RESEARCH AS A CUMULATIVE PROCESS OF INQUIRY: THE FIELD OF ETHNIC CONFLICT

by Ashutosh Varshney
University of Notre Dame

> Let me tell you why I hate critics. Not for the normal reasons: that they are failed creators (they usually aren't; they may be failed critics, but that's another matter); that they are by nature carping, jealous and vain (they usually aren't; if any thing, they might better be accused of over-generosity.). No, the reason I hate critics – well, some of the time – is that they write sentences like this.
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> Flaubert does not build up his characters, as did Balzac, by objective external description; in fact, so careless is he of their outward appearance that on one occasion he gives Emma brown eyes (14); on another deep black eyes (15); and on another blue eyes (16).
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> Julian Barnes, *Flaubert's Parrot* (New York, 1984), 74.

Unlike Julian Barnes' narrator in *Flaubert's Parrot*, I admire critics, especially the good ones. If I were to write the above lines, I would say that I love good critics because:

1. they force elimination of phrases that are inadvertent and details that tend to cloud, rather than clarify, the argument;
2. they give me another chance to explain what I mean; and
3. most of all, they push the research programme forward by suggesting new questions for further research, by intention or as a consequence of their critique.

Indeed, should not we celebrate critics if we believe, as I do, that research is a cumulative process of inquiry – that it solves some puzzles now, leaving other puzzles for later?

It is gratifying to note that all three of my commentators agree that my book has new systematic data;[18] that it has a new argument about an important and big question; and that it makes contributions of a kind that should provoke further research. In the world of social sciences, where different approaches and arguments clash for survival, such remarks can only be acknowledged with gratitude. If I proceed further to reply to some of the more critical remarks, at times vigorously, it is only in the hope that our next research steps in the field of ethnic conflict will be more informed as a result. My book is neither a final statement on the problem of ethnic or communal violence, nor could it possibly be. I do hope, however, that it will provoke newer questions, newer avenues of research, a more

comprehensive search for data, and a spirit of critical engagement. Many more books will be needed to sort out why there is so much ethnic or communal violence in the world, and even more importantly, why different communities in so many other places, despite violence around, continue to conduct their lives reasonably peacefully.

In their critical remarks, Chandra, Chhibber and Laitin have raised three kinds of concerns: are the data mustered for some claims, if not all, accurate? Given data, are some interpretations plausible? And is an overall theory of ethnic violence or peace provided? The third issue is far and away the most serious and ambitious. Let me start with it, for, once tackled, it will set the stage for a good discussion of the first two issues as well.

Deduction and Induction, or Deductive Microfoundations?

David Laitin's critique is two-fold. The primary criticism is that my theorising 'lacks microfoundations', depending basically on induction and relying heavily on a macro-orientation. His secondary critique is that I 'unwittingly conflate two alternative theories': one based on civil society, another on cross-cutting cleavages. Chapters 10–12 of the book speak to the second point. I concentrate here on the first critique.

At the heart of Laitin's critique is the idea that theoretical work and causal explanations *require* microfoundations. Among the most creative scholars of ethnicity and ethnic conflict today, Laitin's research programme has been evolving, as all serious research programmes do. Though his earlier work was in a tradition not governed by the centrality of deductive methods, nor wedded to the idea that microfoundations are necessary for a good explanation, he has of late been moving in both directions.

I find myself in disagreement with Laitin's claim that our theories should be microfoundational, or they will be '*ad hoc*'. Consider some examples from economics, where the analytic value of microfoundations has been explored quite exhaustively. Philosophers of social science have also discussed the matter at some length,[19] but I will rely on economics for two reasons. First, it is a field I know well, having done half of my research on the political economy of development.[20] Second, economics, not philosophy, is the field which has basically inspired the attempts in political science to insist on microfoundations.[21]

To illustrate why microfoundations are not necessary to causal argumentation, let me select two kinds of problems from economics. Some questions in economics are of the following kind: why does currency devaluation sometimes lead to the opposite of what it is supposed to do – namely, redress the balance-of-payments gap? Others can take a different form: why did central planning fail, giving way to a market orientation in most countries that had centrally planned earlier?

A devaluation tends to make imports expensive and exports cheap and should, in principle, rectify a balance-of-payments crisis, which is indeed when devaluation is often recommended as a remedy. However, if the ratio of trade to GDP is high in an economy, or if some key imports (for example, food or oil) affect a lot of prices in the economy, devaluation can raise the overall price-level of the economy, making exports in turn expensive (via inflation) and nullifying the intended effect of devaluation on the balance of payments. That is why it is argued that, to avoid the effect of inflation on trade, devaluation and lowering of tariffs may have to be undertaken together. Stated differently, currency devaluation under import substituting industrialisation (ISI), which is premised upon high tariff walls, may create more problems than it can solve, if trade constitutes a high proportion of GDP, or a key commodity (such as oil, or food in developing countries) having economy-wide effects is imported in large quantities. In the causal explanation above, there is no need for microfoundations. It does not matter whether people, or firms, are self-seeking or altruistic. The macro-effect of devaluation works independently of micro-motives.

Consider now the second question: why did central planning fail? Following Janos Kornai,[22] it is now widely accepted that central planning generated incentives for firms and individuals to cheat. The success of planning depended on the central planner clearly ascertaining the needs of the various industries, matching them on the input and output sides, and allocating a country's economic resources according to a socially rational plan. However, as shortages, for a whole variety of reasons, began to appear in centrally planned economies, firm managers would hoard inputs to reach their output targets, and also give an inflated report of their future needs to the central planner. As more and more firms cheated, it became impossible to obtain good information on the needs of the various sectors. As a consequence, there would often be a mismatch between inputs and outputs in the economy, and the central planner would find it impossible to allocate resources in a way that would allow their optimal use. Strategic (micro)-behaviour on the part of firms and managers, thus, decimated central planning.

Microfoundations are critical to explaining why central planning failed, but not to why devaluation under ISI often led to inflation. For the former question, micro-motives constitute the foundation of the macro-result we observe, but for the latter, a *structural* explanation, relying on how trade/GDP ratio, or the weight of a key import in the overall price index, is enough to establish cause and effect.

For a moment, consider now the theoretical stance adopted by Amartya Sen, a seminal choice-theoretic scholar of modern times. As is well known, much of his work explores the microfoundations of economic behaviour,[23]

but he has also dealt with questions of a different kind, such as why democracies have never had famines. Sen's answer does not begin with individuals, but concentrates on how some key institutions – the press, political parties and governments – operate and how information flows in a democracy.[24] The consequence of such institutional functionings and information flows is simply that a democratic government, which allowed famines to take place, would either not survive, a result that can be foretold *ex ante*, or would have to take action before a drought or flood turned into a famine.

Such examples of a question-specific method of inquiry, as opposed to a fundamental commitment to microfoundations, are available in the discipline of political science as well. In an attempt to explain why workers in advanced capitalist countries went for social democracy rather than a socialist revolution, as Marx had expected, Adam Przeworski sought to ground his explanation in the behavioural microfoundations of workers, suggesting that in the decision-making of workers a certain benefit here and now was better than a considerably greater but uncertain benefit in the distant future, for which, moreover, heavy costs might have to be paid for an indefinite length of intervening time.[25] More recently, however, Przeworski dealt with a different question: do democracies, as the standard theory starting from Lipset in 1959 has suggested, emerge as a consequence of economic development.[26] The answer is that democracies can emerge at any level of economic development, but once democracy is established the chances of its survival are greater if the country is richer. The analysis is in an inductive mode. It deals with two aggregate categories – development and democracy – and it is not presented in terms of microfoundations. Indeed, Przeworski, one of the earliest exponents of microfoundations in comparative politics, has explicitly argued that the search for microfoundations is basically a methodological stance, and he cannot give it primacy in an *a priori* fashion.[27]

Thus, whether microfoundations are necessary to a causal, or theoretical, explanation depends on the question we ask. A search for microfoundations is a methodological stance; it is not necessarily a theory. A theory establishes a cause-and-effect relationship between two sets of substantive phenomena. If microfoundations are necessary for a causal explanation, we need them in our theory; if not, we don't. The appropriateness of method depends on the nature of the problem.

Civic Engagement and Pluralist Human Motivations

In light of the methodological considerations above, what can we say about whether the basic question asked in *Ethnic Conflict and Civic Life* requires provision of microfoundations? Two interconnected responses can be made.

I will first argue that, given what we observe on the ground, identification of microfoundations does not appear to be necessary; and second that Laitin's attempt at providing a game-theoretic model, while intellectually formidable, leaves much to be desired.

I do not, I might add, rule out the possibility that as Laitin's remarkable research programme proceeds further it may well be possible to come up with a better microfoundational model of ethnic violence, but we do not have it yet and its evolution and delineation, if possible at all, would have to fight against some serious intellectual odds listed below. In and of itself, the idea of microfoundations is not problematic. What is implausible is the belief that microfoundations for ethnic conflict and peace will either be necessarily found, or have been found, or the suggestion that all other theoretical stances, especially those relying heavily on empirical work and induction, are '*ad hoc*', as Laitin puts it. As I have argued elsewhere, good research will continue to require a combination of deductive and inductive methods; neither by itself will do.[28]

The question my book asks is: why are there such remarkable local variations in Hindu–Muslim violence? The answer it gives is that the variations in local civic life explain why some cities have Hindu–Muslim riots, and others do not. The key link is that people engaged with each other across ethnic or religious boundaries, especially in associations, define the local environment for politicians, and that environment constrains politicians and their henchmen, if they wish to use communal violence for electoral advantage. Riots and inter-ethnic associational engagement are strongly negatively correlated.

Laitin calls this explanation macro. I do not think the term macro can be used for the kind of explanation offered in the book. In macro-economics, macro factors – exchange rates, budget deficits, inflation, terms of trade and so on – tend to have economy-wide effects. My explanation is based on civic engagement, which is very local in character, not system-wide. Indeed, one of the central claims of the book is that there is something irreducibly local about civil society and its character and texture tend to vary locally. Civil society, in other words, is not a variable like exchange rates, which, once changed, affect the entire traded sector of a country's economy. It would be more precise to say that my explanation is pitched at the intermediate level. It is neither macro nor micro.

Why did I not reduce the explanation further to a parsimonious microfoundation – to individual motivations, either of a altruistic or a self-interest kind? Empirically, I found a whole variety of motivations at work in the field. Some associations or organisations that resist attempts at communal violence do so for fear their members would be hurt by a communal split; others do so because they are ideologically committed to

class-based, caste-based or interreligious politics and opposed to politics that seeks to divide Hindus and Muslims. My empirical research suggests that several motivations – self interest, collective interest, long-run ideological visions, norms – are involved in the functioning of associations. Business associations may be driven by the self-interest of businessmen who constitute them, but social and educational institutions – the NGOs, the schools, 'homespun' (*khadi*) organisations, reading libraries, prohibition societies, linguistic societies promoting the use of vernaculars – as well as some political parties and trade unions, if not all, can often be driven by ideals and norms.

Let me give a concrete example. When Indians joined the national movement against British rule in the 1920s and responded to Gandhi's call for mass mobilisation, a transformative moment, identified in my book as inaugurating India's associational order, emerged in the nation's public space. Joining the movement in the 1920s, as opposed to the late 1940s, entailed serious risks, including incarceration, injury, even death, and not simply opportunity costs measured in terms of the value of the time lost in organising and mobilising. Moreover, the rewards were far off in the future. *Ex ante*, no one knew how long the battle for freedom was going to be, or whether there would be a chance at all of enjoying power. Thus, while Indians who joined the national movement in the 1940s could be said to be pragmatic and self-interested, for it was sensible to be on the side of the likely victors, those whose involvement began in the 1920s and early 1930s took huge risks at the level of individual and family welfare.

To illustrate the enormity of risks, let us briefly discuss India's civil disobedience movement, especially by the famous Salt March in 1930. British rulers had the monopoly over the making and selling of salt. Trying to use a symbol which even the poorest and illiterate masses would understand, Mahatma Gandhi argued that the British insulted Indians by not allowing them to make something as basic as salt in their own country. He then led a non-violent mobilisation, broke the law, and was later arrested. The protest continued. The pre-agreed form of protest *inter alia* required (i) making salt in British factories near the sea, and (ii) not resorting to retaliatory violence even if the British used force. An American journalist gave the following eye-witness account of the early phase of the movement:

> The salt deposits were surrounded by ditches filled with water and guarded by four hundred native ... police in khaki shorts and brown turbans. Half a dozen British officials commanded them. The police carried ... five-foot clubs tipped with steel. Inside the stockade twenty five native riflemen were drawn up.

> In complete silence, the Gandhi men drew up and halted a hundred yards from the stockade. A picked column advanced from the crowd, waded the ditches, and approached the barbed-wire stockade. ... Police officials ordered the marchers to disperse. ... The column silently ignored the warning and slowly walked forward. ...
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> Suddenly, at a word of command, scores of ... police rushed upon the advancing marchers and rained blows on their heads with their steel-shod clubs (*lathis*). Not one of the marchers even raised an arm to fend off the blows. They went down like tenpins. From where I stood I heard the sickening whacks of the clubs on unprotected skulls.
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> ... In two or three minutes the ground was quilted with bodies. ... The survivors without breaking ranks silently and doggedly marched on until struck down. ... Then another column formed while the leaders pleaded with them to retain self-control. They marched slowly toward the police. Although every one knew that within a few minutes he would be beaten down, perhaps killed, I could detect no signs of wavering or fear. ... The marchers simply walked forward until struck down. There were no outcries, only groans after they fell.
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> ... I went back to the temporary hospital to examine the wounded. They lay in rows on the bare ground in the shade of an open, palm-thatched shed. I counted 320 injured, many still insensible with fractured skulls, others writhing in agony.[29]

The description above suggests a high degree of commitment to a larger cause and a sense of dignity in individual and collective conduct, rather than self-interest.

Consider now the analytic implications of a pluralism of motivations. Even though in my empirical account, says Laitin, 'ideological beliefs of certain politicians motivated them to take great personal risks to cauterize violence', 'ideological beliefs', he notes, 'play no role in the theoretical model'. Indeed, they do not, for my explanatory strategy does not rely on the omnipresence of self-interest. I find evidence of many motivations in communal and anti-communal politics – self interest, commitment to group interest, ideology, social visions, emotions – and I do not see the need to focus unremittingly on self-interest, only to be surprised by the presence of deep ideology in many political actors. What is important for my explanation is not that any one of these motivations predominates decision-making, but that given the presence of intercommunal associational ties, a variety of motivations can lead to interpersonal trust among members of the association; and associational resistance to the possibility of communal

violence can result from such trust.[30] Interpersonal trust can emerge for all sorts of reasons: self-interest connecting people in repeated interactions, a shared ideology, a shared commitment to a group or nation, a shared interest in films, sports and profession.

If I do not find evidence of rampant self-seeking but of a pluralism of motivations, I am not required to solve problems such as follows: 'why anyone ... would pay ... a heavy personal cost for intervention when it is cheaper to free ride on others' interventions'. This problem is a methodological artefact; it is not a puzzle to explore in an analytical framework not driven by a self-interest assumption. It would suffice to say that some people find free-riding immoral, and even those who do not have moral objections to free-riding may not actually free-ride, for, as is well-known even within rational choice theory, they may suffer from 'weakness of will'; and, moreover, if getting caught, or escaping undetected, is radically uncertain, they may not be sure whether it is rational or irrational to free-ride.[31] In the non-philosophical literature on rationality, too much has been presumptively made of the rationality of free-riding, despite evidence that even when we leave aside moral issues, which may bother those who believe in acting out of commitment,[32] it may not make sense to free-ride even on rational grounds.

Lest I should be misunderstood, let me make it clear that I am not making an argument against parsimony. As a matter of fact, I do have an analytic bias in favour of parsimony. All I wish to state is that parsimony depends solely neither on demonstrating microfoundations, nor on showing that only self-interest should be seen as constituting the sole microfoundation of human action. Just as a parsimonious explanation of the relationship between devaluation and inflation, or the relationship between democracy and economic development, does not depend on establishing microfoundations, the relationship between civic engagement and ethnic peace can also be explained parsimoniously by simply explaining how the presence of intercommunal associations has a likely effect, irrespective of whether self-interest or ideological belief sustains these associations or organisations. The trust that forms in associational life – due to self-interested, group-motivated or ideological interaction – allows people to come together in times of tension to protect their many neighborhoods, or resist the depredations of those political entrepreneurs who would benefit from communal riots. The very fact that vibrant civic associations locally exist make an effect likely, if not entirely certain. *Demonstrating that effect parsimoniously does not depend on one kind of motivation, but on stating the link clearly and economically without giving up the pluralism empirically discovered.*

Do Aggregate Entities Have Preferences?

Let us now look at the model Laitin proposes by adding microfoundations to my explanation about civic engagement. He constructs two three-person games. The three players are politicians, rioters and the members of civil society (MCS). He does admit that his assumption of perfect information may be 'wildly unrealistic'. But that is not the only problem. Consider two other serious issues.

First, the term politicians, one of the designated players, stands for 'government officials, party leaders and their agents, police'. Second, civic organisations have been represented by a unitary actor as well, the so-called MCS.

A serious problem undermines the potential value of such simplification (not simplification *per se*, which may be necessary for parsimony). How can politicians and MCS *as aggregates* be said to have preferences? In rational choice theory, to which Laitin subscribes, only individuals have preferences, not aggregate entities. One of its basic premises is that there is a fundamental distinction between individual and aggregate group interest, hence a collective action problems exists. Beyond individuals, only firms or corporations are supposed to act like individual entities, for under the rigorous constraints of a market they would be left behind if they did not pay attention to the bottom line.

That is why in microeconomics, both firms and individuals can equally legitimately be units of analysis. But the same assumption is not made with respect to other aggregate entities. Rather, while examining the behaviour of larger entities like industries or sectors as a whole, it is shown how individual, or firm, preferences might aggregate. In a similar vein, Elster has recently argued that 'to be taken seriously, any account that imputes goal-oriented behavior to aggregate entities has to come up with some reason why we should expect consistency in their behavior'.[33] Are politicians like firms or like industries? Is MCS an individual or a collective entity? If it is the latter in each case, we need a statement about aggregation of the preferences of the politicians and MCSs. Theoretically, it will not do to give all politicians the same preferences.

Empirically, too, it is clear to me that we have different kinds of politicians. In India, some like the Hindu nationalists are – often – committed to Hindu–Muslim polarisation; others like the caste-based or Marxist politicians are entirely opposed to it. One can make the classification a little more complex, but this elementary contrast itself throws doubt on the validity of constructing one of the players the way Laitin has. And if one can hardly justify putting such contrasting political preferences in one entity called politicians, how does one legitimately add

'government officials' (meaning presumably bureaucrats) and 'police' also in this category? How does such parsimony help us? The inevitable key question is: are there game-theoretic models that can work with pluralism of motivations?[34] If there are, we will be greatly enlightened by game theory.

In the field of ethnicity, there are few like Laitin who are by now systematically pinning faith in, and working relentlessly with, game-theoretic models. Some of the breakthroughs are indeed likely to be of seminal value. For example, the idea of 'self policing' that Laitin and Fearon have game-theoretically proposed as an agent of ethnic peace is likely to be precious in further research.[35] Laitin should see the problems posed above as challenges. Perhaps in future he can come up with models that are parsimonious in a way that can address the central problems of these models: namely, accommodating a pluralism of motivations, especially the non-rational ones.

Economic Interdependence versus Civic Associations

Chandra suggests that my argument supports a narrower interpretation of what generates peace. It is economic interdependence between Hindus and Muslims, not civic engagement, that forces Hindu and Muslim politicians to moderate their strategies, and where such economic interdependence is missing they choose extremist strategies for electoral advantage, which in turn leads to violence.

This is a selective retrieval of the argument, and it needs to be corrected. Take the three peaceful cities. Two of them – Lucknow and Surat – are indeed marked by economic interdependence. In Lucknow, Hindu entrepreneurs and Muslim workers have developed a vertical, though a symbiotic, relationship in a key sector of the local economy, and in Surat, Hindu and Muslim businessmen are horizontally linked.

However, the third city and two more cases discussed in the book – Ahmedabad, which was peaceful until 1969 (and became riot-prone after that); and the town of Bhiwandi, discussed in the concluding chapter – give an account of civic linkages that are not primarily economic. Calicut is a city of 'joiners', and organisations ranging from business groups to reading clubs, from political parties to trade unions, from popular theatre to popular science movements bring Hindus and Muslims together. And as discussed in the final chapter, in Bhiwandi, a previously riot-prone town, riots were prevented between 1989 and 1993, the worst period of Hindu–Muslim tensions since India's partition, by neighbourhood-level social, not economic, associations, painstakingly created by the police chief of the city.

But it is the city of Ahmedabad which should be by far the most troubling for Chandra's retrieval of economic linkages as primary. Ahmedabad's Muslims have not been businessmen on the whole. Like so

many Northern cities and unlike the city of Surat, which has traditionally had Muslim business classes, Ahmedabad's Muslims were either court-based aristocrats, or poor weavers and workers. In the famous Ahmedabad Millowners Association, there was only one Muslim businessman – and that too for a limited period in the first two decades of the twentieth century.

Rather than business integration, it is India's most famous and a legendary trade union, a whole host of Gandhian social institutions, such as homespun societies, and educational institutions, such as schools and colleges, and the widely recognised organisational strength of Congress Party that brought the communities together. Ahmedabad was the home of India's biggest political figure of the century, Mahatma Gandhi. It came closest in the first half of this century to the civic vision of Gandhi. But even Gandhi could not invent economic interdependence if there were no Muslim industrialists and all millowners were Hindu. An all-Hindu industrialist association contributed to peace not because it had Muslim representation but because it was driven by Gandhian ideology and gave freely to educational and social institutions set up by Gandhi. These institutions contributed to the Hindu–Muslim civic integration of the city; the integration in the economic sphere was largely absent.

Chandra's economic reconstruction of my argument, however, does raise a serious issue for future research. Is it the case that economic networks last longer than other civic networks? The trade union, Congress Party and social and educational institutions, after all, did decline in Ahmedabad in the 1950s and 1960s, and when the 1969 shock came, the city degenerated into a horrific blood-bath, a pattern repeated since then. On the other hand, despite the decline of all of these institutions, Surat's business networks were able to keep the old city quiet, even when the shanty towns burned in 1992–93. Similarly, Lucknow's symbiotic industrial structure has expanded in the last few decades.

Is it that political parties, unions and other institutions can be associated with the origins of civic linkages, but are unable to sustain them without economic networks? Testing this hypothesis will require a very different research design than the one employed in the book, but it is possible to make a distinction between the origins and sustenance of civic networks, and also to come to the conclusion, if the empirical evidence so demands, that all kinds of networks can bring communities into being, but the economic ones last the longest. This hypothesis is not what my book sought to test, so its research design does not permit any firm conclusions, but future research may well do so. Indeed, once we have established the value of civil society in general, it would be natural to disaggregate it and seek finer causal relationships.

Did the Congress Party Bring Communities Together?

Chandra suggests that the Congress Party was primarily a 'Hindu organization indistinguishable in its composition from the Hindu nationalist BJP'. Chhibber has doubts about the vibrancy of the Congress Party, even between the 1930s and 1950s, but he does concede that it quite possibly allowed Hindu and Muslims elites, if not the masses, to come together in several parts of India.

So how does one judge the impact of the Congress Party on Hindu–Muslim relations? How important was its role in integrating Hindus and Muslims in the past, if not today? The controversy over this issue cannot be settled on the basis of numbers alone. To my knowledge, at no point, now or in the 1920s, when Congress Party first took to mass politics and started developing organisational units all over the country, were reliable national or aggregate membership statistics put together by any scholar or party organisation. Nor is it clear that local data about membership are precise, or available in a usable form. Chhibber's belief in the document that puts the Congress Party membership in 1958 at 58,000 may be as far off the mark as any other attempt at aggregate precision.

Basically, given how membership records were kept in the Congress Party, it is implausible to claim statistical exactitude on the size of membership, or its composition. However, empirical reasonableness with respect to many cities and regions is certainly possible, and it may be enough for sound reasoning. In the 1970s and 1980s, the Nehru Memorial Museum collected oral histories about the Congress Party, based on detailed testimony given by leaders who were involved in the freedom movement in the 1930s and 1940s, some going along with the Congress Party and some in opposition to it but fighting nonetheless for India's freedom. In addition, there are also detailed accounts by scholars for the 1950s and 1960s. These materials can form the basis for sound locally based reasoning, but not for arguments about aggregate data.

Chandra cites Paul Brass against my claims to say that the Congress Party was like the Hindu nationalist BJP in the state of Uttar Pradesh (UP). Whether or not that has happened in recent years, it is a massive overstatement for the entire historical period since the rise of the Congress as a mass organisation. Brass studied only five districts in UP, and those districts were not selected according to a methodological principle that would permit state-level generalisations. There are two UP cities, the peaceful Lucknow and the riot-prone Aligarh, among my six case studies. Of the two, Brass studied the Congress Party in only Aligarh, and our conclusions are also similar. Both of us argue that the Congress Party was unable to bring Hindus and Muslims together in Aligarh. For Lucknow,

however, one cannot rely on Brass, for he did not study the party in that city or district. Hard or reliable data in a time-series on party membership are not available for Lucknow, but my detailed interviews with the local political actors of the time – of all shades and opinions – lead me to conclude that the Congress Party in Lucknow enjoyed substantial Hindu as well as Muslim support – in the decades before and after independence. Part of the reason simply was that Shia–Sunni divisions made intra-Muslim cleavages much more central to Lucknow's city politics than a Hindu–Muslim cleavage. Due to an acute Shia–Sunni contestation, Hindu–Muslim integration was objectively easier in Lucknow than in many other cities.

Similarly, studies by Weiner, several oral histories available at the Nehru Memorial Library, and other materials suggest that Gujarat was, *beyond doubt*, the centre of organisational strength for the Congress Party, especially, though not only, because two of the greatest Congress leaders of this century, Gandhi and Patel, hailed from the state and were actively involved in party-building there. By the 1960s, however, it is also clear on the basis of available materials that organisational decay had set in, making the Congress Party no longer as capable of mass and citizen action as it was before independence.

Thus, even though precise data are not available, city-level research would suggest that instead of generalising about the Congress Party at the national level and arguing that its integrative project was an empty slogan, it is best to view it as a party that had local variations in its organisational vibrancy and integrative strength. Once we do so, we can also see why its capacity and/or willingness to bring Hindus and Muslims together differed in different parts of the country. Whatever one may say of the Congress Party on other questions, country-level and, in some cases, even state-level generalisations may be hard to justify on its organisational and ideological strength.

Concluding Remarks

While I am unable to agree with some of the criticisms of commentators, I hope to have demonstrated that some other arguments made by them are intellectually highly valuable and will undoubtedly take the agenda of future research forward. Especially productive would be two avenues of inquiry. First, can game-theoretic models in future accommodate a plurality of human motivations? Second, is it the case that while all kinds of associations and organisations may be able to integrate different ethnic or religious groups, only the economic organisations can sustain integration over a long period? As research on ethnic conflict proceeds further, answers to these questions will allow us to fill some important gaps in our collective knowledge.

NOTES

1. Historical evidence supports Varshney's conjecture. See, e.g., T. Wright, 'The Politics of Muslim Sectarian Conflict in India', *Journal of South Asian and Middle Eastern Studies* (Spring 1980), 67–73.
2. P. Brass, *Theft of an Idol* (Princeton: Princeton University Press, 1997).
3. S. Wilkinson, 'The Electoral Incentives for Ethnic Violence: Hindu–Muslim Riots in India' (Paper prepared for delivery at the Annual Meeting of the American Political Science Association, Boston, MA, 1998).
4. J. Fearon, 'Commitment Problems and the Spread of Ethnic Conflict', in D. Lake and D. Rothchild (eds.), *The International Spread of Ethnic Conflict* (Princeton: Princeton University Press, 1998).
5. B. Posen, 'The Security Dilemma and Ethnic Conflict', in Michael E. Brown (ed.), *Ethnic Conflict and International Security* (Princeton: Princeton University Press, 1993).
6. R. Bates, R. Figueiredo Jr. and B. Weingast. 'The Politics of Interpretation: Rationality, Culture and Transition', *Politics and Society*, 26/4 (1998), 603–42.
7. Brass, *Theft of an Idol*, 284.
8. Wilkinson, 'The Electoral Incentives'.
9. Brass himself would object to this portrayal of his argument in the vocabulary of traditional social science. His intention in *Theft of an Idol* is not to construct an objective theory explaining ethnic violence, but to demonstrate that such a theory cannot be constructed. However, despite this intention, his argument looks suspiciously like traditional social science.
10. See, for instance, P. Brass, *Factional Politics in an Indian State* (Berkeley: University of California Press, 1965); B. Graham, *Hindu Nationalism and Indian Politics* (Cambridge: Cambridge University Press, 1990).
11. M. Weiner, *Party Building in a New Nation: The Indian National Congress* (Chicago: University of Chicago Press, 1967), 108.
12. Calculated from Table 11 in ibid., 120.
13. Social and educational institutions are also said to play a role but neither their form nor their actual role is as clearly delineated as that of the Congress Party, business associations, and trade unions. I will focus most of my remarks on the role played by political parties and business associations in fostering communal peace.
14. In this review I shall attempt to place the argument offered in *Ethnic Conflict and Civic Life* within the sets of questions outlined above. Setting up the questions in such a manner is merely a heuristic device and the distinctions imposed are often not so watertight. Consequently, and solely in the spirit of raising some issues for further discussion, this review takes some liberties with the text.
15. Distinguishing thin and thick notions of trust is important because there is some debate over the role played by business associations. R. Hunt, 'Business Associations and the Small Manufacturing Sector', *International Journal of Comparative Sociology*, 21/2 (1980), 254–67, notes that small business associations were controlled by small cliques that and members of these associations did not identify with them. M. Namjoshi and B. Sabade, *Chambers of Commerce in India* (Bombay: Asia Publishing House, 1967), make a similar argument about Chambers of Commerce.
16. This emphasis on the Congress Party is appropriate because the party is presented by Varshney as a key formulator of civicness. The Congress Party's organisational decay is used to explain the transition from communal peace to communal violence (a transition that Varshney suggests is vital to 'proving' his theory).
17. That the Congress was a patronage machine is widely accepted (Brass, *Factional Politics*; Weiner, *Party Building*; J. Manor, 'Karnataka: Caste, Class, Dominance and Politics in a Cohesive Society', in F. Frankel and M. Rao (eds.), *Dominance and State Power in Modern India*, Vol.1 (Delhi: Oxford University Press, 1981); A. Kohli, *Democracy and Discontent* (Cambridge: Cambridge University Press, 1990); S. Mitra, *Power, Protest and Participation* (London: Routledge, 1992).
18. Collected in collaboration with Steven Wilkinson, formerly my junior colleague at the

Harvard Academy of Area and International Studies, and currently teaching at Duke University.

19. On microfoundations, Jon Elster's work is quite seminal. For a recent statement of his view, see Elster, 'Rational Choice History: A Case of Excessive Ambition', *American Political Science Review*, 94/3 (2000), 685–95.
20. I have published one authored and two edited volumes in that field. The authored book is *Democracy, Development and the Countryside* (Cambridge: Cambridge University Press, 1995 and 1998). The edited volumes are *Beyond Urban Bias* (London: Frank Cass, 1993), and *India in the Era of Economic Reforms* (New Delhi: Oxford University Press, 1999 and 2000), co-edited with Jeffrey Sachs.
21. W. Riker, in J. Alt and K. Shepsle (eds.), *Perspectives on Political Economy* (Cambridge: Cambridge University Press, 1991)
22. J. Kornai, *Contradictions and Dilemmas* (Cambridge, MA: MIT Press, 1985).
23. A. Sen, *Choice, Welfare and Measurement* (Cambridge, MA: MIT Press, 1984).
24. J. Dreze and A. Sen, *Hunger and Public Action* (Oxford: Oxford University Press, 1989), esp. chapters 5–8 and 11.
25. A. Przeworski, *Capitalism and Social Democracy* (Cambridge: Cambridge University Press, 1984).
26. A. Przeworski and F. Limongi, 'Modernization: Theories and Facts', *World Politics*, 49/2 (1997), 155–75.
27. A. Przeworski, 'The Role of Theory in Comparative Politics: A Symposium', *World Politics*, 48/1 (1995), 16–21.
28. For a concrete application of this methodological strategy – combining deduction and induction – see Varshney, *Democracy, Development and the Countryside*, chapter 5, especially pp.120–38.
29. W. Miller, *I Found No Peace* (New York: Simon and Schuster, 1936), sections reproduced in H. Jack (ed.), *The Gandhi Reader* (New York: Grove Press, 1994), 250–53.
30. Chhibber is right to point out that the trust so formed – thick or thin – should be studied in the next round of research. My hunch at this point is that it is more akin to a Granovetter-like thin trust.
31. J. Elster, *The Cement of Society* (Cambridge: Cambridge University Press, 1989).
32. A. Sen, 'Rational Fools', in *Choice Welfare and Measurement* (Cambridge, MA: MIT Press, 1984).
33. J. Elster, 'Rational Choice History: A Case of Excessive Ambition', *American Political Science Review*, 49/1 (2000), 693.
34. Milton Friedman, of course, famously argued that so long as the prediction come out right, realism of assumptions in a model does not matter. But Laitin should explicitly state that, if he agrees that realism of assumptions is irrelevant in modeling. If so, he will, among other things, have a whole host of theoretical literature in economics to contend with. I do not construe him to be taking that position.
35. J. Fearon and D. Laitin, 'Explaining Interethnic Cooperation', *American Political Science Review*, 90/4 (1996), 715–35.