Critical Dialogue

Ethnic Politics and State Power in Africa: The Logic of the Coup–Civil War Trap. By Philip Roessler. Cambridge: Cambridge University Press, 2016. 418p. $99.99 cloth, $34.99 paper.
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— Kate Baldwin, *Yale University*

This outstanding book casts new light on the relationship between power sharing, coups, and civil wars in sub-Saharan Africa. In a piece of scholarship that is remarkable for its analytic clarity, methodological transparency, and commitment to comparative testing, Philip Roessler provides an exemplar of how inductive theorizing from a single case can be used to develop theory with much broader applicability.

The book's central argument is that African rulers face a perilous trade-off when deciding how much power to share with other ethnic groups. Building on his 2011 article, Roessler argues that rulers face a coup–civil war trap ("The Enemy Within: Personal Rule, Coups, and Civil War in Africa," *World Politics*, 63(2), 2011). If they share power with a particular ethnic group, rulers are at greater risk of being thrown out of office via a coup orchestrated by group members. But if they exclude the group from power, they are at greater risk of the group's successful launch of a civil war. Given the lower and less immediate probability of their losing office via civil war, Roessler argues that African rulers have chosen to mitigate the first risk at the expense of the second in most places: "One of the devastating implications that follows from this theoretical framework is that civil war represents the consequences of a strategic choice by rulers, backed by their coethnics, to coup-proof their regimes from their ethnic rivals" (p. xvi).

The book also makes an original secondary argument about the mechanism by which ethnic exclusion leads to increased civil war risk. Ethnic exclusion does not only instigate grievances; it also leads to weaker counterinsurgency efforts as the government cannot tap into local networks to provide critical information on rebels. In contrast, ethnic inclusion facilitates counterinsurgency but also increases the ability of ethnic leaders to mobilize military force against the incumbent ruler in a coup d'état.

The theoretical framework also makes predictions about the circumstances under which rulers trade off the risk of civil war for the risk of a coup and power sharing emerges. If a group is well positioned to capture the capital city in a civil war, its chance of seizing power via rebellion begins to approximate its chance of doing so via a coup. In these instances, rulers may be willing to share power with the group, especially if their own group also has high threat capacity and is therefore likely to be included in any future power-sharing agreement if a coup d'état does materialize.

Ethnic Politics and State Power in Africa begins by outlining its argument at length in the first section of the book. Then the author provides empirical evidence drawing on multiple methods, including a qualitative study of Darfur that was used to build the theory, cross-national quantitative tests, and qualitative tests using the case of Africa's "Great War" in the Democratic Republic of Congo.

The study of Darfur in the second section is particularly remarkable. It draws on elite interviews in Khartoum, Darfur, Asmara, N'Djamena, Abuja, Europe, and the United States to explain why a full-scale civil war did not break out in the region in the early 1990s but did in the early 2000s. Roessler shows both that Sudan's Islamic Movement was critical in keeping the peace in Darfur and that Omar al-Bashir recognized this even as he decided to purge Hassan al-Turabi and his supporters from the government after December 1999 to avoid a coup.

The author then tests the effect of power sharing on coups and civil wars in the book's third section using the Ethnic Power Relations (EPR) data set developed by Andreas Wimmer, Lars-Erik Cederman, and Brian Min based on expert surveys ("Ethnic Politics and Armed Conflict: A Configurational Analysis of a New Global Dataset," *American Sociological Review*, 74(2), 2009). Utilizing data on all ethnic groups in sub-Saharan Africa included in the data set, from independence to 2005, Roessler shows that ethnic groups that are included in the central government are significantly more likely to be involved in coup attempts and significantly less likely to be involved in rebellion. Because he places great theoretical emphasis on power sharing as a strategic decision based on groups' threat capabilities, it is surprising that his empirical models do not account for this. However, if his theoretical predictions about when rulers share power with groups are correct (i.e., power sharing occurs when civil war threat is high or coup threat is low), he is biasing his empirical results against his predictions about the effects of power sharing by not accounting for the selection problem.

A bigger issue is that Roessler's empirical evidence does not actually demonstrate that power sharing decreases the security of rulers. His analysis shows that the exclusion of an ethnic group from power reduces the likelihood of the group producing any coup perpetrators, *not that it decreases the overall likelihood of a coup against a ruler.* Ultimately, what matters for a ruler is not the number of ethnic groups who participate in a coup against him but whether a coup materializes at all in his country, and it is not evident that this risk can be decreased by excluding members of non-coethnic groups. Indeed, rulers in Africa appear subject to coups from opportunistic individuals regardless of whether they share power across ethnic lines or not: To this point, more than 40% of successful coups in sub-Saharan Africa are orchestrated in part by monopoly or dominant groups (see Table A3.2 in the book's appendix), and many of the ethnically homogenous countries excluded from Roessler's analysis (Burkina Faso, Djibouti, Lesotho) have fallen into coup traps.

The third section of the book also includes more direct evidence of the commitment problems bedeviling power sharing. Roessler shows that coconspirators—groups that helped the ruler come to power in a violent transition—frequently launch rebellions after being excluded from power. He also draws on qualitative evidence from Africa's Great War, collected in partnership with Harry Verhoeven, to test the importance of coup-proofing in civil war outbreaks.

The test of when power sharing emerges is left to the final section of the book. Here, Roessler draws on joint work with David Ohls to argue that leaders substitute civil-war risk for coup risk by engaging in power sharing in cases where both the ruling group and its rival are in strong positions to seize power in a civil war. This hypothesis is a little bit difficult to square with the author's hypothesis that coconspirators—many of whom have demonstrated capabilities to help win civil wars—frequently launch rebellions after being excluded from power. Instead, he operationalizes threat capabilities by averaging groups' relative size and absolute distance to the capital, and shows that power sharing is significantly more likely in the case of strong opposition and ruling groups.

The book provides a powerful theoretical framework with broad implications, for Africa and beyond. But despite my overall admiration for the book, I disagree with Roessler's claims about the inherent instability of power sharing in sub-Saharan Africa due to the pervasiveness of ethnic security dilemmas.

The theory and evidence in my own book also provide a framework for understanding the prevalence of inclusive governance in Africa. Focusing on an important example of "ethnic" big men in Africa—traditional chiefs—I show that chiefs play critical roles in mobilizing local development projects, and that their relationships with politicians are relevant for the flow of resources to communities. As a result, politicians have incentives to share power with these "development brokers" so that they can tap into their networks to ensure more effective delivery of goods and services. In this view, inclusive governance is beneficial because it allows the state to do more with the same amount of government resources. This framework helps us understand the pervasiveness of power sharing and ethnically inclusive governance across sub-Saharan Africa.

My main difference with Roessler is in the extent to which I think that ethnic security dilemmas undermine power sharing. As mentioned, I do not think the evidence shows that power sharing increases political instability across sub-Saharan Africa, and I am not convinced that commitment problems between rulers and potential challengers are especially great when rulers share power with non-coethnics. Ultimately, Roessler and I make fundamentally different arguments about the importance of ethnicity in organizing politics in our respective books. He argues that "ethnic followers" almost always line up behind "ethnic patrons" (p. 95). Instead, I view citizens as primarily concerned with the performance of their rulers. I provide evidence that citizens may consider the opinions of their local chiefs insofar as they provide information on politicians' performance, but neither ethnics nor non-coethnics of chiefs blindly follow them. In this framework, politics need not break down along ethnic lines.

Inevitably, the extent to which citizens weigh performance versus ethnic alignment in deciding whether to support politicians varies from setting to setting, with some places looking more like Zambia and others more like Sudan. I found Roessler's discussion of ethnic commitment problems most compelling in countries that have already experienced ethnic conflict—as in Sudan, the Democratic Republic of Congo, and several of the cases of coconspirator civil wars. But this begs the question of how we get on the path of ethnic polarization in the first place, something that is not sufficiently explained by either Roessler's theoretical framework or my own. Understanding these inflection points is an important research agenda for future scholarship.

Response to Kate Baldwin's review of *Ethnic Politics and State Power in Africa: The Logic of the Coup-Civil War Trap*

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— Philip Roessler

Thanks very much to Kate Baldwin for her incisive review of my book. Her thoughtful engagement with the text raises some important questions on the nature of African politics and how we study it. Because she is a scholar who focuses on Zambia and other more institutionalized states in Africa, her critique is especially valuable and illuminating. From her perspective, I overstate the explosiveness of power sharing. Rather than a source of violent competition for state power and a potential pathway to civil war, Baldwin sees the institution as a stabilizing, and even a democratizing, force. How can we reconcile these seemingly diametric analyses of African politics?

In many ways, our contrasting perspectives nicely encapsulate the two equilibriums that have arisen in postindependence Africa that I address in Chapter 10 of *Ethnic Politics and State Power in Africa*. In one equilibrium, power sharing has too often broken down into a vicious cycle of ethnic exclusion and civil war. In another, it has proven more durable with few, if any, bouts of large-scale political violence and has paved the way for democratization. That our formative research experiences have been in countries in these different equilibriums—Sudan and Zambia, respectively—it is perhaps not surprising that we draw such sharply different conclusions. Yet strikingly, despite working in these different contexts, we ultimately derive similar models of politics: Both revolve around the incentives that rulers of weak states have to share power with societal brokers to effectively deliver goods and services and overcome low-state capacity. Baldwin shows how brokers play an integral role in facilitating the implementation of development projects in response to voters' demands; I show how they play a critical role in the provision of security and cooperative counterinsurgency.

Why, then, do these similar models lead us to draw different conclusions about the efficacy of power sharing? Baldwin suggests that it is because we treat ethnicity differently. In her view, I assume that citizens blindly line up behind ethnic patrons, making political breakdown along ethnic lines inevitable, whereas she suggests that citizens are motivated by performance, not social identity. I disagree with this characterization. On the contrary, we have similar understandings about the microfoundations of brokerage networks in weak states. Like Baldwin, I believe that citizens absolutely care about regime performance and their access to resources, security, and opportunities. However, the challenge that citizens face is how to hold politicians accountable to ensure that they deliver the goods promised.

Baldwin draws inferences about this process in the context of an electoral democracy that has formally devolved power to local chiefs. What about in other states in which the institutions of democracy and decentralization are weak or nonexistent? How do citizens mobilize to gain access to vital services in these circumstances? In these contexts, networks need not be organized along ethnic lines—as I go to great lengths to show in Chapter 5 in analyzing the Islamic movement in Sudan. But they often are—not because ethnicity socially conditions citizens to organize around this shared identity (though that matters and is understudied), but because ethnicity as a social institution has a number of technological, sociological, and geographic attributes (e.g., common language, overlapping and dense social and kinship ties, physical proximity, shared normative principles) that lower the costs of collective action and coordination.

In such states, the main culprit of the coup–civil war trap is not ethnicity but politics: how elites embedded in these different networks can credibly commit to share power in the absence of strong institutions and when the threat of force is necessary to uphold it. Ethnicity compounds this bargaining problem via its effects on information flows and networks of reciprocity (or trust), but it is ultimately rooted in weak institutions and political uncertainty. As pernicious as this uncertainty is, it can be managed, opening the door to durable power sharing. In addressing both sides of the power-sharing coin—its explosiveness but also its persistence—*Ethnic Politics and State Power in Africa* has sought to advance an integrated theory of war and peace in weak states.

The Paradox of Traditional Chiefs in Democratic Africa. By Kate Baldwin. New York: Cambridge University Press, 2016. 253p. $99.99 cloth, $32.99 paper.

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— Philip Roessler, *College of William and Mary*

Over the past quarter century, the spread of democracy has transformed politics in Africa. The transition to multiparty rule has forced incumbents to expand the political coalitions they need to stay in power. Among the many consequences of the shifting institutional bases of power has been the political resurgence of rural Africa. Rural voters have long been neglected and, even worse, exploited for the benefit of urban constituents, but democratization has induced politicians to pay heed to them. The downstream benefits for these communities have been measurable: Primary school fees are lower if not slashed altogether; farm gate prices tend to be more competitive; and roads and health facilities are improved. Rural areas across many countries in Africa remain some of the poorest parts of the world, yet now they at least have a fighting political chance. But are the effects of democratization merely redistributive, or are they bringing about more fundamental changes in the configuration of political authority in rural Africa? In *The Paradox of Traditional Chiefs in Democratic Africa*, Kate Baldwin argues it is the latter. Written with great lucidity and insight, this compact, well-crafted book tackles the preceding question, and in doing so advances an innovative theoretical model that changes how we think about voting in developing countries.

Baldwin motivates the book by pointing to a striking empirical regularity that speaks to the question of how democratization may be reconfiguring political authority in rural areas. She shows that democratization in Africa has led to the devolution of power to chiefs—unelected traditional leaders whose legitimacy comes from their recognition as customary authorities of a "place-based community" (p. 21). Not only are democratic governments more likely to include constitutional provisions that protect chiefs' authority; they also tend to increase their power over land tenure.

As the book's title suggests, however, this is a seeming paradox: Why would elected governments reconfigure political authority in a way that strengthens the hands of a group of actors who are often dismissed as "antidemocratic local despots" (p. 3)? One possibility is that like the party bosses of Tammany Hall, unelected chiefs are effective *vote brokers*. Embedded in local communities and equipped with the tools of patronage, coercion, and social sanctioning, traditional leaders help politicians solve the agency problems that arise from voter mobilization and vote buying. From this perspective, elected politicians may actually prefer the autocratic nature of these despots because they are cheaper to buy off and monitor than the "democratic masses."

Although it is often assumed that local electoral politics in Africa follows this "vote-broker model," as Baldwin notes, there is little systematic evidence to support the claim. Moreover, she questions the tenability of the framework's core assumptions. Vote-broker models tend to assume that citizens are motivated by petty patronage or social identity. But building on a growing scholarship in comparative politics, Baldwin conceives of citizens as "first and foremost . . . evaluative voters"—and in local elections the key evaluative criterion is which candidate can best deliver local public goods or development projects (pp. 69–70). From this perspective, if traditional chiefs merely operate as "decentralized despots" who whip votes, it is not clear how they help on this key dimension: If anything, they may hurt politicians, as their use of patronage and coercion undermines public goods provision.

It is on this point that Baldwin makes one of the most important contributions of her book—a reappraisal of traditional leaders as political actors in contemporary Africa. (Chapter 2 is an excellent primer on traditional authorities and their continued relevance in Africa.) Such leaders are often regarded as an anachronistic institution that blocks modernization and development and undermines democracy, but, Baldwin suggests, this does not reflect reality. Instead, she illuminates the integral role that traditional leaders play as development actors, especially in the process of "coproduction"—a concept she draws from Elinor Ostrom. With the state too weak to enact top-down development, governments lean heavily on local communities for help. Governments provide some financing and resources, but they rely on the contributions of communities, both financially and in terms of labor (e.g., to help make bricks or dig boreholes). Coproduction will break down, however, unless communities are able to collectively organize and coordinate their efforts with government officials. Baldwin argues that the *electoral* influence of chiefs comes from their power as *development brokers* who facilitate coproduction. In playing this role, chiefs are thus critical to government responsiveness to meet the demands of its citizens for the delivery of local public goods. She argues that it is for this reason—not because they increase the efficacy of clientelism or ethnic voting—that democratic governments have readily increased chiefs' political authority.

Baldwin tests the "development broker model" primarily in Zambia and reports the results across Chapters 6 through 8. The empirical chapters systematically build evidence to support the book's central argument. Chapter 6, on leveraging chiefs' deaths as a natural experiment, shows that the absence of a chief leads to a significant decrease in schools built and rehabilitated and boreholes dug. Chapter 7 tests the flip side of coproduction and shows that the stronger the ties between chiefs and members of parliament, the greater the provision of public goods in the form of classrooms built and the better condition of roads. Chapter 8 then assesses the potential electoral implications of coproduction. It shows that citizens are most responsive to a chief's political endorsement of their MP *if* they believe that MPs and chiefs are jointly important for local governance. Chapter 9 considers the generalizability of the model beyond Zambia.

Overall, *The Paradox of Traditional Chiefs in Democratic Africa* represents the best in the Batesian tradition of the study of African politics: a concise but powerful monograph that advances an elegant theoretical framework—drawing heavily on empirical evidence from Zambia nonetheless—that alters how we think about fundamental political processes, not just in Africa but in developing countries more broadly. Consider the study of clientelism on voting. Few research programs have been as active in political science in recent years. Yet Baldwin's book challenges the underlying assumptions of this research program head-on. The author argues that this literature's narrow focus on patronage and identity fails to capture the "sophisticated" nature of rural voters and the discerning manner in which they evaluate politicians and pursue their own interests. With regard to the African politics literature, the book speaks to an important debate on the resilience of precolonial institutions after the short but devastating shock of European colonialism. Her analysis, in line with other recent scholarship on the enduring effects of precolonial institutions, calls for a critical reassessment of the *degree* to which colonialism "divorced" chiefs from their communities (p. 182). Subnational variation on this dimension seems to be of particular significance as Baldwin shows that it is traditional leaders' *embeddedness*—the degree to which they live and have an "encompassing interest" in their communities (to use Mancur Olson's term)—that determines their effectiveness as development brokers.

This study also has important policy implications for aid donors, on which many democratic governments in Africa rely heavily. Donors' aversion to working with traditional chiefs, whether for ideological, normative, geographical or technological reasons, may hinder aid effectiveness.

There are also reasons to be cautious, however, about the generalizability of the development broker model and the implications that follow from it. First, Baldwin relies heavily on the Zambian case because, as she acknowledges, the organization of political authority around "a fixed number of recognized chiefs," about which there are "detailed and highly disaggregated data," facilitates rigorous empirical analysis (pp. 95–96). But the historical and institutional factors that gave rise to this organizational structure make Zambia a most-likely case. That so much of her analysis draws from this case, with only cursory evidence from other countries, leaves open the question of how far the development broker model travels, especially to Francophone Africa. In Chapter 2, she aims to show that chiefs are not irrelevant in former French colonies, but their electoral influence as development brokers is never systematically tested. And the limited cross-national analysis that is presented in Chapter 9 points to the weakness of the power of traditional leaders in former French colonies.

Another important question is how the development broker model works in national elections. Baldwin subsets her analysis to focus primarily on the model's implications for constituent-level parliamentary elections. She acknowledges that the model is probably less relevant for national-level elections given chiefs' weaker influence and ties at the top echelon of the state. But how then do presidential candidates mobilize local support? Given the scale of the coalitions that national politicians need to cobble together, such campaigns seem to lend themselves more to the use of patronage and identity politics—where one would expect chiefs to play a key role in voter mobilization. Is this the case? If so, to what degree does this demand drive the devolution of power to chiefs, and what are the consequences for the development broker model? Can chiefs serve as vote brokers and development brokers simultaneously?

Finally, what is the overall impact of chiefs as "development brokers?" The local benefits are clear, as are the effects on state–society relations—it helps to underwrite a positive feedback loop of mutual engagement between local governments and their citizens—but to what degree is this leading to "state building via traditional leaders" (p. 16) and helping to solve the "democratization backwards" problem that plagues countries in Africa (p. 16)? Baldwin advances this claim as one of the main implications of the book's analysis. Here I am more skeptical. The contributions of the chiefs to state building are limited. They coordinate between politicians and citizens to increase the production of boreholes and schools, but as the empirical analysis shows, they have little influence on large-scale development projects that are ultimately necessary to help these countries escape the weak state trap. More problematic is the discretionary authority that chiefs have over the siting of these projects and the extent to which this may skew local development to disproportionately favor those close to the chief (p. 77).

This raises one of the most important questions for future analysis: What is the effect of the development broker model on local bureaucratic capacity? The answer is not obvious. On the one hand, it seems likely that this mode of development, which relies heavily on personal and ad hoc arrangements between politicians and traditional leaders, subverts the building of a meritocratic and impersonal bureaucracy tasked with impartially providing public goods. At the same time, however, the development broker model does seem to be stimulating greater citizen engagement and elevating expectations of the government's role in development. Without this bottom-up demand, politicians have little incentive to invest in building bureaucracies that can more efficiently meet citizens' requests for better public services. But herein lies a new paradox. It is hard to envisage the implementation of such bureaucratic reforms except at the expense of the authority of traditional leaders. If chiefs resist such change—and it is hard to see how they embrace it—this raises the possibility that ultimately the devolution of power to chiefs may hinder as much as aid development in democratic Africa.

Response to Philip Roessler's review of *The Paradox of Traditional Chiefs in Democratic Africa*

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— Kate Baldwin

Philip Roessler's attentive review of my book concludes with questions about the scope of the theory's applicability, the possibility for vote brokering in presidential elections, and the implications of traditional chiefs for state building. Although these questions will only be answered satisfactorily once additional scholarship has accumulated on traditional chiefs, I use my response to outline what my theoretical expectations are and how they diverge from Roessler's hypotheses.

The Paradox of Traditional Chiefs in Democratic Africa emphasizes the role that chiefs play in brokering local development projects and the motivations this gives citizens for preferring electoral candidates with stronger relationships to chiefs: The rationale is that more resources are likely to flow to local communities through this brokered relationship if the connections between chiefs and politicians are stronger. The theory depends on the existence of locally embedded chiefs with some influence over the day-to-day governance of their communities. In addition, it only gives citizens motivations for supporting one candidate over another in cases in which the local chief is differentially connected to the competing candidates. I would expect the development broker model to apply in all instances in which these conditions hold. These include some regions of Francophone Africa (on this theme, see Lauren Honig, "Selecting the State or Choosing the Chief? The Political Determinants of Smallholder Land Titling," *World Development* 100, 2017) and some chiefdoms in presidential elections.

Importantly, I disagree with Roessler's hypothesis that chiefs are likely to serve as traditional vote brokers in presidential elections, relying on vote buying, coercion, and social sanctioning to mobilize citizens. Instead, my hypothesis is that the development broker model will operate, but with regional-level brokers. Voters will consider the strength of the connections between major presidential candidates and regional leaders (including some higher-level chiefs), because these leaders are the most relevant brokers of resources from the president. Thus, in both parliamentary and presidential elections, I hypothesize that voters are primarily concerned with evaluating how well politicians will perform in delivering to their communities if elected.

Roessler also raises important questions about the relationship between the strength of chiefs and the strength of the formal bureaucracy. In particular, he raises concerns that politicians might be discouraged from investing in the bureaucracy in cases in which they can respond to citizens through traditional institutions. Does this in turn harm the ability of the state to provide public goods that must be provided on a wide geographic scale? Would investments in the bureaucracy eventually result in a fairer allocation of local public goods? These questions follow naturally from the framework in Roessler's book, which emphasizes the inherent instability of regimes dependent on informal alliances with ethnic big men.

In contrast, I am both more sanguine about the types of governance that result from alliances with traditional chiefs and more pessimistic about the types of politics that would emerge if politicians invested directly in the formal bureaucratic apparatus. Strong local chiefs are very effective substitutes for the formal bureaucracy when it comes to delivering local public goods. This helps solve the "democratization backwards" problem because it allows elected officials to meet the highest-priority demands of rural citizens, even without a formal bureaucratic apparatus to assist them.

Furthermore, I am skeptical that weakening traditional chiefs would lead to better delivery of either national or local public goods in the medium term. In the absence of traditional leaders, I *would not* expect states to invest more in bureaucratic agencies that provide national public goods. In fact, the ability to deliver local public goods through traditional leaders—a cheaper substitute apparatus—frees up funds for the army, the electrification authority, and the anti-corruption commission. If there is resistance to investing in these agencies, I do not think it is coming from local traditional chiefs.

Without chiefs, I *would* expect states to invest more in the arms of the bureaucracy that deliver local public goods like schools, clinics, and wells. But, unfortunately, I would not expect the bureaucratic apparatus to be fairer in its allocation of these goods. Partisan bureaucracies are widespread in developing democracies (see Allen Hicken, "Clientelism," *Annual Review of Political Science* 14, 2011). A strengthened bureaucratic apparatus would almost certainly continue to favor certain citizens over others, creating a distribution of resources that may even be *less* equal than when politicians must work with traditional leaders to provided targeted goods.