leaving the truly heavy-going work for the appendix. At the very least, references in the text to where the statistical analysis appeared in the appendix would have been helpful.

Irrespective of stylistic issues, Bates has clearly issued a methodological challenge. Tying game-theoretic models explicitly to national outcomes and then developing credible proof using a range of evidence is clearly a powerful approach. It requires an impressive knowledge of the political science literature, quantitative techniques, and area studies work. The author also makes clear that his extensive fieldwork in Africa has influenced the models he chooses to develop. *When Things Fell Apart* has a lofty set of methodological ambitions and goes a considerable way to defining what "rigor" should actually mean in the study of national outcomes, not only in Africa but also the rest of the world.

Overall, Bates has produced a book that will serve as an important summary of the political dynamics of state failure in Africa, especially as it was experienced in the 1980s and 1990s. Some African countries have moved on. As a result, state failure today does coexist with some countries that are on the rise and with many others that are somewhere in between. The great challenge for the future will be to explain this increasingly complex set of outcomes using all of the methodological tools that Bates has deployed.

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When Things Fell Apart manages to be wonderfully concise but still compelling. The thing Robert Bates seeks to explain is the secular trend in sub-Saharan Africa toward civil war, although he often characterizes this in broader terms, as a trend toward "political conflict" or "political disorder." He explains the trend as follows: Public revenues fell in the 1970s and 1980s as a result of commodity price declines, effects of the second oil shock, and bad economic policy choices that overtaxed farmers so that politicians could dispense patronage to smaller, politically more important urban constituencies. The decline in public revenues led elites to become more predatory, which caused an increase in political conflict by mobilizing opposition. Popular demands for political reform, along with increased international pressure for the same at the end of the Cold War, heightened elite insecurity and led to more predation. This had the effect of "provoking their citizens to take up arms" (p. 109). Further, state decline and national-level conflicts exacerbated simmering subnational conflicts, typically in the form of land disputes between locals and migrants from other tribes.

I think that this composite, stylized sequence captures important aspects of the story for many sub-Saharan countries, even if it does not fit any one country's decline into civil war perfectly. The comments that follow are mainly suggestions about how further research might flesh out Bates's theoretical argument and better test its main empirical claims.

The first is a matter of theory. Bates suggests that political order is like a cooperative equilibrium in a repeated prisoner's dilemma game between citizens and the ruler. Order is more likely to break down when the ruler's temptation to "defect"—to prey on citizens rather than to protect their productive activities—increases. The temptation to defect increases when the value of the cooperative status quo declines for the ruler. The author associates the value of the cooperative equilibrium for African rulers with their tax revenues. Thus, revenue declines from external economic shocks and the effects of politically driven bad economic policies are said to have increased leaders' incentives to destroy political order.

This argument does not work, however, if the decline in tax revenue causes a proportional decline in the ruler's payoffs from predation. When tax revenue is smaller, there is less to grab; when the economy is weaker, license monopolies are worth less; when peasants are poorer, there is less to steal from them. Bates notes this himself when he argues against a common proposal for explaining the association

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between low per capita income and civil war: "If, as many argue, lower per capita incomes imply lower wages and therefore lower costs of rebellion, so too do they imply lower gains from predation" (p. 13). Unfortunately, this same argument cuts against his own proposal concerning public revenues, to the extent that these are based on direct or indirect taxes on peasant incomes.

One possible way out is to propose a "two sector" economy, where the gains from predation come more from one sector than another, and the sectors are differentially affected by international price changes. Bates suggests natural resource rents as one source of rewards from predation. If these increased in value as public revenues from taxes on peasant production declined, then this might have increased elites' "temptation to defect"—although it is not clear to me how to think about this, given that elites already controlled these rents. A simple alternative to "increased temptation to defect" would be that competition for resource rents increased as the non-resource-based economy declined, and this competition was more likely to turn violent. To test this argument, we would really like to look directly at changes in different commodity prices for different sub-Saharan countries in this period.[1]

There are other possible reasons for why a decline in tax revenues and international pressure to democratize might increase civil conflict, even without Bates's next step of increased elite predation. Negative shocks to state coercive capabilities can open up windows of opportunity for contenders to try to grab local or national power by force; rulers might like to buy them off with promises of future redistribution, but cannot credibly commit to keep their promises after the state recovers.[2] The author expresses some skepticism about this possibility, but I am not sure why. Of the 26 post-1970 sub-Saharan civil wars listed in his Figure 1.1 (p. 4), 10 began between 1989 and 1991. It seems implausible to me that the mechanism was often international pressure to democratize, causing leader insecurity causing repression causing rebellion. Rather, in a number of cases, international pressure to democratize acted in part as a shock to a regime's strength against potential challengers. In any event, state weakening due to lower tax revenues and international pressure to liberalize could well have had both "push" (predation) and "pull" (opportunity) effects on opposition mobilization and rebellion.

The empirical support for Bates's argument comes from case evidence in the body of the text and from cross-national statistical models in the appendix. These two forms of evidence actually address slightly different research questions. In the chapters, the question is what explains the secular trend since the early 1970s toward more civil war or "political disorder" across sub-Saharan Africa. In the appendix, the implicit question is why some African states developed bad economic policies/policy reform/militias while others did not. In both sections, more data on two of the main "moving parts" in the theory—commodity prices and government revenues—as well as more data and discussion about the measurement of the dependent variable could have been helpful.

In the chapters, Figure 6.1 (p. 100) shows a secular decline in the share of government revenues from trade, and slowing but still *positive* growth of average government revenues from 1970 to around 1990.[3] In the appendix, Bates shows some evidence that countries with low government revenues were more likely to have a militia, but finds at best very weak evidence that declining revenue over time within a country on average raises the risk of militia formation. The latter relationship is more important here, since the argument is about falling government revenues causing predation causing conflict, and the cross-sectional relationship could be spurious for a number of reasons. As the author notes, there is a serious concern about endogeneity with this key independent variable, both in the pooled cross-section analysis and in the models with country fixed effects. Political disorder, or even the expectation of it, plausibly causes lower government revenue. Contrary to his suggestion (p. 171), failing to account for this would lead us to overestimate rather than underestimate the causal impact of low tax revenues on the occurrence of militias. So while the argument seems plausible, the evidence he presents that declining government revenue was an important cause of the rise of political disorder for many African countries in this period appears a bit thin to me.[4]

On the dependent variable, in the chapters Bates provides many country-specific examples of violent conflict in sub-Saharan Africa. But these examples range over the whole postindependence period, and he also often illustrates his arguments with examples from countries that did *not* experience significant political disorder (at least by comparison to Sierra Leone, Liberia, Somalia, and so on). For instance, Bates discusses Zambia and Benin at some length in Chapter 3 to illustrate his argument about how political dynamics led sub-Saharan rulers to narrow the political arena, reducing public goods provision and creating widespread popular grievances. But in neither country did the state ever "fall apart" into civil war or even much lower-level political violence. If the same political dynamics that he sees as causing state failure are present in these countries as in Sierra Leone, Somalia, and so on, then what explains the different outcomes? Should this lower our confidence in the causal story?

The cross-national analysis in the appendix might have helped here, for instance, by means of a presentation of predicted probabilities of militias and actual militia presence across countries. Which of Bates's independent variables distinguish Zambia, Benin, et al. from Sierra Leone, Somalia, et al., and are these variables the ones that Bates puts the most weight on in his theoretical account?

More broadly, I would have liked a fuller discussion of the militia presence variable, its coding and distribution

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in time and space. Almost all studies before Bates—and, for the most part, Bates in the book's chapters—have used civil war as the thing to be explained. How are measures of civil war and militia presence related and how do they differ? This seems like an interesting and promising measure that is underexploited—a cost of concision. The author presents a graph (Figure 6.3, p. 121) that is probably reporting the results of fitting a logit with militia presence as the dependent variable against a quadratic function of year. The estimated percent of countries with militia presence declines a bit from 1970 to 1977 and then almost doubles from about 20% to 38% in 1995. This is consistent with his suggestion that commodity price changes and the second oil shock in the late 1970s kicked off a chain of events leading to widespread political disorder. However, time series of civil war incidence in Africa tend to show a strong and steady rise throughout the 1970s, arguably starting right at independence (early 1960s). So an alternative measure of political disorder might lead us to put less weight on the importance of international economic forces.

It is remarkable how much Bates brings together in this book of 174 short pages. He provides insightful discussions of many specific countries' politics in the context of a "big picture" presentation of continent-wide patterns and trends. In laying out a simple and clear theoretical argument, he manages a nearly seamless integration of ideas from anthropology, classical and modern political economy, game theory, and econometrics. And this is a book that can be read with equal profit and engagement by an academic specialist or a nonacademic. Bates pioneered this format, or style, with his influential *Markets and States in Tropical Africa* (1981; 132 pages). It is a shame that more of us have not followed his lead, but it is also not surprising, given how hard it is to carry off. The complaints and queries here do not take much away from the achievement.

Notes

1 Besley and Persson (2008) and Olsson and Fors (2004) develop models of conflict in economies that have a natural resource sector and another productive activity; they get results on how conflict varies with the relative value of production in the two sectors. Besley and Persson present an empirical test in a global sample that finds, consistent with their model, that conflict incidence increases with both a country's import and export prices for raw materials. Dal Bó and Dal Bó (2008) model incentives for conflict in an economy with capital- and labor-intensive sectors, showing how conflict increases with shocks that reduce returns to labor (as workers shift into appropriation). Fearon (2008) explores the neutrality of conflict to per capita income in standard conflict models.

2 See Fearon (2004) and Fearon and Laitin (2007). Dal Bó and Powell (2009) show that negative shocks to state revenues can increase conflict risk in a model where the ruler has private information about state revenues and other elites or groups need to threaten conflict to ensure that he or she does not "eat" it all.

3 These graphs present output from a parametric statistical model. Why not just plot the actual averages by year (slightly smoothed if need be)?

4 Direct measures of African states' public revenues are not included in the logit models for bad economic policies ("control regimes") or political reform. They probably should be, given the theoretical argument.

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