

Contents

Published in the United States of America in 2010 by
Lynne Rienner Publishers, Inc.
1800 30th Street, Boulder, Colorado 80301
www.rienner.com

and in the United Kingdom by
Lynne Rienner Publishers, Inc.
3 Henrietta Street, Covent Garden, London WC2E 8LU

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Library of Congress Cataloging-in-Publication Data

Collective violence in Indonesia / edited by Ashutosh Varshney.
p. cm.

Includes bibliographical references and index.

ISBN 978-1-58826-687-3 (hardcover : alk. paper)

1. Violence—Indonesia. I. Varshney, Ashutosh, 1957–

HM886.C627 2010

303.609598—dc22

2009042176

British Cataloguing in Publication Data

A Cataloguing in Publication record for this book
is available from the British Library.

Printed and bound in the United States of America



The paper used in this publication meets the requirements
of the American National Standard for Permanence of
Paper for Printed Library Materials Z39.48-1992.

5 4 3 2 1

<i>List of Tables and Figures</i>	vii
<i>Preface</i>	ix
1 Analyzing Collective Violence in Indonesia: An Overview <i>Ashutosh Varshney</i>	1
2 Patterns of Collective Violence in Indonesia <i>Ashutosh Varshney, Mohammad Zulfan Tadjoeddin, and Rizal Panggabean</i>	19
3 Conflict in Post-Suharto Indonesia: What Local Newspapers Tell Us <i>Patrick Barron and Joanne Sharpe</i>	51
4 Ethnic Conflicts in Indonesia: National Models, Critical Junctures, and the Timing of Violence <i>Jacques Bertrand</i>	77
5 Explaining Ethnic Violence in Indonesia: Demilitarizing Domestic Security <i>Yuhki Tajima</i>	99
6 Local and National: Lynch Mobs in Indonesia <i>Bridget Welsh</i>	119
7 Final Reflections: Looking Back, Moving Forward <i>Patrick Barron, Sana Jaffrey, Blair Palmer, and Ashutosh Varshney</i>	145
<i>References</i>	161
<i>The Contributors</i>	179
<i>Index</i>	181
<i>About the Book</i>	195

Final Reflections: Looking Back, Moving Forward

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Blair Palmer, and Ashutosh Varshney*

In this final chapter, we look back on the findings presented in this book, reflect on some other publications on conflict—both in Indonesia and elsewhere—note what we have learned, and sketch out potential research directions. More than a decade after the fall of Suharto and the New Order, we have arrived at a meaningful retrospective moment, allowing us to identify domains that remain analytically untouched, at least partially if not fully. We combine four sets of considerations in these final remarks: comparative, Indonesia-specific, methodological, and theoretical.

A belief that conflict has de-escalated in Indonesia has crept into popular and policy circles. However, it is not clear whether the movement toward de-escalation is cyclical or permanent. Nor is it clear how common and deadly smaller forms of violence are or whether newer forms of violence will erupt in Indonesia. Comparative evidence indicates that violence often reappears in areas that previously had acute conflict. Theory also suggests that unless suitable institutions and policies are imaginatively devised and put in place, a multiethnic or multi-religious society, especially at low or middle levels of income, is vulnerable to the possibility of long-term violent conflict. As Indonesia seeks to consolidate the democratic gains of the post-Suharto decade, understanding violent conflict is of utmost importance.

By now, of course, the literature on conflict in Indonesia is quite substantial, and many elements of the story are reasonably clear.¹ The fall of Suharto was accompanied by the outbreak of intense group violence in several parts of the country. As a result—and in dramatic contrast to studies of Indonesia during the late New Order when the literature emphasized order, stability, and economic dynamism—conflict

became an important concern in scholarly and policy circles. The literature that emerged has especially advanced our understanding of some large-scale conflicts, such as in Aceh, Kalimantan, Sulawesi, and the Maluku.²

Yet, as is often expected in the first flush of research, there are limitations to much of the existing literature on Indonesian conflict. Four are particularly worthy of note. First, as the introductory chapter noted at length, the vast majority of the literature on Indonesia has remained by and large unincorporated into the larger theoretical and methodological literature on conflict. Very little is known outside of Indonesia about the nation's conflict dynamics beyond a small circle of country specialists. The conflict dynamics in Indonesia are likely to have relevance for multiethnic and/or multireligious societies that used to have authoritarian political orders and have of late gone through a democratic transition accompanied by considerable group violence. Nigeria, post-Communist eastern and central Europe, and Central America easily come to mind, but the list can be expanded. A creative engagement with theory and comparative experience nearly always illuminates uncharted dimensions of a problem, inaugurating newer ways of thinking.

Second, the emphasis in the literature has been almost entirely on the colossal episodes of collective violence in Indonesia, especially in the Maluku, in Central Sulawesi, the May 1998 riots in Jakarta, and the war in Aceh. The most comprehensive dataset on Indonesian violence to date has focused on these incidents (see Chapter 1 of this book). This focus is understandable in light of how horrific these violent episodes were, but it has several serious drawbacks. The literature has more or less ignored routine acts of violence, such as fights over land or vigilante justice, which appear to be common in some parts of Indonesia. In earlier chapters of this book, Patrick Barron and Joanne Sharpe (Chapter 3) and Bridget Welsh (Chapter 6) suggest that, though each act of small violence kills or injures only a few, the overall magnitude is likely to be quite high.³ Additionally, if these forms of violence are a precursor to larger outbreaks of unrest, an important part of the picture is missing.

Third, the methods with which cases of large-scale violence have been studied have led to incomplete explanations. Books by Jacques Bertrand (2004), John Sidel (2006), and Gerry van Klinken (2007a) look for commonalities among multiple case studies to determine causal factors. These scholars may well be right about the causes of violence, but without a comparison with peaceful cases, they cannot, in principle, be sure that the causes of violence they have identified are in-

deed the right ones.⁴ As argued in Chapter 1, for a causal theory to be empirically correct, it is not only important to identify what is common across the many episodes of violence, but it is also critical to demonstrate that the factors associated with violence are absent in peaceful cases.⁵ We need variation in the research design.⁶

Finally, there is almost no systematic information available at all on the post-2003 years of conflict—its forms, causes, and trajectories. Ashutosh Varshney et al. (in this book) put together a database for the United Nations Support Facility for Indonesian Recovery (UNSFIR) that records incidents of large-scale violence for the period 1990–2003; Barron and Sharpe (2005, and in this book) have created a dataset on small-scale violence in Flores and parts of East Java for 2001–2003. Yet relatively little is known about conflict since 2003.⁷ With the massive decentralization initiative, a whole host of new institutions have come into existence, altering the sites, group incentives, and dynamics of conflict. Post-2003 data are critical for understanding conflict and its impacts in Indonesia.

We begin with a discussion of the relative merits of quantitative or qualitative work for understanding the causes, patterns, and dynamics of Indonesia conflict. Though some have critiqued the use of quantitative datasets (Sidel 2006), we offer our views and make a case for a mixed methods approach. We then concentrate on three substantive areas that, in our view, call for further attention of researchers.

Methodological Issues

Methodological arguments in the social sciences are increasingly headed toward the view that both quantitative and qualitative approaches have distinct utilities and limitations, and an exclusive use of either approach can unduly confine the scope of analysis. Ideally, for a whole range of questions the two should be combined (Gerring 2007).

Quantitative work typically relies on large-*n* datasets (Barron and Sharpe, in this book; Varshney et al., in this book). Such datasets typically allow two kinds of analyses: (1) identification of broad patterns and trends, and (2) establishment of correlations between “independent” and “dependent” variables. On the whole, if not always, large-*n* datasets are unable to establish causality,⁸ whereas qualitative research, by systematically looking at which events led to violence (“process tracing”), permits us to separate causes and effects.

Of course, there are conditions under which large-*n* datasets can

move beyond correlations. They can allow us to assign causality if good “instrumental variables” can be identified. However, even under such conditions, we need qualitative case studies. Instrumental variables can give us a good sense of *causal effects* (what is the effect of X on Y?), but not of *causal mechanisms* (how did X cause, or lead to, Y?).⁹

Following this reasoning, one can generate a large-n dataset, building on and supplementing existing datasets, which will allow for identification of trends in conflict types and forms and their impacts. Based on the empirical results of the quantitative work, targeted qualitative studies can then be undertaken to determine the causes of conflicts and of differing patterns of conflict escalation and de-escalation. For example, the large-n dataset may show that large episodes of violence are concentrated in cities, not villages. If so, the case studies could be aimed at exploring why this is so.

It should be noted that there is another way to proceed. Sometimes, it is said that case studies are good at theory building, not theory testing, for which large-n datasets may be required (Gerring 2007). Conceptualized in this vein, case studies can be used for theory generation, and the theory so generated can be tested with the aid of a large-n dataset. Alternatively, with a large-n dataset, one can test whether some existing theories in the larger literature—for example, the ethnolinguistic fractionalization (ELF) argument—are applicable to Indonesia.

Either way, it is best to combine the qualitative and the quantitative data. Each complements the other, and using them together in these ways will make for a sounder understanding.

Quantitative Data: The Role of Newspapers

How, then, should we generate a large-n dataset? Considerable research shows that newspapers are often the best source of quantitative data on ethnic conflict (Barron and Sharpe, in this book; Varshney 2002; Varshney et al., in this book; Wilkinson 2004). Household surveys are weak at measuring conflict incidence and impacts, as they tend to record perceptions of conflict and have a tendency to underreport because violent conflict, especially large-scale, is a generally rare event that does not affect a whole community. Key informant interviews, as used by the PODES survey in Indonesia, create perverse incentives to under- or overreport conflict depending on the expectations about how the survey results will influence policy decisions and resource allocations (Barron

2009a). Furthermore, survey methods rely on the memory of respondents and are therefore less reliable for recording the details of older incidents of violence, making it difficult to create time series data. Some preliminary work in parts of Indonesia that compares police, hospital, and NGO sources with newspaper data, reveals that police and hospital data systematically underreport fatalities, because they fail to notice cases that are not reported to the police or victims who are not admitted to hospitals.¹⁰ Moreover, these records do not contain the level of detail that would allow for a distinction to be made between incidents of group conflict and violent crime, such as assault and arson.¹¹

In contrast, the newspaper monitoring methodology has been shown to be effective in both high- and low-conflict regions. The UNSFIR data, outlined in Varshney et al. (in this book), showed that building a national dataset that records conflicts reported in newspapers was both possible and worthwhile. That dataset provided useful estimates of large incidents of violence, but did not adequately cover forms of small violence given the news sources it used. Other newspaper data collected in East Java and NTT (Barron and Sharpe, in this book) and Aceh have permitted for finer-grained analyses, but to date have had limited temporal and geographic coverage.

Existing datasets have enhanced our understanding of violence in Indonesia. For example, we know that large-scale violence is highly spatially concentrated in Indonesia (Varshney et al., in this book), that small-scale violence has a wider spread (Barron and Sharpe, in this book), and that the total casualties in small-scale violence may well be surprisingly large (Welsh, in this book). We did not know these trends for sure until the large-n studies gave us empirical evidence. Drawing on the insights provided in many of the chapters in this book, it appears that it is possible to put together a more comprehensive database tracking large and small violence across Indonesia.

Newspaper Reports: How to Interpret Reliability

While newspapers appear to be the best source of data for mapping conflict patterns and trends in Indonesia, they are not without weaknesses. Three potential issues may limit the utility of using newspapers to understand conflict. However, we believe these can largely be overcome; where reliability issues remain, understanding these can help ensure that interpretations of the data are reasonable.

First, there is widespread consensus that the press in post-Suharto In-

Indonesia is relatively free. However, previous research shows the existence of self-censorship in editorial policy continuing the SARA legacy of the New Order, or to prevent conflict escalation.¹² Also present is “envelope” journalism, where newspapers sponsored by certain local groups or individuals become advocates of those parties (Barron and Sharpe 2005). Such biases do exist, but are not widespread enough to invalidate the use of newspapers as a source of data on conflict. Nevertheless, it is of utmost importance that the process of creating the dataset involves an assessment of institutional biases of different newspapers. Extensive interviews with newspaper staff, eliciting responses about not only their own reporting standards but also the reputations of other papers in an area, can help us choose which papers are most reliable and to evaluate the strengths and weaknesses of our data and how they can be analyzed. For example, if a newspaper acts as a mouthpiece for a politician, it cannot be used for an accurate count of conflicts related to elections. However, it may still be useful for reports on lynching and other forms of violence.

Second, accurate reporting, especially as it pertains to assessing deaths, injuries, and property damage, is crucial for the validity of data. Some newspapers may lack systematic procedures to ensure accurate collection and checking of facts prior to publication, and this may be particularly true for subprovincial news sources. Gathering information about a newspaper’s sources of information and its policies on fact checking can enable us to select newspapers with high standards of reporting and to establish how accurate our data are likely to be. Using more than one newspaper in each location will also improve the accuracy of the data.

Third, even if we use provincial- and district-level newspapers, it is likely that the reporting coverage will be uneven across districts and subdistricts. If this is the case, it may invalidate intertemporal and inter-area comparisons, because different levels of violence may be an artifact of differential conflict reporting rather than incidence. However, by compiling information about areas where each newspaper has permanent offices, permanent reporting staff, and freelance reporters, we can (1) select newspapers with the best regional coverage for data collection, (2) supplant a weaker-coverage paper with others in that region, and (3) identify the stronger and weaker sections of our data.

It is important that the construction of a newspaper database be preceded by a *comprehensive media assessment* that includes an analysis of archive coverage, newspaper reach, potential biases, and quality of reporting. In short, newspaper data have to be interpretively collected and used.

Routine Violence

Routine violence is one of the most neglected aspects of the current scholarship.¹³ Such incidents involve local actors struggling over local issues rather than large-scale mobilization by identity characteristics (such as ethnicity, religion, or region).

There are several important justifications for the study of routine violence. First, although fatalities tend to be limited in each incident, the total number of those killed through routine violence can be large if such episodes are common or frequent (Barron and Madden 2004; Barron et al. 2009; Barron and Sharpe, in this book; Welsh, in this book; Tadjoedin and Murshed 2007). Second, frequent small-scale violence can have serious systemic consequences. For example, if some regions of a country develop a tradition of lynching—a mob killing a suspected culprit instead of handing him or her over to the police or administration—it can impede the growth of the rule of law. Moreover, if the frequency of such acts is high, it can deaden popular sensibilities, arguably creating greater acceptance of large-scale violence as well. Finally, sometimes small incidents initiate a process that leads to huge conflagrations. Often, if not always, the starting point of a big episode of violence is a small clash between two groups or individuals. If we develop a better understanding of why small acts of violence occur and which people or groups are in conflict in these forms of violence, we can perhaps generate a theory that can identify institutions and strategies relevant to minimizing the occurrence or limiting the effects of such violence.

Escalation of Violence

Extensive literature has emerged on large-scale violence in post-Suharto Indonesia. However, we still do not have a good theory for why the small sparks of localized violence and tensions erupt into the large fires of intergroup collective violence. Developing such a theory is important for understanding not only the deadly outbreaks of communal violence in the past, but also (1) the potential for small-scale conflict and routine violence to escalate in various parts of the archipelago, including those areas that have *not* been hit by large-scale communal violence; and (2) the scope for intervention by the government and/or civil society. If, with the aid of theory, we can understand how to prevent sparks from becoming fires, perhaps we can also hope for fewer and less deadly violent conflicts in the future.

Intercommunal ethnic or religious violence in West and Central Kalimantan, the Malukus, and Central Sulawesi, separatist conflicts in Aceh and Papua, and the Jakarta riots of May 1998 have received the greatest attention of Indonesia experts. Initially, most analyses focused on individual cases with few attempts at cross-case analysis. More recently, three scholars (Bertrand 2004; Sidel 2006; van Klinken 2007a) have written books on the broader issue of violence in Indonesia, examining multiple conflicts. Cross-case analysis has been used to develop frameworks to understand why different forms of conflict arose in different places at different times, concentrating largely on similarities in structural conditions that predated the outbreak of widespread violence.

This new work has undoubtedly enhanced our understanding of the specific conflicts and has pointed to general systemic factors, all largely a product of Indonesia's post-Suharto transition. Yet the books have also left some key gaps in the research.

The greatest issue is methodological. None of these works is based on the idea of variation in research design. All have concentrated only on episodes of violence, mostly large-scale episodes, and none systematically compares why violence occurred in some places and not others. Bertrand (2004) studies violence in East Timor, the Malukus, and Kalimantan; Sidel (2006) focuses on the burning of churches in Java in the early to mid-1990s, the violence in Jakarta, and intercommunal conflict in the Malukus and Sulawesi; and van Klinken (2007a) concentrates on riots in Kalimantan, the Malukus, and Sulawesi. A good theory requires showing that the factors identified as causal in making violence possible were missing in places that did not experience violence.¹⁴ As discussed earlier, if we do not study peace and violence together, we cannot conclusively show which factors were really causal in producing either.¹⁵

Three more analytical issues, especially related to the analysis in Bertrand (2004) and Sidel (2006), are worth noting.¹⁶ First, the comparative work has not fully considered the processes of *escalation*, which turned existent social tensions into conflicts ranging from small-scale acts of hostility to large-scale episodes. Second, the explanations have largely been structural and often rather deterministic, focusing on demographic shifts, economic balance, and changing access to political power, and they have underplayed the importance of the *processes* of mobilization. Third, there has been an overriding emphasis on macroexplanations for the outbreak of violence in certain localities. Bertrand (2004), for example, concentrates on differential group

access to power in Jakarta, and the role of different groups in the Indonesian nation and polity, to explain why the Dayaks, Christians, and Muslims rose up at certain points. As Bertrand (in this book) readily admits, this sort of approach can explain why violence gets clustered around certain periods (temporal variation), but it cannot help us understand why violence occurs in certain places (spatial variation). For understanding the latter, there is a need to pay attention to *micro* or *local factors* in explaining violence (Aspinall 2008; Varshney 2002, and in this book).

De-escalation of Violence

In Indonesia and beyond, there has been little consideration of processes of de-escalation after episodes of large-scale violence have taken place, or of the conditions under which remaining tensions can re-escalate into new outbreaks of severe violence. Development of a theory that explains variations in the success of peace stabilization in areas that experienced massive unrest, that identifies why violent conflict reemerges in some areas and not in others, and that accounts for why "postconflict" violence takes different forms in different areas would be of enormous utility, for both the policy and academic communities.

Paul Collier et al. (2003) have demonstrated that there is a significant chance of violent conflict reemerging within five years in areas where civil wars have formally ended. Even where countries do not return to civil war, new forms of postconflict violence can emerge (see for example, Rodgers 2007; Chaudhary and Suhrke 2008; Fortna 2008). In some cases, the human security impacts of such violence can be as great as those experienced during the initial period of war (Muggah 2009). In El Salvador, Guatemala, and Nicaragua, for example, homicide rates are now higher than they were during the conflict period (Waiselfisz 2008).

Indonesia provides an excellent site for studying postconflict violence and conflict de-escalation. We know that while many of the large-scale intercommunal and separatist conflicts have subsided in recent years, small-scale violence has occurred in their wake. In Aceh, for example, the Helsinki peace agreement officially brought to an end a three-decade conflict between the Indonesian government and GAM, a rebel group. Yet while the peace process has by and large gone well, there have been rising levels of localized routine violence since the signing of the peace agreement (Figure 7.1).

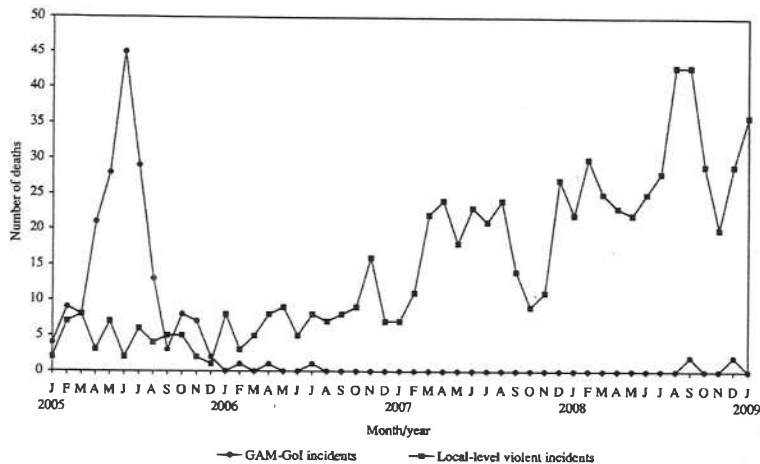
Beyond this, the sheer range of large-scale conflicts that hit Indonesia about the time of the post-Suharto transition provides ample scope for comparative analysis. Whereas many studies focusing on peace consolidation have compared countries, in Indonesia there appears to be tremendous variation in conflict patterns over time across different provinces and districts. Comparing areas that suffered similar levels of conflict but are now having different experiences of “peace” can help us generate a theory on how and why peace consolidates.

A number of comparisons would be useful. First, it is interesting to look at how patterns of routine violence vary across different postconflict areas of the country. The four hypothetical scenarios laid out in Figures 7.2a–d illustrate the point. In each, the dotted area represents the trajectory of the original, large-scale violence, with routine, small-scale violence represented by the line with square markers. All four figures show steadily declining large-scale violence but four different trajectories in the evolution of routine violence. In Figure 7.2a, routine violence continues, unaffected by the decline in large-scale violence. In Figure 7.2b, both large-scale and routine violence decline roughly simultaneously. In Figure 7.2c, the two rates of decline are different, with

routine violence taking longer to decline. In Figure 7.2d, large-scale violence declines over time, but routine violence increases instead.

Second, variation in the forms of postconflict violence is also analytically important. Figures 7.3a and 7.3b break down further the different patterns of postconflict violence observable in two hypothetical areas. In each, levels of violence remain similar, but violence has taken

Figure 7.1 Violent Conflicts in Aceh, January 2005–January 2009



Sources: Barron (2009a and 2009b).

Notes: The Helsinki MoU was signed in August 2005. Data are generated from local newspaper monitoring.

Figure 7.2 Patterns of Routine Postconflict Violence

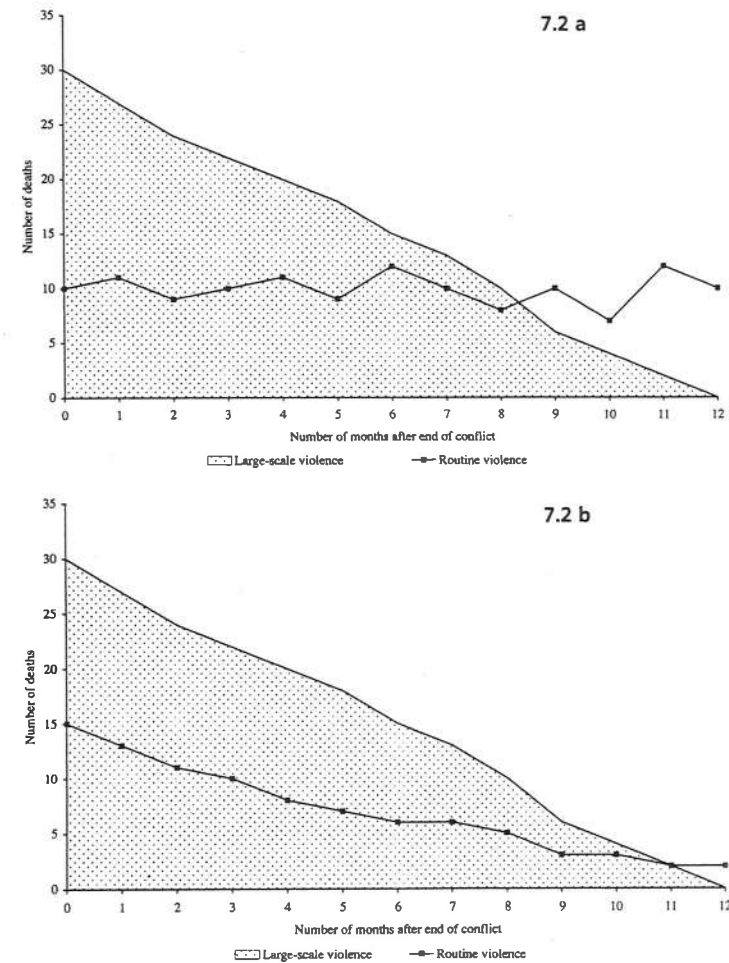
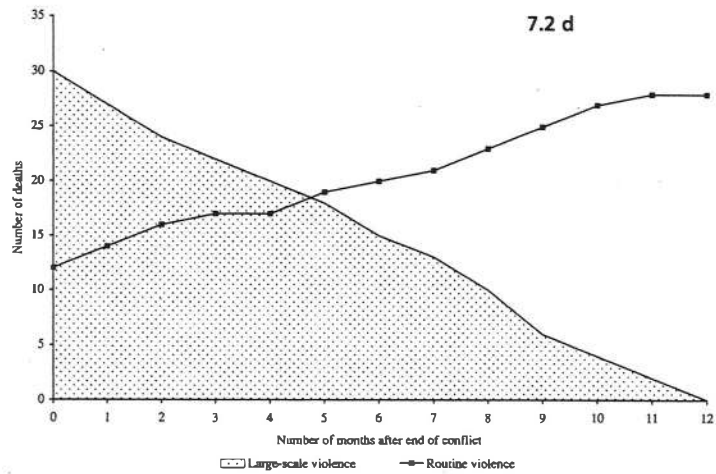
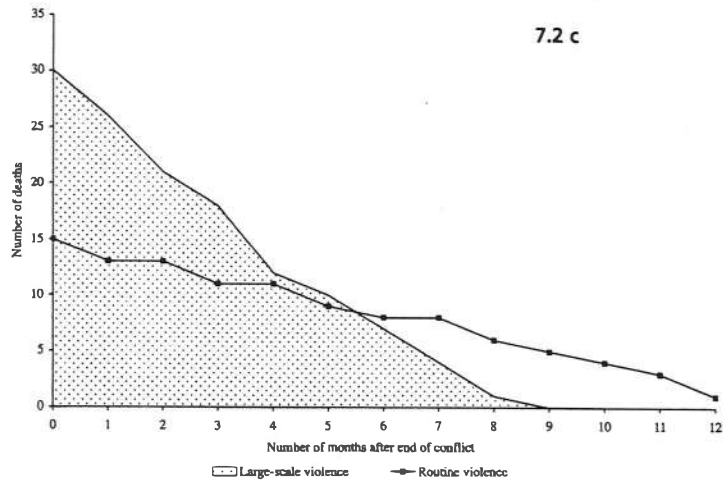


Figure 7.2 continued



different forms. Figure 7.3a shows a rise in vigilantism, while Figure 7.3b shows a rise in land conflicts.

Third, much can also be learned by looking at variations within cases in levels and forms of violence. A quantitative dataset can allow the identification of sharp rises and falls in violence over the postconflict period. Qualitative cases, on the other hand, can focus on understanding

Figure 7.3a Postconflict Vigilantism

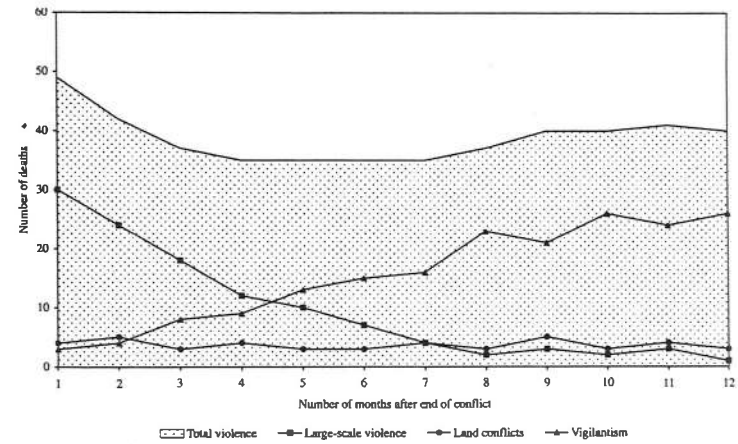
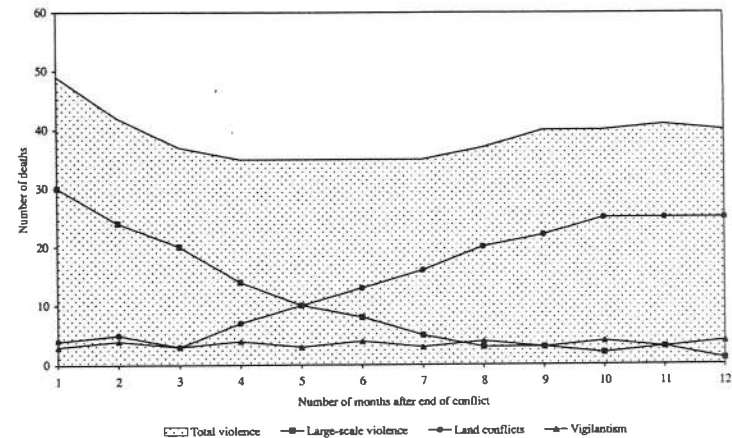
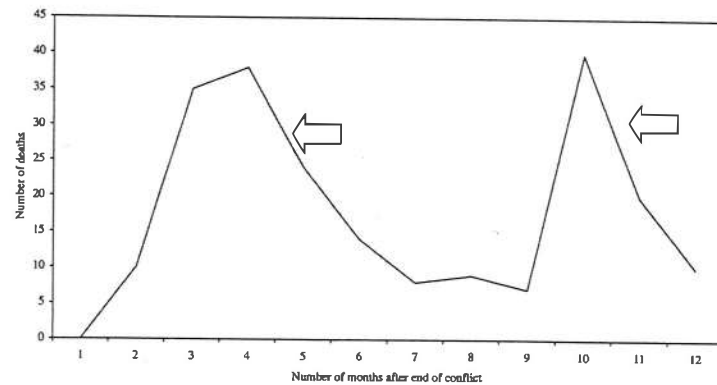


Figure 7.3b Rise in Postconflict Land Conflicts



the reasons why such rapid changes occurred. Figure 7.4 shows the pattern of violence over time in a hypothetical district. Are the factors that led to the initial violence missing or do they remain in the second round? If these underlying factors remain even after the second de-escalation, can we posit that violence may reemerge again in the future?

Figure 7.4 Within-Case Analysis of De-escalation

Conclusion

We have suggested that scholars have understudied three kinds of issues: (1) What is the magnitude and spread of small-scale, routine violence in Indonesia, and why does it appear to be so large in its cumulative impact? (2) Why do some small episodes of violence escalate while others remain relatively contained or disappear altogether? (3) Do the patterns of de-escalation after periods of large-scale conflict markedly differ across the various theaters of violence, and why might that be so? We have also consistently made a methodological point that a mix of quantitative and qualitative methods will help us answer these questions best. Neither alone will do.

Notes

1. Although, for the most part, the policy implications of the existing research are unclear.

2. See, for example, van Klinken (2001) and Wilson (2008) on Maluku and North Maluku; Acciaioli (2000), Aragon (2001), and McRae (2008) on Central Sulawesi; McGibbon (2004) on Papua; Davidson (2008), van Klinken (2000), and Smith (2005) on West or Central Kalimantan; Aspinall (2006, 2009), Schulze (2004), and Sukma (2004) on Aceh; and Mietzner (2008), Purdey (2006), and Siegel (1998b) on the Jakarta-riots. The reports of the International Crisis Group (ICG), available at www.crisisweb.org, have also illuminated many of the conflicts. A number of edited volumes have brought together pieces of conflicts, often drawing parallels with historical patterns of

violence in Indonesia: see Anderson (2001), Anwar, Bouvier, Smith, and Tol (2005), Colombijn and Lindblad (2002), Coppell (2006), Hüsken and de Jonge (2002), Tornquist (2000), and Wessel and Wimhöfer (2001).

3. Also see Vel (2001) and Herriman (2007). The edited volume by Colombijn and Lindblad (2002) shows how “everyday” forms of violence are not new to Indonesia, being prominent in the precolonial and immediate post-colonial eras. The World Bank’s Conflict and Development program has also analyzed “local conflict” in Lampung (Barron and Madden 2004; Tajima 2004), and Flores and East Java (Clark 2005; Barron and Sharpe 2005 and in this book).

4. One other type of research—large-n in inspiration—ought to be noted. Barron et al. (2009) and Mancini (2005) use survey data to determine factors associated with conflict propensity. This sort of work does cover variation in the dependent variable, but as is true in general of large-n work, it is unable to identify the mechanisms through which the independent and dependent variables might be connected.

5. We say more on this later. See also Varshney (2007) and Aspinall (2008).

6. On the whole, a research design based on comparing similar episodes is useful in theory building, not in theory testing. Under one condition, however, theory testing is also possible through this method. If a theory is deterministic, not probabilistic, then even one case—let alone a few—where violence takes place in the absence of factors identified with violence is enough to invalidate the theory. Karl Popper’s famous example is relevant here: any number of white swans that we observe will not prove that all swans are white, but one black swan can prove that not all swans are white (Popper 2002). The Popperian observation, it should be noted, does not apply to probabilistic theories, which theories of violence, along with a lot of other social science arguments, tend to be. In a probabilistic scheme of things, one black swan could simply be an outlier.

7. Aceh is an exception. Here, the World Bank has been monitoring conflict incidents reported in local media since the tsunami (e.g., World Bank 2008). Some case evidence (e.g., van Klinken 2007b) and reports by the International Crisis Group also provide useful information. But these have not compared current conflict incidence and patterns with those in earlier periods, making it difficult to know how serious violence is today compared with that of the immediate post-Suharto period. Another data source is the Statistik Potensi Desa (PODES) survey conducted by the government’s Bureau of Statistics. The 2005 survey contained a question on the incidence and impacts of conflict, for all Indonesian villages. While the 2002 PODES data have been used (Barron et al. 2009), no one has published analysis of the 2005 data. Though the scale of the PODES is impressive (it is implemented in every village in Indonesia), the fact that it collects data at a single point in time prevents analysis of how conflict evolves over time, and there may be reliability issues, given incentives for respondents (primarily village heads) to over- or underreport conflict.

8. This is particularly true for research on violence, where the direction of causality can be impossible to determine (see Barron et al. 2009).

9. For details, see Gerring (2007, 43–48). See also George and Bennett (2005).

10. A pilot conducted in Maluku and North Maluku compared deaths reported by various sources between January and June 2005. It found that newspapers reported twenty-four deaths, police recorded only twelve, UN Incident Tracking found seventeen, and the Maluku Interfaith Association recorded only four. Hospital records recorded only one death in Ambon, compared with the seven reported in newspapers (for Ambon). The level of underreporting was most pronounced outside of the provincial capital (Sharpe 2005).

11. For these issues in India, see Varshney (2002, ch. 4).

12. On ethnocommunal issues, the New Order government had a so-called SARA policy. SARA was an acronym for ethnic (*suku*), religious (*agama*), racial (*ras*), and intergroup (*antar-golongan*) differences. These differences were not to be discussed in the public realm.

13. For example, routine violence is not a focus of attention in the three most recent book-length works on group conflict in Indonesia (Bertrand 2004; Sidel 2006; van Klinken 2007a). Routine violence refers to forms of violence (such as the beating of suspected thieves, intervillage brawls, or fights over a plot of land) that are not part of a large or widespread conflict, and where the impacts of single incidents are typically low.

14. Van Klinken (2007a) does develop a "vulnerability index" to compare the provinces of high violence with other provinces where large-scale violence did not break out. He identifies factors of rapid deagrarianization and high dependence of the local economy on the state as important in differentiating high-violence and low-violence provinces. However, the focus of the book is not on establishing how these factors led to violence through a comparison of dynamics in high-violence and low-violence provinces, but rather on tracing the evolution of conflict in the high-violence provinces.

15. For a more detailed discussion, see Varshney (2007).

16. Van Klinken (2007a) does concentrate on escalation, processes, and local dynamics—the three points we make below. However, his primary focus was not to isolate causal factors in order to develop policies for conflict mitigation. Van Klinken discussed one aspect of escalation for each of five big conflicts: West Kalimantan, Central Kalimantan, Maluku, North Maluku, and Central Sulawesi. As a result, we learn how Indonesian violence supports Tilly and his colleagues' conceptual categorization of the dynamics of contentious politics (McAdam et al. 2001), and how elements of that theory can shed light on understanding violence in different provinces. But the lack of a comparative framework (even within high-violence locations) makes it hard to generate a broader understanding of why escalation occurs in some places and not in others.

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