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Analyzing Collective Violence in Indonesia: An Overview

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In 2001, using violent junctures in the life of a seventy-year-old Indonesian as a metaphor for the whole nation, Benedict Anderson summarized the history of violence in Indonesia in a poignant manner:

> A seventy year old Indonesian woman or man today will have observed and/or directly experienced the following: as a primary school age child, the police-state authoritarianism of . . . Dutch colonial rule . . . ; as a young teenager, the wartime Japanese military regime, which regularly practiced torture in private and executions in public . . . ; on the eve of adulthood, four years (1945–49) of popular struggle for national liberation . . . at the cost of hundreds of thousands of lives; as a young mother or father . . . the cataclysm of 1965–66, when at least 600,000 and perhaps as many as two million people . . . were slaughtered by the military; in the middle age, the New Order police-state, and its bloody attempt to annex East Timor, which cost over 200,000 East Timorese lives . . . ; in old age, the spread of armed resistance in . . . Aceh and West Papua, the savage riots of May 1998 . . . and . . . the outbreak of ruthless internecine confessional warfare in the long peaceful Moluccas. (Anderson 2001, 9–10)

The articles in this issue, written by a new generation of scholars working on Indonesian violence, do not disagree with Anderson's portrayal. But they do choose a different *methodological* path to sketch and analyze violence in Indonesia. Instead of focusing on the aggregate picture, these studies on the whole disaggregate violence and focus on *variations*,[1] on the assumption that, following Gary King, Robert Keohane, and Sidney Verba (1994), studying variation in collective violence is one of the best ways, if not the only way, to explore causation.[2] The variations covered here are of various types: across space, across time, and in the forms of collective violence. While we cannot claim that we have fully sorted out the causes of Indonesian collective violence—a great deal more work is clearly required—we do take necessary steps in that direction.

A second distinctive aspect of these writings is their comparative intellectual anchorage. Scholars of Indonesian violence have, on the whole, communicated with one another,[3] but innovative research often emerges in a dialogue with other subfields, in an engagement with newer methods, in a critical encounter with recent theories.[4] In studies of Indonesian violence, an intellectual engagement with debates emerging in other parts of the world, or with the evolving methods and theories in the field of ethnic conflict, is generally missing.[5] As a result, despite its potential relevance, Indonesia has not been adequately incorporated into the larger body of theoretical and comparative literature.[6] Indonesian materials on ethnicity and conflict are simply too meaningful to be left outside the emerging domain of comparative theory.

Finally, these articles also raise some new questions about the sources of information scholars of Indonesian violence tend to use. Comparative scholars on ethnic conflict have come to believe that newspapers, not government records, are the best, though not a perfect, source of information on collective violence. Is this true for authoritarian settings as well? If so, what kind of newspapers—national, provincial, or those at the subprovincial or district level—should scholars rely on?

Indonesia's "national newspapers" have focused mostly on the largest and most visible episodes of violence, the biggest riots, and selective acts of terrorism, leaving a lot that is analytically important out of their reporting gaze. We find that provincial newspapers are on the whole better than the national newspapers for tracking such large-scale violence. But the most important insight of these works is that subprovincial or district-level newspapers, rarely a favorite of "high scholarship," are a better source for analyzing *small-scale* collective violence such as lynching, which can also end up taking a lot of lives. Once we look at this level and include small-scale violence in our overall figures of casualties, the amount of collective violence recorded in Indonesia goes up significantly. Research elsewhere might benefit from this insight emerging from the studies of Indonesian violence. For what might be called small-scale, or routine, group violence, local newspapers, despite not having star reporters and distinguished editors, may be the best sources of information.

In the remainder of this introductory overview, I elaborate on these themes and touch on some allied matters as well. My attempt is primarily to bring scholarship on Indonesia in conversation with the arguments emerging in other parts of the world, and to initiate a systematic interaction between chronicles of Indonesian violence on the one hand and the theories and methods in the subfield of comparative ethnic conflict on the other. Indonesian violence needs theory, and theory in turn needs Indonesian case materials.

How Violent?

Before turning to the causes of Indonesian violence, it is worthwhile to place the country in comparative perspective. How violent is Indonesia? Is it more violent than other societies?

There is a consensus in the field that at higher levels of income, the incidence of collective violence, especially riots and civil wars, declines (Fearon and Laitin 2003; Horowitz 2001). This generalization is, of course, a statement about averages. It should not be construed to mean that high-income societies have no riots. US riots in the 1960s and 1990s and riots in the UK in the 1980s and in France in 2005 show that high-income societies are not entirely free from collective violence. But such societies have lower levels, and lower frequencies, of rioting. While the links between income and collective violence are not fully understood, a negative correlation is robust.[7] It follows that Indonesian collective violence should be compared with patterns and magnitudes of such violence in low-income, or at best middle-income, countries.

How good are the statistics on collective violence in such societies? While we do have some usable, if imperfect, datasets on civil wars, we do not yet have reliable cross-country datasets on riots, pogroms, and lynchings.[8] Since the primary focus of the articles in this issue is on the latter, not on civil wars, we cannot be absolutely confident that for forms of collective violence that stop short of civil wars, Indonesia is more violent than other societies at the low- or middle-income level.

Still, a few sensible comparisons with some, if not all, developing countries can be made. Also relevant are the materials from Western Europe and North America in the late nineteenth century, a period often used for suggestive comparisons with developing countries in the second half of the twentieth century.

Lynching

Consider, first, lynching as a form of collective violence. A comparison of Indonesia and the American South during 1882–1930 is revealing.

Sometime after the end of the American Civil War and the abolition of slavery, lynching of African Americans became common in the American South.[9] Available studies suggest an annual average of a little over 100 lynchings during 1882–1930, or roughly one lynching every third day during those forty-nine years.[10]

These late nineteenth- and early twentieth-century US statistics pale in comparison, when juxtaposed with the evidence presented by Bridget Welsh on lynchings in Indonesia in this issue. Welsh studied only four of the thirty-six Indonesian provinces: West Java, Bali, Bengkulu, and South Kalimantan. "These provinces were selected because they were seen as less violent than others." Still, and much to her astonishment, she found that in a mere ten years (1995–2004), over 5,500 people were victims of lynching in the four provinces, giving us an annual average of more than 500 victims.

Can we estimate the total annual numbers of lynching victims for all thirty-six provinces of Indonesia in this ten-year period (and even more demandingly, for all provinces for a longer period)? Until further empirical research is done, there is, of course, no good way of doing this.[11] Still, some revealing conclusions can be drawn. Even though, compared with Indonesia today, the United States had a smaller population during 1880–1930, the per capita lynching in Indonesia is almost certainly higher.[12] Not only has Welsh studied only four out of thirty-six Indonesian provinces but, as noted earlier, these provinces were chosen because they were known to be low-conflict provinces. Unless we wish to make the unlikely argument that lynching as a form of collective violence was absent in the remaining provinces, the numbers killed in lynchings in the country as a whole for the period 1995–2004, or for a longer period, are bound to be significantly larger than Welsh's estimates.

In short, to punish theft, robbery, hit-and-run accidents, rape, adultery, and witchcraft, a very large number of Indonesians appear not to rely on the police or the law at all. Taking law into their own hands, they opt for the "popular forms of justice." Indeed, Welsh also discovered that often the local police were informed ahead of time about planned lynchings, and they willingly acquiesced. Moreover, in a number of cases, if not all, leaders of the local communities were not only aware that preparations for lynchings were being made, but they also authorized the acts of lynching or participated in the rituals.[13] Unless later empirical research surprisingly disconfirms the reasoning here, it is safe to argue that lynching as a form of punishment was not something that occurred primarily in Indonesia's past (Colombijn 2002); it is also widely practiced in contemporary Indonesia, in contrast to the United States, where it has become quite rare.[14]

Religious Violence

Let us now turn to religious violence. Indonesian data can be fruitfully compared with the statistics available on Hindu-Muslim riots in India. Both countries have remarkable ethnocommunal diversity. Moreover, for most of their independence, they have also been low-income countries. Since the incidence of collective violence, as already argued, is (negatively) correlated with income, an India-Indonesia comparison is much tighter than a comparison of Indonesia with the United States in the late nineteenth and early twentieth centuries.

For India, Steven Wilkinson and I have constructed a dataset for the period 1950–1995 and provided evidence for over 7,000 deaths in Hindu-Muslim riots (Varshney 2002).[15] For Indonesia, Mohammad Zulfan Tadjoeddin, Rizal Panggabean, and I have documented more than 5,400 deaths in Muslim-Christian violence over a fourteen-year period, lasting from 1990 through 2003 (Varshney, Tadjoeddin, and Panggabean, this issue).[16] Can something comparatively significant be said about religious violence in India and Indonesia?

Let us first note that even if a period of forty to fifty years is covered for Indonesia, the number of deaths in Muslim-Christian violence—roughly 5,400 during 1990–2003—is unlikely to be larger than what we have for Hindu-Muslim violence in India (over 7,000 deaths during 1950–1995). Muslim-Christian violence is of recent origin in Indonesia, whereas Hindu-Muslim violence has a history of more than a hundred years. Indeed, given this difference in longevity and the political significance that such longevity conveys, it makes sense to suggest that in several if not all ways, Indonesia's *Pribumi* (sons of the soil)–Chinese cleavage, not its Muslim-Christian cleavage, is conceptually similar to India's Hindu-Muslim divide. Associated with frequent violence in history, the Pribumi-Chinese cleavage has been a master cleavage of twentieth-century Indonesia, just as the Hindu-Muslim cleavage has been one in twentieth-century India.[17] In the future, Muslim-Christian differences may outstrip the Pribumi-Chinese animosities in significance, but that is not yet true in Indonesia.

How does Indonesia's Pribumi-Chinese violence compare with India's Hindu-Muslim violence? In the fourteen-year period (1990–

2003) covered by Varshney, Tadjoeddin, and Panggabean (this issue), anti-Chinese riots claimed 1,259 lives.[18] Could the number be as large as it is for Hindu-Muslim violence in India—over 7,000 deaths—if a larger swath of time were covered?

We know that there was significant anti-Chinese violence during Indonesia's independence struggle (1945–1949).[19] More importantly, we also know that the anti-Chinese killings of 1965–1966, perpetrated as part of anti-Communist massacres, were massive and horrendous. While no clear estimation is available, there is a relative consensus that half a million people, including many Chinese, were slaughtered, making it the largest episode of violence in twentieth-century Indonesia (Cribb 2001). Finally, there were massacres of the Chinese in West Kalimantan during 1967–1968. "Estimates in the written accounts of the number of Chinese massacred range from two to five thousand, although these figures exclude deaths that later occurred in detainment camps" (Davidson 2008, 68).

If revolutionary violence during 1945–1949 is to be included on the Indonesian side, we should also perhaps incorporate on the Indian side the gruesome Hindu-Muslim carnage during India's partition in 1947. Though available numbers are not precise, most scholars believe that roughly a quarter million people perished during the partition of India. Still, there is no parallel to the 1965–1966 slaughter in India's postindependence history. At no point after India's independence were half a million people massacred. Riots after the destruction of Baburi mosque in December 1992 and anti-Muslim pogroms in the state of Gujarat in 2002 were the biggest episodes of postindependence Hindu-Muslim violence. Each took slightly over 1,200 lives, mostly Muslim (Varshney 2002, 2002–2003).[20] The master cleavage of Indonesian polity has arguably been much more brutal.

How should we conclude? Even after these comparisons are made, I don't think we can confidently say that Indonesia is more violent than other societies; a major point of our collective work is that the incidence of violence in many developing countries is either unknown or almost certainly underestimated. But regardless of what later comparisons might show, the scale of ethnocommunal violence in Indonesia does appear to be enormous. Anderson's formulation about the experience of violence that a seventy-year-old Indonesian is likely to have witnessed, or experienced, is largely correct, meaningful, and worthy of further exploration.

Variations

In search of greater understanding, as already indicated, the scholars who have contributed to this issue choose spatial and temporal variations in the incidence of violence over commonalities as their preferred method. Varshney, Tadjoeddin, and Panggabean seek to ascertain the distribution of all forms of collective violence short of civil war and test some existing theories of violence. Patrick Barron and Joanne Sharpe take the investigation down to the district level, asking whether violence is as spatially concentrated as Varshney, Tadjoeddin, and Panggabean believe. Welsh asks whether it is possible to understand why some places experience lynching as a mode of "justice" and others do not, unless one probes local conditions. Jacques Bertrand concentrates not on spatial but on temporal variation. He compares those periods in Indonesian postindependence history that have been marked by large-scale violence with those that were peaceful and asks what systemic features explain why violence was so concentrated around the breakdown of the New Order but has declined since 2001. Yuhki Tajima's primary, if not exclusive, focus is on temporal variation: why violence became so intense in the 1990s, compared to the 1980s.

A Brief Methodological Detour

Before I present an overview of the substantive findings, we need to ask why there is such emphasis on variations in these writings. The answer is quite simple. Although their arguments about qualitative research are rooted in statistical theory, King, Keohane, and Verba (1994) have significantly altered the way *qualitative* research is done in contemporary political science. King, Keohane, and Verba contend that a case study of violence, or several case studies of violence, would not give us a theory of violence.

Why should that be so? Why would a study of violence alone not generate an adequate theory of violence? What might explain this paradox? Here is how one can summarize the crux of the matter:

> Suppose on the basis of commonalities, we find that inter-ethnic economic rivalry (a), polarized party politics (b), and segregated neighborhoods (c) explain ethnic violence (X). Can we, however, be sure that our judgments are right? What if (a), (b) and (c) also exist in peace-

> ful cases (Y)? In that case, either violence is caused by the intensity of (a), (b) and (c) in X; . . . or there is yet another factor (d), which differentiates peace from violence. It will, however, be a factor that we did not discover precisely because peaceful cases were not studied with the conflictual ones. . . . In short, until we study ethnic peace, we will not be able to have a good theory of ethnic conflict. (Varshney 2002, 6)

Since the publication of King, Keohane, and Verba nearly a decade and a half back, further methodological interventions have taken the debate forward. Arguments reviving case studies, or a quest for commonalities, have resurfaced, but their form has changed. Case studies may not fit well with statistical modes of inquiry, argues John Gerring (2006, 2007), but case studies remain the best way of understanding causal *mechanisms* (why A causes B), whereas statistical research, when done well, can at best give us causal *effects* (what the effect of A on B is). Large-n statistical research, which is the foundation of King, Keohane, and Verba's arguments, typically has too many cases or observations. In and of its own, it does not allow scholarly intimacy with any of the empirical cases. As a result, we can't quite figure out the *process* through which a given outcome occurs. And without understanding the process—what led to what—it is hard to sort out causal mechanisms.

Moreover, studying similar cases is not without value. While it will not allow us to clinch arguments precisely for the reasons King, Keohane, and Verba identified, studying similar cases can knock down an existing theory or allow us to propose a theory that may be tested later.[21] Amartya Sen's theory of famines is among the best examples. Sen (1983) proposed a theory based on five famines; there were no nonfamines in his research design. Sen could not fully prove his argument about how famines represented entitlement failures, but the evidence he gathered was enough to knock down the conventional theory: that sharp declines in food supply cause famines. Moreover, a new theoretical idea—that entitlement failures cause famines—was put on the table for further investigation.[22]

What implications do these larger methodological arguments have for the studies of violence in Indonesia? First, studying violence alone may permit us to undermine existing theories and may even allow us to construct new theories, but we cannot be sure that our theories are right on the basis of commonalities alone. As Tajima in this issue points out, Gerry van Klinken's recent book falls in this category (van Klinken 2007). Van Klinken only studies major acts of rioting, building no variations into his research design. Second, case studies will continue to give us a sense of causal mechanisms, but again we can't be sure that the mechanism we identify in a case, or a set of cases, indeed produced the outcome observed, unless we select and study varying cases: cases of high violence versus low violence, or cases of violence versus peace. King, Keohane, and Verba may have underestimated the value of studies that compare similar outcomes, but their point about why variation is necessary for understanding causality—or to be more precise, "causal mechanisms"—remains valid.

This intellectual background should explain why these writings are different from how violence used to be studied in Indonesia. They reflect the methodological zeitgeist of our times. However, despite sharing methodological concerns, these scholars do have important substantive differences, toward which I now turn.

How Spatially Concentrated?

Though the levels of violence in Indonesia may be very high, Varshney, Tadjoeddin, and Panggabean claim that Indonesia fits the comparative pattern of *distribution* noted in recent studies of violence elsewhere. As in India, sub-Saharan Africa, and the 1960s United States, collective violence in Indonesia is highly locally concentrated.[23] Varshney, Tadjoeddin, and Panggabean find that fifteen districts alone, holding a mere 6.5 percent of Indonesia's total population in 2000, account for as much as 85.9 percent of all deaths in all forms of collective violence short of civil war.

Barron and Sharpe disagree. Varshney, Tadjoeddin, and Panggabean based their figures on a reading of provincial newspapers. Barron and Sharpe coded district-level or subprovincial newspapers in twelve districts of two provinces—East Java and East Nusa Tenggara (NTT)—for 2001–2003 and discovered evidence of more widespread violence. Barron and Sharpe selected these provinces because they were generally viewed as low-conflict areas. Still, compared with Varshney, Tadjoeddin, and Panggabean, they found "over three times as many deaths." They also think Varshney, Tadjoeddin, and Panggabean missed "thousands of deaths from collective violence."

Are Barron and Sharpe right? In a larger conceptual sense they are, but in a statistical sense some unresolved points require greater empirical scrutiny in the future. Let me explain.

Provincial newspapers are the basis for the dataset constructed by Varshney, Tadjoeddin, and Panggabean. If the speculation of Barron and Sharpe turns out to be right *for the nation as a whole*, it will be a damning critique of how even provincial newspapers, let alone national newspapers, covered collective violence in Indonesia. But it is worth pausing for a moment to ask: What can twelve districts in two provinces, covering an estimated 3.3 percent of Indonesia's population in 2000, establish for the country as a whole? The Varshney, Tadjoeddin, and Panggabean dataset covered fourteen provinces accounting for 72.4 percent of the country's population. Indonesia now has thirty-six provinces and over 400 districts. Before we can extrapolate from twelve districts studied during 2001–2003, we need to know whether these districts were representative of Indonesia in general. At issue is a key conceptual matter, about which we have no empirical knowledge yet: Is small-scale violence, such as lynching, more common in low-conflict areas? If high-conflict areas have more riots and very few lynchings and, contrariwise, low-conflict areas have a lot of small-scale violence but very few riots, then it is quite possible that the twelve districts of Barron and Sharpe may not be generalizable to the country as a whole. One of the greatest challenges for conflict research in Indonesia, as elsewhere, is to sort out the relationship between small-scale violence and large-scale violence.[24] No existing theory allows us to predict this relationship.[25]

However, in a deeper conceptual sense, Barron and Sharpe have made a compelling point. By systematically comparing—*for the first time*—the reportage in provincial and district-level newspapers, and showing that "provincial sources . . . picked up only 39 percent of deaths from group violence in our research areas," they have empirically established what was at best a hunch before: that district newspapers are much better at reporting small-scale group violence than newspapers at higher levels of the polity. Evidence provided by Welsh (this issue) lends further credibility to this argument. Barron and Sharpe make it abundantly clear that by relying on provincial newspapers, Varshney, Tadjoeddin, and Panggabean underestimated the incidence of small-scale violence, if not of large-scale violence. Future studies of conflict in Indonesia, and possibly elsewhere, will have to pay attention to this finding. The sources one chooses should depend on the nature of the violence to be studied.

Other than the argument about sources, a new substantive conclusion is also worth noting. As elsewhere, large-scale violence (riots and pogroms) may be heavily locally concentrated in Indonesia, but small-scale group violence (lynchings and intervillage brawls) is quite widespread. The overall magnitude of each kind of violence—small or large—may not yet be fully settled, but the variation in the pattern of distribution is no longer in doubt. Why the two patterns—concentration of large-scale violence and wider spread of small-scale violence—are different will require more investigation in the future. No theory in the field of ethnic conflict predicts this pattern.

Macro Versus Micro

Bertrand critiques Varshney, Tadjoeddin, and Panggabean as well as Barron and Sharpe. His basic argument is that a large-n statistical exercise that maps the distribution of violence in a finite time period focuses too much on the local factors and microprocesses, ignoring the "broader changes occurring at the macro-level." According to him, one analytically fatal consequence of such emphasis is that large datasets are unable to explain the temporal clustering of collective violence in Indonesia: during the mid-1960s and around the breakdown of the New Order.

Bertrand's own explanation for why one gets these temporal spikes revolves around the notion of critical junctures. As in Bertrand (2004), he defines a critical juncture as a political period, when the basic features of a "national model" are renegotiated. At such moments,

> groups struggle for inclusion, or more frequently, for renegotiation of their terms of inclusion. They mobilize either violently, or through extra-institutional protest to renegotiate unfavorable terms, eliminate discrimination, gain greater recognition, representation or resources. States turn to address demands and mobilization through a mix of repression and accommodation. When the critical juncture comes to an end and institutions are stabilized, new terms of inclusion are in place, which define relations between ethnic groups and the state. (Bertrand, this issue)

Pursuing self-admittedly a "historical institutionalist" mode of inquiry, Bertrand thus seeks to restore "structure" to the studies of violence. He calls attention to two critical junctures in the recent history of the nation: Suharto's downfall in 1998, ending a thirty-year-long polity; and, before that, the embrace of Islamic groups by Suharto in the early 1990s, upsetting the multireligious balance of a Pancasila state and leading to serious Christian anxieties. "The emphasis on critical junctures," he adds, "also explains why violent ethnic conflict diminished very significantly after the stabilization of the new regime. After the accession to power of Megawati Sukarnoputri in July 2001, the new regime became much more strongly entrenched and stabilized." Thus, with new ethnic relations renegotiated, Indonesia, according to Bertrand, has entered a long phase of highly diminished large-scale violence.[26]

As a mode of inquiry, historical institutionalism is basically about the *longue durée,* about the continuation of obdurate historical patterns, and about changes in the basic rules of the game, given long-standing patterns. In a social science world increasingly preoccupied with rigor and method, a narrow inquiry is often favored, for it is easier to be rigorous about short time frames and sharply delimited problems. Longer processes or historically rooted underlying factors tend to get neglected, even if they are highly significant.

Bertrand's emphasis on broader processes should therefore be welcomed. Moreover, there is something fundamentally incontrovertible about the claim that after repressing Islam until the mid- to late 1980s, Suharto's renegotiation with Islam in the early 1990s altered the basic political parameters of the system, thereby arousing new fears among certain groups, especially the Christians, and generating new confidence among other groups, especially the Muslims. In a number of societies, such fundamental alterations in group relations have led to violence. It should not be surprising that we have no evidence of serious Muslim-Christian clashes in Indonesia before the mid-1990s. Suharto's shift is thus most probably causally related to Muslim-Christian violence. And the clustering of violence around 1998–2001 also cannot but be connected to the breakdown of a long-lasting New Order.

However, we need to ask a methodological question: Do large-n datasets necessarily direct explanation toward the microprocesses, ignoring macrotrends? Are they prone to such biases?

Typically, large-n datasets map the distribution of a phenomenon—in this case, how violence was distributed over the years covered; over the forms it assumed (religious, ethnic, or economic; riots, pogroms, or lynchings); and over geographical spaces. *Datasets are not about causes; they are a way to describe the empirical universe*. Once the distribution of violence is ascertained, either causal inquiries are launched or some existing theories are tested. Causality firmly resides outside the dataset.

Therefore, there is no fundamental contradiction between summoning a macroexplanation for violence and creating a dataset. Welsh, after collecting data in her four provinces for a period of ten years and identifying its varying distribution, argues that the causes of lynching "are national *and* local." And after presenting patterns of violence, Varshney, Tadjoeddin, and Panggabean argue:

> The notion of critical junctures—the decline and end of the New Order—is of great significance in terms of timing, but this systematic transformation did not produce collective violence everywhere. Group violence had local theaters. Some of the local questions that need to be explored systematically are: how the New Order upset a traditional local equilibrium of communities—communities rooted in traditional (*adat*) forms of governance—in the process of installing uniform, all-Indonesia forms of local institutions; how migration altered local equilibria; whether different ethnic or religious communities are integrated or segregated in different local settings; how the patterns of local governance have vastly varied; and how economic penetration of previously self-sufficient communities led to dramatically new results, marginalizing some communities and privileging others. (Varshney, Tadjoeddin, and Panggabean, this issue)

Thus, Bertrand and microresearchers can both be right. *Temporal variation* is best explained by macrofactors, but *spatial variation* is best analyzed when we pay attention to local processes. Macrofactors vary over time, but for any given point of time, macrofactors are, by definition, a constant for the entire nation or province, and a national or provincial constant cannot logically be called on to account for *intra*national or *intra*provincial variations, as opposed to *inter*national or *inter*provincial differences. Anticipating this logical truth, Bertrand concedes that "an emphasis on critical junctures and institutional change . . . explains the emergence of clusters of ethnic violence, but it cannot in itself explain why violence emerges in some locations but not others" (Bertrand, this issue). A more thorough explanation of Indonesian violence will clearly require both macro- and microexplanations.

An Alternative Explanation for Temporal Variation

Tajima (this issue) provides an alternative to Bertrand's theory of temporal variation. Instead of emphasizing critical junctures, defined as something related to the basic properties of "national model," Tajima concentrates on the changing role of the military. Taking his central cue from one of the principal theories of violence in the larger field of ethnic conflict, a theory that focuses on "state weakness," and combining it with Indonesian particularities, Tajima argues:

> What is surprisingly absent from the Indonesianist literature is the role of the military. Where the military is mentioned, it is as an *agent provocateur*. . . . Given the central role of the military in both politics and security in Indonesia and the massive changes in state security forces during the transition from authoritarian rule, the lack of attention toward the military as a factor in communal violence is striking. This is even more so given the prominence of weak state capacity in comparative theories of ethnic conflict and civil wars. (Tajima, this issue)

Whatever one's normative position on how the Indonesian military deployed coercion and whether its coercive role was in the long term tenable, according to Tajima, we should not forget that rightly or wrongly, the military was the state's preeminent apparatus for maintaining domestic order during the New Order. Even the police reported to the military. The military used excessive violence to put down disorder but, in the process, large-scale social violence was averted.

One should emphasize that Tajima does not defend the military's impunity but only tracks down its implications for social violence: "The idea that greater concerns over human rights led to ethnic violence by restraining the formerly repressive military is not to counsel against the adoption of human rights reforms in authoritarian regimes. Rather, it suggests that the manner by which political liberalization is achieved deserves greater attention from scholars and policymakers."

According to Tajima, starting in the late 1980s, three "liberalizing" developments progressively undermined the power of the military, opening up space for large-scale group violence. First, the rise of the human rights discourse in the international system led to increasing criticism of the Indonesian military. Especially after the 1991 massacre in Dili, even Suharto, unable to ignore international pressure, "dismissed two generals and court-martialed 19 soldiers involved in the affair" and subsequently established the National Commission on Human Rights. Second, in the early 1990s, serious differences emerged between the highest rungs of the military and Suharto. The clash led to Suharto clipping the wings of the military. And finally, in 1999, President Abdurrahman Wahid separated the military from the police, making the latter responsible for law and order. The police, however, were not ready to perform their new role. For decades, they had been dependent on the military and had no autonomous capacities of their own. This was precisely the time violence peaked in Indonesia.

In short, rather than the abrupt changes in the national model disturbing the preexisting equilibrium between groups, it is the eroding power of the military and the incapacity of the police to fill the gap that, according to Tajima, explains the clustering of violence around 1998–2001. This argument, consistent with a Hobbesian view of security common in the field of international relations, is likely to generate a lot of debate in Indonesian circles.

Conclusion

Displaying knowledge of the methodological and theoretical developments in the comparative field of ethnic conflict, the scholars assembled in this issue craft original datasets and reinterpret Indonesian violence. Many gaps of understanding, of course, remain, and much more work will be required to take the emerging explanations further. But these writings set the stage for a systematic dialogue among theory, methods, and the specificities of Indonesian violence. Too important to be left out, Indonesia must return to the mainstream of scholarship on ethnocommunal conflict.

Notes

For comments on an earlier draft, the author is grateful to Benedict Anderson.

1. Bertrand is a partial exception. He does not disaggregate violence as much as he seeks to explain why aggregate violence had such variations over time.

2. The emphasis placed on variation is, of course, not incontestable. See Brady and Collier (2004).

3. See, for example, Hefner (2008). Hefner reviews three recent books on Indonesian violence. As the term *Indonesian violence* suggests, the literature dealing with it should, in principle, intersect two kinds of works: those dealing with Indonesia, and those analyzing violence. Hefner's review makes no reference to the main findings of the enormous literature on group violence that has emerged in different parts of the world. The judgment is entirely framed in light of Indonesian studies.

4. A great example of such innovation is a chapter titled "Creole Pioneers" in Anderson (1983). Anderson, originally a scholar of Indonesia, looked at nationalism in Latin America with his Southeast Asianist eyes, producing insights that could not emerge from within the Latin Americanist literature. Intellectually barging in from the outside—or what is sometimes called intellectual trespassing—has its conceptual and methodological benefits.

5. Recent works that have begun to incorporate insights emerging from non-Indonesian materials include Davidson (2008), Sidel (2006), and van

Klinken (2007). Also relevant is a book of essays that probes the relationship between political science as a discipline and studies of Southeast Asian politics (Kuhonta, Slater, and Vu 2008).

6. For an overview of the theoretical literature in the field, see Varshney (2007, 2003).

7. This does not mean that at higher levels of income, ethnic or racial prejudices in the functioning of the police and administration, or hate crimes among the citizenry, disappear altogether. Prejudices can indeed be shown to exist (Hattam 2007) and hate crimes also continue (Green, McFalls, and Smith 2001). But *collective* forms of overt violence tend to diminish. Before small tensions, clashes, and individually perpetrated hate crimes trigger riots, the police and administration intervene more often than not.

8. For civil wars, the COW (Correlates of War) dataset is most used. For a well-known use of the COW dataset, see Collier and Hoeffler (2004). The MAR (Minorities at Risk) dataset is also often referred to, but rarely used with confidence. The coding problems of the latter, as of now, appear to be formidable. Nonetheless, see Gurr (1993).

9. To be more precise, lynchings became more frequent after the so-called period of reconstruction.

10. The *Chicago Tribune* "began tracking lynchings in the late 19th century. It reported . . . 4,951 lynchings in the United States from the year 1882 through 1930. . . . Of the victims, 3,513 were black and 1,483 white; 92 were women and 76 of those were black. Eighty-two percent of the recorded lynchings were in the eleven [southern] states" (Howard 2007).

11. In a project in which I am currently involved with Patrick Barron and Blair Palmer, we are collecting statistics on lynchings for the period 1998–2008 for twenty-three provinces, covering nearly 80 percent of the Indonesian population.

12. One should also note that American lynching statistics cover only deaths, not injuries, whereas Welsh covers both. But even after enough subtractions have been made for injuries, Indonesian figures on lynching deaths would be considerably higher.

13. One should note that the state of affairs in the American South in the late nineteenth century was not terribly different. "In the South, those who participated in lynchings . . . were likely to be public officials, members of the Ku Klux Klan, the poor, and the working class" (Howard 2007, 392–293).

14. The 1998 "dragging death" in Jasper, Texas, of James Byrd Jr. is the last recorded lynching in the United States (Howard 2007, 396).

15. Though India, like Indonesia, has several religions, Hindu-Muslim violence is the primary form of religious violence in the country.

16. Varshney, Tadjoeddin, and Panggabean wanted to cover as long a period in Indonesia as in India, but compared to Indian newspapers, Indonesian newspapers turned out to be less usable, reliable, and available. A great deal of double-checking was necessary.

17. Though the key texts on Indonesian Chinese do not use the term "master cleavage," the following scholars in effect go in that direction: Coppel (2004), MacKie (1976), and Shiraishi (1997).

18. Not all of those killed were Chinese, even if the Chinese were the target of violence. It is not possible to generate a precise ethnic breakdown of deaths on the basis of newspaper reports. And police records, even if available, would not be reliable.

19. Under Japanese occupation (1942–1945), too, the Chinese were targets of violence. Such violence was perpetrated both by the Japanese and Pribumi (Heidhues 2003).

20. Though civil wars are not the focus of writings in this issue, it is worth adding that India's civil wars have also been less violent. For East Timor, scholars suggest that 200,000 people were killed, though the truth commission puts the number at 100,000. Having a population of about 750,000 in 1973, East Timor is a very small place. In no civil war in India, including the one in Kashmir, which had a population of over 7 million in 2001, have the numbers of those killed been so large.

21. For a fuller development of these methodological ideas and their implications for research in the field of ethnic conflict, see Hopf (2006), Laitin (2006), and Varshney (2006). These three essays were part of a special issue on David Laitin's work, sponsored by the American Political Science Association.

22. Varshney (2008) analyzes the fuller implications of selecting only famines for Sen's theory of famines.

23. See Fearon and Laitin (1996), Horowitz (1983), and Varshney (2002).

24. To buttress their claims, Barron and Sharpe also use the data from PODES, a survey conducted by the government of Indonesia. On conflict, it is worth asking whether the PODES data are reliable at all, because the survey administrators, representing the government, questioned village heads, not a representative sample of citizens. From the data so produced, it can't be firmly established whether village heads gave strategic, or honest, answers. I helped write the conflict questions for PODES, but I did not know beforehand that the survey would be administered to village heads only.

25. Another technical statistical matter is worthy of brief consideration. It would have been, statistically speaking, quite stunning if Barron and Sharpe had found "three times as many deaths" in high-conflict areas such as Maluku, North Maluku, Central Kalimantan, West Kalimantan, and Central Sulawesi. The number of deaths in each of these provinces is very high; the discovery of three times, or even two times, as many deaths would have made an enormous difference to the overall numbers. In contrast, East Java and NTT are low-conflict areas. For 2001–2003, based on a reading of provincial newspapers, Varshney, Tadjoeddin, and Panggabean found only twenty-three and twenty-two deaths in East Java and NTT, respectively, in acts of collective violence, whereas Barron and Sharpe found eighty-three and seventy-three, respectively. Isn't the statistical base here too small to permit a serious claim about multiples ("three times")?

26. Small-scale group violence may be another matter. Bertrand's argument does not deal with small-scale violence.

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