

Correspondence

Civil War Settlements and the Prospects for Peace

Laurie Nathan

Monica Duffy Toft

To the Editors (Laurie Nathan writes):

In “Ending Civil Wars: A Case for Rebel Victory?” Monica Duffy Toft questions whether policymakers are correct to have a strong preference for terminating civil wars through negotiated settlements.¹ Her main endeavor is to undertake a statistical analysis that compares the effect of negotiated settlements, military victories, and cease-fires/stalemates on war recurrence and on the state’s level of democracy and economic growth. With respect to war recurrence, which is the focus of this letter, she finds that negotiated settlements are largely ineffective, that civil wars ending in military victory by one side are less likely to recur, and that rebel victories produce the most durable settlements (pp. 7–8). Toft’s purpose is not only to understand these phenomena but also to provide policymakers with guidance (p. 22). She recommends that third parties should pay greater attention to security-sector reform (SSR) during negotiations, leading to settlements that can credibly guarantee both benefits from cooperation and harm from defection; failing that, support in pursuit of victory, especially rebel victory, may be a worthy objective for policymakers (p. 36).

Given that the stakes and risks of external intervention in a civil war are high, it is worth reflecting on the merits of Toft’s results and recommendations. I suggest below that her findings on war recurrence and the stability of rebel victories are not dependable and helpful for policy purposes. Advice to policymakers on war termination ought to be based on case studies that explore causal relationships and examine the role actually played by policymakers in seeking to end civil wars.

CONTESTING THE STATISTICAL FINDINGS

Toft finds that in the period 1940–2000, war recurrence was 22 percent after negotiated settlements (5 of 23 cases), 12 percent when wars ended in military victory (10 of 81 cases), and only 6 percent when wars ended in rebel victory (2 of 33 cases) (p. 16). These findings are questionable because they rest on contestable judgments about some of the cases. A small number of classification changes based on plausible interpretations that differ from those of Toft can have significant statistical implications.

In this regard, Toft notes that scholars disagree on “matters of interpretation about how to aggregate or disaggregate certain conflicts.”² She points out that Michael Doyle

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1. Monica Duffy Toft, “Ending Civil Wars: A Case for Rebel Victory?” *International Security*, Vol. 34, No. 4 (Spring 2010), pp. 7–36. Further references to this article appear parenthetically in the text. In response to my query to Toft regarding her data set and coding, I was referred to, and have used extensively, “Data Appendix for Monica Duffy Toft,” *Securing the Peace: The Durable Settlement of Civil Wars* (Princeton, N.J.: Princeton University Press, 2010), <http://belfercenter.ksg.harvard.edu/files/MTcodebook2010.pdf> (accessed February 11, 2011).

2. Toft, “Data Appendix,” p. 10.

and Nicholas Sambanis consider Afghanistan to have had three separate wars from 1978 to 2001; Guatemala to have undergone separate civil wars in 1966–72 and 1978–94; Nicaragua to have experienced separate conflicts with the Sandinista and Contra uprisings; and the Democratic Republic of Congo (DRC) to have had separate wars in 1996 and 1998.³ Toft combines the conflicts in each of these countries on the grounds that their combatants were essentially the same, they were fighting over the same issues, and the war could not be considered “terminated” in the interim.⁴

What is the statistical effect of these different interpretations? The rate of war recurrence after rebel victories would rise from 6 percent to 16 percent (6 of 37 cases) if the following four cases, omitted from Toft’s data set as separate conflicts, were included and coded as recurring wars: the leftist victory in Afghanistan in 1978; the mujahideen victory in Afghanistan in 1992; the Sandinista victory in 1979; and the Kabila victory in the DRC in 1996. Furthermore, whereas Toft codes the Liberian civil war in 1989–97 as a rebel victory and as nonrecurring,⁵ the subsequent war in 2002 can plausibly be seen as a recurrence of the earlier war.⁶ A coding change in this respect would raise the war recurrence rate of rebel victories to 19 percent (7 of 37 cases), which is close to that of negotiated settlements.

For policymakers the crucial point is not whether Toft or some other scholar is correct on the disputed cases. The point is rather that statistical findings that can fluctuate notably as a result of a few legitimate differences of opinion are not a dependable guide to decisionmaking on the most appropriate means of ending civil wars.

It is also necessary to challenge Toft’s interpretation of her finding regarding the durability of negotiated settlements. In the introduction to her article, she states, “Although since 1990 the preferred means for ending civil wars has been negotiated settlements, these have proven largely ineffective: civil wars ended by negotiated settlement are more likely to recur than those ending in victory by one side” (p. 7). There are two separate claims here. The first is that negotiated settlements have been largely ineffective, and the second is that wars ending in negotiated settlements are more likely to recur than those ending in victory. The first claim is not justified in light of Toft’s finding that war recurrence after negotiated settlements is 22 percent. If 78 percent of negotiated settlements were not followed by further war, the reasonable conclusion is that these settlements have been largely effective. Given the long-term success rate of 78 percent, moreover, it is wrong to conclude that negotiated settlements are “not self-sustaining” (p. 34) and that peace after negotiated settlements has “tended to be short lived” (p. 35).

THE INADEQUACY OF THE EXCLUSIVE FOCUS ON “RECURRING WARS”

Toft defines success in civil war termination as “ending the violence and establishing the political space for enduring peace” (p. 8). Yet her statistical analysis covers only “recurring wars,” excluding other subsequent civil wars. She states that “wars are considered to have recurred if they take place when the principal combatants and the

3. *Ibid.*, pp. 10–11. Toft refers here to Michael W. Doyle and Nicholas Sambanis, “International Peacebuilding: A Theoretical and Quantitative Analysis,” *American Political Science Review*, Vol. 94, No. 4 (December 2000), pp. 779–801.

4. Toft, “Data Appendix,” pp. 10–11.

5. *Ibid.*, p. 4.

6. International Crisis Group, “Liberia: Unravelling,” *Africa Briefing*, No. 10 (Brussels: ICG, August 19, 2002).

principal stakes are the same as those of a previous conflict.”⁷ This approach is problematic. If scholars and policymakers want to gauge the effect of different war termination types on “enduring peace,” then they are surely interested in all subsequent civil wars, especially those that are causally linked to the way in which the previous war ended.

Toft’s narrow focus on “recurring wars,” ignoring other civil wars, has three negative consequences. First, we cannot see how different types of war termination have led to further wars with different combatants and stakes. Second, the extent of peace and stability after rebel victories is grossly overstated. For example, Toft classifies the following wars as rebel victories and as nonrecurring: the Chinese Revolution of 1945–49; the Vietnam war in 1946–54; the Algerian war of independence that ended in 1962; the victory of the Front for the Liberation of Mozambique (FRELIMO) in 1975; and the Afghanistan war in 1978–2001.⁸ So classified, these cases contribute to Toft’s conclusion that rebel victories are “stable” and “durable” (pp. 8, 10, 33, 34). In reality, hundreds of thousands of people died in the Chinese Cultural Revolution in the 1960s; Vietnam underwent a terrible war in 1957–75; Mozambique experienced a devastating war with the Resistencia Nacional Mocambicana, or RENAMO, between 1976 and 1992; Algeria returned to civil war in 1992; and there are few places where peace and stability have been more absent than in Afghanistan. Third, Toft’s definition of “recurring war” gives rise to a statistical bias in the category “rebel victory.” Seven of the thirty-three cases she includes in this category were wars of independence where the colonial power withdrew from the country, making it impossible to have had a further war with the same combatants.⁹

Problems also stem from the fact that “recurring wars” must meet the definition of a civil war. The relevant criteria include an average 1,000 battle deaths per annum and the requirement that at least 5 percent of the battle deaths must have been suffered by the stronger side (p. 12). Consequently, Toft does not include postwar violence that falls short of these criteria. For example, she codes the 1955 coup in Argentina and the 1973 army revolt in Chile as rebel victories and as nonrecurring.¹⁰ Given that thousands of people were killed, imprisoned, and tortured under the two ensuing dictatorships, the effect of Toft’s approach is once again to overstate the stability of rebel victories.

Policymakers wanting to end civil wars are concerned not only with the risk of recurring civil war but also, more broadly, with postwar stability and the absence of violent conflict.¹¹ Indeed, the broader concern is reflected in Toft’s definition of “success” in civil war termination, quoted above. Her narrow focus on “recurring wars” is not consistent with this definition.

THE LIMITED POLICY UTILITY OF THE STATISTICAL FINDINGS

Aside from the problems with Toft’s definitions and classifications, her statistical findings have limited policy utility. There are three reasons for this. First, the averages

7. Toft, “Data Appendix,” p. 6.

8. *Ibid.*, pp. 2–4.

9. The cases are Algeria, Cameroon, Guinea-Bissau, Indonesia, Morocco, Mozambique, and Tunisia. See *ibid.*, pp. 2–4, 10.

10. *Ibid.*, pp. 2–3.

11. See United Nations, “Prevention of Armed Conflict: Report of the Secretary-General,” UN doc.

and correlations tell us nothing about causal relations. If policymakers want to develop general policy on negotiated settlements, or if they seek to promote a settlement in a current war, the fact that five of twenty-three previous negotiated settlements failed is insufficiently instructive. The vital question is why they failed and whether there are any lessons that might be relevant to other cases. As discussed further below, this is a matter that requires investigation through case studies.

Second, if scholars want to offer guidance to policymakers, it is not enough for them to analyze the situations in which the policymakers might intervene. They should also review critically how policymakers have actually intervened in these situations in the past, and with what consequences. More specifically, it is not sound advice to encourage policymakers to promote military victories without any regard to the impact of their efforts historically to promote military victory by governments or rebels (e.g., the United States in Latin America).¹² Nor is it sound to offer advice on external support for negotiated settlements without considering the results of such support in previous instances. The involvement of third-party actors in civil wars and fragile postwar environments is always complicated and sensitive, and it often has unintended outcomes that include exacerbating the conflict.¹³

Third, Toft's statistical findings reveal only averages and general patterns, which provide little guidance on the best response in a specific case. If policymakers want to be effective and avoid doing harm, they must pay close attention to the peculiarities of each war, the strategic possibilities and constraints, the preferences and conduct of the parties, and the fundamental ethical issues at stake. Because of the variability of these factors from one case to another, the statistical findings do not offer a solid basis for decisionmaking.

This observation is most pertinent in relation to Toft's conclusion that it may be a worthy objective for policymakers to support military victory (p. 36). The conclusion ignores the all-important contextual questions of politics, strategy, and norms. In relation to the Sudanese civil war that ended in 2005, for example, it would have been fanciful for policymakers to have promoted victory by the southern rebels and inappropriate to have promoted victory by the extremist National Islamic Front that had seized power through a coup in 1989. Similarly, policymakers would have been ill advised to have discouraged South Africa's negotiations in the early 1990s and to have encouraged instead a military victory by the apartheid regime or the African National Congress. As with many negotiated settlements, the South African negotiations were the choice of the belligerent parties, which recognized that military victory was unattainable.

Even where military victory is attainable, policymakers are constrained by international law and norms. For example, a victory by the Sudanese government was probably feasible at the start of the Darfur conflict in 2003, but it was not a tenable option for

A/55/985-S/2001/574, 2001; U.K. Department for International Development, *Preventing Violent Conflict* (London: Department for International Development, 2006); and Organization for Economic Cooperation and Development, *Helping Prevent Violent Conflict* (Paris: OECD, 2001).

12. See Lars Schoultz, *Beneath the United States: A History of U.S. Policy toward Latin America* (Cambridge, Mass.: Harvard University Press, 1998).

13. See Organization for Economic Cooperation and Development, *Do No Harm: International Support for Statebuilding* (Paris: OECD, 2010).

Western policymakers because of Khartoum's brutal campaign of ethnic cleansing if not genocide.¹⁴ The actual facts of this case, as with every case, were vastly more relevant than statistical findings derived from a range of diverse cases.

The limited utility of statistical generalizations drawn from diverse cases is acute with respect to "rebel victories," which in Toft's data set include the Bolivian popular revolt of 1946, the coups in Chile and Argentina, the North Yemeni civil war in 1962, FRELIMO against Portugal, the Khmer Rouge, the Taliban in Afghanistan, the Tutsi invasion of Rwanda during the 1994 genocide, and other dissimilar cases.¹⁵ Apart from the analytical fallacy of treating these disparate conflicts as constituting a single category, for both normative and strategic reasons policymakers view terrorist groups, national liberation movements, and soldiers mounting a coup as distinctly different.

SPECULATING ON THE CONTENT OF NEGOTIATED SETTLEMENTS

Toft claims to offer "a general theoretical explanation for civil war termination that is consistent with [her] article's empirical findings" and that accounts "for why peace eludes negotiated settlements" (p. 27). As indicated above, however, her statistics show that in most cases peace did not elude negotiated settlements. A general explanation for civil war termination should cover this finding. Yet aside from a brief comment on El Salvador (p. 35), Toft does not try to explain the reasons for successful negotiated settlements.

Toft states that the failure of negotiated settlements results from two factors: the settlements did not pay adequate attention to SSR (p. 32); and the settlements fell short in the credibility of their promise to inflict harm if the parties failed to comply with the terms of the settlement (p. 34). This assertion is largely speculative, because Toft does not substantiate it with reference to the negotiated settlements she classifies as failures (i.e., Angola, Iraq, Lebanon, the Philippines, and Sudan).¹⁶ The assertion would be credible if it were shown to account for the breakdown of these settlements, and it would be compelling if it were shown to be one of the key differences between the negotiated settlements that succeeded and those that failed. With one exception, though, Toft does not examine these relationships. The exception is El Salvador, where she says the negotiated settlement lasted because "it contained robust provisions for the reconstruction of El Salvador's security forces. It contained both carrots and sticks" (p. 35).

Toft's main recommendation is that policymakers should promote SSR during negotiations to ensure harm from defection (pp. 32–36). She does not explain how, in El Salvador or elsewhere, SSR can accomplish this. Although she believes that SSR has received little consideration in academic research (p. 32), there is in fact a sizable literature that includes many case studies.¹⁷ A prominent theme of this work is the difficulty

14. Nicholas D. Kristof, "Genocide in Slow Motion," *New York Review of Books*, Vol. 53, No. 2 (February 2006), pp. 14–17.

15. Toft, "Data Appendix," pp. 2–4.

16. Toft describes Colombia as a case of neglected SSR, but it seems clear from her own account that the problems she identifies—the frustration of the armed forces, leading to the emergence of militias that attracted state military personnel—resulted from the government's approach to dealing with the Revolutionary Armed Forces of Colombia, or FARC, rather than from a failure to reform the security sector. Toft, "Ending Civil Wars," p. 33.

17. See, for example, Eboe Hutchful, "Security Sector Reform Provisions in Peace Agreements" (London: African Security Sector Network, January 2009); Alan Bryden and Heiner Hänggi, eds., *Security Governance in Post-Conflict Peacebuilding* (Münster, Germany: LIT Verlag, 2005); Gavin Cawthra and Robin Luckham, eds., *Governing Insecurity: Democratic Control of Military and Security*

of achieving security reform in postwar countries because the reforms challenge vested interests, power relations, and dominant paradigms and because they require a level of expertise, resources, institutional stability, and political cohesion that is typically lacking in these countries. The research also illustrates my contention that advice to policymakers should be informed by a critical review of their activities. External actors that promote SSR have been ineffective when they have intervened in an overly prescriptive fashion based on Western models, with insufficient flexibility and responsiveness to local actors, and lacking a good appreciation of the sensitivities of the security sector.¹⁸

In conclusion, policymakers who support negotiated settlements in civil wars would benefit from knowing whether there are any general patterns or specific problems among past cases that have a bearing on the settlement of current and future cases. Such patterns and problems have to be investigated through case studies that explore the complex reasons for subsequent wars and the ways in which external actors have facilitated and retarded war termination.

—Laurie Nathan
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To the Editors (Monica Duffy Toft writes):

Laurie Nathan makes some excellent points regarding my article “Ending Civil Wars: A Case for Rebel Victory?”¹ He correctly notes, for example, that a critical objective of my analysis is to provide better guidance to policymakers and that I argued that negotiated settlements should not be the default approach to ending civil wars. I make this argument for two reasons. First, negotiated settlements are more likely to break down than military victories, leading to a higher likelihood of civil war recurrence. Second, even though negotiated settlements are less common than military victories in ending civil wars, they receive far more attention in both the scholarly and policy literatures.

In my article, I examined military victories in civil war to determine why they are more likely than negotiated settlements to result in lasting peace. Two questions guided my analysis: “What is it about negotiated settlements that has produced failure, and what is it about victories that has led to success? Might it be possible to incorporate the more effective mechanisms of victory into negotiated settlements, so as to determine a new type of settlement that achieves all of the political objectives being sought?” (p. 32).

Establishments in Transitional Democracies (London: Zed, 2003); Anja H. Ebnöther and Philipp H. Fluri, eds., *After Intervention: Public Security Management in Post-Conflict Societies—From Intervention to Sustainable Local Ownership* (Vienna: GKS, 2005); Albrecht Schnabel and Hans-Georg Ehrhart, eds., *Security Sector Reform and Post-Conflict Peacebuilding* (Tokyo: United Nations University Press, 2006); Mark Sedra, “Security Sector Reform in Afghanistan: The Slide towards Expediency,” *International Peacekeeping*, Vol. 13, No. 1 (March 2006), pp. 94–110; Hans Born and Marina Caparini, eds., *Democratic Control of Intelligence Services: Containing Rogue Elephants* (Aldershot, U.K.: Ashgate, 2007); and Laurie Nathan, *No Ownership, No Commitment: A Guide to Local Ownership of Security Sector Reform* (Birmingham, U.K.: University of Birmingham, 2007).

18. Nathan, *No Ownership, No Commitment*.

1. Monica Duffy Toft, “Ending Civil Wars: A Case for Rebel Victory,” *International Security*, Vol. 34, No. 4 (Spring 2010), pp. 7–36. Additional references to this article appear in parentheses in the text.

To answer these questions, I provided a statistical analysis of all civil war terminations from 1940 to 2002 and demonstrated that civil wars ended through negotiated settlements were more likely to recur than those ended through military victory. I then presented a general theory to explain why victories might be more stable than negotiated settlements; and I applied the logic of the theory to the case of Uganda. I concluded the article with the following assessment: "A key strength of negotiated settlements is their credible promise to provide mutual benefit as a reward for continued compliance. The collective goods of (1) no more violence and (2) the opportunity to participate in an electoral process and a government that promises to represent and protect the interests of the citizens in some form are important benefits. Where negotiated settlements fall short, however, is in the credibility of their promise to inflict harm should one or both sides in a settlement fail to comply with its terms. . . . Thus, while negotiated settlements are good at providing benefits, they are less effective in following through on their threats and are therefore not self-sustaining" (p. 34). I posited that military victories might be more successful in keeping the peace when there is a robust security sector and that this factor is vital to the long-term stability of post-civil war states.

Nathan criticizes the statistical analysis that I presented in the article, arguing that differing interpretations of cases lead to different conclusions. He claims that this issue of case selection and coding is a general one and not exclusive to my article. This leads him to conclude that comparative case studies provide the only appropriate method for discerning the patterns and problems associated with war termination. This is the most contentious of his claims, but also the least justifiable.

In the remainder of this response, I first assess his critique of my statistical analysis and, in particular, my coding of variables and cases. I then consider his criticism that my article does not address why negotiated settlements might fail. Third, I counter his central claim that case studies are the only suitable method for understanding civil war termination and informing policy.

First, Nathan and I agree that case selection and coding are critical in quantitative analysis. Where we disagree, however, is that Nathan argues against the usefulness of statistical analysis altogether. In my article, I did not claim that statistics are definitive, but I did argue that they are nevertheless helpful in elucidating some important relationships. I discussed the coding of cases and highlight how different coding of variables can lead to different conclusions and, consequently, different policy implications. Prior studies that employed statistics to determine the implications of war termination were inconsistent in the cases selected for inclusion in their data sets (for example, colonial wars were sometimes included and sometimes not),² and then how those cases were coded in terms of war termination.

My article and related book, *Securing the Peace: The Durable Settlement of Civil Wars*, therefore went to great lengths to ensure a consistent set of criteria for case selection and coding of critical variables.³ Unless Nathan can demonstrate that these variables are somehow inadequate or inconsistently applied, his critique and choice of cases to

2. For a comparative analysis of my data set with those of Michael Doyle and Nicholas Sambanis, and James Fearon and David Laitin, see <http://isites.harvard.edu/fs/docs/icb.topic890694.files/MITcodebook2010.pdf>, pp. 10–11.

3. Monica Duffy Toft, *Securing the Peace: The Durable Settlement of Civil Wars* (Princeton, N.J.: Princeton University Press, 2010).

dismiss my statistical analysis amount to little more than the selective use of a handful of cases to make his point (e.g., his discussion of the South Africa case). Nathan also mentions that the statistical analysis did not include Liberia in 2002. The Liberia case, however, occurred outside the time parameters of the analysis (see, in particular, page 4, note 19, which states, “Only civil wars that ended by 2002 are included in the logit analysis”).⁴ Moreover, Nathan’s argument that the 2002 Liberia case represents a recurrence of the first war is difficult to sustain, given that both the combatants and the issues over which they fought were different from those in the first Liberian civil war (1989–97).

Further, Nathan questions why I coded some conflicts in the data set as single wars instead of dividing them into multiple observations. Here, too, he appears to use cases to support his argument but mentions only those that, if disputed, would increase the number of civil war recurrences following military victories. Yet there are vast numbers of attempted cease-fires and peace agreements that failed to resolve civil wars. Again, consider the case of Liberia’s first civil war. Liberia experienced no fewer than a dozen settlements that failed to secure peace in half as many years (1990–96).⁵ Given that my coding criteria require that peace be sustained for at least two years, I did not include these as separate observations. If I had included these failed settlements in my analysis, along with a host of other such settlements (Angola also suffered a series of failed agreements),⁶ I could have bolstered my argument considerably. To support Nathan’s argument that “disaggregating cases matters,” one would have to disaggregate all cases, not just those that help to make one’s point. Quantitative analysis requires scholars to exert considerable effort in defining and defending their case universe precisely because it is crucial that the definitions one uses be consistent. My codings might be disputable, but they are systematic. Nathan’s critique would be plausible only after he, too, spent the time and effort to define the appropriate case universe and applied his definition without favor toward any particular argument.

Second, Nathan asserts that scholars need to examine not only whether settlements fail but also why. I agree, which is why explaining settlement failure was a core focus of my article. In it, I presented a theory about the importance of security sectors across all types of civil war settlement types, including negotiated settlements. So, it is more than a little puzzling when Nathan writes that I need to explain why negotiated settlements fail but neglects to mention my theory. I would be curious to know whether he agrees that security-sector reform is critical in maintaining peace and stability but, at the same time, is one of the most overlooked aspects in the formulation of agreements and formation of governments after these civil wars have ended. As I wrote in the article, a number of agreements included provision for demobilization, disarmament, and reintegration, yet few included provision for creating a robust security sector in the aftermath of civil war (an exception is El Salvador, whose civil war has not recurred) (p. 32). Given his publication record, I believe that Nathan would agree.⁷ It is nevertheless striking that this insight and the support it receives from both the statistics and the case

4. The peace settlement was signed in 2003. For a copy of the agreement, see http://www.usip.org/files/file/resources/collections/peace_agreements/liberia_08182003.pdf.

5. Dorina Bekoe, *Implementing Peace Agreements: Lessons from Mozambique, Angola, and Liberia* (New York: Palgrave Macmillan, 2008).

6. *Ibid.*

7. Laurie Nathan, *No Ownership, No Commitment: A Guide to Local Ownership of Security Sector Reform* (Birmingham, U.K.: University of Birmingham, 2007); and Laurie Nathan, *The Changing of the*

study of Uganda are missing in his correspondence. What is critical for readers to understand is that I could not have made such a strong argument had I not included an assessment of all types of war endings, not just negotiated settlements.

Third, Nathan argues that scholarship on civil war termination requires case studies. The debate about the relative merits of different methodologies has been going on since the advent of the microprocessor and the behavioral revolution in the social sciences.⁸ Are scholars still at the point where we need to defend the merits of a given methodology every time we use it? Nathan seems to think so. Given my research, with its use of multiple methods, we obviously disagree. Regardless of this disagreement, what makes Nathan's critique untenable in this instance is that, as I note above, he fails to mention that my article contains a case study of Uganda. Moreover, my book *Securing the Peace* contains four in-depth case studies, three of which ended through negotiated settlements: Sudan (comprising two settlements) and El Salvador. I included the case study of Uganda because I agree with Nathan that qualitative evidence is critical in furthering scholars' understanding of civil war termination.

Clearly, there are just as many perils with findings based on case studies. As I stated in the article, "Ultimately, readers must decide whether the Ugandan example is reflective of the larger universe of relevant cases or merely an outlier" (p. 31 n. 56). But given Nathan's emphasis on the notion that every case is unique, how are scholars ever to know which case study is appropriate?

In conclusion, I was not trying to grind any methodological axes in writing my article or my book. Indeed, I employed statistical analysis and case studies in both publications to leverage the relative inferential strengths of these two methods, and to convince readers that a combination of quantitative and qualitative evidence supports my broader theoretical and empirical claims. As a theorist, I welcome criticism so long as my critics bring their own systematic evidence to bear. I remain less sympathetic to critiques that dismiss certain kinds of social science out of hand, or that challenge my findings based on evidence selected to make the rather obvious point that 100 percent of the variance has not been explained. The basic question in my research is whether, all else being equal, some civil war termination types lead to more stable outcomes, and if so, why? Doubtless there are gaps and other weaknesses with my argument, which is why I look forward to future research that addresses these weaknesses in an effort to advance understanding of an issue with significant policy implications.

—*Monica Duffy Toft*
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Guard: Armed Forces and Defence Policy in a Democratic South Africa (Pretoria, South Africa: Human Sciences Research Council 1994).

8. On the relative merits of different methodologies, see Gary King, Robert O. Keohane, and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton, N.J.: Princeton University Press, 1994).