Economic & Political WEEKLY

Review: An Electoral Theory of Communal Riots?
Author(s): Ashutosh Varshney
Source: *Economic and Political Weekly*, Vol. 40, No. 39 (Sep. 24-30, 2005), pp. 4219-4224
Published by: Economic and Political Weekly
Stable URL: http://www.jstor.org/stable/4417194
Accessed: 15/04/2011 19:08

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An Electoral Theory of Communal Riots?

Votes and Violence: Electoral Competition and Communal Riots in India
by Steven I Wilkinson;
Cambridge University Press,
New York, 2004;
First South Asian Edition, 2005;
pp 310, $ 75 (hardcover).

ASHUTOSH VARSHNEY

Simultaneity of research programmes is a double-edged sword. Acrimony is quite common in political and popular debates, but scholarly exchanges are generally expected to be less feverishly pitched. The conventional bounds of tone and temper, however, do sometimes collapse, when several scholars are working on roughly the same subject at roughly the same time.

The coincidence of simultaneity need not inevitably go in this direction. It can also be viewed as an opportunity for a healthy intellectual exchange, which allows the scholarly community to advance the understanding of complex problems, and also provides them the building blocks for better explanations.[1] It is in this spirit that I would like to view this book – as an invitation to dialogue, as a way to move our knowledge forward.

My own arguments about Hindu-Muslim riots, based on research that began in 1990, are contained in my book first published three years ago.[2] With Steven Wilkinson, the author of the book under review, I have collaborated twice: to put together a dataset on Hindu-Muslim riots in India (1950-95);[3] and to publish some early findings based on the first cut of our database (1960-93).[4] Our arguments in the beginning were not very different [Varshney and Wilkinson 1996], but by now we appear to have radically diverged. This essay is an occasion to take stock of this divergence, and appraise the electoral theory of riots that Wilkinson proposes.

Wilkinson's book is based on his PhD dissertation completed at MIT in 1999, but it is manifestly more than that. For more than 10 years, Wilkinson has worked on Hindu-Muslim conflict. He was trained first as a historian, wedded to qualitative methods and to the power of the archive. He has increasingly moved towards quantitative modes of analysis. It has been a long professional journey, which will doubtless evolve further. This book is his first large product of his emerging quantitative methodological orientation.

The Argument

Why do communal riots take place? Wilkinson's central argument is that "democratic states protect minorities when it is in their government's electoral interest to do so" (p 6).[5] But when, one might ask, is it in the government's interest to protect minorities?

Wilkinson points to two principal possibilities: (a) "when minorities are an important part of their party's current support base, or the support base of one of their coalition partners in a coalition government" (p 6); or (b) when the overall electoral system in a state is so competitive ...that there is therefore a high probability that the governing party will have to negotiate or form a coalition with minority supported parties in the future, despite its own preferences" (p 7, emphasis added).

Of the two parts of the argument, thus, the first concentrates on the support base of the governing party or coalition; and the second focuses on the level of electoral competition. The higher the competition between political parties, says Wilkinson, the lower will be the incidence of riots. "Politicians in government will restrict the supply of security to minorities if...overall levels of party competition in a state are so low that the likelihood of having to seek the support of minority-supported parties in the future is very low" (p 7). In short, in highly competitive electoral contexts, Muslims would be protected by ruling parties, even when targeted by mobs in riots, because small groups could be critical in swinging closely contested elections.

How is the level of electoral competition to be measured? In terms of the effective number of parties (ENPV), says Wilkinson. The virtue of ENPV, a commonly used measure to determine the level of party competition, is that it does not count the total number of parties contesting an election and weigh them equally, for that can only demonstrate the formal, not real, level of competition. ENPV gives greater weightage to parties with a larger vote share than to parties with a smaller vote share, thus showing what the degree of effective, not formal, competition is.[6] The state of Madhya Pradesh (MP) may have as many political parties participating in the elections of late as Uttar Pradesh (UP), but MP normally has a straight contest between the Congress and the BJP, whereas in UP the electoral field has for over a decade been four-cornered, split between the Samajawady Party (SP), Bahujan Samaj Party (BSP), BJP and the Congress. The effective number of parties, and therefore the level of party competition, in UP is higher than in MP, even if the total number of political parties contesting in the two states is about the same.

In my critical appraisal below, I will first deal with the second argument. The reason is quite simple. The argument linking electoral competition and riots is quite novel. Moreover, claims about electoral competition – what it is, how to measure it, and how it is connected to riots – dominate the book, and also provide its title. I turn subsequently to the argument about the role of the state, which has been standard fare in the literature for a long time.

Party Competition and Riots

Does higher competition for votes between political parties lead to lower communal rioting and lower party competition to higher rioting, as Wilkinson claims, or is it the other way round? Consider the Figure, derived from the 46-year database (1950-95) that Wilkinson and I jointly created. It presents data (a) on the number of deaths and (b) on the number of riots for each year between 1950-95. Note that between 1950 and roughly 1975-76, the figures reveal no upward or downward trend. The ups and downs in riots appear to be a random walk. Starting with the late 1970s, however, there is a clear upward trend – ups and downs in riots each year are around a rising trend. Compared to 1950-76, more riots took place in India between 1977-95 and more people died in them.

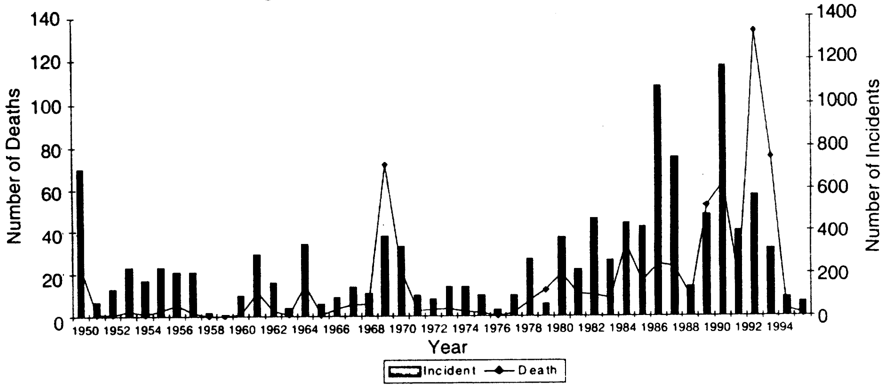
Wilkinson's central argument says: the higher the competition, the fewer the riots. Was party competition between 1977 and 1995, a period of rising riot frequency, less vigorous than during 1950-76, when riots were fewer?

The answer is an unambiguous no. Roughly till 1967, the Congress Party had a nearly hegemonic hold over power and no other parties came close. The Congress began to lose power in 1967 at the state level and by 1977, the Congress was voted out in Delhi as well. Since then, the party competition, if anything, has been more vigorous. Indeed, at least since the late 1970s, incumbency has not been an advantage in Indian politics. Roughly three out of four incumbents have been thrown out of office in elections.

How do these considerations tie up with ENPV? ENPV is, by definition, higher for a period when many more parties become effective contestants for power than for a period when the domination of one party was rarely challenged. In short, India has had more riots in a period of greater party competition, not less. Wilkinson's theory proposes the opposite: "The effect of the decline of the dominant Congress Party and the resulting party competition in recent years has not, as some have argued, been to increase the level of communal violence. On the contrary, the increasing party competition for minority voters has led to a reduction in Hindu-Muslim violence, as politicians are forced by electoral incentives to take firm action to prevent Hindu-Muslim riots" (pp 169-70).

Here, then, is a direct clash between theory and evidence. But Wilkinson does have a way out. He can say that his argument applies to the state level, not to the national level. Logically speaking, it is possible for the country as a whole to have more riots and higher party competition at the same time, because (i) a few states may have low party competition and a lot of riots, even when (ii) most states have higher party competition and fewer riots. In theory, an increase in (i) can not only fully cancel out the decrease in (ii), but also produce an overall national increase in riots if the jump in (i) is big and the decline in (ii) low.

Figure: Hindu-Muslim Riots 1950-1995



It would have been instructive to discover whether states with small effective number of parties in the post-1977 period – let us say, Gujarat, Rajasthan, Madhya Pradesh, Andhra Pradesh, Himachal Pradesh – have indeed more than spoiled the overall downward effect that the states with higher number of effective parties – Uttar Pradesh, Bihar, Haryana, Maharashtra, Tamil Nadu – have, according to Wilkinson, brought about. No such exercise is undertaken. Instead, Wilkinson calculates the ENPV for various states in 2002 (p 143) and then presents his regression results for the period 1961-95 (p 151), leading to his major conclusion that "the number of Hindu-Muslim riots goes down as the effective number of parties goes up" (p 150). If this argument does not work for India as a whole, does it for the state level?

Surprising Exclusions

Wilkinson's book is silent on two major issues, raising serious doubts about the validity of the argument even at the state level. First and foremost, one of Wilkinson's key causal variables – low or high party competition, or closeness of the election race – is at the level of electoral constituencies, but his riot data are town-based. It is well known in India's election studies that administrative units like towns and electoral units like constituencies do not normally coincide. Most towns of India are part of a larger or smaller electoral constituency, both for the national parliament and state assemblies, or alternatively, they are so large that they have split into several constituencies.

Potentially, this creates insurmountable problems for Wilkinson's argument. Let me give an example. The town of Aligarh is split into two state assembly constituencies, Aligarh and Koil. Aligarh constituency incorporates a large part of the town, but Koil is both urban and rural, made up of some parts of Aligarh town and some contiguous rural areas. Wilkinson can measure electoral competitiveness at the level of Koil or Aligarh constituency, but the riot data we jointly collected are not for constituencies.[7] Incidents that are coded as riots in Aligarh town (including when the riot took place, how many people died, where the riot took place), may have taken place either in Aligarh constituency or in Koil constituency.

There is no way of knowing this, unless one meticulously disaggregates riot locations below the town level – down to the mohalla or neighbourhood level – and then begins to map neighbourhoods with the constituencies in which they fall. Our database, as it currently exists, does not allow this. Wilkinson simply cannot be sure that riots at the town level are connected to the competitiveness of elections at the constituency level.

If this is true for Aligarh, a medium-size town, consider the enormous measurement problems that this incorrect equation raises for riots that took place in the bigger riot-prone cities – Ahmedabad, Vadodara, Hyderabad, Mumbai, Delhi. These five cities accounted for a very large proportion (nearly 38 per cent) of all deaths in Hindu-Muslim riots during 1950-95 [Varshney 2002a: 104-5]. Moreover, each of these cities has many more than two state assembly constituencies that Aligarh is split into. The city of Delhi, for example, has seven national parliamentary and, currently, 70 state assembly constituencies. Our dataset simply does not allow us to determine which Delhi riots fell in which of Delhi's many constituencies. No analyst can derive conclusions about towns from electoral constituencies unless we incorrectly assume that the town and constituency tend to coincide.[8]

Another big silence, perhaps inadvertent, calls attention. Why have the regressions been run only for truncated period, 1961-1995 (p 151)?[9] Why not for the entire period of 1950-95 included in our database? This matter is important, for the period left out of regressions, 1950-60, saw a much smaller number of riots than the later period, and also had a low level of party competition. Is the negative relationship between party competition and riots by any chance an artefact of the truncation, or does it hold up even when the entire period is covered? Wilkinson provides no assistance in the matter.

Inconsistent Inclusions

Wilkinson's argument becomes even more troubling, when the reported regression results are carefully examined. Instead of a clearly demonstrable and robust inverse relationship between the degree of party competition and occurrence of riots, we get radical indeterminacy and a lot of what statisticians call 'noise'.

Wilkinson has presented his regression results at two levels – for UP, 1970-95 (pp 43 and 45) and for 15 major states of India, 1961-95 (p 151).[10] When the latter is analysed, "there is a negative relationship between the degree of electoral competition in a state and its level of communal riots" (p 150). But in UP, the relationship is positive (Table 2.1, p 43). "The closeness of the previous Vidhan Sabha election ...seems to be positively related to the likelihood of a riot taking place before the next election" (pp 42-43). Are close races likely to reduce, or increase, the odds of riots?

When one looks at the relationship between electoral competition and the number of deaths, not number of riots, the plot thickens even further. Closeness in previous national election is negatively correlated with the number of deaths, but positively correlated with the closeness in the previous state election (p 45). In short, the relationship between electoral competition and riots is not uniformly negative. Rather, it is both positive and negative, depending on (a) whether we are looking at India as a whole, or at UP, and (b) whether we are looking at national or state elections. Closely contested national elections appear to produce a tendency towards fewer, smaller or no riots, but closely contested state assembly elections generate the opposite result.[11]

More confusion is added by the variable "proximity to elections". In UP, the odds of riots go up if national or state level elections are within six months (p 43), but at the all-India level, riots are positively correlated with "state elections within six months" and negatively correlated with "national elections in six months" (p 151).[12] Statistically, then, we have no uniquely acceptable and generalisable pointers to whether elections and riots are clearly related.

The indeterminacy, one should emphasise, is not confined to statistical results only. The text also says quite contradictory things. In the opening chapter, as quoted above, the relationship between electoral competitiveness and riots is called negative (pp 6-7), an assertion repeated in several other places (p 137, 144, 147, 150 and 152). Elsewhere, however, Wilkinson equally clearly states: "towns with a close electoral race are considerably more likely to have a Hindu-Muslim riot" (p 16); "increases in electoral competition are associated with a rise in the likelihood of communal riots" (p 47); "violence...was most likely to break out in those places where political competition was most intense" (p 205).

What might account for these contradictions? Let us give the argument the benefit of doubt, and point to two theoretical possibilities. First, it may be that the relationship between electoral competition and riots is neither uniformly positive, nor uniformly negative, but curvilinear. That is, it is positive (or negative) up to a threshold of competitiveness and turns negative (positive) once party competition crosses that threshold. There is, in other words, a 'tipping point' somewhere which transforms a negative or positive linear relationship into its opposite. If so, Wilkinson should have tested for it, just as he has for the percentage of Muslims in a town's population, which he finds curvilinearly related to riots (p 41).

Second, it may be that Wilkinson has measured electoral competitiveness for his India-level analysis in one way and measured it for the state of UP quite differently. As we look carefully, that does turn out to be the case. For Wilkinson's national level analysis, the indicator of electoral competitiveness is ENPV, and for the analysis of UP, the indicator is "how close the last election race was".

These are, of course, two different things, for it is possible for races to be quite close even if the contest is bipolar, not multicornered. Of late, election races between the Congress and BJP have been roughly as close in MP and Rajasthan as in the multi-party contests of UP. Wilkinson should have specified what matters most for his theory: closeness of races, or number of effective parties. They are conceptually separable. Wilkinson should also have used an identical measure for India and UP, and at the very least, might want to re-run the regressions.

To conclude, if the exclusions generate doubts about Wilkinson's central argument, the inconsistencies and contradictions of what has been included seem to undermine it. This does not, of course, mean that there is no link between elections and riots. All it suggests is that Wilkinson has not been able to establish it.

State and/or Civil Society?

Let me now turn to the first part of Wilkinson's argument. It concerns the relationship between the state and riots. "The response of the state", says Wilkinson, "is the prime factor in determining whether ethnic violence breaks out" (p 6). Further, "in virtually all the empirical cases I have examined, whether violence is bloody or ends quickly depends not on the local factors that caused violence to break out but primarily on the will and capacity of the government that controls the forces of law and order (p 5).

A great deal has been written about the role of the state in communal riots.[13] But before I analyse Wilkinson's version of it, let us first note that Wilkinson's views have dramatically changed over the last 10 years. In his first publication on the subject, jointly authored with me [Varshney and Wilkinson 1996], Wilkinson had drawn a distinction between the normative and empirical sides of a state-based explanation, suggesting that if the state repeatedly does not stop riots, even though it is constitutionally supposed to, scholars should stop asking what the state should do and start investigating why the state in fact does not perform its constitutionally assigned functions:

> Justice Raghubar Dayal's commission summarised almost all of the main measures to reduce violence as long ago as 1968: that state governments should not undermine or interfere with local law enforcement; that speedy and firm action should be taken at the first sign of trouble; that prosecutions of offenders should not be withdrawn for political reasons; that

officers in service should not be transferred for reasons other than the requirements of service; that the press should be stopped from making false and deliberately inflammatory statements; and that textbooks should be cleansed of deliberately communal interpretations of history (Varshney and Wilkinson 1996:38, italics added).

After outlining this list, Wilkinson, along with me, concluded that "another riot commission which tells us what ought to be done is not as helpful as finding out why previous recommendations have not been followed and then deciding which policies can be carried out" [Varshney and Wilkinson 1996, ibid: p 46].

Moreover, Wilkinson at that time firmly veered towards an explanation based on civil society: "Local inter-group ties...seem to provide the best defence against the spread of communal violence" (Ibid, p 39). Many examples of integrated ties preventing riots were cited. One such example stood out:

> Disha is a women's NGO working in Saharanpur district of Uttar Pradesh. Disha's workers (sakhis and sahyoginis) are drawn from both religious communities.. . As part of the Mahila Samakhya (MS) initiative in the state, Disha worked to improve women's education and awareness about a wide range of social issues...In Bateda village, before MS started, the women from the Muslim *mohalla* did not speak to the women from the Hindu basti....Gradually, a relationship of trust was built. ...(T)he sakhis and sahyoginis did not find it difficult to mobilise the two communities on a common basis like ration cards. Once the barrier was broken there was no going back. ....They were even supported by the men in their efforts.
> Such contacts helped to stop communal violence in the autumn of 1990. Women mobilised by the NGO were active in stopping the controversy over the Ram janmabhoomi from destroying local communal harmony. Hindu women who had met Muslims through Disha's social work persuaded local Hindus to reassure Muslim families – who were about to flee their homes – that they would be in no danger if they stayed" (ibid, pp 43-44).

This previous writing is not referenced at all in this book. If considerably greater research since 1996 has led to a change in Wilkinson's views, it should of course be accepted as something to do with professional evolution. But does the new argument stand up to scrutiny?

A proof of why – and how – the state would ignite or stop riots is typically not found in regressions, which can suggest correlations, and are rarely enough for establishing causality [Achen 1982]. For ascertaining cause-and-effect relationships, one normally needs to go to qualitative empirical materials and do what has come to be called process-tracing – sorting out when a riot took place, or was likely to occur, what came before that, what after, and in what ways the state could be said to be involved.[14]

Politics and the Muslim Vote

To support his theory, Wilkinson has marshalled qualitative empirical materials primarily from five states: Gujarat, Kerala, Tamil Nadu, Bihar and Uttar Pradesh. When the 2002 riots broke out in Gujarat, the riots on the whole did not spread beyond the state,[15] though it should be noted that despite the enormity of violence, several towns within Gujarat, including Surat, remained quiet, or had small incidents only.[16] Wilkinson argues that the clearest support for his theory comes from how Indian states other than Gujarat handled the 2002 riots. In 2002, Gujarat was among the states having the lowest number of effective parties, the BJP was in power in the state all by itself (not in a coalition) and it had no need for Muslim votes. The states of MP and Rajasthan – adjacent to Gujarat and like Gujarat, having only two main parties, the BJP and Congress – had no riots because the political party in power, the Congress, needed Muslim votes to compete with the BJP. In short, for riots not to take place, a state does not have to have a high ENPV, though it would be better if it did. All that is required is that a ruling party should have need for the Muslim vote.

Wilkinson's interpretive account of qualitative political materials, which he needs to establish causality, is remarkably selective. First of all, it is worth asking if the Gujarat riots in 2002 had something to do with the fact that the central government in Delhi, in which the BJP was a primary partner, did not suspend the BJP government in the state, using Article 356 of India's constitution. Second, more critically, one can think of not one or two, but many instances when the ruling party was not the anti-Muslim BJP, or its analytic equivalent, the Shiv Sena, but deadly Hindu-Muslim riots nonetheless took place.

If Gujarat in 2002 was ruled by the BJP, the Congress party ruled Gujarat on the following occasions when riots broke out: January 1982; March 1984; March-July 1985; January, March and July 1986; January, February and November 1987; April, October, November and December 1990; January, March and April 1991; and January and July 1992.[17] The BJP came to power in Gujarat state only in 1995. Moreover, in the early to mid-1980s, the Congress in Gujarat also unveiled the so-called KHAM (Kshatriya, Harijan, Adivasi and Muslim) strategy, aimed at putting together an alliance of these groups as a basis for power. The Congress aggressively courted Muslim vote; it was in power, yet Hindu-Muslim riots were endemic.

Consider now the period of Ayodhya agitation, 1990-92. During 1989-90, Uttar Pradesh was ruled by Mulayam Singh Yadav, whose commitment to Muslims and cultivation of the Muslim vote is well known. In 1990 he did order the police under his command to shoot on the masses mobilised by Hindu nationalists, earning the famous epithet 'Maulana Mulayam'. Yet, awful riots took place in Uttar Pradesh in 1989-90. In supporting his theory, Wilkinson notes how Mulayam Singh Yadav in 1994-95 succeeded in preventing riots (p 93), but surprisingly omits his failure to do so in 1989-90.

To make sure that these illustrations are not simply viewed as a few exceptions, to only proving the rule, let me give some more critical examples. During the infamous Mumbai riots in January 1993, the Congress ruled the state of Maharashtra. During the 1980s riots in the Moradabad, Aligarh and Meerut towns of UP, the Congress ran state governments in UP. Between 1978 and 1983, riots repeatedly rocked the city of Hyderabad, even as the state of Andhra Pradesh had Congress governments. And most remarkably of all, the riots of 1961, the worst year for riots in the first decade and a half of independent India, occurred when Nehru was India's prime minister, with an unquestionable commitment to India's Muslims, and almost all states then were Congress ruled. Counterexamples undermining Wilkinson's theory are simply too many to be brushed aside as occasional deviations from the rule.

Why should the Congress, even under Nehru and Indira Gandhi, have failed to prevent riots? Why was Mulayam Singh Yadav successful in 1994-95 but failed in 1989-90? Why did Gujarat government fail to prevent riots in the early to mid-1980s? Can we prove that the Congress governments (and Mulayam Singh Yadav in 1989-90) were not cultivating, or did not need the Muslim vote? Not having used the many counter-examples above, Wilkinson does not formulate the question this way.

Fundamentally, Wilkinson's understanding of the state is at issue here, especially the relationship between the ruling party on the one hand and the permanent bureaucracy and police in a parliamentary system on the other. The theory that riots would not take place if only the government could order the police or armed forces "to use deadly force to stop them" (p 20) betrays a monolithic and omnipotent view of the state. The state is not a monolith, nor is it omnipotent or omniscient. Ruling politicians are indeed the bosses of bureaucrats and police officials, but that does not mean that ruling parties will always get what they want.

Consider Indira Gandhi, who had a remarkable hold over state chief ministers after 1971. Can we really show that the many riots during her years, especially during 1981-84, took place because she allowed her chief ministers to order the police not to protect Muslims during riots — in the cities of Aligarh, Moradabad, Meerut, Hyderabad, Bhiwandi and Ahmedabad? In all of the states where these cities fell, the Congress Party ran governments during 1981-84.

Quite often, if not always, the relationship between ruling parties and the bureaucratic-police establishment is one consisting of serious principal-agent problems. If peasants can subvert the landlords through 'weapons of the weak' without frontally defying them [Scott 1985], police officers and bureaucrats, with much greater power than the peasants, can also subvert the ruling parties through subterfuge, dissimulation and feigned compliance, even when one can demonstrate that the ruling party would not benefit from having riots and would like to prevent or control them.

If riots took place under Nehru and Indira Gandhi's Congress party rule, despite their pro-Muslim political ideologies, why might that be so? Because some police officers and bureaucrats had very different ideological persuasions; or because officers at the top levels shared the ideological proclivities of the rulers but officers at the district level did not; or because police officers were heavily compromised in that the criminals who led the mobs in riots had developed an extensive network of relationships with them and with important local politicians; or because the police officers and bureaucrats, despite political orders and their desire to control riots, were simply unable to do so, either due to the fact that information flows on the ground were defective, or the ruling politicians were divided on what to do, or opposition politicians, especially the Hindu nationalists, were strong enough to ignite riots and had enough links in local society to incite mobs. Other than ruling parties, opposition parties in a democracy also matter, and even while not ruling, can wield a lot of power. The argument that the state can stop riots at will can only be premised upon an assumption that there are either no principal-agent problems at the level of state institutions and/or the state is all-powerful and opposition parties of no consequence. Both assumptions are flawed.

This view of the state, of course, does not mean that the state is never interested in riots. It may be, but that, as the examples above show, is not always the case. That being so, one can't build a general theory of state involvement in riots. State involvement of the kind Wilkinson talks about is linked conceptually to pogroms, not riots. Gujarat comes closest to Wilkinson's theory because it was one of the few pogroms in independent India. Riots have often taken place in India; but there have been very few pogroms.[18]

This view of the state also does not mean that one should stop critiquing the state for failing to protect the lives of its citizens. In terms of action, citizens should of course exercise pressure on the state to behave better. But in terms of analysis, one needs to draw a distinction between empirical and normative theories of the state. The state quite often does not, or is unable to, do what it should. Political scientists have long known that.

Conclusion

Methodologically speaking, Wilkinson's book is quite unusual. Both qualitative and quantitative methods have in the past been used to explain why Hindu-Muslim riots have been such a persistent feature of 20th century Indian politics. But no scholar of communal violence has thus far so heavily relied on statistical methods, and applied them with the kind of commitment Wilkinson demonstrates in this book. If the argument is still unconvincing, it is not because regression analysis itself is a fundamentally flawed mode of analysis. Rather, we may need a more imaginative mix of methods, some more interpretive than statistical, some more logical than purely empirical. If Wilkinson had supplemented his regression analysis with some in-depth empirical research into case materials, and if he had used a combination of deduction and induction, relying on logical reasoning whenever necessary, his results would have been superior. The argument falls because of inadequate logic and lack of empirical depth.

However, the implausibility of Wilkinson's argument does not detract from his tenacity of purpose, seriousness of inquiry and spirit of innovation. These are qualities one simply cannot do without in a serious scholarly enterprise. Perhaps his next work will address the puzzles, paradoxes and contradictions this book leaves unresolved, and enlighten us more about communal violence. EPW

Email: Varshney@umich.edu

Notes

[The author would like to thank Yogendra Yadav for helpful discussions on electoral issues raised in this essay.]

1 A model debates come to mind between James Scott (1976) and Samuel Popkin (1979) on the moral versus rational foundations of peasant revolutions.

2 See Varshney, 2002a, published by Yale University Press in the US and by Oxford University Press in India. A Pakistani edition of the book appeared in 2003, and a Hindi translation in India in 2005.

3 Our database was not based on government statistics, which most previous scholarship on riots had used, and we also covered a longer period than available in previous work: 46 years, 1950-95. For what procedures were used to generate a new dataset, see Varshney, 2002a, Appendix B.

4 See Varshney and Wilkinson, 1996. Some quotes from this paper are used below.

5 I should add that I only concentrate in this essay on the main argument of the book [Wilkinson 2005]. Wilkinson has chapters on testing other people's theories as well as some cross-country materials. I am not persuaded by them, especially on Malaysia where I have been doing research of late. But that is not the main issue here.

6 The formula is ENPV = $1/\Sigma\, v_i^2$, where v_i is the vote share of the ith party.

7 For each district, our riot data is split into two parts: urban and rural. For our template, see Wilkinson, 2004, p 256; and Varshney, 2002a, p 309. No further disaggregation of urban and rural is available in the template.

8 One can theoretically connect riots in towns and the degree of electoral competition, if in each of the many constituencies of the city, the election race is equally close or has an

equal number of effective parties. This assumption, if true, can allow Wilkinson to equate a lot of towns and constituencies, but still not all of them, for some will continue to be in constituencies that have significant rural citizens as well. One should, however, note that election studies in India have often shown that constituencies in the same city – whether Bombay, or Delhi, or Hyderabad – often seriously differ in how close the election races are.

9 This period is covered by Wilkinson for the country as a whole, but for UP, regressions cover a still shorter period, 1970-95 (pp 43 and 45).

10 I am only concentrating on those results reported to be statistically significant by Wilkinson – at 105, 5 per cent or 1 per cent. For one of the clearest statements on how to interpret statistical significance by one of the best practitioners of the craft, see Christopher Achen, 1982, pp 46-51.

11 At the very least, this is a paradox that calls for an explanation in a book that presents an electoral theory of riots. Sadly, Wilkinson does not provide an explanation, deductive or empirical.

12 It is not obvious what this variable really means. In presidential systems, the election dates are typically cast in stone, and politicians know for sure how far the elections are. In parliamentary systems like India's, the legislature can be dissolved by the existing governments and elections must typically be held within 10-12 weeks of dissolution, unless the Election Commission overrules the incumbents. To have riots within six months of elections seems to add a fixity to legislative elections that has not existed in India since 1962.

13 See Brass, 2003, and Varshney, 2002a, Ch 12, and Varshney, 2004, Preface.

14 See King, Keohane and Verba, 1994, as well as Ragin, 2004.

15 There were small riots in Andhra Pradesh and Maharashtra.

16 See Varshney 2004b. See also Wilkinson 2002.

17 See Varshney, 2002a, Ch 10.

18 See Varshney, 2002b.

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