnic Conflict: How Eelam War IV was Won," Manekshaw Paper 22, Centre for Land Warfare Studies (CLAWS), New Delhi, 2010; Paul Staniland, "Between a Rock and a Hard Place: Insurgent Fratricide, Ethnic Defection, and the Rise of Pro-State Paramilitaries," *Journal of Conflict Resolution* 56: 1, 2012.

[3] Jon Lindsay and Austin Long, "Counterinsurgency Theory and Stabilization of Iraq's Anbar," working paper, 2009; Stephen Biddle, Jeffrey A. Friedman, and Jacob N. Shapiro, "Testing the Surge: Why did Violence Decline in Iraq in 2007?" *International Security* 37: 1, 2012.

Christia's theory has the important merit of explaining in an elegant way the incentives of all sides involved in a realignment. Often realignments entail decisions by at least two actors.[4] These decisions can be thought of as symmetrical – i.e., two belligerents abandon their respective coalitions to join in a new one – or asymmetrical – i.e., an insurgent group decides to abandon the rebel camp and support the government counterinsurgency effort (in the latter case it may make sense to speak of demand and supply for side-switching). By contrast, existing arguments tend to focus only on one of the two sides of the 'equation.' For example, Patrick Johnston argues that in certain circumstances governments may be interested in coopting rebel groups to support them in their counterinsurgency efforts but he does not clarify the conditions under which insurgents are likely to take the bait.[5]

Christia's book also has significant empirical merits. It relies on an impressive amount of fieldwork in challenging political environments (Afghanistan and Bosnia), including interviews with key decision-makers, which results in fine-grained measures of her key independent variable (relative power). In addition, the author combines in an ingenious way case studies and quantitative methods, thus offering both in-depth examinations of a few conflicts and evidence that the patterns she identifies may hold more generally. The use of original micro-level data (for municipalities in Bosnia and insurgent commanders in Afghanistan) to show that the macro-logic of alliances holds at lower levels of analysis is especially innovative.

Nonetheless, careful reading also reveals certain problematic aspects of Christia's book. At the theoretical level, the argument is not fully specified in an important respect, which derives from the fact that MWC logic does not travel well from the field of electoral competition to civil war. One key difference between the two realms is that in civil war it takes time for potential material superiority to translate into victory and a long war entails both higher risks and costs for the potential winner. As Christia points out, other things being equal, a MWC will reach victory through a longer process of power accumulation than an oversized coalition (41). However, she glosses over the fact that a longer war will typically entail higher costs and risk of ultimately winding up on the losing side. Unpredictable developments like group fragmentation and changes in levels of external support to belligerents may radically alter the balance of power; the closer a dominant alliance is to the MWC threshold, the smaller the magnitude of the shock that is necessary to reverse the balance of power. If belligerents care about both benefits and costs of victory and somehow take into account probabilities of different outcomes, they will face a trade-off in deciding whether to stick to an oversized coalition or switch to a MWC. Christia's

[4] Cases in which one actor decides to attack an ally represent an exception to this statement.

[5] Patrick B. Johnston, "Negotiated Settlements and Government Strategy in Civil Wars: Evidence from Darfur," *Civil Wars* 9, 2007. Other studies that implicitly or explicitly develop explanations that focus only on the incentives of one of the two sides include: Chaim D. Kaufmann, "Rational Choice and Progress in the Study of Ethnic Conflict: A Review Essay," *Security Studies* 14: 1, 2005; Stathis N. Kalyvas, "Ethnic Defection in Civil War," *Comparative Political Studies* 41: 8, 2008; Staniland, 2012; Navin A. Bapat and Kanisha D. Bond, "Alliances between Militant Groups," *British Journal of Political Science* 42: 4, 2012.

conclusion that groups will opt for a MWC holds only under the (implicit and arguably heroic) assumption that belligerents only care about the size of the spoils of victory and the risk of exploitation by a stronger ally and disregard the costs of prolonged fighting and the risk of losing the war.

Another theoretical problem is that Christia's notion of MWC relies on belligerents' assessment of *actually manifested* relative power: by observing patterns of territorial control (or other measures of military strength, such as troop numbers, armaments and organization) belligerents can figure out which alliances have sufficient power to win. However, this is at odds with the prevailing conceptualization of irregular warfare (i.e., guerrilla, the most common form of warfare in civil wars):[6] rebel groups typically start as small bands of lightly armed individuals operating in remote areas of the country, too weak to openly face the overwhelming firepower of the government. The insurgents' theory of victory entails the gradual accumulation of strength (through a combination of mobilization and intimidation of the population as well as hit-and-run attacks on government targets) and a corresponding erosion of government's resolve up to the point when the rebels can achieve outright victory or extract important concessions from the incumbent. If this conceptualization is roughly correct (and there is plenty of supporting case study evidence[7]), rebel groups typically do not constitute a MWC (in the sense proposed by Christia) at the onset of rebellion. So insurgents are willing to fight despite the long odds or believe to constitute a *potential* (as opposed to actually manifested) MWC despite their momentary weakness; when the outcome is determined by a difference in resolve between incumbent and rebels, in a sense the rebels do not form a MWC even when they achieve victory.[8]

This observation raises a fundamental question about the scope conditions of Christia's argument: should we really expect MWC theory to hold in the prototypical cases of weak insurgents waging guerrilla warfare or just in cases characterized by a more balanced distribution of power (as in the wars in Afghanistan and Bosnia that Christia focuses on)? Many cases of groups that are too weak to constitute a MWC but fight each other as they also battle against the government suggest that her theory's applicability may be significantly less broad than the author claims. For example, in the first years of the Algerian war of independence, the National Liberation Front (FLN) fought and then wiped out its rival, the National Algerian Movement (MNA), despite the fact that it was facing an overwhelmingly more powerful foe – the French army.[9] The National Front for the

[6] Stathis N. Kalyvas and Laia Balcells, "International System and Technologies of Rebellion: How the End of the Cold War Shaped Internal Conflict," *American Political Science Review* 104: 3, 2010.

[7] See, for example, Robert B. Asprey, War in the Shadow*: The Guerrilla in History*, Lincoln, NE: iUniverse, 2002.

[8] Andrew Mack, "Why Big Nations Lose Small Wars: The Politics of Asymmetric Warfare," *World Politics* 27: 2, 1975.

[9] The bulk of the fighting between FLN and MNA in Algeria occurred in the years 1955-1957. By 1956 the French had deployed 400,000 troops, while the FLN's ranks included 15-20,000 regulars; the MNA was

Liberation of Angola (FLNA) and the Popular Movement for the Liberation of Angola (MPLA) fought each other from the beginning of the anti-Portuguese struggle rather than cooperating against the vastly superior incumbent.[10] This pattern is not limited to anti-colonial wars. The Eritrean rebel groups Eritrean People's Liberation Front (EPLF) and Eritrean Liberation Front (ELF) clashed in 1975 and 1980, in moments in which the Ethiopian forces had the upper hand in the region.[11] In Iraq, the Kurdish rebel groups Kurdistan Democratic Party (KDP) and Patriotic Union of Kurdistan (PUK) fought each other in the late 1970s and early 1980s, their military inferiority vis-à-vis the Baath government's forces notwithstanding.[12] In Sri Lanka, the Tamil Tigers wiped out most of their Tamil rivals in 1986 while engaged in an insurgency against the Sri Lankan government, which was the most powerful actor in the conflict both in terms of troop numbers and territorial control.[13] Similarly, the massive military power of the Indian state did not deter Kashmir's pro-Pakistan insurgent groups from targeting the pro-independence Jammu and Kashmir Liberation Front (JKLF).[14]

These cases are not randomly selected and thus do not amount to a falsification of Christia's probabilistic argument, but the abundance of anomalies in conflicts that have attracted substantial amounts of academic and policy attention suggests, at the very least, that there is quite a bit of variation that MWC logic cannot explain, warranting a complementary theory of civil war alliances. Here it is only possible to provide a potential sketch of such a theory: rebel groups often value achieving 'hegemony' in the insurgent camp due to a complex mixture of considerations about military effectiveness (essentially, economies of scale of rebellion), fear of future defection/exploitation by other groups, and spoil maximization. This is likely to be especially true when groups' overlapping social bases (e.g., when insurgent organizations claim to represent the same ethnic group) make it plausible that the winner of the inter-rebel struggle would absorb the membership and supporters of the groups it defeats. Such 'hostile take-overs'(in particular when they can be

probably always weaker than the FLN (Alistair Horne, *A Savage War of Peace: Algeria 1954-1962*, London: Macmillan, 1977, pp. 136, 222; Rasmus Alenius Boserup, "Collective Violence and Counter-state Building", in Bruce Kapferer and Bjorn Enge Bertelsen (eds.), *Crisis of the State: War and Social Upheaval*, New York: Berghahn Books, pp. 249-250).

[10] John H. Marcum, *The Angolan Revolution: Exile Politics and Guerrilla Warfare (1962-1976)*, Cambridge, MA: MIT Press, 1978, pp. 9-61.

[11] Michael H. Woldemariam, *Why Rebels Collide: Factionalism and Fragmentation in African Insurgencies*, Dissertation Manuscript, Princeton University, 2011, pp. 110-111, 127-128.

[12] David McDowall, *A Modern History of the Kurds*, London: I. B. Tauris, 2004, pp. 344-347.

[13] Staniland, 2012, pp. 31-33; John Richardson, *Paradise Poisoned: Learning about Conflict, Terrorism and Development from Sri Lanka's Civil Wars*, Kandy, Sri Lanka: International Centre for Ethnic Studies, 2005, p. 528.

[14] Staniland, 2012, pp. 25-26; Navnita Chadha Behera, *Demystifying Kashmir*, Washington, DC: Brookings Institution Press, 2006, p. 167.

executed quickly and when the government is, for political or military reasons, unable to bring to bear its military superiority in a decisive way) may be one of the processes through which initially weak rebel groups accumulate sufficient power to become serious threats to the incumbent.

More troubling for the empirical record of Christia's theory than the anomalies mentioned above is that in her case studies it is sometimes hard to tell whether an existing alliance constitutes a MWC: the author usually points out that, as a coalition's power grew, a weak member jumped ship, without clearly showing that the former ally had in fact become stronger than the new one or that no smaller winning coalition than the one actually established was possible. However, in one of the book's cases – the Bosnian civil war, 1992-1995 – it is quite clear that MWC logic does not hold. Christia's theory can only explain the initial alignment pattern: Bosnian Muslims and Croats against Bosnian Serbs, as the latter were clearly the strongest actor. The fight between Muslims and Croats in 1993 and 1994 is an anomaly as the Serbs were by that point even stronger than at the onset of the war. To make sense of this fact, Christia argues that by 1993 the key drivers were the regional balances of power within the wider Bosnian theater: if one looks at Eastern Bosnia, Central Bosnia, and Herzegovina as three separate arenas, there is evidence of balancing behavior consistent with MWC theory. However, Christia does not clarify why and under what circumstances MWC logic stops operating at the conflict-level and lower levels of analysis become the relevant ones. Finally, the Muslim-Croat rapprochement in 1994 cannot be explained by MWC logic (the Serbs were the strongest actor from the beginning) – it was the result of intense U.S. pressure.[15]

Despite the points addressed above, Christia's book represents a welcome and well-researched contribution to the field: she asks important and difficult questions and addresses them creatively with a variety of empirical methods and original data. Her work will represent a central milestone and source of inspiration for scholars struggling to make sense of civil war behavior for years to come.

[15] Steven L. Burg, Paul S. Shoup, *The War in Bosnia Herzegovina: Ethic Conflict and International Intervention*, New York: M.E. Sharpe, 1999, pp. 292-298.

Review by Paul Staniland, University of Chicago

Fotini Christia has written an important book on alliances in multi-party civil wars. As violence in eastern Congo, Syria, Afghanistan, and Burma waxes and wanes, the question of how different factions align is central to predicting the trajectories of conflicts. Christia provides an intellectual framework for understanding the alignment decisions of factions trying to pursue power in an environment of uncertainty and fluidity. Her basic argument is that groups are attempting to simultaneously be on the winning side of a war while also extracting the greatest possible benefits relative to other members of their coalition. Shifts in relative power unsettle coalitions and lead to side-switching, as a group that feels it can get a better deal by joining the other side defects. This leads to a pattern of power balancing that only ends with decisive military victory. Christia's book is empirically thorough and impressive, and it clearly adds to our understanding of the dynamics of civil war. As I argue after summarizing the argument and evidence, there are some limitations to the book that open pathways to future research.

The basic argument of the book is admirably clear. Armed groups in multi-party civil wars have to make intricate calculations about both who is likely to win the war and what the distribution of spoils will be after victory. Building on neorealist International Relations (IR) theory, Christia argues that balancing behavior is pervasive in these wars. Groups want to be on the winning side but they also want the best possible deal they can get. As a result, we see shifting coalitions because it is hard to lock in a balance of future benefits in the face of changing power relations among the groups within an alliance. As groups decline in power relative to their alliance partners, they begin considering changing sides in order to get a better deal. We see coalescing, fracturing, and re-coalescing alliances that reflect different configurations of a minimum winning coalition: "fear of betrayal drives groups to also worry about their relative power compared to other alliance members – in other words, warring groups will ally or affiliate with the weaker side in an anarchic all-out civil war to balance the distribution of power" (34).

Alliance complexity also extends within groups. Though "identity is indeed a powerful bond" that constitutes groups , there are nevertheless pre-existing cleavages that can lead to intra-group fractionalization when there are asymmetric losses that lead these sub-groups to try to either split or takeover the overall group (35). This form of fractionalization are more rare than alliance switches by groups, but can happen as a result of local elites' responses to unfavorable battlefield dynamics (8, 42-45).

'Bandwagoning' – when groups ally with the stronger party – only occurs when outright military victory by a powerful group appears likely and the others decide to accommodate themselves to reality. Short of this kind of military victory or international intervention that decisively alters the balance, we should see "constant realignment and fractionalization" (49). These dynamics are most likely in very weak states (7).

Several further claims arise from this core argument. First, Christia argues that elite leaders of groups will strategically mobilize different narratives to justify changing alliance choices.

While groups themselves are built around trust and institutions that minimize (though do not eliminate) commitment problems, when it comes to inter-group relations, the cold-eyed logic of power seeking leads to an instrumental use of narratives that can justify essentially any alliance (46-48). Christia suggests that these narratives serve the purpose of signaling to followers which side they should be on (49). Fundamentally they are "endogenous to alliance preferences" (46). Second, she provides predictions about the overall conflict dynamics in multi-party civil wars: those with even balances of power will see extensive side-switching, multi-party wars will last longer than binary conflicts, and more fractionalization will lead to more alliance switches.

Christia provides a wide variety of evidence to support her argument. The depth and breadth are ambitious, and go well beyond what can be surveyed here. Briefly, she uses the history of alliances in Afghanistan between 1992 and 1998 and Bosnia between 1992 and 1995 to build her theory, which she then tests against Afghanistan's alliances during the 1978-89 period, local alignments at the regional level between 1978-1998, and Bosnia between 1941-45 and a medium-N dataset of 53 multi-party civil wars. Christia's empirics are thorough and often exhaustive; she has gotten on the ground to do impressive fieldwork, delved into various sources, and gathered substantial new data.

I was persuaded by the evidence that simple ethnic-distance arguments for explaining alliances are not compelling and that the narratives produced by warring actors shifted at least in part according to strategic considerations. I wish there had been some cases where Christia flagged her argument as missing something important or used the cases to identify anomalies that point to new theoretical areas for study – a theory that explains everything is worrisome - but I nevertheless came away convinced that she has identified a key dynamic in an important category of civil wars. The book is a model of mixing theory, nitty-gritty fieldwork, and broader empirical generalizations, one that is made even more striking by the challenges of gathering all types of data in these war contexts.

I can continue praising the book; it is a serious piece of research that should be read by everyone interested in civil wars, international interventions, and peace-building. The incredible amount of work that went into it comes through on every page. But, like all research, it has important limits.

First, in order to claim novelty Christia regularly contrasts her findings to those that one would expect given an identity-based theory of alliance choice. But it is unclear who makes such an argument. There are many scholars who argue that ethnicity provides a crucial basis for collective action, but Christia agrees with them that identity can help to form groups and subgroups. Her real targets are theories that claim that identity distance leads to alignment choices. I had trouble figuring out exactly what these theories are and who advances them. A primordialist straw man is decisively beaten to a pulp, but the literature review reveals almost no work that makes the specific alliance claims that the author is attacking (23, footnotes 22-28).

This leads to a confusing mix of claims about how identity does and does not 'matter.' Identity seems to matter enormously to Christia in constituting the actors ("identity attributes do have psychological and emotional import for the rank and file,"; "subgroups. . . have identity ties to their groups that they sever only in times when the group's survival is at risk,"; "in-group identity does play a key role at the group level that is absent at the alliance level," (7, 33, and 46). But it does not 'matter' in the more narrow and specific set of strategic decisions that these constituted actors make about allying with other actors. Christia notes the circumscribed nature of her claims on pages15, 48, and 242, and suggests that her framework "does not speak to the larger questions involving the use of identity and symbols in civil war onset, targeting, or violence" (15). The book oscillates between grand attacks on a set of theories that might not actually exist and much more careful, but far less provocative, claims about a specific aspect of civil wars; rhetorical whiplash can ensue.

Second, I worry about a 'Goldilocks' problem. For the most part, Christia takes groups as a given, though pre-war social cleavages may open space for intra-group tension when there are asymmetric losses. The group therefore has a 'just right' position – not as localized as subgroups, but not as broad as inter-group relations. I worry that the most interesting action in alliance formation actually happens in the formation of 'the group.' A delicate balance becomes necessary in which for some reason identity bonds are crucial to actors in some contexts but not others. This leads to a simple question: if leaders are power seekers then why do in-group dynamics ever matter?

The alliances among sub-groups (and existence of sub-groups in the first place) that create groups themselves are black-boxed and taken as unproblematic givens. The group therefore is an alliance that mostly holds together (with some exceptions), but that is excluded from the explanation. In a strange way, Christia's work is not actually constructivist "all the way down," despite her claims to carry the banner of constructivism into the grim lands of apparently unreconstructed primordialist IR scholars (23).

In the hands of a less careful researcher, the danger is that groups and hence alliance formation can 'begin' where the analyst finds convenient, with everything else simply being treated as exogenous. This opens itself up to slipperiness. I should be clear that I have a stake in theorizing the emergence of armed organizations, and don't want to suggest that Christia should have written a different book. But strategic action requires actors and I was much more satisfied by Christia's account of the former than the latter.[1]

This is an issue, for instance, in the rise of the Taliban: Christia argues that Pakistani support was the key that allowed the Taliban to seize power but it is not clear why this support succeeded where it had failed in backing Gulbuddin Hekmatyar's armed group. The nature of the groups themselves mattered in building up sustainable organization as a group and converting resources into war-fighting. The Taliban's ability to both expand and remain cohesive was central to its ability to tip the balance of power. The group faced setbacks in the mid-1990s that perhaps should have triggered coups or splits, but instead

[1] For my approach to armed group organization, see Paul Staniland, *Networks of Rebellion: Explaining Insurgent Cohesion and Collapse*, book manuscript, University of Chicago, 2013.

remained relatively unified. Treating the emergence of the Taliban as exogenous seems to miss the crucial politics of coalition and institutionalization that were bound up in its rise.

Third, what Christia is studying is fundamentally a struggle over political order and control. As such, it is a process of contested state building. Yet she misses an opportunity to speak to important questions about when and how violent contestation can be transformed into durable political control through processes of bargaining and conflict. She notes that war is an extension of the bargaining process (5) but does not follow through to the implications of this claim for the broader politics of civil war. Despite cases in which new state power was forged – like the Taliban in the 1990s and Partisans in 1945 Yugoslavia – there is more on the intra-war back-and-forth dynamics than on the crucial question of how new arrangements of power and authority were ultimately established (for instance, why do winners sometimes accept bandwagoners and why do they sometimes purge or repress them? Do wartime groups become the state or does state building create new coalitions?). The politics of violence become simply a question of military side-switching: ideological visions, institutional structures, economic interests, and the other stuff of politics are excluded or subsumed into something vague called 'power.' Does a post-war outcome in which a group gets favorable language policy but unfavorable agricultural policy provide more or less power than one that has the reversed distribution?

More specifically, I wonder how Christia's framework explains protracted asymmetric resistance, refusals to ally between particular actors, and dynamics of fratricide and hegemony. It may be that enduring resistance to state power is best approached through a two-actor (state vs. insurgent) lens, but it is not clear how the long-running multi-party insurgencies in places like Burma, northeastern India, or the southern Philippines can be explained in this framework. It's not apparent that actual victory is likely, and so it's unclear why these actors fight and ally in the ways they do.

There are also situations in which certain actors simply never ally even when a pure power-seeker might do so (and when other similarly-situated groups are defecting). In Afghanistan, Ahmad Shah Massoud and remnants of the Northern Alliance continued to balance even as others defected to join the Taliban or fled, and the Communists in Yugoslavia did not align at any point with the fascist Ustashe (Chapter 7). Even within alliances the depth of cooperation varies dramatically in ways that may reflect competing political interests: Serb support to Muslims against Croats in some areas of Bosnia did not involve a substantial change in bargaining position or distributional outcomes, but was instead temporary and opportunistic rather than a mutually-adjusting arrangement with costs to defection (160-161). There may also be contexts in which prospective defectors are deterred by their coalition partners or annihilated through fratricide by aspirant hegemons once they start to change sides; the assumption of frictionless side-switching needs to be justified better.

In sum, Christia's Hobbesian *problématique* tells us useful things but stops short of giving us an understanding of where political order actually comes from. Pure power-seeking is clearly a valuable analytical starting point, but actors' preferences need more specification and contextualization; the book would have benefited from more politics. Giving Thomas Hobbes due attention is no reason to ignore Charles Tilly and Max Weber.

These questions notwithstanding, it is clear that Christia's book is a major contribution, one that will rightly attract wide attention and engagement in the civil war field and beyond.

Author's Response by Fotini Christia, Massachusetts Institute of Technology (MIT)

I would like to start by thanking the editors of H-Diplo/ISSF for offering to feature my book in a roundtable and the three reviewers -- Zachariah Mampilly, Costantino Pischedda and Paul Staniland -- for their rigorous engagement with my work. In their assessments of my argument, they raise concerns with how I deal with the costs of war and the role of identity in conflict; my views on how warring elites relate with their masses; as well as on how warring groups cohere and fragment. All are issues that I grappled with while writing the book and I will use this response to further illustrate how I addressed them.

On the costs of war, Pischedda raises the point that switching sides leads to a longer and more costly war and thus may not be rational. The book does not disregard the costs of war but rather argues that they must be weighed against the costs of winning as the weaker alliance partner. The main cost confronting the group is the concern over the group's potential demise. In particular, loss or reliance on a non-credible commitment from a stronger victor risks the destruction of the group. As the theory points out, "each group's notion of winning entails its survival as an autonomous entity. It is an outcome-oriented conflict, with losses and gains understood in terms of survival" (40). Since the stronger alliance partner cannot credibly commit to "dividing power fairly" (34) and not preying upon (and potentially destroying) the weaker partner upon victory, continuing the war may be the *less* costly option in many circumstances, which explains why weaker groups would defect when their own alliance grows too strong.

Pischedda, along with Staniland, also questions whether the minimum winning coalition logic is applicable when no combination of rebels is strong enough to defeat the state. The theory does have a prediction on irregular warfare between weak rebels and strong governments: it predicts a lower likelihood of alliance switches in this type of conflict since the government holds a "preponderance of power" (35) and therefore changes in relative power that alter what constitutes the minimum winning coalition are less likely. Because the theory is probabilistic, the discrepant cases cited by Pischedda and Staniland do not falsify the theory. The relevant test for the theory is whether this type of alliance switching is *less likely* in these sorts of hegemonic conflicts, a proposition for which the book provides empirical support (216-225).

Relatedly, Pischedda argues that it is hard to measure what constitutes a minimum winning coalition in conflict. While it is hard to definitively measure whether an alliance constitutes a minimum winning coalition, the book goes to pains to provide objective metrics for the relative power of the respective alliances (see pages 76, 112, 166, 194). He specifically uses the case of the Bosnian civil war to suggest that given the overwhelming strength of the Serbs in 1993 and 1994, the theory only explains why the Croats and Muslims allied with the Serbs during this period by lowering the level of analysis from national to regional. The book indeed argues that during periods of relative weakness, the meso-level of regional elites becomes important. As such, during this period, the role of regional factors became critical in both Croat and Muslim alliance decisions. And though the question of when the minimum winning coalition logic operates on the regional vs. the national level is worthy of further research, it is important to note that regardless of the level of analysis, it is still relative power considerations and not identity that are doing the work.

On identity, both Staniland and Mampilly state that there are few arguments in the conflict literature where identity overwhelms power and thus suggest that the identity-driven alliance hypothesis that the book disproves is a straw man. However, the multitude of scholarly works on civil conflict that have identity as the central variable counter this critique. In International Relations, an example of this approach is epitomized by the title to Stephen Van Evera's 1991 piece, "Primordialism Lives!"[1] While Barry Posen and Chaim Kaufmann move beyond primordialist arguments, they still suggest that when civil wars erupt, ethnic groups behave as cohesive actors with strong in-group preferences that limit the possibility of agreements between different sides in the conflict.[2] On the Comparative Politics side, many arguments similarly assume that identities are firm within a conflict and determine behavior. For example, Roger Petersen argues that pre-existing ethnic identities become fixed upon the onset of war and shape conflict dynamics since choices about who to work with and who to target are based on emotional mechanisms guided by the hierarchy of ethnic groups.[3] Staniland himself argues that pre-existing community ties drive the course of conflict, shaping the actors and limiting the potential for creating organizations that span different communities.[4] There is thus no scarcity of respected works that assert that ethnic identities shape the targets of violence as well as the prospects for cooperation in civil war. Indeed, it was exactly the prevalence of these types of arguments that prompted Stathis N. Kalyvas and Matthew Adam Kocher to attack the assumption of fixed identities during civil conflict.[5]

Mampilly goes even further to suggest that my work misconstrues constructivist ideas about identity formation and does not take into account the fact that power relations between actors and identities are co-constituted. As with all paradigms, there is certainly a diversity of views within constructivism. Precisely for this reason, the merits of a particular constructivist theory should be judged based on its logical soundness and explanatory power, not on whether it is in agreement with a particular set of works. That said, the

[1] Stephen Van Evera, "Primordialism Lives!," *APSA-CP: Newsletter of the Organized Section in Comparative Politics of the American Political Science Association* 12, no. 1 (2001): 20–22.

[2] Barry Posen, "The Security Dilemma and Ethnic Conflict," *Survival* 35, no. 1 (1993): 27–47; and Chaim Kaufmann, "Intervention in Ethnic and Ideological Civil Wars: Why One Can Be Done and the Other Can't," *Security Studies* 6, no. 1 (1996): 67.

[3] Roger Petersen, *Understanding Ethnic Violence: Fear, Hatred, and Resentment in Twentieth-century Eastern Europe* (Cambridge University Press, 2002).

[4] Paul Staniland, "Organizing Insurgency: Networks, Resources, and Rebellion in South Asia," *International Security* 37, no. 1 (July 1, 2012): 142–177; Paul Staniland, "Networks of Rebellion: Explaining Insurgent Cohesion and Collapse" (University of Chicago, 2013).

[5] Stathis N. Kalyvas and Matthew Adam Kocher, "Ethnic Cleavages and Irregular War: Iraq and Vietnam," *Politics & Society* 35, no. 2 (June 1, 2007): 183–223.

book draws upon a well-established line of research: the ethnic politics literature that treats the salience of ethnic identities as primarily a function of strategic calculations and power relations.[6]

Mampilly also argues that I grant too little agency to the masses. The book contends that elites have wide latitude to manipulate their followers, an argument that is also consistent with influential work in the ethnic politics literature.[7] The limitations of the identity narrative manipulation is certainly deserving of further research, although the fact that portions of the rank-and-file may react negatively to their subgroup's realignment does not invalidate the core argument of the book, which specifies the conditions under which these realignments occur.

Relatedly, Mampilly provides evidence from his work in Sudan and Sri Lanka that decisions by elites to join alliances with opposing forces weakened those elites and resulted in them losing the allegiance of some of their foot soldiers. The fact that there were long-standing subgroup cleavages within South Sudan does not contradict the book's theoretical framework; on the contrary, one of the book's contributions is to point out that warring groups are not internally homogenous and are prone to fracturing (42-43). Moreover, the argument that the Liberation Tigers of Tamil Eelam (LTTE) fractured partially because "Eastern Tamils had contributed a larger numbers of troops and suffered significantly higher casualties while failing to gain equal representation among the rebel leadership as compared to northern Tamils" is in accordance with the book's theory, which states that battlefield losses borne asymmetrically across subgroups are likely to spur alliance fractionalization (43-45). There is no doubt that fragmentation may occur following a decision to ally with former enemies—and as the theory points out, it is far more likely during periods of relative decline when subgroups question whether they will survive.

Moreover, there are many reasons that fragmentation within conflict groups may happen that are beyond the scope of the theory.[8] For example, in both his review and a recent article, Staniland argues that the internal politics of groups, in particular whether there is internal violence, is an important predictor of fragmentation.[9] However, my theory

[6] For example, see James Fearon and David Laitin, "Violence and the Social Construction of Ethnic Identity," *International Organization* 54, No. 4 (Autumn 2000); 845-877; Stathis Kalyvas, *The Logic of Violence in Civil War* (Cambridge: Cambridge University Press, 2006); and Stathis Kalyvas, "Ethnic Defection in Civil War," *Comparative Political Studies* 41, No. 8 (August 2008): 1043-1068.

[7] See Fearon and Laitin 2000; V.P. Gagnon, "Ethnic Nationalism and International Conflict: The Case of Serbia," *International Security* 19, No. 3 (Winter 1994-1995): 130-166; John Mueller, "The Banality of 'Ethnic War,'" *International Security* 25, No. 1 (Summer 2000): 42-70; and Steven Wilkinson, *Votes and Violence: Electoral Competition and Ethnic Violence in India* (Cambridge: Cambridge University Press, 2004).

[8] See, for example, Wendy Pearlman and Kathleen Gallagher Cunningham, "Nonstate Actors, Fragmentation, and Conflict Processes," *Journal of Conflict Resolution* 56, no. 1 (February 2012): 3–15.

[9] Paul Staniland, "Between a Rock and a Hard Place: Insurgent Fratricide, Ethnic Defection, and the Rise of Pro-State Paramilitaries," *Journal of Conflict Resolution* 56, no. 1 (February 2012): 16–40.

provides a parsimonious explanation of alliance and fragmentation dynamics that is not dependent on pre-existing community structures. Unlike works that focus on exogenous, pre-conflict variables, my theory is able to explain cases of fragmentation and alliance changes that cannot be accounted for by variables that do not change during the conflict. As such, while divided communities may be more likely to fragment cross-sectionally, the book is uniquely able to show that battlefield losses are a consistent driver of group fragmentation across all conflict actors.

Staniland also questions the cohesiveness of warring groups, underscoring the processes that may have led to the creation of the groups studied. To get to causal inference, all theories must choose to treat something as exogenous; group formation is outside the scope of the book's theory in the same way that social network structure is 'black-boxed' in Staniland's theory. Moreover, in order for this to affect the findings in the book, there must be an argument that some omitted variable influences both (1) group emergence and (2) subsequent alliance choices, which is not an argument made in Staniland's critique.

Shifting from pre-conflict to post-conflict dynamics, Staniland calls for a greater role for politics in assessing both the reasons for and implications of alignment decisions. He argues that the book overlooks "important questions about when and how violent contestation can be transformed into durable political control through processes of bargaining and conflict." While these are good questions for future research, they are simply not within the purview of this book's research question, nor do they affect the validity of the arguments in the book. Nonetheless, at the time of state collapse, the period under study in my book, survival, not political inclusion, is the focus. During such periods, it is difficult to disentangle promises from powerful groups to weaker groups about future state institutions, let alone "agricultural" or "language policies," from the broader commitment problem the strong groups have in ensuring the continued autonomy and *existence* of weaker groups. Thus, while state-building is definitely shaped by conflict dynamics, conflict actors facing destruction are focused on acquiring enough power to ensure their survival and role in shaping state institutions *after* the conflict so as to maintain their autonomy. Indeed, in the 1,000 year state-building process put forward by Charles Tilly,[10] much of the progress could only be made after a victorious warring actor destroyed or subsumed its enemies.

[10] Charles Tilly, *Coercion, Capital, and European States, AD 990–1990*. Cambridge, MA; Blackwell, 1990.