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A Review Symposium

Ethnic Conflict and Civic Life: Hindus and Muslims in India

A Review Symposium

In spring 2000, the Center for Asian Studies at the University of Texas at Austin hosted a review symposium on Ashutosh Varshney's *Ethnic Conflict and Civic Life: Hindus and Muslims in India*, forthcoming from Yale University Press. During the course of his research, Professor Varshney had published several articles that attracted wide attention, and few books in comparative politics have been awaited with such anticipation. For the symposium, three scholars, David Laitin (Stanford University), Pradeep Chhibber (University of California at Berkeley), and Kanchan Chandra (MIT), examined theoretical and substantive issues in the book. Professor Varshney responded.

Scholars have either worked on civil society or on ethnic conflict, but until Varshney took up his work on Hindu–Muslim conflict in India, no systematic attempt had been made to connect the two. *Ethnic Conflict and Civic Life* posits that there is an integral link between the structure of civic life in a multi-ethnic society on the one hand and the presence of ethnic or communal violence on the other. To illustrate these links, Varshney makes two interconnected arguments.

First, a distinction must be drawn between inter-ethnic and intra-ethnic networks of civic engagement. Because they build bridges and manage tensions, inter-ethnic networks are agents of peace, but if communities are organised only on intra-ethnic lines and the interconnections with other communities are very weak (or do not exist), a multi-ethnic society can become very vulnerable to ethnic violence.

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Second, civic engagement can also be broken down into two other types: associational forms of engagement and everyday forms of engagement. Business associations, professional organisations, non-governmental organisations (NGOs), trade unions, and cadre-based political parties are some of the examples of the former. Everyday forms of engagement consist of simple, routine interactions of life, taking place typically in the neighbourhoods and unorganised work-places. Both forms of civic engagement, if they cut across ethnic lines, promote peace. Of the two, however, the associational forms turn out to be sturdier than everyday engagement, especially when confronted with attempts by politicians to polarise the people on ethnic lines. Vigorous associational life, if inter-ethnic, acts as a serious constraint on the polarising strategies of political elites.

These arguments are based on a controlled comparison of Hindu–Muslim relations in six Indian cities, three peaceful (Calicut, Lucknow and Surat) and three riot-prone (Aligarh, Hyderabad and Ahmedabad). The book also asks whether the arguments above are extendable to other social contexts and countries.

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THE THEORETICAL CONTRIBUTION OF *ETHNIC CONFLICT AND CIVIC LIFE*

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Ashutosh Varshney in his *Ethnic Conflict and Civic Life: Hindus and Muslims in India* provides cogent data that show considerable variation between rural and urban India and among India's cities in regard to the number and magnitude of violent communal (that is, Hindu vs. Muslim) confrontations. He theorises that the key factor differentiating rural society from most cities is the fact in rural society of everyday informal interactions between Hindus and Muslims. In an ingenious research design, he demonstrates that the key factor differentiating cities that experience periodic and gruesome communal violence from those that do not is the urban parallel to everyday informal interaction, namely the existence of networks of associations that include members from both religious communities. Cities that have political parties, business associations and labour unions whose membership transcends the communal divide resist violence when opportunities present themselves to transform local incidents into communal riots. Moreover, these networks in parties, business associations and unions were constructed for reasons having little to do with cauterising communal conflict. Varshney concludes from this that the networks ameliorate violence, rather than the counter-proposition that peaceful cities are good breeding grounds for such networks.

In the course of establishing through painstaking empirical research (done in collaboration with Steven Wilkinson) wide variation between rural and urban India, and between cities themselves, Varshney is able to diminish the explanatory power of four distinct traditions of inquiry, each of which would place communalism in India squarely in its explanatory domain. The very fact of variation across contexts is sufficient to discredit the essentialist tradition, whose adherents point to the existence of communal differences themselves as sufficient to serve as an account for their violent confrontations. Those instrumentalists who examine elite interests in setting one ethnic group against the other for purposes of electoral gain will also have trouble in explaining variation across similar cities in the motivation of those elites in manipulating them as they do. More important, according to Varshney, while it is plausible that elites can manipulate masses into voting for candidate 'A' because candidate 'B' is an ethnic other, it is incredible that elites can for their own electoral gain manipulate those very masses to maim ethnic others and thereby risk their own lives. The masses must have powerful reasons to help politicians in this nasty business, and these reasons are absent from elite-centred instrumentalist accounts. Constructivist theorising (and its more radical formulation in post-modernism), in positing the historical processes in which modern identities become embedded in popular consciousness, focuses on the divide and rule strategy of British colonialism to account for reified Muslim/Hindu oppositional identities. However, if the identities across cities and across urban and rural settings are equally oppositional and reified, the constructedness of these identities could not serve as an account for communal violence. Finally, institutionalist arguments, or at least those that stress electoral institutions, face similar problems, as do the other traditions of inquiry. While single-member districts with first-past-the-post rules provide very different incentives to ethnic entrepreneurs than do multiple-member districts with proportional representation, India's electoral rules are the same across all districts. With a controlled set of cases, in which cities are compared with similar demographics but different outcomes in regard to violence, electoral institutions cannot be the factor to explain variation among those cities. While essentialist, instrumentalist, constructivist and institutionalist theories may well be able to account for other sorts of variation in regard to ethnic violence, Varshney, by the very way he sets up his problem, discredits them as viable accounts for the variation across India of communal violence.

Varshney's alternative theory is that of civic engagement. All cities, he points out, are subject to exogenous shocks. In India, for example, the ugly confrontation between Hindu nationalists and the state for control over the Ayodhya mosque rippled through all Indian cities. In some cities, where there were already in place associations with Hindu and Muslim members, peace committees formed spontaneously from among the membership of these associations to cauterise the violence in their city. In other cities, where there were no such associations, peace committees had to be created by police authorities. The peace committees in the former cities were built on trust, mutual understanding, and an interest in peace. Those in the latter cities were built on mistrust, mixed motives, and sometimes an interest in using the violence for their own group's gain. Given the expected reactions by peace committees in cities that are civicly engaged, politicians in these cities are less likely to see exogenous shocks as opportunities to ally with thugs in order to foment an electorally useful riot. Thugs, without a promise of leniency (or even encouragement) by political authorities, are not likely to rampage at the expense of the ethnic other. Thus politicians in civicly engaged cities should appear more moderate, police officials more competent, and thugs less thuggish. In the three paired comparisons that constitute the empirical meat of the manuscript, a research design that brilliantly demolishes all claims that structural aspects of the city have any explanatory power or that there is some general Indian propensity toward communal violence, Varshney adds considerable credence to his theory of civic engagement.

More attractive still, the theory has observable implications that Varshney leaves to others to test. In two of his low-violence cities, the communal divide is not the pre-eminent cleavage that drives political conflict. In Calicut it is the high- vs. low-caste Hindus that divides the political space. In that space the Communist Party has been able to unite the low-caste Hindus and the Muslims (who are, in historical imagination the descendants of low-caste Hindu converts) against high-caste Hindus. In Lucknow, another of the cities with low levels of communal violence, the principal divide was sectarian within Islam, separating Shias from Sunnis. In the pre-independence period the Shias allied with upper-caste Hindus against the poorer Sunnis. Although no systematic data are presented, Varshney's narratives suggest that inter-caste violence in Calicut is low but inter-sectarian violence in Lucknow is high. Further research, as a test of the theory, should determine whether this is the case, and whether, correspondingly, there are higher levels of inter-caste civic engagement in Calicut than there is inter-sect civic engagement in Lucknow. Indeed, Varshney suggests (but provides no empirical evidence) that inter-sect civic engagement in Lucknow, at least on the mass level, is low.[1] The elaboration of such a finding would help strengthen the thesis by showing the importance of civic engagement outside one particular type – Hindu/Muslim – of cleavage. Not only does Varshney present cogent evidence on communal violence, but his theory has clear observable implications waiting to be tested in the field.

The question posed for this review is not that of empirics but rather the book's contribution to our theoretical understanding of ethnicity in general and ethnic violence in particular. Here I have three answers. First, there can be no doubt that Varshney has shown limits to the explanatory domain of several competing theories. To be sure, it will always be possible to find variation across cases for which a reigning theory will not be able to account, and therefore the competing theories that Varshney addresses are hardly disconfirmed. Nonetheless, the unaccounted-for variation across cities in India is quite significant in magnitude, and one should lose confidence in those forms of essentialism and instrumentalism that cannot account for such variation.

Second, Varshney unwittingly conflates two alternative theories, as suggested in my discussion of observable implications, without seeking to isolate their independent impact on the differential outcomes. Much of the work in explaining communal violence throughout the text is being done by the venerable theory of cross-cutting cleavages, going back to the writings of Georg Simmel. In this theory, it is suggested that the more cleavages intersect (for example, if religion and language divide a society along a different dimension), the lower will be the overall level of violence. The logic behind this claim is that a person who differs on religion but shares a language with his neighbour might find himself in political conflict with that neighbour in regard to prayers in school, but in coalition with his neighbour in regard to respect for minority languages. Your neighbour cannot be all-evil if on some issues he is on your side. Cross-cuttingness, as opposed to cumulative cleavages, thereby reduces the chances of Manichean oppositional debate.

Cleavage structure does a lot of work in *Ethnic Conflict and Civic Life*. In Calicut, Varshney reasons, the Muslim League needed a coalition partner, as it could never come to power on its own. This need for low-caste Hindu allies compelled Muslim League leaders to cool down tensions on religious issues. 'Coalition governments moderate its politics', Varshney concludes. That politically consequential cleavages (rich/poor; Muslim/Hindu) cross-cut carries explanatory weight, but that weight is not measured. Similarly in the Lucknow case, Varshney shows that the Sunni/Shia cleavage cuts into the Muslim/Hindu cleavage such that the latter cleavage does not cleanly divide all residents on all political issues. In Surat as well, a cross-cutting cleavage argument creeps in beside the civic engagement thesis. We learn

that after the attack on the Babri mosque at Ayodhya, Muslims in Surat organised a demonstration in the old city, and rumours were rife. But the civicly engaged business community, many of whom were strong communalists in their personal beliefs, formed peace committees and exposed the rumours as falsehoods. In analysing their motivations, Varshney reveals that they acted as they did because 'they were simply not ready to risk disruptions in business'. Here we see cross-cuttingness between businessmen/workers and Muslims/Hindus, and that confluence of interests rather than organisations of trust may be carrying the explanatory weight. Varshney is clearly aware of this, and in his conclusion writes that 'India's encounters with ethnic violence … and its equally frequent return from the brink … have a great deal to do with the self-regulation that its … cross-cutting civil society provides'. But cleavage structure relies on mechanisms of interest while civic engagement relies on mechanisms of trust. It is important to sort out which mechanisms carry decisive weight.

Third, Varshney's approach is basically inductive and macro in orientation. It lacks micro-foundations. Varshney connects his independent and dependent variables with *ad hoc* reasoning and a multitude of examples. But he provides no deductive framework, and gives insufficient attention to the mechanics by which civic embeddedness yields institutionalised peace systems. By juxtaposing his theory to macro versions of competing theories whose explanations are ruled out immediately, merely by presenting the cross-city data, Varshney's own approach has no competing theory. In a sense, Varshney wins the war of the paradigms before the empirical chapters begin! Active theorising ends after Chapter 2. If Varshney had a more micro-foundational perspective, his empirical chapters would have been in closer touch with a continually probing theoretical apparatus. Many of the materials useful for such theorising are available in Varshney's book, and I propose now to give an outline to a complementary micro-theory of civic engagement.

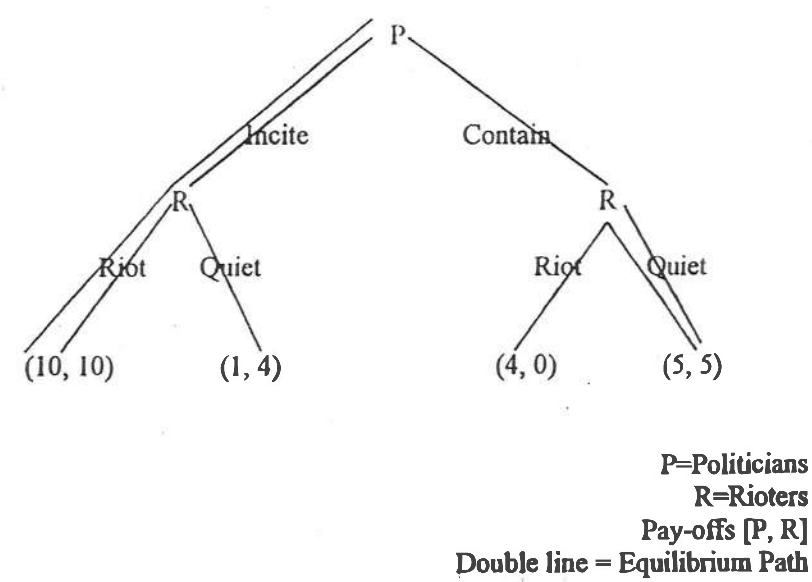
Let us begin with a counter-theory closer to the ground than the traditions of inquiry with which Varshney juxtaposes his own contribution. Consider a formalisation of an account that pervades Paul Brass's representation of a riot system in his *Theft of an Idol*.[2] His Rashomon-like stories all begin, as does Varshney's theory, with an explosive incident. He would not categorise them, as has Varshney, as an 'exogenous shock' because Brass sees the production of at least some of these incidents as endogenous to the riot system he seeks to uncover. To be sure, some incidents that Brass describes in *Theft* are local and exogenous to the macro-stories of Hindu/Muslim communal violence. An example would be the alleged abduction of a young girl from her father. In a subsequent manuscript, the driving incident is endogenous to those macro-stories. In it, Brass examines reactions to the shock of Ayodhya across North Indian cities. In all cases, however, there is some event that crystallises the Hindu/Muslim divide. Enter the riot professionals, who are a congeries of opportunistic politicians, policemen and notables, who seek to frame this incident as a religious affront and an assault upon the interests and dignity of their religious community. These riot professionals have a variety of motives for escalating the stakes in these incidents. They may see it, and this is confirmed in work done by Steven Wilkinson, as an instrument for furthering electoral advantage when a Muslim/lower caste alliance is on the horizon. By politicising an incident to illustrate the intractable Muslim/Hindu divide, and to demonstrate powerfully its violent aspect, these politicians hope to sustain lower and upper caste Hindus in an electoral bloc.[3] Whatever their motives, once young men, unemployed and without hope of social mobility, get a signal from these riot professionals that they can riot without fear of punishment, and take advantage of its spoils, they are easy to enlist. Furthermore, in giving a frame to these young thugs that their predations not only will give them chance for theft but also dignity as shock troops for the honour of their religious group, they are that much easier to recruit. Thus, the interaction of interests between riot professionals and thugs accounts for the escalation of a variety of incidents into communal violence.

This analysis can be represented in a simple two-person game as illustrated in Figure 1. Here we have two players, politicians (who stand for government officials, party leaders, and their agents, the police) and rioters (who stand for the lumpen elements in society who are indifferent between the gains to be procured through the legitimate economy and those to be gained through predation). The politicians receive an electoral pay-off of 10 if they manage to transform an exogenous shock into a Muslim/Hindu riot, and the rioters similarly gain a ten as they will get a large reward from theft and rape without paying any cost, as they will not be risking arrest. If politicians ignore the shock and rioters remain quiescent, both receive pay-offs of 5, reflecting the rewards of peaceful politics and rioters working at their normal jobs. To be sure, riots are not everyday events. Politicians need to know which exogenous shocks will provide for them scores greater than 5 – for example, those that occur before an election where their support is waning from low-caste Hindus who might be inclined to vote with Muslims. Riots are incited only when rewards for politicians are greater than 5, here represented as 10.

Co-ordination here is the key. If the rioters riot without being incited by politicians, they risk arrest and lose their salaries from their regular jobs; meanwhile the politicians pay a cost in putting down an unwanted riot. If politicians incite but rioters remain on seat, the rioters get a 4, reflecting a small loss in future co-ordinations with politicians, and politicians get a 1, reflecting a strong loss in prestige for not being in touch with the populace. With this pay-off structure, politicians (if expected returns for inciting are greater than 5) and rioters have a powerful incentive to co-ordinate on rioting, and making the most of exogenous shocks. As the game is portrayed, rioters simply wait for a signal from politicians whether to riot or not. If they follow the signal correctly, both parties win. A theft of an idol gives politicians the opportunity to alter the colour of a traffic signal, and once politicians send a green light having determined that the times are propitious, they will incite with full confidence that the armed gangs will follow.

Varshney's contribution to this model is to add a third actor: the Members of Civic Society (MCS). I am of course taking liberties here with the text. Varshney differentiates two forms of MCS: those organically linked to the populace and those who are not. This allows him (in a rather *ad hoc* way) to account for the evidence of powerful civic associations in Hyderabad yet disturbingly high levels of communal violence. But, for purposes of illustration, let us now imagine a unified MCS, and analyse the introduction of this third actor to see how it affects equilibrium predictions. This entails a reformulation of our initial game so that the MCS can choose either to contain or not contain the riot.

FIGURE 1
EXOGENOUS SHOCK



Determining the pay-offs to the players is not possible here, as, on this score, Varshney's data are radically incomplete. Sometimes the method of computing pay-offs is obscure. He tells us, for example, that people in Calicut 'agree on the futility of violence as a way to deal with differences', but this formulation does not compute the benefits of peace less the costs of cauterisation. Elsewhere he brings in factors that have implications for pay-offs that are hardly analysed at all. Consider the following analysis of Ahmedabad's riots in 1941 and 1946.

> Instead of spiraling to engulf large parts of the city, they were brought under control by the pre-existing networks. Congressmen worked hard to prevent their spread. On occasion, some Congressmen, committed to the party's ideology of Hindu-Muslim unity, went to the extent of risking their own death to stop the killers.

Twice again in this chapter, Varshney reckons that ideological beliefs of certain politicians motivated them to take great personal risks to cauterise violence. Yet ideological beliefs play no role in the theoretical model.

The pay-offs for winning elections (important for Brass and Wilkinson's theories) are similarly underplayed, yet there are subtle mentions of them. In accounting for changes in Ahmedabad in 1989–90, Varshney is shocked that Congress did not launch counter-mobilisations to those of the Bharata Janata Party (BJP) oriented toward the liberation of Rama's birthplace in Ayodhya. This counter-mobilisation 'did not happen', Varshney reasons, 'for the Congress has lost its ideological stamina and much of its organizational strength'. But the reader might wonder whether the Congress was moving towards the interests of the median voter, and this was in accord with tacit support for anti-Muslim pogroms. The electoral pay-offs to parties for inciting or cauterising violence are not well articulated in Varshney's empirics. His criticism of electoral arithmetic in his theoretical chapter suggests that his cross-city comparisons control for that factor. But they do not. If the key to incitement by politicians is the probability of a Muslim/low-caste electoral alliance, such a probability could easily vary across cities with similar demographics.

The pay-offs to other players also require reckoning. The economic returns for peace to businessmen (who are MCS) in Lucknow, where Hindu entrepreneurs hire skilled Muslim workers, and where Hindu replacements are not available, are quite high, and an investment in peace committee work would be worth the effort for them. Surely the comparative opportunity costs for business of communal violence plays some role in explaining variation in its occurrence, but this is not computed.

Even without clear directions on pay-offs for each end-point in the game, in the next section I provide two plausible interpretations of

Varshney's theory, with quite different implications for analysis, for behaviour, and for useful interventions.

Two Games of MCS Intervention

In this section, I structure a game similar to that of Figure 1, except that the third player, the MCS, chooses whether to contain the riot once an exogenous shock hits the radar screen. This game is also one of sequential moves with full information. The structure of the game is illustrated in Figure 2. Because knowledge of the pay-offs is consequential before an equilibrium prediction can be made, I can only show plausible pay-offs and their consequences for understanding the path of play. Rather than illustrating the full range of possible specifications of pay-offs, I will here vary a single pay-off to show the equilibrium consequences.

Table 1 lists the entire pay-off schedule. The basic intuition is that if they co-ordinate on a riot without any other forces providing containment then rioters and politicians get very high pay-offs (10 for each). Rioters get a 0 if they do not riot, but pay a cost of 2 if they riot and are contained by the MCS (who are likely to penalise them economically), a cost of 4 if they riot and are contained by the police (who are likely to incarcerate them) and a cost of 6 if there is joint containment. The costs for containing rioters borne by the MCS and the politicians once there is an exogenous shock are 4 if rioters actually riot, and 2 if they do not riot but vigilance is still required. The cost is halved between the politicians and MCS if both contain the rioters in joint action. MCS get a high score (5) if there is no riot (with a cost subtracted depending on whether they contributed to its cauterisation), and an equally low score (-5) if a riot takes place and there is much property damage.

It is the pay-off for the politicians that I will vary. Suppose in model 1 they receive a 0 if they incite a riot that is contained by the MCS, since they get no benefits from the riot but also pay no containment costs. In model 2 the politicians receive a –5 score, reflecting the political humiliation they suffer from inciting a riot that does not occur. This small change has important implications for strategic play.

Backward induction has (in both models) the MCS contain when R riots, but not contain when R is quiescent. If rioters know this, they will always in both models be quiescent, as there are no benefits to rioting if there are strong forces making them pay a cost for attempted predation. But here the two models diverge. In model 1, the politicians get a 0 if they incite the riot (as the MCS will pay the full cost of containment) and a –2 if they contain (carrying the burden of containment by themselves). In equilibrium, the politicians will incite a riot, and leave it to the MCS to pay the cost of containment. In model 2, the politicians get a –5 if they incite a riot that

FIGURE 2
EXOGENOUS SHOCK

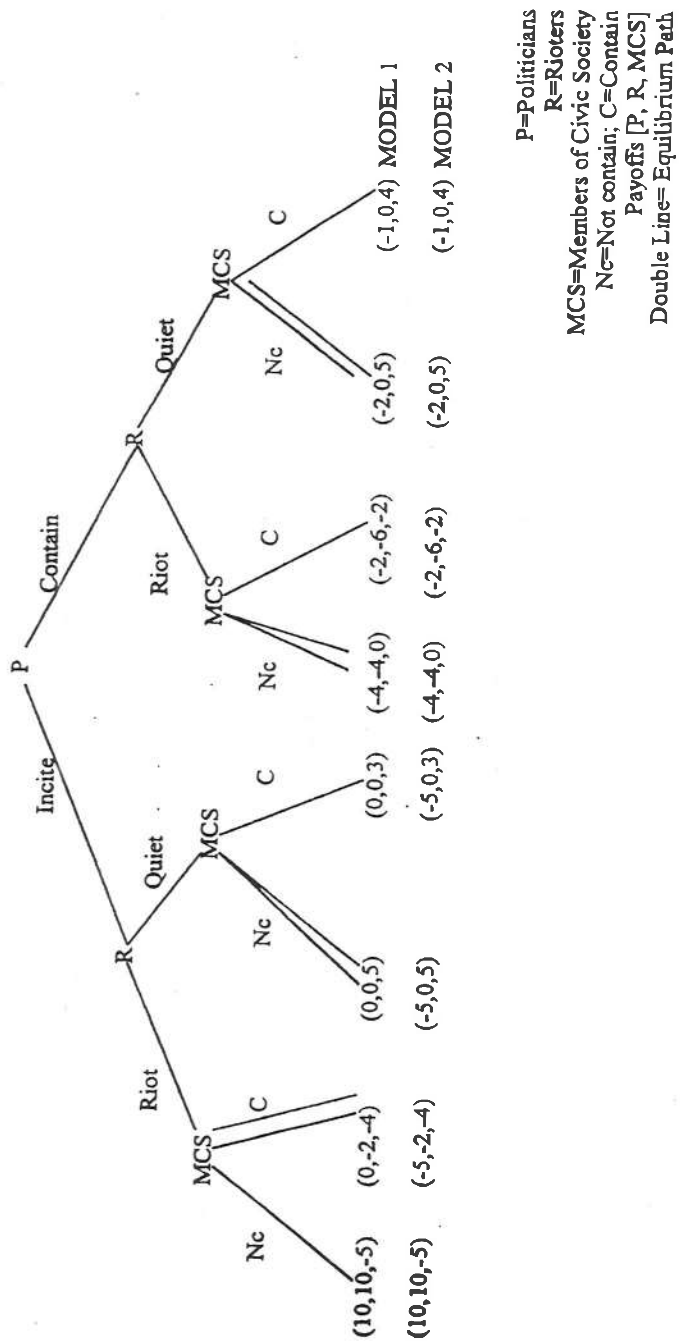


TABLE 1
PAY-OFF SCHEDULE FOR FIGURE 2

Outcome	Player	Model 1	Model 2
Riot, uncontained	P	10	10
	R	10	10
	MCS	-5	-5
Riot, contained only by P	P	-4	-4
	R	-4	-4
	MCS	0	0
Riot, contained only by MCS	P	0	-5
	R	-2	-2
	MCS	-4	-4
Riot, contained by P and MCS	P	-2	-2
	R	-6	-6
	MCS	-2	-2
No riot, no containment	P	0	-5
	R	0	0
	MCS	5	5
No riot, contained by P	P	-2	-2
	R	0	0
	MCS	5	5
No riot, contained by MCS	P	0	-5
	R	0	0
	MCS	3	3
No riot, contained by P and MCS	P	-1	-1
	R	0	0
	MCS	4	4

Note: P=politicians; R=rioters; MCS=Members of Civic Society

does not occur. This is lower than the –2 that they would get for containment, and they would therefore contain.

What is interesting about model 2 is that in equilibrium you should (hardly) ever observe any containment activities by the MCS, even if the strategic situation of the MCS is driving the quiescence. This situation is faithful to one of Varshney's formulations, as, for example, when he holds that 'Calicut's intercommunal civic depth has restrained politicians from polarizing religious communities'. This suggests that politicians' off-the-path expectations of what the MCS will do lead them to play down exogenous shocks without inciting rioters. If this is correct, then a curious gap in Varshney's narratives – the extremely rare and rather thin descriptions of the MCS actually putting down riots – begins to make sense. To be sure, there are exceptions. Chapter 10 has a gripping account of a leading Muslim businessman in Surat announcing a prize of Rs 100,000 to any one who could deliver a rumoured video of a Hindu goon-rape of Muslim women. No such video appeared, and this heroic action by a member of a peace committee was crucial in holding back the potential spiral of violence. But such accounts are rare in this empirically rich volume. For example, the actions of the peace committees in Lucknow are reported in the passive voice, so who was doing the acting remains obscure. Perhaps the reason for this is that most of the 'action' in the game consists in politicians seeking to avoid the humiliation of the off-the-equilibrium path outcome of a riot contained by the MCS. To the extent that the MCS will cauterise, at least in model 2, politicians will not incite. Therefore, in equilibrium we would (almost) never see the MCS engaged in what the theory claims they will do.

In both model 1 and model 2, we can say that with the presence of the MCS in the game there will be no riots. What can be gained from the provision of micro-foundations to Varshney's theory? For one, this exercise suggests that Varshney was not as attentive to the mechanisms driving the peaceful outcomes as would be optimal. Questions such as why anyone in the MCS would pay such a heavy personal cost for intervention when it is cheaper to free-ride on others' interventions are answered once we get more specific information about the pay-offs. Second, we get a clue as to why ethnographically based accounts, such as Brass's, may be methodologically flawed. Ethnographers not attentive to off-the-path expectations could easily overlook the cauterisation of violence induced by the MCS in model 2 because on the equilibrium path the MCS need do nothing, so there would be nothing to observe. Yet without politicians' expectations of what the MCS would do if they (the politicians) incited a riot, the politicians would have incited violence and the rioters would have rioted. When action is caused by off-the-path expectations, observations from the field that show this causal force would require innovative ethnographic techniques that are attentive to beliefs about what other actors would do under conditions that have never (or rarely) occurred. The game-theoretic formulation should therefore allow us to posit causal influences that cannot be directly observed through traditional means. If model 2 is a correct rendition for some cities, Varshney would have a powerful retort if an ethnographer concluded that Varshney's theory was wrong because in a particular city, although the MCS were present, they did nothing in response to an exogenous provocation, and no violence ensued. Third, a game-theoretic approach to the cauterisation of communal violence has policy implications. Suppose it is the case that only in cities where the costs of rioting to the MCS are extremely large would a game-theoretic rendition predict cauterisation by any party in equilibrium. In this case, the policy recommendation would be to induce business leaders to train specialised labour forces from the other side of the communal divide. In this case the cross-cuttingness would be the key to peace. Suppose now that only when the threat of MCS is highly credible will the institutionalised riot system break down. In this case, civic associations

might be more important a signal to politicians and rioters than would be the prospect of business losses. Better specification of the micro-incentives to all players after an exogenous shock would help us think through efficient strategies of intervention.

I have hardly scratched the surface in exploring the micro-foundations for civic engagement, at least for two reasons. First, much of the work in the model illustrated in Figure 2 takes place inside the circle 'MCS'. That is to say, the more the Members of Civic Society can credibly commit to collective cauterisation, the less likely politicians will be to use an exogenous shock to incite a riot. It may well be that the associational ties that Varshney emphasises increase the credibility of MCS threats. If this were the case, we would need to model a co-ordination game within the MCS itself. Such a model would compel us to observe more closely the types of assurances that are offered within those associations assuring collective action.

Second, the assumption of full information in Figure 2 may be wildly unrealistic. Once an exogenous event mobilises riot professionals, both the politicians and MCS must act quickly to respond (or ignore). Each cannot be fully sure how the other will act. Despite this lack of realism in the model, its major recommendation is that it is simple. It allowed me to trace equilibria without the use of mixed strategies. Future theorising would surely relax this assumption, better to uncover the strategic dynamic among rioters, politicians and the MCS.

Third, my model may have missed important elements in the strategic setting. For example, I have modelled politicians as if they, along with the police, were a single actor. But the forces of law and order in India are at the level of the state, not the city. They are hardly true agents of urban politicians. Under what conditions, one should ask, might state governments have a different interest concerning riots than a particular city within those states? Questions such as these cannot be addressed within the confines of the model proposed here. Also, I have modelled the game (consistent with Varshney's interpretation) as if all shocks were exogenous to the strategic interaction among the players, ignoring Brass's invitation to endogenise those shocks.

But my general points, even with these unexplored paths, should nonetheless hold. Specifying the micro-foundations for communal peace compels any researcher to specify more precisely the key actors, their information, the path of play, and the pay-offs for each actor. Also, researchers will be compelled to ask more theoretically probing questions about the cities under investigation, leading to observations that would enable us better to discriminate among a variety of mechanisms that might be driving the outcomes. Doing so would have pushed Varshney to theorise at every stage of empirical work, and not just in Chapter 2.

Varshney has taken us a long way in understanding intra-Indian variations in communal violence, and he leaves a set of unanswered questions for future research to address. What more can be asked from a work of social science?

CIVIC LIFE OR ECONOMIC INTERDEPENDENCE

by Kanchan Chandra
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Ethnic violence is fast becoming the best studied of subjects within the theoretical literature on ethnic mobilisation. Many of the theories of ethnic violence that we have so far have been developed from a small number of paradigmatic cases. Sinhala–Tamil violence in Sri Lanka, for instance, is the paradigmatic case for the theories of ethnic 'outbidding' proposed by Rabushka and Shepsle and Donald Horowitz. Serb–Croat violence in the former Yugoslavia is the paradigmatic case for Fearon's model of ethnic war as a commitment problem,[4] Posen's model of ethnic war as a security dilemma,[5] and Bates and Weingast's spatial model of the process of ethnification preceding violence.[6] Hindu–Muslim violence in India is emerging as a third such theory-producing case. Ashutosh Varshney's book *Ethnic Conflict and Civic Life: Hindus and Muslims in India* joins Paul Brass's 1998 study *Theft of an Idol*,[7] which identifies 'institutionalized riot systems' as a key variable in the production of Hindu–Muslim violence, and, more recently, Steven Wilkinson's work linking Hindu–Muslim violence in India with electoral incentives.[8] The study of the same question, using the same case materials, by a body of scholars with different points of view, different methods and research designs, and independently collected data, provides an unparallelled opportunity for theoretical advancement through the accumulation of findings. Ashutosh Varshney's book makes three important contributions to this collective body of research.

The first contribution of the book is the identification of localised variations in the pattern of ethnic violence. The book is based upon an original dataset, constructed in collaboration with Steven Wilkinson, which provides the most systematic data that we have so far on Hindu–Muslim violence in post-colonial India. Varshney shows that such violence is highly concentrated in nature: it occurs in towns rather than villages, in some towns rather than others, and in some neighbourhoods within these towns rather than others. Further, he argues that we should expect such localised variation in the incidence of ethnic violence in other countries as well, and correctly points out that much of the theoretical literature on ethnic violence