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IMPLICATIONS OF AN ECONOMIC THEORY OF CONFLICT: HINDU-MUSLIM VIOLENCE IN INDIA

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IMPLICATIONS OF AN ECONOMIC THEORY OF CONFLICT:

Hindu-Muslim Violence in India

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ABSTRACT

We study inter-group conflict driven by economic changes within groups. We show that if group incomes are "low", increasing group incomes raises violence *against* that group, and lowers violence generated *by* it. These predicted relationships demonstrate the complex connections between economic growth and violence, and in particular serve as tests for group aggression or victimization, which we apply to Hindu-Muslim violence in India. Our main result is that an increase in per-capita Muslim expenditures generates a large and significant increase in future religious conflict. An increase in Hindu expenditures has negative or no effect. This robust empirical finding, combined with the theory, has direct implications for the origins of Hindu-Muslim violence in post-Independence India.

Journal of Economic Literature Classification Numbers: C33, D31, D74.

Keywords: Conflict, income distribution, religious violence, uneven growth.

1. INTRODUCTION

We study Hindu-Muslim conflict in post-Independence India through the lens of economics. We allow for two equivalent (though conceptually different) channels that link economics to conflict. Under the first, Hindu-Muslim violence is the systematic use of a particular marker (religion, in this case) for appropriating economic surplus, either directly through resource-grabbing or looting, or indirectly through exclusion from jobs, businesses or property. Under the second, existing inter-group hatreds are re-ignited or exacerbated by economic progress within one of the groups. Both approaches have the same formal representation, which makes robust predictions regarding the effect of group incomes on inter-group violence. We examine these predictions empirically.

The recurrent episodes of Hindu-Muslim conflict in India (going back to Partition and earlier) form the motivation for this paper. Even if we exclude the enormity of human losses from religious violence during Partition, such conflict has continued through the second half of the twentieth century, accounting for over 7,000 deaths over 1950–2000. There is reason to believe that the situation may not have changed much since: witness, for instance, the rampant Hindu-Muslim violence unleashed in the Indian state of Gujarat in 2002. It may be argued that these numbers

[1]Mitra: University of Oslo; Ray: New York University. Ray is grateful for funding from the National Science Foundation (SES-0962124), the Fulbright Foundation and for hospitality from the Indian Statistical Institute during a year of leave from NYU. Thanks to Abhijit Banerjee, Sam Bowles, Sugato Dasgupta, Oeindrila Dube, Joan Esteban, Mukesh Eswaran, Raji Jayaraman, David Ludden, Michael Manove, Kalle Moene, Andrew Oswald and Rohini Pande for useful discussions, and to Steve Wilkinson for granting us access to a dataset on religious conflict. We also thank Jay Dev Dubey for his able research assistance.

are small relative to the overall population of India. From a pure arithmetical perspective they are, but they do not capture the less measurable consequences of conflict: displacement, insecurity, segregation, loss of livelihood, widespread fear and the sapping of the morale of an entire society.

Like the many episodes of ethnic violence that have occurred all around the world, it is *prima facie* reasonable that there is an economic component to Hindu-Muslim conflict. There is, of course, no getting away from the facts of sheer hatred and mistrust, or what one might call the "primordialist explanations" for ethnic violence. Nor does one necessarily *need* to get away from primordialism, provided that we entertain the possibility that the economic progress of one's enemies may heighten the resentment and spite that one feels. But equally, there could be the systematic use of violence for economic gain, for the control — via appropriation or systematic exclusion — of property, occupations, business activity and resources (see, e.g., Das (1992), André and Platteau (1998), Collier and Hoeffler (1998, 2004), Dube and Vargas (2013), Field, Levinson, Pande and Visaria (2008), Iyer and Do (2009) and the recent survey by Blattman and Miguel (2010)). This economic perspective is no contradiction to the use of noneconomic markers (such as religion) in conflict.[2]

In this paper, we take the economic approach to conflict seriously, and apply it to Hindu-Muslim conflict. We construct a simple theory that allows us to link observable economic variables to conflict outcomes. But our goal isn't just to establish a link. We use the theory to interpret the empirical findings that we subsequently obtain. In the model, there are two groups: Hindus and Muslims. Depending on the circumstances, members of either group can be aggressors or victims in an inter-religious conflictual encounter. We view such violence as decentralized, though it may place against a backdrop of religious antagonism and orchestrated support from group leaders.

Consider "encounters" across members of different religious groups: an accident, an assault or confrontation, an isolated murder or rape. When religion is involved, if only by chance, such encounters could boil over into a larger conflict or riot. A potential aggressor involved in the confrontation must decide whether to take advantage of the situation and frame it as a religious conflict, in which members of the other religion can be targeted. The act itself may be motivated by the prospect of economic gain (via direct appropriation or economic exclusion of the victim) or it may be the expression of animosity and resentment, as long as that resentment is sensitive to the economic situations of aggressor and victim.

At the same time, a potential victim can try to defend himself. We consider two technologies of protection. One is "human": the recruitment of community members to safeguard against the possibility of attack. The other is "physical": the use of barricades and gated communities, or the acquisition of weapons. We allow for both avenues, but recognize that their relative use will depend on the economic status of the potential victim.

Our main result (Proposition 1) states that if a group is relatively poor to begin with, *an increase in the average incomes of the group — controlling for changes in inequality — must raise violence perpetrated against that group*. In contrast, the effect on violence perpetrated *by* that group on members of the other group is generally negative. This assertion — that a positive correlation between group incomes and subsequent violence is an indicator of victimization of the group, while

[2]Indeed, as Esteban and Ray (2008) and Ray (2009) have argued, there may be good economic reasons for conflict to be salient along noneconomic ("ethnic") lines, rather than along the classical lines of class conflict long emphasized by Marxist scholars.

a corresponding negative correlation is indicative of group aggression — informs our empirical exercise. More generally, these nuanced connections between economic growth and conflict suggest that the overall relationship between economic development and violence is a complex one, even more so if that development is essentially uneven, a theme pursued in Ray (2010).[3]

We use a unique dataset on Hindu-Muslim violence between 1950 and 1995, compiled by Ashutosh Varshney and Steve Wilkinson, and extended by us to 2000. It summarizes reports from *The Times of India* on Hindu-Muslim conflicts in India in the second half of the twentieth century. We use different count data from the dataset: such as the number of people killed, or injured, or the number of riot outbreaks.

We match the data to the large scale household surveys that are conducted quinquennially as part of the National Sample Surveys (NSS). Because we seek spatially disaggregated economic information by religion, the earliest large-sample or "thick" round we can use is the 38th (1983), followed by two subsequent thick rounds, the 43rd in 1987–88 and the 50th in 1993–94. This enables us to compute average per capita monthly expenditure of Hindu and Muslim households,[4] and at some sacrifice of disaggregation (described below), we obtain a 3-period panel at the regional level.

Table 3 contains the basic results with time-varying controls. In several different panel specifications with different sets of controls, Hindu per-capita expenditures have a negative effect on conflict (measured by total casualties; killed + injured), while the coefficient on Muslim per-capita expenditures is significant and positive. The coefficients are also large. Depending on the exact specification, a 1% increase in Hindu per-capita expenditure is predicted to decrease casualties by anywhere between 3–7%, while the same increase in Muslim per-capita expenditure *increases* casualties by 3–5%. We conclude that an increase in Hindu prosperity is negatively associated with greater religious fatalities in the near future, while the opposite is true of Muslim prosperity. The remainder of the paper subjects these findings to a number of different robustness checks. In all these exercises, the effect of Muslim expenditures remains strong and significant. By and large, the same is true of Hindu expenditure, though in some specifications significance is lost.

As we argue, our preferred explanation for this strong and curious relationship rests on the theory outlined in Section 3. The fact that Muslim expenditures display a significant and positive connection with later conflict, while Hindu expenditures have a negative link, suggests that (statistically speaking) Hindu groups have been largely been responsible for Hindu-Muslim violence in India, or at least for violence driven by instrumental, specifically economic considerations. We do not mean to suggest that aggression is an intrinsic quality of Hindu groups while inevitable victimization is the lot of the Muslims. Indeed, our findings do not speak to baseline levels of violence, but to their sensitivity to economic change.

It is important to note that the empirical analysis does not by itself allow us to draw such a conclusion. The reader must jointly entertain both the theory and the empirical analysis. Whether that is a stance that one is justified in taking is largely left to the reader, though Section 5.3, which examines the alternative hypothesis that the observed correlations are driven by the funding of violence, is

[3]A leading example is Dube and Vargas (2013), in which a resource boom — such as one in oil — could be conflictual if it represents lootable spoils, while a similar boom that raises wages and therefore the opportunity cost of conflict — such as one in coffee — could reduce violence. The Dube-Vargas approach builds in turn on Dal Bó and Dal Bó (2011), which allows for both effects and connects the dominant effect to the capital-intensity of the sector in question.
[4]NSS does not collect data on incomes.

written to advance our interpretation a step further; see in particular, Propositions 2 and 3 and the succeeding discussion. As Section 2 emphasizes, the theory has historical context. It is not hard to find case studies in which attacks on the Muslim community can be traced to various forms of Muslim economic empowerment. These studies are supportive of our main approach, but to our knowledge no one has pointed out the general relationships we observe here.

2. Background

Political scientists, sociologists and anthropologists have written extensively on ethnic violence. The literature is vast and we do not pretend to review it here: the monumental treatise by Horowitz (2000) on ethnic conflict is an excellent entry point. It is probably fair to say that the *economics* of violence have not been given center-stage in most of these writings, the focus being more on other sources of conflict, which include both politics as well as historical antagonisms. This lack of focus is not surprising. Despite ample ethnographic studies that document an economic component to conflict, there is relatively little by way of firm statistical evidence that the two phenomena are connected.

One view is that conflict should be related to economic poverty and deprivation. Indeed, at the broad level of cross-country correlates, there is evidence that per-capita income, or shocks to such income, are negatively correlated with conflict (Collier and Hoeffler 1998, Fearon and Laitin 2003, Miguel, Satyanath and Sergenti 2004). One might extrapolate a step further and assert, as Sen (1973) does, that "the relationship between inequality and rebellion is indeed a close one." But there is little evidence for the argument that the *relative* deprivation of a group (or economic inequality more generally), is conflictual; see, for instance, Midlarksy (1988) or the survey by Lichbach (1989). This ambiguity shows up not just at the cross-country level, but also in specific studies such as those by Spilerman (1970, 1971, 1976), Wilson (1978) and Olzak and Shanahan (1996) on race riots in the urban United States. Theories of relative deprivation have just not worked very well to explain violence.

One reason for the lack of a connection is that cross-group inequality is correlated with increased segregation of the groups. They interact little and so the frictions are low: as in a caste-based or feudal society, each group knows its place. It is only when the fortunes of a previously deprived group begin to improve, that economic interaction across groups begins to increase. The improved fortunes mean that the deprived group may have less of an incentive to engage in conflict, though the associated changes in aspirations may have the opposite effect. But the previously advantaged groups will feel threatened, and react accordingly. That reaction may well lead to violence. Sociologists have dubbed this view *competition theory*. In the words of Olzak and Shanahan, "..[W]hen groups come to occupy the same niche, the historically more powerful or advantaged group attempts to exclude competitors. When the less powerful resist these attempts, racial conflict and violence ensues." [5] In particular, economic *progress* can be conflictual. Our paper builds on this point of view by linking group incomes to violence.

[5]Olzak and Shanahan (1996) consider two measures of urban labor market competition: first, an interaction term between percentage change in population and the overall unemployment rate, and second, an interaction between the nonwhite unemployment rate and percentage change in the nonwhite population in a city. These measures are unconnected with the approach we adopt. See also the "split labor" market theory of Bonacich (1972), which argues that labor from clearly demarcated groups of weaker economic strength, such as immigrants, are often used to wear down organized labor, leading to inter-group violence.

Indeed, ethnographic studies show quite unequivocally that in many instance, religious or ethnic violence has a strong economic component; see, for instance, Bohr and Crisp (1996) on Kyrgystan, André and Platteau (1998) on Rwanda, Horowitz (2001) on the Ivory Coast and other regions, or Mamdani (2010) on Darfur. More to the point of our study, the economic component is specifically visible in Hindu-Muslim riots. For instance, Upadhyaya (1992) documents the targeting of Muslim sari dealers in the 1991 Varanasi riots. They were clearly viewed as business rivals. A similar targeting of Muslim cloth manufacturers is seen in the case of the 1984 Bhiwandi riots; see Rajgopal (1987) and Khan (1992).

"[T]he 1984 riots were largely the outcome of business rivalry, though the immediate provocation was provided by the Shivaji Jayanthi procession. The well-entrenched and the newly emerging traders came to perceive competition between them in trade along religious lines. When the competition happens to be between merchants belonging to two religious groups, communal motives are imputed for the success or the failure of the different groups."

Of Meerut, where Muslim powerloom owners had started to diversify economic activity from cloth weaving and printing into other sectors, such as transport and auto-repair, Engineer (1987) writes:

"If [religious zeal] is coupled with economic prosperity, as has happened in Meerut, it has a multiplying effect on the Hindu psyche. The ferocity with which business establishments have been destroyed in Meerut bears testimony to this observation. Entire rows of shops belonging to Muslims . . . were reduced to ashes."

Economic targeting during conflict is not confined to eliminating rival businesses or workers. It can consist in direct attacks on entire localities, so as to drive out an ethnic group and affect either housing prices or the opportunity to buy and build. In their analysis of the 2002 Gujarat conflict, Field, Levinson, Pande and Visaria (2008) study locations in which valuable housing was retained by mill workers in residential colonies when the textile mills shut down:

"Once the mills closed, preferential treatment of these lands under the Bombay Rent Control Act implied that residents were granted stronger than average tenancy rights. Since tenancy rights are not transferable on formal real estate markets, mounting tensions between Hindus and Muslims in Gujarat led to a territory war rather than segregation in these locations. As tension mounted, acts of violence and intimidation were used to push out residents belonging to the religious minority group."

This is only one of several studies in which housing is implicated as a factor influencing violence. For instance, Das's (2000) report on the Hindu-Muslim riots in Calcutta in 1992 observes that

"[I]t appears that that 'promoters' played a crucial role in inflaming the riot whose victims . . . were slum-dwellers. Their obvious aim was to clear the *bustees* [or slums] for construction projects. . . The expectation was that once such people could be forced to abandon their establishments the realtors would have 'an easy way to rake in the fast buck'. . . What actually took place in 1992 was a land-grabbing riot under a communal garb."

For more on direct economic targeting in Hindu-Muslim violence, see Bagchi (1990), Khan (1992), and the discussion in Wilkinson (2004, Ch. 2).

It seems reasonably clear that in most of these accounts, Muslims suffer a share of the losses that is entirely out of proportion to their population representation (though there are instances running the other way, as in the certain parts of Calcutta during the 1992 riots, such as Metiabruz). That isn't particularly surprising as Muslim populations are generally minorities, and implicit political or police support for Hindu rioters has often been alleged. Drawing on the 9th and 10th Annual Reports of the Minorities' Commission, Wilkinson (2004, p. 30) observes that

"Muslims suffer disproportionately as a result of Hindu-Muslim riots. Hard numbers are difficult to obtain, but of 526 Hindu-Muslim incidents that occurred from 1985 to 1987 in 10 major states, Muslims (12% of the population) accounted for 60% of the 443 deaths, 45% of the 2,667 injuries, and 73% of the property damage. Given that Muslims are, as a community, much poorer than Hindus the relative effect of communal riots on Muslims economic life is even greater than these percentages suggest. . . The fact that Muslims suffer disproportionate losses in riots and that Muslim businessmen are more often the victims of looting has convinced many scholars and activists that riots are nothing more than a particularly brutal method of protecting Hindu merchants market share."

Yet writers such as Wilkinson and Horowitz only flirt with the economic argument, in part because they have entirely legitimate non-economic axes to grind (political factors in the case of Wilkinson, and a host of historical and anthropological correlates, including ethnic hatreds, in the case of Horowitz). While open to the possibility that economic causes may be afoot, their point is that it is one thing to state that conflict has a strong economic component, and another to say that economic changes *precipitate* conflict. So, for instance, Wilkinson (2004, p.30–31) asserts:

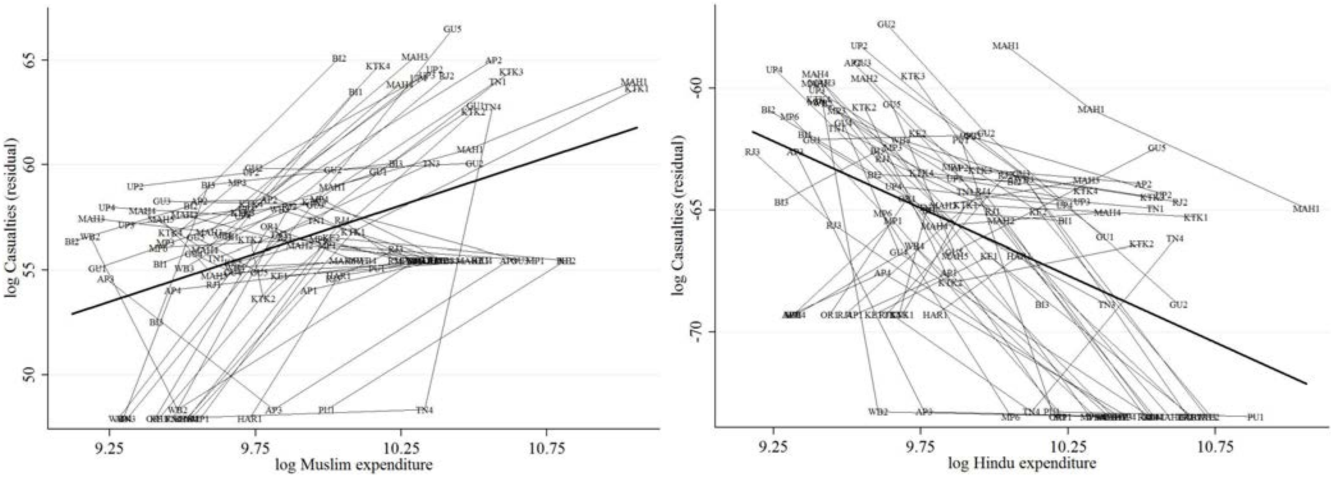
"Despite the disparate impact of riots on Hindus and Muslims, however, little hard evidence suggests that Hindu merchants and financial interests are fomenting anti-Muslim riots for economic gain. . . The fact that economically motivated violence against Muslims occurs *after a riot breaks out* does not necessarily prove that this is why the violence broke out in the first place."

This echoes the earlier cautionary note sounded in Horowitz (2001, p. 211):

"It is difficult to know how seriously to take commercial competition as a force in targeting choices. In some north Indian cities serious competition has subsisted without any violent episodes. The role that commercial competition is said to play is said to be a covert, behind-the-scenes role, which makes proof or disproof very difficult."

A partial objective of this paper is to address the difficult question of whether economic considerations play a role in precipitating violence. To this end, we study changes in Hindu and Muslim group incomes by using data on expenditure from the National Sample Survey of India (NSS). The data come from the 1980s and 1990s (more detail below), during which several changes impacted differentially on Hindus and Muslims, thereby allowing for degree of independent movement in their incomes. Here are two examples. First, positive shocks to oil prices, starting with the concerted efforts of OPEC in the 1970s, resulted in a huge increase in the demand for labor from the Gulf countries. That resulted in a substantial emigration of workers from India to the Gulf over the next few decades. In particular, members of the Muslim communities in Kerala, Tamil Nadu and Andhra Pradesh contributed to this steady flow of migrant workers (see, e.g., Azeez and Begum (2009)). In turn, this flow resulted in remittances back to India from the Gulf, some of it resulting in highly visible real estate booms.[6] Second, the trade liberalization process in India, set in motion

[6]See, for instance, Rajagopal (1987, p.35): "The boom in the economy of the Arab countries in the Middle East has been a blessing . . . the youths and the entrepreneurs among the Muslims have also capitalized on this boom. This accounts for a distinct spurt in the economic affluence of Muslims in certain parts of the country."



(A) Conflict and Muslim Expenditure

(B) Conflict and Hindu Expenditure

FIGURE 1. HOW CONFLICT REACTS TO PER-CAPITA EXPENDITURE

in 1991, has led to the continuation and heightening of changes with even earlier origins. In particular, while some sectors made substantial gains from this liberalization process, the unorganized tertiary sector has suffered, certainly in relative and perhaps in absolute terms. After all, this sector has practically no safety nets to cope with the structural changes accompanying globalization. It is well known (see, e.g., Basant (2012)) that Muslims are heavily concentrated in this sector; furthermore, they mostly happen to be poor and self-employed. Therefore, such Muslim households were more at the mercy of the broad, sweeping changes which liberalization brought in its wake.

We combine these economic changes with information on Hindu-Muslim conflict from the Varshney-Wilkinson dataset. As a starting observation, Figure 1 considers (the logarithm of) Hindu and Muslim per-capita expenditure by region at each of three rounds of the NSS, and conflict measured by (the logarithm of) total "casualties" — killed plus injured — in the five-year period starting immediately after the rounds. Because regions vary so widely in average conflict levels and because there are nationwide trends over the three periods, we remove region-specific and time-specific effects. With no other controls in place, the figure plots the two sets of residuals.

The remarkable pattern that emerges is one we will repeatedly verify over several robustness checks: conflict appears to react significantly and positively to an increase in Muslim per-capita expenditures, while the opposite reaction occurs to an increase in Hindu per-capita expenditure: conflict declines. Indeed, we display each regional observation as a line segment joining three observations, so the reader can even see the effect "region by region": the line segments are generally upward-sloping in the first panel of the Figure, and downward-sloping in the second. A second objective of our paper is to interpret these two different effects by constructing and applying a simple theory of economic violence.

In what follows, then, we attempt to unearth such the economic basis of violence by proposing a particular theoretical link between changes in group incomes and a subsequent tendency towards inter-group conflict. The central idea is simple: an increase in individual income has two effects. To the extent that the recipient of the increase is a potential victim, it makes him (or his group, to the extent that the income increase is correlated within the group) a more attractive target of violence. But to the extent that the recipient is a potential aggressor, it makes him less likely to participate in conflict, because an income increase raises the opportunity cost of engaging in violence. It follows that groups with a large proportion of potential victims will exhibit a positive relationship between group income and subsequent conflict, while the opposite is true for groups with a large proportion of potential aggressors. We then investigate these relationships from an empirical perspective.

3. Theory

3.1. **A Model.** There are two groups. Members of one group can attack those of the other, possibly by exploiting a past confrontation or violent incident with a possible religious interpretation. The individuals involved — in their role as aggressors — decide whether or not to take matters further by "communalizing" the incident.[7] At the same time, members of either group — in their role as potential victims — seek security against the possibility of such attacks. Indeed, any individual could be an aggressor or a victim, depending on the specific context.

A potential victim is characterized by his income or wealth, which we denote by y. Let α be the perceived probability of this person being attacked. A victim can seek protection against attack; think of this as "defense" d. While not directly affecting α itself (though in equilibrium α will be endogenous), an individual's investment in defense lowers the probability that the attack will be effective. Write this probability as $p = p(d)$, with p decreasing in d and $p(d) = 0$ for all d exceeding some threshold $\bar{d}$. While we regard d somewhat abstractly here, it has several interpretations to which we return below. For now, we simply view a potential victim with income y as picking d to maximize

$$(1-\alpha)[y - c(d)] + \alpha\left\{p(d)[(1-\mu)y - c(d)] + [1-p(d)][(1-\beta)y - c(d)]\right\},$$

where $c(d)$ is the direct or opportunity cost of defense, μ is the fraction of gross income lost by the victim in the event of successful attack, and β (presumably smaller than μ) is the fraction lost in case an attack occurs and turns out to be unsuccessful, where the word "successful" is used from the aggressor's point of view.[8] This specification incorporates the fact that an attack, successful or not, may still be costly to the victim: $0 \leq \beta < \mu \leq 1$.

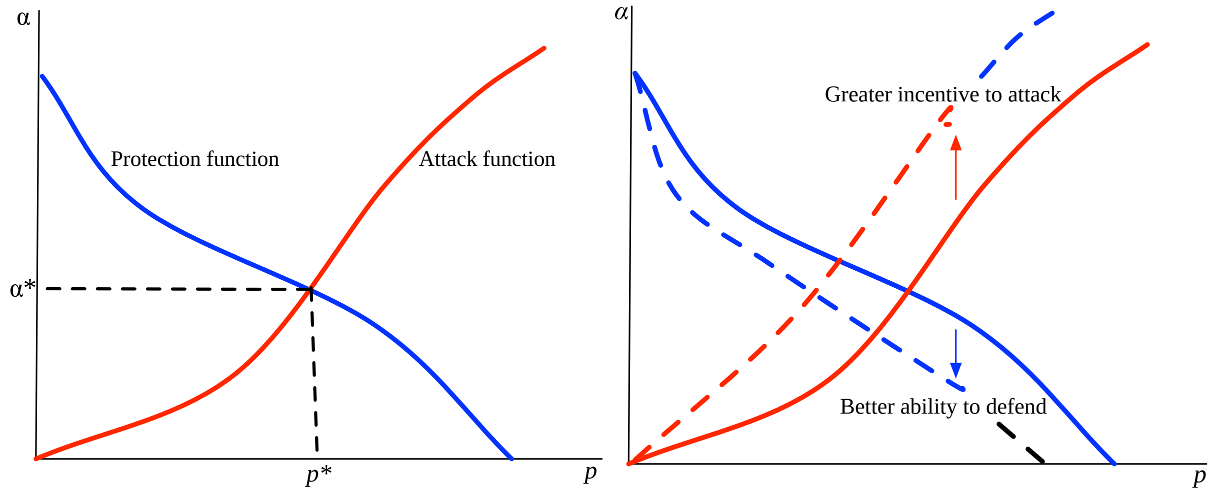
This problem is equivalent to the one of choosing d to minimize

$$\alpha(\mu - \beta)p(d) + [c(d)/y], \tag{1}$$

where the first term details the extra loss that will accrue from a successful attack, and the second term is the cost of lowering the success probability.

[7]Of the Moradabad riots in 1980, Rajagopal (1987, p. 75) observes that "[t]he incident was sparked off by the entry of a pig towards the Namazis (Muslims offering prayers)...". A more common list (p. 87) includes "encroachment on places of worship", "music before mosques", "teasing of girls belonging to the other community", and "provocative articles in magazines".

[8]It makes for easier exposition (but it is by no means necessary) to collapse the defense and attack scenarios into one single period; one could just easily write this out in a more sequenced way. For instance, there could be some prior stage at which defense resources are chosen, followed by a second stage in which attacks possibly happen. Our results are also robust to the use of a constant-elasticity utility function defined on net income.



(A) Equilibrium

(B) Cross-Section

FIGURE 2. EQUILIBRIUM

Under the assumption that p is decreasing and c is increasing (and both functions are continuous), there is always a solution to the maximization problem (1). As we track these reactions over different values of α, we obtain a best response mapping, which we call the *protection function*.

The second best response mapping yields the probability of attack as a function of the perceived probability of success p. Call this the *attack function*. Suppose that a potential aggressor with income z must decide whether or not to participate in violence against an individual with income y. Participation involves an opportunity cost, incurred in the fraction of time t spent on conflict. That time could have been spent in productive work. The income loss is therefore tz. (We extend this setting to include the expenditure of financial resources in Section 5.3.) The gain could be economic or psychic but, as discussed above, it is positively related to the victim's income y. Denote the gain by λy. Then an attack will be launched if

$$(1-p)[1-t]z + p([1-t]z + \lambda y) > z.$$

Rearranging, we may rewrite this condition as

$$z < (\lambda p/t)y, \tag{2}$$

The value $(\lambda p/t)$ establishes a threshold ratio of attacker to victim income below which the attacker will willingly engage in conflict. It is intuitive that a higher probability of success p makes it more attractive to attack, and that an increase in the opportunity cost t makes it less attractive.

It follows that a potential victim with income y faces a likelihood α of being attacked, given by

$$\alpha = \pi A(\lambda p/t)y, \tag{3}$$

where π is the probability of a cross-religious encounter, p is the perceived probability of success, and A is the cumulative distribution function of aggressor incomes. Call this the *attack function*.[9]

[9]Note that in deriving the attack function, we've used the exogenous income y of the potential victim. In actuality, y may be depleted by expenditures on defense, and it may be augmented by the economic gains of the victim in *his* role (in other contexts) as aggressor. Similarly, we've used the exogenous income z of the aggressor, and haven't adjusted it for his attack or defense activities elsewhere. It is easy enough to adjust the model to take these endogenous

3.2. **Equilibrium.** We may now formalize an equilibrium notion for conflict. This is a collection of attack and success probabilities, $\alpha^*(y)$ and $p^*(y)$, one such pair for every victim income y, such that α^* is determined by the optimal decisions of potential attackers, given p^*, while p^* is determined by the optimal decisions of potential victims, given α^*. A simple single-crossing argument, which we record in the Appendix, assures us that the protection function is decreasing, while the attack function is increasing. Their unique intersection determines the equilibrium for every y:

OBSERVATION **1.** *For every y, the protection function generates success probabilities p that weakly decrease in α, while the attack function generates attack probabilities α that weakly increase in p.*

If the distribution of income is strictly increasing everywhere, there is a unique equilibrium.

The Appendix contains a proof. Panel A of Figure 2 summarizes an equilibrium. The upward-sloping line is the attack function that generates α as a function of p.[10] The downward-sloping line is the protection function[11] Either function may have jumps, but we can use indifferences (and the assumption of a large population) to fill in these jumps so that the resulting graph is closed. These jumps will actually arise in our later specification of two kinds of protection technologies. The two lines intersect once, telling us there is a unique second-stage equilibrium, as in Observation 1.

In what follows, we are interested in conflict outcomes; specifically, whether or not they are "successful" from the point of view of the aggressor. With large populations, this is equivalent to studying the overall probability of attack.

3.3. **The Two Faces of Economic Fortune.** This model, elementary though it may be, can be used to address a variety of different questions. In the present exercise, we focus on the effects of group income changes on the likelihood of conflict, which is the value of α averaged over all potential victim incomes.

First traverse a cross-section of victim incomes. Imagine drawing a variety of attack and protection functions for different values of the income of a potential victim. It is obvious that the net effect of such changes on α will be ambiguous. Richer victims are a more attractive target for attack, but on the other hand they invest more on protection. The net impact of victim wealth on the probability of attack can, therefore, go in either direction. Panel B of Figure 2 summarizes this situation.

However, the effect of an across-the-board change in *group* incomes is different. To understand this, one must study the technology of protection or defense, because the cost of deploying that technology will vary with group incomes. Think of two components to protection. The first component is human: protection provided by other individuals in the same community. This is ensured, first and foremost, by *living* in that community, or at least within easy reach of community members.[12] Yet that choice cannot but come at a cost. The principal component of that cost lies in the implicit contract of protection. It may well be the case that compatriots would spontaneously

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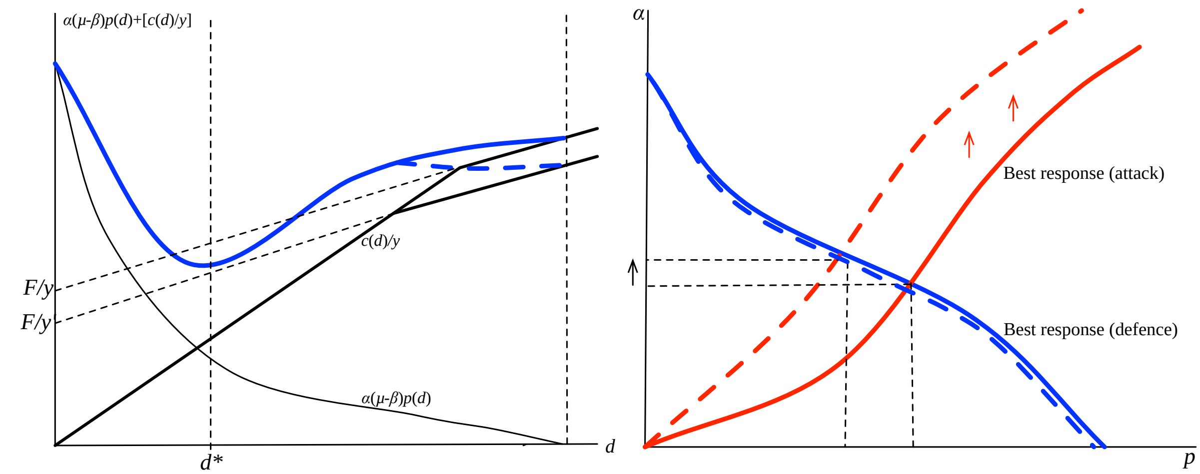
adjustments into account. There is no difference in the results, but the resulting model is just more complicated in terms of exposition.

[10] It is indeed upward-sloping if the distribution function A is strictly increasing.

[11] It is actually weakly downward-sloping.

[12] In the Hindu-Muslim case, see, for instance, Mahadevia (2002) and Chandoke (2009) on the high residential segregation in Ahmedabad. Over 70% of the Ahmedabad sample studied in Field, Levinson, Pande and Visaria (2008) lived in segregated communities.



(A) The Defense Response (B) Equilibrium

FIGURE 3. THE EFFECT OF A CHANGE IN GROUP FORTUNES: LOW INCOME

defend a potential victim, but such defense is rarely free: by and large, equal contributions will have to be made to the community or obligations incurred, such as the reciprocal protection of others. But the cost of that reciprocity must be commensurate with the opportunity cost of providing protection services, which is related to the average of group incomes to which our victim belongs. We therefore expect that *the cost of "human protection" will be proportional to group incomes*.

The second component of protection largely involves the use of physical capital: the purchase of security through the use of high walls, barricades, and firearms. This sort of protection is generally extremely effective in reducing attack, but involves high fixed costs: the purchase of weaponry (and the hiring of security guards to use them), the erection of high walls around one's property, and so on. Unlike human protection, the cost of this component will be less-than-proportionately related to group incomes, and to the extent that it is fully reliant on physical capital, not related at all. Specifically, we suppose that

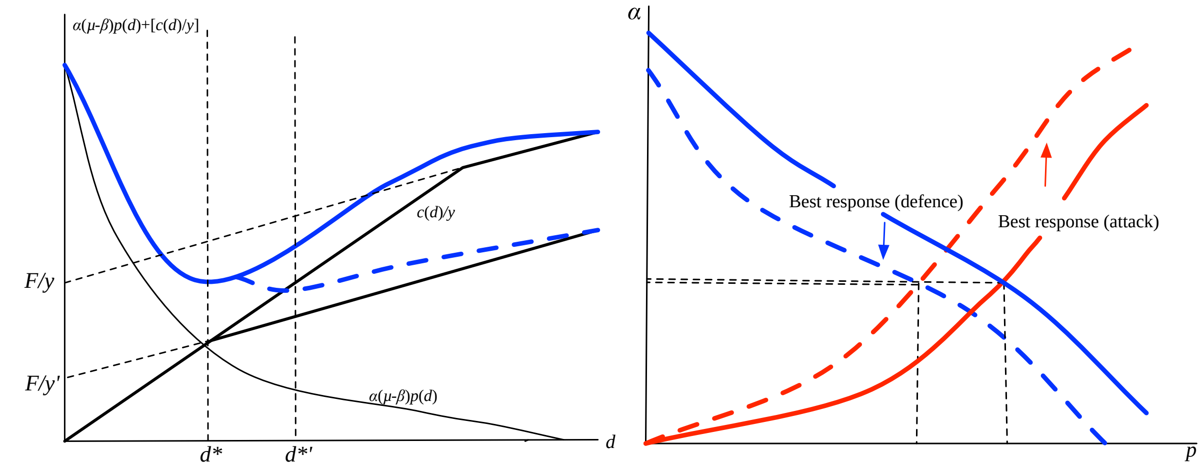
$$c(d) = \min\{wd, F^* + w^*d\},$$

where the first entry represents a protection technology with a dominant human component, and the second a technology with a dominant physical component, with the potential advantage that it has lower variable costs. That is, $w > w^* \geq 0$. The important assumption that we make is that the variable costs w are fully human (and borne by individuals in the same group), and therefore proportional to average group incomes.

PROPOSITION **1.** *Assume that w is proportional to average group incomes. Then an equiproportionate increase in the incomes of a group has the following effects:*

(a) *There exists a threshold income y^*, such that higher group income elevates attacks perpetrated on members of that group, provided all group incomes are lower than y^* before and after. The effect persists as a small number of incomes cross the threshold, but turns ambiguous as more incomes exceed y^*.*

(b) *It unambiguously lowers attacks instigated by members of that group.*



(A) The Defense Response

(B) Equilibrium

FIGURE 4. THE EFFECT OF A CHANGE IN GROUP FORTUNES: HIGH INCOME

Parts (a) and (b) represent the two faces of economic fortune. An improvement in the fortunes of a potential victim make him a more lucrative target, so that violence increases. An improvement in the fortunes of a potential aggressor increases the opportunity cost of engaging in conflict, so that violence decreases. The sign of the correlation between group incomes and subsequent violence tells us something about whether that group contains a preponderance of victims or attackers.

To understand how the proposition works, consult Figure 3. Consider a potential victim, whose income increases (in the same proportion as his group's) from y to y'. The thin downward-sloping line in Panel (a) is the function $\alpha(\mu - \beta)p(d)$, which is the expected loss per unit of victim income in the event of an attack. The piecewise linear segment in that panel is the function $c(d) = \min\{wd, F^* + w^*d\}$, deflated by victim income y. The thick nonlinear curve is the sum of these two functions, which our individual seeks to minimize via choice of d.

Given that our individual's income shift mirrors the overall group shift, and that w/y is unaffected by group income, there is no change in the sum of the two curves up to some threshold, after which it moves down. This happens because fixed costs are effectively reduced when deflated by rising income, and the ratio of subsequent variable cost w^* to income could be reduced as well. The sum of the two functions therefore moves as shown in Panel A. However, in this panel, the individual in question has low income, and the capital-intensive technology is not attractive even after the effective fixed cost shifts down. A change in group incomes then has *no effect* on the optimally chosen defense expenditure of that individual.

Moving over to Panel B with this information, we see that when incomes are low, the variable cost of defense expenditure moves in tandem with incomes, and the protection function does not shift with a change in group incomes. At the same time, each individual in the group becomes a more attractive target: the attack function shifts upward, and it becomes more profitable to launch an attack for any fixed value of p. The net effect is an increase in equilibrium attack probability.

It is easy enough to define a threshold y^* which is sufficient to generate all the effects above. Note that the highest probability of an attack is bounded above by π, the probability of a cross-religious confrontation. If, at this level, it is optimal for an individual to choose the "human protection" technology, then by the first part of Observation 1, it is optimal to do so for all lower levels. It is straightforward to see that such a threshold must exist.[13]

For individuals with incomes that exceed this threshold, the capital-intensive technology may be attractive. If it is attractive both before and after the change in group incomes, then the effect on d will depend on the ratio of w^* to y. If w^* is a fully human cost and involves the use of fellow group members, it will again be proportional to group means, and previous arguments apply. The ambiguity arises from individuals whose incomes cross the threshold. Figure 4 shows what happens with incomes that rely on the human technology before the change, but move into the fixed-cost technology after the change. Panel A shows that it is now possible for there to be a sharp upward jump in defense expenditures.[14] The protection function shifts downwards, as in Panel B, while the attack function (as before) shifts upwards. The net effect will depend on the relative strengths of these two shifts, and it is ambiguous.

The effect on overall attacks will depend on the proportion of individuals who fall below the threshold for which the capital-intensive technology is never used. The more individuals there are in this category, the more likely it is that economic improvement will generate greater violence directed *against* the group in question.

In contrast, consider a potential aggressor, whose income increases from z to z', and look at the attacks perpetrated *by* him. Given the assumptions of our model, there is no ambiguity here at all: the opportunity cost of engaging in violence goes up, and aggression must decline. Formally, the inequality (2) is less likely to hold for any aggressor-victim pair, and so — all other things being equal — the probability of attack, as given by (3), must come down. In summary, then:

(i) Group economic improvements are likely to lead to greater violence overall if (a) the group is relatively poor to begin with, and (b) the greater the proportion of potential victims relative to aggressors in the group.

(ii) If the group is relatively well-off, the effect of group income changes on violence is ambiguous.

(iii) If the group is more likely to consist of aggressors rather than victims, then group improvements will lead to a decline in overall violence.

This is the interpretation we take to the empirical study, in which we connect the mean income of Hindus and Muslims to subsequent outbreaks of conflict. We emphasize that the theory is not tested by the study that follows: it is only a device to make sense of the empirical observations.

One might object that we have nothing in the theory that permits groups to differ in terms of "aggressiveness". But this is easily dealt with; for instance, by presuming that the opportunity cost t is group-specific. That cost may be driven by a multiplicity of factors: relative income, numerical majorities, elite funding, or historically determined notions of who is more "Indian" than whom. In the particular case of Hindu-Muslim conflict, a number of these considerations intertwine and intersect. But the present paper has little to say on these extremely important matters.

[13]Recall that w is linear in average incomes and is therefore bounded above by a fraction of Y, if all incomes in society are smaller than Y. Moving Y down lowers w and must create a cross-over to the human protection technology at some positive level even if $w^* = 0$. This level is sufficient for our needs (it may be far from necessary).

[14]By mixing across individuals who are indifferent between making this change, we can always make sure that the graph of the protection function is continuous, so that an equilibrium exists.

4. Empirical Analysis

4.1. **Data and Descriptive Statistics.** Systematic statistical information on outbreaks of religious violence in India is relatively hard to come by. We use a dataset compiled by Steven Wilkinson and Ashutosh Varshney.[15] It summarizes reports from *The Times of India*, a leading national newspaper, on Hindu-Muslim conflicts in India in the second half of the twentieth century. This dataset has information on deaths, injuries, and arrests. It does not provide hard information on which side initiated the violence, for in most cases that issue would necessarily be mired in subjectivity. For every report of Hindu-Muslim violence, the dataset provides the date of incidence of the riot, the name of the city/town/village, the district and state, its duration, the number of people killed, injured and arrested and the reported proximate cause of the riot.

The following summary provides some sense of the pervasiveness and intensity of Hindu-Muslim riots in post-Independence India. Between 1950 and 1995, close to 1,200 separate riot episodes were reported, with over 7,000 individuals killed. Between 1950 and 1981, the average number of Hindu-Muslim riots in India was 16 per year. This same number for the period between 1982 and 1995 happens to exceed 48. Over these 14 years, a total of 674 riots were reported with close to 5000 deaths. Therefore, over half the reported riots between 1950 and 1995 (and around 2/3 of total deaths) occurred during a period that was less than one-third as long as the total period for which we have data. In other words, religious conflict appears to have sharpened significantly as we move from 1950–81 to 1982–95.

In this paper, we primarily utilize the Varshney-Wilkinson data from 1979 to 1995. Furthermore, we have extended this conflict dataset by a period of five years, i.e., from 1996 to 2000.[16] The main reason for limiting ourselves to this time period is the non-availability of reliable data on economic conditions (by religious group) for earlier years. At the same time, the observations made above highlight the importance of religious violence in the 1980s and 90s.

We use different count data from the dataset: the number of people killed or injured ("casualties"), the number of people killed or the number of riot outbreaks over the period. In all cases, we take aggregates over a five year period in each location.

Although incidents of Hindu-Muslim violence have been reported all over India, there are some regions that appear to be particularly prone to such outbreaks. The "conflict" columns of Table 1 tell us that the states of Gujarat and Maharashtra have witnessed major outbreaks whereas states like Punjab, Haryana and Orissa have experienced very few such incidents.

The "expenditure" columns of Table 2 provide a quick guide to Hindu-Muslim expenditure disparities in different states of India. The table provides state averages as well within-state regional variations. On the whole, Hindu households have a higher average monthly per-capita expenditure than their Muslim counterparts. But Table 2 also reveals the large variation in Hindu-Muslim expenditure ratios across the regions of India. This ratio was as low as 0.36 in a region in Orissa in 1983 and as high as 1.93 in a region in Haryana in 1993–94.

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[15] See, in particular, the recent use of this data in Wilkinson (2004). We acknowledge, with gratitude, Steve Wilkinson's generosity in letting us have access to this data.

[16] In conducting this exercise, we have adhered to the same data collection protocol as followed in the construction of the original dataset. To ensure consistency, we have kept the source of these data (from 1996-2000) the same as that used by Varshney and Wilkinson; namely, the reports from *The Times of India*.

State	1984-88			1989-93			1994-98		
	Casualties	Killed	Outbreak	Casualties	Killed	Outbreak	Casualties	Killed	Outbreak
Andhra Pradesh	320	48	14	226	165	11	141	8	2
Bihar	62	18	4	647	485	29	187	42	6
Gujarat	1932	329	97	1928	557	75	639	2	3
Haryana	0	0	0	6	4	2	0	0	0
Karnataka	300	38	19	430	82	32	235	39	7
Kerala	17	0	2	42	5	3	0	0	0
Madhya Pradesh	139	17	8	794	174	12	22	2	1
Maharashtra	1250	333	57	2545	808	29	238	9	11
Orissa	0	0	0	62	16	6	0	0	0
Punjab	13	1	1	0	0	0	0	0	0
Rajasthan	14	0	4	302	75	15	66	6	3
Tamil Nadu	21	1	1	125	12	5	67	33	5
Uttar Pradesh	963	231	38	1055	547	48	217	50	22
West Bengal	71	19	7	148	59	12	0	0	0

TABLE 1. Descriptive Statistics: Conflict. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots. "Conflict" is measured by aggregates of casualties (killed + injured), killed and outbreaks over a five-year period.

State	1983			1987-8			1993-4		
	H-M exp. ratio	Min	Max	H-M exp. ratio	Min	Max	H-M exp. ratio	Min	Max
Andhra Pradesh	0.99	0.96	1.09	0.99	0.92	1.17	0.99	0.84	1.16
Bihar	0.98	0.88	1.12	1.07	1.02	1.12	1.03	0.93	1.16
Gujarat	1.02	0.89	1.19	0.98	0.78	1.14	1.06	0.88	1.13
Haryana	1.20	1.07	1.53	0.96	0.85	1.05	1.60	1.39	1.93
Karnataka	0.98	0.84	1.19	1.00	0.83	1.07	1.01	0.69	1.15
Kerala	1.10	1.07	1.19	1.15	1.15	1.16	1.01	0.92	1.16
Madhya Pradesh	0.92	0.78	1.38	0.86	0.71	1.04	0.88	0.62	1.16
Maharashtra	1.04	0.97	1.25	1.04	0.74	1.29	1.12	0.87	1.42
Orissa	0.69	0.36	1.04	0.85	0.58	0.93	0.96	0.73	1.13
Punjab	0.86	0.75	1.15	1.21	1.19	1.22	1.18	1.08	1.34
Rajasthan	0.97	0.43	1.18	1.02	0.46	1.19	1.22	1.06	1.35
Tamil Nadu	1.06	0.82	1.44	0.88	0.80	0.94	0.98	0.85	1.05
Uttar Pradesh	1.12	1.01	1.23	1.11	0.95	1.54	1.08	0.93	1.31
West Bengal	1.18	1.05	1.26	1.21	1.05	1.31	1.25	1.07	1.38

TABLE 2. Descriptive Statistics: Economic Data. *Sources and Notes. National Sample Survey* 38th, 43rd and 50th rounds. H-M exp. ratio = Hindu per-capita expenditure/ Muslim per-capita expenditure, average value for the state. The range for the state comes from the constituent regions of the state.

Table 1 makes it clear that there is enormous spatial variation in conflict, certainly at the level of the state. It is therefore important to exploit a panel structure with fixed effects,[17] which we deploy at

[17]Jha (2013) provides a fascinating account of regional variation in religious tolerance, based on inter-ethnic complementarities in medieval trading ports.

the regional level. Indeed, this is the structure that underlies the construction of Figure 1. However, in the choice of the time period we are constrained by the available overlap of conflict data and economic information. In India, large scale household surveys are conducted quinquennially as part of the National Sample Surveys (NSS). The survey rounds cover the entire nation and capture monthly expenditure incurred by the sample household for the purpose of domestic consumption.[18]

We seek spatially disaggregated economic information by religion. The earliest "thick" round that permits us to do this is the 38th (1983).[19] So we use three such "thick" rounds: the 38th (1983), the 43rd (1987–1988) and the 50th (1993–94). For all of these rounds there is information on the religious affiliation of the household, or more precisely, the head of the household. This enables us to compute the per-capita monthly expenditure of Hindu and Muslim households.

However, we are further restricted by the relative lack of spatial disaggregation in the 38th and the 50th rounds, which do not permit identification of the surveyed households all the way down to the district level. To use all three rounds (and thereby exploit the panel structure), we must aggregate the Varshney-Wilkinson dataset up to the regional level in India, "regions" being formally defined areas that are midway between the state and the district. We do so for 55 such regions, which together span 14 major Indian states and account for more than 90% of the Indian population.[20]

4.2. **Specification.** For the reasons given in the theoretical section of this paper, we are interested in the effect of Hindu and Muslim per-capita expenditures on religious violence. As already described, our dependent variables are different measures (or specifically, counts) of Hindu-Muslim violence. The independent variables and the expected signs on them come from the theory. Recall that in equilibrium, violence is proportional to the total number of attacks, given by

$$\pi \left[\nu_1 \int_{y_2} F_1(\lambda_1 p_2(y_2) y_2 / t_1) dF_2(y_2) + \nu_2 \int_{y_1} F_2(\lambda_2 p_1(y_1) y_1 / t_2) dF_1(y_1) \right],$$

where π is the probability of a cross-match and subscript i stands for variables pertaining to group i. The first term within the square brackets denotes attacks generated by aggressors in group 1 on victims in group 2, and the second term switches the roles of the two groups. The weights ν_1 and ν_2 tell us how important each configuration is in generating the overall conflict that we observe.

The cross-match probability π will be increasing in both the extent of Hindu-Muslim polarization as well as in overall population. Proposition 1 tells us, additionally, that attack data will depend on average incomes in each group. Taken together, this motivates a Poisson specification in which

[18]Unfortunately, a well-known problem in the case of the NSS is that we do not have income data on a nationwide scale, and expenditure is the closest we can get.

[19]NSS surveys which occur annually utilize smaller samples and hence are referred to as "thin" rounds. However, the rounds performed quinquennially draw upon larger samples (about 120,000 households per survey) and hence the epithet "thick".

[20]We leave out border states with their own specific sets of problems: Jammu & Kashmir and Himachal Pradesh in the north, and the north-eastern states of Assam, Arunachal Pradesh, Manipur, Meghalaya, Nagaland, Sikkim and Tripura. There are two specific issues with these areas: (i) NSS does not survey all regions within these states (owing to hilly terrain, safety issues, national security reasons due to border skirmishes, etc.), and (ii) for the border states it is sometimes difficult to tell whether a reported riot is indeed civilian in nature or due to the Army clashing with extremist groups. In addition, the north-eastern states (which happen to be sparsely populated) have an insignificant Muslim population: they are primarily Hindus, Christians, Buddhists and Scheduled Tribes. So even in the violence dataset there are almost no reports of riots there.

the parameter depends on all these variables, with possibly additional region- and time-specific variation. This motivates the baseline Poisson specification that we use:[21]

$$\mathbb{E}(\text{Count}_{i,t}|\mathbf{X}_{it}, \gamma_i) = \gamma_i \exp(\mathbf{X}'_{it}\beta + \tau_t)$$

where we add in region effects γ_i as well as time effects τ_t in the panel regressions below. Note, the subscript i represents region while t denotes time.

The most important variables in $\mathbf{X}$ are, of course, Muslim and Hindu per-capita expenditures (our proxies for per-capita income), and in some variants their ratio. Population and some measure of Muslim presence are always included as controls in every specification (despite the region fixed effects, these are important variables that potentially vary with time). Muslim "presence" is measured in two ways: we use either the share of Muslim households in the region, or a measure of Hindu-Muslim polarization along the lines proposed by Esteban and Ray (1994) and Montalvo and Reynal-Querol (2005).[22] To be sure, in all the regressions we either control for Muslim percentage or religious polarization but never both simultaneously. The correlation between these two variables is very high, though not perfect.[23]

The basic controls are constructed using the data from the NSS rounds. In some specifications, we also use an expanded set of controls; more on these below. In all the specifications, expenditures and population are entered logarithmically, and all other controls are brought in linearly.

We look at the effect of these expenditure variables on Hindu-Muslim conflict starting the year right after the corresponding expenditure round. Specifically, expenditures from the 38th round (1983) are matched with conflict during 1984-88, the 43rd round (1987-88) expenditures are matched with conflict during 1989-93 while the 50th round (1993-94) expenditures are matched with conflict during 1994-1998. Lag specifications and issues of endogeneity are discussed in some detail below. All specifications utilize both regional fixed effects as well as time dummies.

Here is a quick glossary of the relevant variables, beginning with the independent variables and controls. Hindu and Muslim *per-capita expenditure* will refer to per-capita monthly expenditures for each religious group at the regional level and entered using their natural logarithm (the same being true of per-capita expenditures overall), and the term *Muslim-Hindu exp. ratio* will refer to the ratio of Muslim per-capita expenditure to Hindu per-capita expenditure. Region *population* will also enter in logarithmic form. Controls include *religious polarization*, constructed by considering only two religious groups: Hindus and Muslims, the *literacy rate*, the completion rate for *primary education*, *urbanization*, calculated as the percentage of urban households in the region, and the share of regional Lok Sabha seats won by the Bharatiya Janata party (BJP). We also use Gini coefficients as controls for expenditure inequality among Hindu and Muslims, as our predictions pertain to balanced increases in income for either group. Our baseline dependent variable is *casualties*, given by the sum of individuals killed or injured in a riot; we also use just the *killed* variable as well as *outbreaks*, which counts the number of riots in any given period.

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[21] We also use the Negative Binomial and OLS specifications as robustness checks. Moreover, OLS has the advantage of easier interpretation of the coefficients vis-a-vis count models like Poisson and Negative Binomial.

[22] The degree of religious polarization for a region is defined by $4\sum s_j^2(1-s_j)$ for $j = H, M$ where H denote Hindus and M Muslims and s_j denotes the population share of j in the region.

[23] In some areas, there are other dominant religious groups (like Sikhs in Punjab), so that Muslim percentage and Hindu-Muslim polarization measure different things. But these cases are exceptions rather than the rule.

4.3. **Basic Results.** Our baseline specification is defined by the choice of dependent variable: "total casualties" (killed + injured) is used as the outcome of interest. Table 3 contains the main results. We present three different regression models: the first two are count model specifications (Poisson and Negative Binomial, respectively) and the third is OLS.[24] Each of the nine columns uses fixed effects. Hence, region-specific effects and time dummies are present in all regressions.

For each of the three regression models, we display three columns. The first column has minimal controls (only population and a measure of Muslim presence), while the second column controls in addition for literacy and urbanization. The third column further includes measures of within-Hindu and within-Muslim inequalities. In all panel specifications with or without controls, the coefficient on Muslim expenditures is significant and positive. In contrast, Hindu expenditures exhibit a negative effect on conflict (measured by total casualties; killed + injured).

The coefficients on both Hindu and Muslim expenditures are also large. A one percent increase in Muslim expenditures is predicted to increase casualties — starting the very next year — by around 5% in the fixed effects Poisson model. The corresponding estimate for the Negative Binomial model is around 3%. The same change in Hindu expenditure has corresponding effects ranging from -7% in the Poisson specification to -3% for the Negative Binomial model. To be sure, a 1% increase in expenditure may require a bit more than a 1% increase in underlying incomes, if the consumption function is concave.[25] But there is little doubt that the effect is significant and big, and strongly suggests that an increase in Muslim prosperity is positively associated with greater religious fatalities in the near future, while the opposite is true of a change in Hindu prosperity.

Below, we discuss several variations. Before we do so, we take explicit note of the controls for within-group economic inequality, as measured by the Gini coefficients on Hindu and Muslim expenditures. The controls, introduced in Column 3 of our basic specification, will be used in all the relevant variations below. It is important to maintain these controls as our theoretical predictions regarding income changes and its consequent effect on violence are based on "balanced changes" in group incomes. To be sure, "unbalanced changes", or changes in inequality, can also have their own set of effects, but this is not something we seriously investigate in this paper.[26] In any case, the inclusion or exclusion of inequality controls makes no serious difference to the main results of the paper.

In what follows, we explore the robustness of the basic finding to alternative specifications, and discuss questions of interpretation.

4.4. **Variations.** The basic results are robust to the many different variations we've tried; we discuss some of them in this section.

[24] For the OLS regressions, to avoid losing observations in cases where reported casualties are nil, we add a very small number (0.01) to the total casualties variable. Therefore, the dependent variable for the OLS regressions is actually $log(casualties + 0.01)$.

[25] Another distinction between consumption expenditure and income should be borne in mind. Consumption expenditure includes both food and non-food components. While income shocks are more transitory in nature, consumption expenditure tends to be more stable (owing to credit markets/informal insurance, etc.).

[26] Unbalanced changes in group incomes can affect conflict. For example, if victim incomes change in a manner that brings more individuals above the attack threshold, then conflict will go up. Such effects are compatible with both an increase and a decrease in inequality, depending on several factors. It is also the case that changes in aggressor inequality can affect conflict, an argument made by Esteban and Ray (2011). We do not, however, focus on these changes here.

	Poisson			Negative Binomial			OLS		
	[1]	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**	**[7]**	**[8]**	**[9]**
Hindu per-capita expenditure	***-8.325	***-7.869	***-6.824	*-2.528	*-2.791	-3.310	**-9.896	**-9.148	*-8.462
	(0.005)	(0.005)	(0.003)	(0.082)	(0.093)	(0.131)	(0.024)	(0.033)	(0.085)
Muslim per-capita expenditure	***5.627	***5.103	***4.670	*2.707	**2.637	**3.872	***7.366	***6.892	***9.523
	(0.000)	(0.000)	(0.001)	(0.052)	(0.040)	(0.023)	(0.003)	0.006)	(0.009)
Population	3.353	4.280	3.914	0.649	0.623	0.744	-3.402	-3.867	-1.230
	(0.554)	(0.468)	(0.496)	(0.122)	(0.149)	(0.132)	(0.659)	(0.614)	(0.877)
Religious Polarization	5.103	*5.552	*5.566	0.607	0.719	1.094	4.962	6.003	6.860
	(0.104)	(0.054)	(0.056)	(0.798)	(0.763)	(0.715)	(0.546)	(0.470)	(0.408)
Literacy Rate		0.021	0.023		-0.015	-0.015		-0.046	-0.043
		(0.298)	(0.242)		(0.411)	(0.525)		(0.514)	(0.552)
Urbanization Rate		-0.020	-0.017		0.019	0.015		-0.070	-0.055
		(0.258)	(0.354)		(0.234)	(0.405)		(0.227)	(0.371)
Gini: Hindu per-capita exp.			-5.426			4.121			-14.473
			(0.317)			(0.521)			(0.342)
Gini: Muslim per-capita exp.			3.399			-5.952			-11.073
			(0.497)			(0.362)			(0.451)
Log-Lik./Adj. R^2	-3,468	-3,416	-3,357	-304.23	-303.39	-302.20	0.331	0.335	0.348
Observations	129	129	129	129	129	129	129	129	129

TABLE 3. The Effect of Hindu and Muslim Expenditures on Regional Conflict: FE regressions with Poisson, Negative Binomial and OLS, respectively. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th, 43rd and 50th rounds. "Conflict" is measured by regional aggregates of casualties (killed or injured) over a five-year period starting immediately after the expenditure data. Robust standard errors clustered by region; corresponding p-values in parentheses. Time dummies included in all regressions. *significant at 10% **significant at 5% ***significant at 1%

4.4.1. *Other Dependent Variables.* The use of alternative count variables generate similar results. We can move to progressively coarser indicators: the number killed in riots or simply the number of outbreaks. Table 4 records some of these findings. As before, we report results for all three regression models: Poisson, Negative Binomial and OLS. For each of the three models, the first column runs the exercise for all killed, while the second column does so for the number of reported riots. All these variants consistently report that an increase in Muslim per-capita expenditure is positively and substantially correlated with later occurrences of conflict.

	Poisson		Negative Binomial		OLS	
	[1] Killed	[2] Outbreak	[3] Killed	[4] Outbreak	[5] Killed	[6] Outbreak
Hindu per-capita expenditure	-0.073	-2.122	-2.249	*-5.369	-4.267	**-6.304
	(0.976)	(0.393)	(0.293)	(0.069)	(0.339)	(0.019)
Muslim per-capita expenditure	0.852	*2.493	**3.692	**4.158	**6.415	***6.421
	(0.636)	(0.067)	(0.030)	(0.016)	(0.043)	(0.006)
Population	*-6.032	0.256	0.833	0.300	-3.310	-0.031
	(0.071)	(0.900)	(0.170)	(0.823)	(0.549)	(0.995)
Religious Polarization	1.306	0.261	0.100	*4.584	4.173	2.729
	(0.659)	(0.875)	(0.970)	(0.085)	(0.556)	(0.603)
Literacy Rate	-0.016	-0.024	-0.030	-0.037	-0.021	-0.034
	(0.609)	(0.289)	(0.406)	(0.127)	(0.746)	(0.320)
Urbanization Rate	-0.019	-0.025	0.009	-0.035	*-0.095	-0.052
	(0.451)	(0.240)	(0.735)	(0.208)	(0.074)	(0.227)
Gini: Hindu per-capita exp.	-2.629	-2.694	6.316	4.560	-8.767	-8.992
	(0.686)	(0.617)	(0.389)	(0.484)	(0.445)	(0.366)
Gini: Muslim per-capita exp.	4.577	-1.112	-11.240	-9.137	-15.055	-11.925
	(0.505)	(0.790)	(0.121)	(0.153)	(0.235)	(0.199)
Log-Lik./Adj. R^2	-730.84	-149.57	-193.27	-128.76	0.402	0.435
Observations	126	132	126	132	126	132

TABLE 4. The Effect of Hindu and Muslim Expenditures on Regional Conflict: FE regressions with Poisson, Negative Binomial and OLS, respectively (variations). *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th, 43rd and 50th rounds. All counts over a five-year period starting immediately after the expenditure data. Robust standard errors clustered by region; corresponding p-values in parentheses. Time dummies included in all regressions. *significant at 10% **significant at 5% ***significant at 1%.

4.4.2. *Expenditure Ratios.* Table 3 has the interesting feature that Muslim and Hindu expenditures have not only the opposite sign, they have roughly the same absolute impact. Indeed, in all our specifications, the two expenditures can be easily replaced by their ratio. As expected, a higher ratio of Muslim to Hindu income, controlling for overall per-capita income, is positively and significantly associated with greater subsequent conflict. See Table 5 for details.

4.4.3. *Politics.* Our empirical findings so far — coupled with the theory — suggest that Hindus have largely been the aggressors in Hindu-Muslim riots in independent India. We recognize, however, that our basic empirical specification does not include a satisfactory variable that captures the ambient political climate, which might influence Hindu-Muslim violence. In particular, the period of our study coincides with the rise of Hindu politics in many parts of India. A useful indicator for this is the strength of the Bharatiya Janata Party (BJP) in the region.[27] We use "BJP share", the fraction of Lok Sabha (national level parliament) seats in the region that is held by that Party.

This variable helps in two ways. First, given that politics plays a major role in determining the extent of Hindu-Muslim rioting in India (see, e.g., Wilkinson (2004)), we can ask if our findings are merely a reflection of the effect that the BJP's presence in a region has on regional violence. Second, the coefficient on this variable — while not of central interest as far as this paper is concerned — would tell us if BJP share is connected to the level of conflict. Theory gives us little clue regarding this connection. Greater Hindu dominance may be more conducive to conflict, because there is more "infrastructural support" for it. At the same time, Hindu dominance may be associated with more peace, simply because there are smaller gains through conflict for an already dominant group.[28] So the effect of heightened BJP presence is likely to be nonmonotonic, depending on the level of presence to begin with.

In Table 6, we report results for our measures of conflict: casualties, killed and outbreaks. We use three specifications each: Poisson, Negative Binomial and OLS. The basic finding that Muslim expenditures significantly and positively affect conflict, while Hindu expenditures exhibit (if anything) an opposite effect, remains entirely unaltered. As for the coefficient on BJP share itself, the results are mixed. In some specifications, the coefficient is positive, suggesting that BJP presence is conflictual; in others, the opposite result holds. In any case, while we are unwilling to speculate further on the sign of the BJP coefficient, it appears that our main result is entirely robust to the inclusion of political considerations.

4.4.4. *Urban Conflict.* It is true — especially if one restricts attention to the conflict dataset — that Hindu-Muslim riots are primarily an urban phenomenon; rural India is witness to fewer cases of religious riots.This is possibly the real picture even if one admits that the TOI might have an implicit urban bias in reporting. Hence, smaller urban riots might get documented while more large-scale rural ones may not make it to the press. One way to deal with this situation is to restrict attention to urban households in our NSS expenditure rounds. We do so, and the results are presented in Table 7. The results are in line with what we have obtained earlier, as the several specifications in that table show.[29]

[27]The BJP is a political party that is traditionally associated with a platform of respect for "Hindu values" and the creation of a state based on those values. The rise of the BJP is correlated with the presence and growth of other social organizations that represent "Hindu values".

[28]Wilkinson (2004) argues that the effect of religious dominance in politics at the state level can be reversed at smaller levels such as a particular electoral constituency. In states with narrower margins across religious or caste groups, the government (which is often a coalition of parties) cannot afford to mistreat any minority: they form important vote-banks. With smaller units like a single municipality, this state level effect could well be ignored and tighter religious margins may be more conflictual. A region lies somewhere in between and it is unclear which effect dominates.

[29]In these regressions, the control for the level of education is the primary education completion rate for the region instead of the region's literacy rate as in the other regressions. For urban households, the changes across regions in terms of literacy is marginal and hence primary education seems a better measure of education in this scenario. However, using literacy rather than primary education provides results very similar to those reported in Table 7.

	Poisson			Negative Binomial			OLS		
	[1] Casualties	[2] Killed	[3] Outbreak	[4] Casualties	[5] Killed	[6] Outbreak	[7] Casualties	[8] Killed	[9] Outbreak
Muslim-Hindu exp. ratio	***4.783	0.800	*2.444	**3.883	**3.553	**4.290	6.976**	4.183	4.852**
	(0.000)	(0.640)	(0.089)	(0.011)	(0.014)	(0.010)	(0.034)	(0.166)	(0.018)
Population	4.763	-5.677	0.494	0.747	0.838	0.319	-1.368	-3.537	-0.107
	(0.417)	(0.101)	(0.804)	(0.105)	(0.162)	(0.821)	(0.858)	(0.502)	(0.982)
Per-capita expenditure	-3.356	0.094	-0.187	0.688	1.396	-1.410	0.796	1.542	-0.002
	(0.208)	(0.971)	(0.915)	(0.671)	(0.540)	(0.471)	(0.876)	(0.717)	(0.999)
Religious Polarization	*5.356	1.214	0.300	1.145	0.137	*4.555	5.693	3.304	1.980
	(0.061)	(0.681)	(0.856)	(0.658)	(0.961)	(0.060)	(0.492)	(0.640)	(0.703)
Literacy Rate	0.023	-0.017	-0.024	-0.015	-0.030	-0.036	-0.046	-0.025	-0.036
	(0.212)	(0.590)	(0.301)	(0.426)	(0.410)	(0.136)	(0.525)	(0.710)	(0.302)
Urbanization Rate	-0.021	-0.020	-0.027	0.014	0.008	-0.036	-0.062	-0.101*	-0.057
	(0.261)	(0.453)	(0.239)	(0.400)	(0.724)	(0.182)	(0.311)	(0.062)	(0.183)
Gini: Hindu per-capita exp.	-4.531	-1.899	-2.205	4.203	6.327	4.728	-17.218	-10.732	-10.861
	(0.413)	(0.774)	(0.681)	(0.499)	(0.413)	(0.485)	(0.256)	(0.357)	(0.268)
Gini: Muslim per-capita exp.	4.052	4.768	-0.895	-6.153	-11.169	-9.077	-7.572	-11.536	-9.837
	(0.421)	(0.482)	(0.832)	(0.310)	(0.127)	(0.136)	(0.606)	(0.368)	(0.286)
Log-Lik./Adj. R^2	-3,318.69	-731.72	-149.61	-302.14	-193.29	-128.63	0.334	0.389	0.425
Observations	129	126	132	129	126	132	129	126	132

TABLE 5. The Effect of Hindu and Muslim Expenditures on Regional Conflict: FE regressions with Poisson, Negative Binomial and OLS, respectively (variations with Muslim/Hindu expenditures ratios). *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th, 43rd and 50th rounds. All counts over a five-year period starting immediately after the expenditure data. Casualties = Killed + Injured. Robust standard errors clustered by region; corresponding p-values in parentheses. Time dummies included in all regressions. *significant at 10% **significant at 5% ***significant at 1%

	Poisson			Negative Binomial			OLS		
	[1] Casualties	[2] Killed	[3] Outbreak	[4] Casualties	[5] Killed	[6] Outbreak	[7] Casualties	[8] Killed	[9] Outbreak
Hindu per-capita expenditure	***-6.825	0.012	-2.105	-2.877	-1.942	*-5.315	-7.332	-3.582	**-5.748
	(0.003)	(0.996)	(0.404)	(0.130)	(0.372)	(0.067)	(0.151)	(0.444)	(0.035)
Muslim per-capita expenditure	***4.668	0.804	*2.461	**4.226	**3.815	**4.155	***10.163	**6.780	***6.790
	(0.001)	(0.628)	(0.083)	(0.026)	(0.025)	(0.018)	(0.004)	(0.031)	(0.003)
Population	3.902	*-6.556	0.224	*0.873	0.879	0.522	-0.373	-2.805	0.365
	(0.507)	(0.065)	(0.911)	(0.050)	(0.166)	(0.749)	(0.961)	(0.597)	(0.938)
Religious Polarization	**5.628	3.815	0.396	1.353	0.159	*4.446	5.985	3.690	2.307
	(0.038)	(0.269)	(0.816)	(0.618)	(0.960)	(0.083)	(0.461)	(0.606)	(0.654)
Literacy	0.022	-0.018	-0.025	-0.012	-0.031	-0.037	-0.035	-0.017	-0.029
	(0.244)	(0.575)	(0.270)	(0.586)	(0.353)	(0.132)	(0.601)	(0.794)	(0.404)
Urbanization	-0.017	-0.017	-0.026	0.004	0.004	-0.036	-0.073	*-0.106	-0.062
	(0.352)	(0.451)	(0.227)	(0.809)	(0.869)	(0.136)	(0.235)	(0.057)	(0.159)
Gini: Hindu per-capita exp.	-5.502	-4.856	-2.809	5.210	7.063	5.014	-12.578	-7.888	-8.102
	(0.288)	(0.481)	(0.615)	(0.442)	(0.403)	(0.491)	(0.407)	(0.499)	(0.417)
Gini: Muslim per-capita exp.	3.422	5.128	-0.976	-7.012	-11.311	-9.554	-15.911	-17.745	-14.545
	(0.500)	(0.425)	(0.832)	(0.244)	(0.157)	(0.132)	(0.266)	(0.150)	(0.113)
% Lok Sabha held by BJP	-0.030	*-1.307	-0.098	*1.224	0.993	0.353	*2.547	1.379	1.345
	(0.965)	(0.092)	(0.856)	(0.067)	(0.155)	(0.546)	(0.100)	(0.280)	(0.179)
Log-Lik./Adj. R^2	-3,357.20	-696.20	-149.52	-299.69	-191.91	-128.37	0.365	0.405	0.444
Observations	129	126	132	129	126	132	129	126	132

TABLE 6. The Effect of Hindu and Muslim Expenditures on Regional Conflict: Fixed Effects with BJP Controls. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th, 43rd and 50th rounds. All counts over a five-year period starting immediately after the expenditure data. Casualties = killed + injured). *BJP*: Bharatiya Janata Party. Robust standard errors clustered by region; corresponding p-values in parentheses. Time dummies included in all regressions. *significant at 10% **significant at 5% ***significant at 1%

	[1] Casualties	[2] Casualties	[3] Killed	[4] Killed	[5] Outbreak	[6] Outbreak
Hindu per-capita expenditure	**-5.096		***-6.615		**-3.022	
	(0.024)		(0.001)		(0.019)	
Muslim per-capita expenditure	*3.617		**3.032		0.689	
	(0.056)		(0.032)		(0.312)	
Muslim-Hindu exp. ratio		**3.772		***3.620		*1.076
		(0.042)		(0.004)		(0.075)
Population	3.435	3.465	1.128	1.289	1.105	1.158
	(0.116)	(0.118)	(0.388)	(0.343)	(0.126)	(0.123)
Per-capita expenditure		-2.477		*-3.630		-1.662
		(0.182)		(0.086)		(0.203)
Religious Polarization	***5.057	**4.624	**3.005	*2.655	0.903	0.740
	(0.008)	(0.021)	(0.034)	(0.067)	(0.331)	(0.419)
Primary Education	***12.930	***12.919	***10.274	***10.212	***5.603	***5.481
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Gini: Hindu per-capita exp.	-8.879	-7.302	***19.490	***19.293	2.874	1.770
	(0.265)	(0.335)	(0.002)	(0.001)	(0.445)	(0.605)
Gini: Muslim per-capita exp.	-0.395	1.454	-0.904	0.341	0.498	0.296
	(0.937)	(0.772)	(0.859)	(0.948)	(0.872)	(0.929)
BJP share	0.915	0.928	-0.190	-0.245	0.402	0.353
	(0.144)	(0.152)	(0.724)	(0.654)	(0.498)	(0.550)
Log-Likelihood	-3,064.43	-3,028.97	-487.41	-483.83	-144.11	-144.96
Observations	123	123	117	117	126	126

TABLE 7. The Effect of Hindu and Muslim Expenditures on Regional Conflict (Urban Households only); Poisson with Fixed Effects. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th, 43rd and 50th rounds, *Election Commission of India*. All counts over a five-year period starting immediately after the expenditure data. Casualties = killed + injured. Robust standard errors clustered by region; corresponding p-values in parentheses. Time dummies included in all regressions. *significant at 10% **significant at 5% ***significant at 1%

However, in our opinion, these regressions do not constitute a conceptual improvement over the ones reported previously, i.e. the ones with both rural and urban households. Running these regressions is equivalent to pretending that rural households in India do not exist as far as Hindu-Muslim riots are concerned. While we do know that Hindu-Muslim conflict is primarily an urban phenomenon, we still do have some cases of rural conflict in the data. If we ignore these cases, or pretend they don't exist, we run the risk of a selection problem. Rural regions presumably have the potential for conflict but for certain reasons (greater locational segregation, limited interaction, rigidly-defined social norms, etc.) they may not experience significant realizations of such conflict. Dropping them simply because they exhibit little conflict is ignoring relevant information.

4.4.5. *Different Lags.* Our main specification relates Muslim and Hindu expenditures "today" to subsequent conflict, or more precisely, to a five-year aggregate of conflict starting the following year. It is clear that *some* degree of lagging is necessary, as there are effects running the other way in contemporaneous correlations, a topic that we take up in more detail in Section 5. At the same

	[1] Cas-2	[2] Cas-1	[3] Cas-0	[4] Cas+1	[5] Cas+2	[6] Cas+3
Hindu per-capita expenditure	0.976	0.103	-0.105	***-6.825	***-11.113	***-10.231
	(0.687)	(0.968)	(0.959)	(0.003)	(0.000)	(0.001)
Muslim per-capita expenditure	-0.147	-0.675	*2.361	***4.668	***6.397	***8.322
	(0.915)	(0.624)	(0.085)	(0.001)	(0.000)	(0.000)
Population	5.180	7.364	**7.841	3.902	5.468	4.483
	(0.187)	(0.117)	(0.018)	(0.507)	(0.385)	(0.410)
Religious Polarization	-2.346	-0.864	**5.990	**5.628	**5.699	***6.395
	(0.440)	(0.786)	(0.038)	(0.038)	(0.038)	(0.008)
Literacy Rate	**-0.056	-0.046	0.017	0.022	**0.046	***0.068
	(0.049)	(0.109)	(0.473)	(0.244)	(0.017)	(0.004)
Urbanization Rate	0.008	-0.012	0.011	-0.017	-0.008	0.022
	(0.760)	(0.692)	(0.666)	(0.352)	(0.684)	(0.305)
Gini: Hindu per-capita exp.	**-21.780	**-23.821	***-16.605	-5.502	5.508	8.413
	(0.011)	(0.013)	(0.002)	(0.288)	(0.336)	(0.179)
Gini: Muslim per-capita exp.	-0.117	6.664	6.548	3.422	1.852	**-11.048
	(0.984)	(0.228)	(0.168)	(0.500)	(0.721)	(0.042)
BJP	*1.248	0.069	-0.674	-0.030	-0.056	0.618
	(0.066)	(0.915)	(0.363)	(0.965)	(0.944)	(0.476)
Log-Likelihood	-3,736.07	-4,001.45	-2,904.03	-3,357.20	-3,070.02	-2,904.07
Observations	129	126	129	129	126	123

TABLE 8. The Effect of Hindu and Muslim Expenditures on Regional Conflict; Different Lags: Poisson with Fixed Effects. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th, 43rd and 50th rounds. Conflict is measured by regional aggregates of casualties (killed or injured) over a five-year period. Cas + n means that the 38th round expenditures are matched with conflict during (1983 + n)-(1987 + n) and so on. Robust standard errors clustered by region; corresponding p-values in parentheses. Time dummies included in all regressions. *significant at 10% **significant at 5% ***significant at 1%

time, it is a safe presumption that our effect should die out with very long lags. It is easy enough to explore the effects of different lag structures on our regressions. That is, we match the three expenditures rounds with different 5-year periods of conflict that start with n years into the future. A summary of the results is to be found in Table 8, which reports on $n = -2, -1, 0, 1$ (our baseline case) , 2 and 3.

Observe that "contemporaneous conflict" (column 1)[30] appears to be negatively related to Muslim expenditures although the coefficient on Muslim expenditures is not significant. We take this question up more carefully below. As the lag is increased, the sign switches and turns positive. In fact, we note that it is significant and positive for five-year aggregates of conflict for several lag

[30]It is contemporaneous in the sense that the 38th round (1983) is matched with conflict during 1981-85, the 43rd round (1987-88) is matched with conflict during 1986-90 and the 50th round (1993-94) is matched with conflict during 1991-1995.

structures (columns 3 – 6). For lags larger than the ones we have chosen, the positive relationship diminishes and then any association between the variables progressively disappears. These results are testimony to the robustness of our basic findings.

There are several other variants that we do not report upon; the majority of which continue to yield the same results, both in magnitude and significance.

5. CONCERNS

We raise three concerns, and describe what we do to alleviate them.

5.1. **Endogeneity.** While we explicitly regress conflict over a five-year period on *anterior* Hindu and Muslim expenditure, the question of endogeneity needs to be addressed. Even though we connect expenditure change to conflict starting a year later, conflict may well be serially correlated. For instance, some regions do exhibit violence more persistently over time than others, and besides, there is truth to the aphorism that "violence begets violence". To be sure, the region-specific fixed effects are meant to capture the time-invariant features of the region which make it violence-prone. But of course, the effects that we're referring to are not generally time-invariant.

If conflict starting a year later is highly correlated with conflict today, there is effectively the possibility of reverse causation. But Section 2 makes it clear that conflict destroys Muslim property and wealth, and therefore reduces Muslim expenditure; see, e.g., Engineer (1984, 1994), Rajgopal (1987), Bagchi (1990), Khan (1992), Thakore (1993), Brass (1997), Das (2000), Engineer (1994), and the excellent summary of these and others in Wilkinson (2004). That the impact is negative is not very surprising as Muslims are a minority and happen to be poorer on average than their Hindu counterparts. It stands to reason that they would be less able to protect their lives and property in the event of a religious riot. However, the lagged relationship we obtain between Muslim expenditure and conflict in every one of the tables so far *is just the opposite*. There is just one exception to this, in Table 8, where regressions of conflict on *contemporaneous* Muslim expenditure yield a negative (though not significant) coefficient, in line with this discussion.

In short, the particular concern of reverse causation appears to runs the other way.

But the serial correlation of conflict could be more subtle, with episodes of conflict followed by periods of relative quiescence. Consider an episode of conflict that depresses Muslim income. In the quiet period that follows, incomes would recover. If conflict flares up again along the violence-peace-violence cycle, that might generate a situation in which Muslim expenditures are positively correlated with future conflict. To be sure, such an argument, even without the empirical resolution we attempt below, rests on a somewhat delicate conceptual foundation. For expenditures to revert to pre-conflict levels, conflict must be temporary. Yet, for those expenditures to be correlated with (without causally influencing) *future* conflict, current and future conflict must be positively correlated. The greater this correlation, the less space there is for the mean-reversion to occur to begin with. But the possibility exists.

Another omitted variable is elite funding of conflict, particularly by inflows from the Gulf countries. To some extent, overall changes in Gulf fortunes are subsumed in the time dummies, but not to the extent that different Indian regions are differentially represented in the Gulf. Because remittances also flow for peaceful purposes, Muslim expenditures (which presumably include the effect of those remittances) could be correlated with Gulf funding for other, conflictual objectives. To address these issues, we undertake two separate exercises, with the second building on the first.

5.1.1. *2SLS With Hindu and Muslim Income Indices.* We begin with a broad classification of occupational groupings, and then use the NSS data on expenditures (by religion and occupation) to form proxies of *national* average returns for Hindus and Muslims in each occupational class. For each religion, construct the weighted average of these national returns, with weights given by the regional employment share over occupational groups. That generates an "income index" for each region and each religion. Changes in occupational composition at the regional level, coupled with changes in national returns by occupation, will affect these indices regionally. We use them as instruments for Hindu and Muslim expenditures. Specifically, we take the ratio of the Muslim income index to the Hindu income index and use it as an instrument for the ratio of Muslim to Hindu expenditures at the regional level.

We therefore attempt to exploit the fact that Muslims and Hindus are concentrated differently over occupational classes. Presumably, such differential concentration stems from their specialization — over time — in different activities, their acquisition of skills and so on.[31]

What does this approach handle? By computing the average returns to occupations at the national level, we presumably obtain numbers that are acceptably exogenous to events at the regional level. It certainly takes out the endogeneity generated by reverse causation and omitted variables, to the extent that these factors precipitate lower expenditure via the impact on property and wealth during conflict. Moreover, the concern about (external) funding from the Gulf is also handled by the use of this occupation-based index, since the remittances channel does not show up in the occupational structure. Nevertheless, the index still allows for occupational changes at the regional level. Might this fail the exclusion restriction?

In principle, it might, as occupational structure by religion might conceivably be influenced by conflict. To avoid this potential objection, we employ a rather broad partition of occupations. We use a total of 18 broad-brush sectors, such as "Agricultural Production and Plantations", "Manufacture of Rubber, Plastic, Petroleum, Coal ; Chemicals and Chemical Products", "Manufacture of Textiles (Cotton; Wool, Silk, Artificial; Jute, Veg. Fibre; Textile Products)", "Manufacture of Transport Equipments and Parts", to name a few.[32] That is, just 18 sectors are used to partition the entire labor force of India.

[31]For instance, an occupational class such as "Manufacture of Leather and Leather Products" has a disproportionate share of Muslims — in relation to their population numbers — and this is true across all the different time periods.

[32]The 18 sectors are: (1) Agricultural Production and Plantations, (2) Livestock Production, (3) Fishing, (4) Mining and Quarrying (Coal; Crude Petrol and Natural Gas; Metal Ore; Other), (5) Manufacture of Food Products and Inedible Oils, (6) Manufacture of Beverages, Tobacco and Tobacco products, (7) Manufacture of Textiles (Cotton; Wool, Silk, Artificial; Jute, Veg. Fibre; Textile Products), (8) Manufacture of Wood and Wooden Products, (9) Manufacture of Paper, Paper Products, Publishing, Printing and Allied Industries, (10) Manufacture of Leather, and of Leather and Fur Products, (11) Manufacture of Rubber, Plastic, Petroleum, Coal ; Chemicals and Chemical Products, (12) Manufacture of Non-Metallic Mineral Products, (13) Basic Metal and Alloy Industries, (14) Manufacture of Metal Products and Parts, except Machinery and Transport Equipments, (15) Manufacture of Machinery, Machine Tools and Parts except Electrical Machinery, (16) Manufacture of Electrical Machinery, Appliances, Apparatus and Supplies and Parts, (17) Manufacture of Transport Equipments and Parts and (18) Other Manufacturing Industries.

	First Stage			Second Stage		
	[1] Casualties	[2] Killed	[3] Outbreak	[1′] Casualties	[2′] Killed	[3′] Outbreak
Muslim index/Hindu index	***0.782	***0.779	***0.759			
	(0.001)	(0.001)	(0.002)			
Muslim-Hindu exp. ratio				***26.831	***24.972	***16.592
				(0.004)	(0.006)	(0.010)
Per-capita expenditure	*-0.593	*-0.597	*-0.544	13.989	14.798	7.209
	(0.079)	(0.082)	(0.089)	(0.131)	(0.115)	(0.188)
Population	-0.164	-0.167	-0.224	3.805	1.711	3.400
	(0.453)	(0.445)	(0.311)	(0.651)	(0.818)	(0.528)
Religious Polarization	**-0.468	**-0.476	*-0.406	12.236	10.778	5.398
	(0.046)	(0.042)	(0.087)	(0.174)	(0.195)	(0.348)
Literacy Rate	-0.002	-0.002	-0.002	0.005	0.024	-0.004
	(0.401)	(0.425)	(0.343)	(0.953)	(0.791)	(0.938)
Urbanization Rate	-0.002	-0.002	-0.002	-0.052	-0.084	-0.052
	(0.331)	(0.349)	(0.410)	(0.459)	(0.158)	(0.306)
Gini: Hindu per-capita exp.	***-1.286	***-1.279	***-1.370	1.822	8.218	1.097
	(0.002)	(0.003)	(0.001)	(0.921)	(0.593)	(0.928)
Gini: Muslim per-capita exp.	***2.772	***2.790	***2.767	**-67.179	**-72.737	**-44.728
	(0.000)	(0.000)	(0.000)	(0.031)	(0.015)	(0.033)
% Lok Sabha held by BJP	-0.012	-0.014	-0.016	2.584	1.598	1.377
	(0.820)	(0.799)	(0.755)	(0.111)	(0.247)	(0.197)
R^2	0.613	0.613	0.606	0.056	-0.048	0.222
F-statistic (first stage)	10.63	10.53	9.64			
Observations	129	126	132	129	126	132

TABLE 9. The Effect of Hindu and Muslim Expenditures on Regional Conflict: 2-SLS IV regressions. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th, 43rd and 50th rounds. All counts over a five-year period starting immediately after the expenditure data. Dependent variable in the first 3 columns (denoting the 1st stage relationship) is the ratio of Muslim to Hindu expenditures: each column corresponds to a sample of regions based on the choice of conflict variable. Column 1 reports the 1st-stage relationship where the sample includes all regions which have experienced positive *Casualties* in at least one period; columns 2 and 3 do the same for *Killed* and *Outbreak*. Columns 4–6 report the second-stage results for the three different measures of conflict. Robust standard errors clustered by region; corresponding p-values in parentheses. Region-specific effects and time dummies included in all regressions. *significant at 10% **significant at 5% ***significant at 1%

Indeed, occupational groupings in the NSS data are based on a detailed classification (using the National Industrial Classification codes), and we could have used finer subdivisions if we so wished. (For instance, we could treat "Manufacture of Cotton Textiles" as a separate sector.) But that would create greater pressure on the exclusion restriction, because it is easier to argue that conflict could affect the labor force in those finer subdivisions.[33] So, even if we allow for the possibility that Muslim workers may be driven out of, say, some segments of the cotton textile industry, it is far harder to argue that such an effect would show up for the *entire* textiles sector, as defined here. (Though we do not doubt that there are identification purists who are up to the task, and for them we take further steps in Section 5.1.2.) The displacement effect is more likely to operate at a finer occupational level than the deliberately coarse partition we employ.

We deploy our instrument in Table 9. We use all three of our dependent variables:*casualties*, *killed* and *outbreak*. Recall that the sample of regions is slightly different based on the measure of conflict as regions reporting overall zero conflict (when summed over the 3 periods) are dropped.[34] We therefore report separate first-stage relationships for each of the three conflict variables in columns [1]–[3] of the table. As these relationships reveal, the instrument is positively and significantly correlated with our potentially endogenous explanatory variable, with a coefficient of around 0.77. The next three columns report the corresponding second-stage relationships. In each of the columns [$1'$]–[$3'$], the coefficient on the instrumented ratio of Muslim to Hindu expenditures is positive and highly significant, entirely in accordance with our previous results.

5.1.2. *GMM With Lagged Conflict and Income Indices.* To explicitly allow for the possibility that past conflict might affect expenditures, we turn to the linear system GMM estimation procedure for dynamic panels suggested by Arellano and Bover (1995) and further developed by Blundell and Bond (1998). This estimation method recognizes the fact that current expenditures may be affected by previous conflict, so that expenditures, while predetermined, are not strictly exogenous. The idea is to use lagged expenditures as instruments for current expenditures after first-differencing (to eliminate unobserved fixed effects). Using this procedure a two-step system GMM estimator is developed based on appropriate moment conditions for both sets of equations — in first-differences and in levels.[35] In addition, we include the Hindu and Muslim income indices described above in the set of instruments.

Moreover, this method is designed to provide consistent estimates for dynamic panels which include a lagged dependent variable as a regressor for short panels ($N \to \infty$ while T is fixed). Table 10 contains some results based on this estimation procedure. In the first two columns of this table, we report results for the dependent variable *casualties* without explicitly controlling for previous levels of conflict. In the remaining four columns, we control for past levels of violence, thereby exploiting the full advantages of the system GMM estimation procedure. In each specification, the (robust) standard errors are based on the two-step GMM estimation method with bias-correction as proposed by Windmeijer (2005). In columns 3–6, the large p-values associated with the Hansen J-test for overidentification are reassuring, as are the small p-values for the measure of overall fit.

[33] In addition, the finer the classification the lower the number of households in each sector leading to small-sample biases.

[34] Hence we have 129 observations (43 regions) for *casualties*, 126 observations (42 regions) for *killed* and 132 observations (44 regions) for *outbreak*.

[35] See Cameron and Trivedi (2005) and Roodman (2006) for a detailed formal exposition.

	[1] Casualties	[2] Casualties	[3] Casualties	[4] Casualties	[5] Killed	[6] Outbreak
Hindu per-capita expenditure	***-14.092		-2.112		-4.708	0.628
	(0.008)		(0.726)		(0.234)	(0.423)
Muslim per-capita expenditure	**10.261		**11.427		***9.485	**1.359
	(0.035)		(0.013)		(0.000)	(0.029)
Muslim expenditures/ Hindu expenditures		*8.587		**11.518		
		(0.085)		(0.035)		
Average per-capita expenditure		***-2.381		**9.515		
		(0.003)		(0.010)		
Population	**2.421	**2.288	***4.489	***4.672	***4.058	***0.839
	(0.038)	(0.013)	(0.000)	(0.000)	(0.000)	(0.000)
Religious Polarization	7.725	*9.698	2.838	0.064	0.809	0.150
	(0.270)	(0.054)	(0.586)	(0.989)	(0.836)	(0.825)
Literacy rate	0.040	0.006	0.046	0.055	0.040	-0.009
	(0.576)	(0.897)	(0.427)	(0.311)	(0.368)	(0.345)
Urbanization rate	-0.095	-0.080	**-0.132	*-0.103	-0.086	**-0.023
	(0.267)	(0.160)	(0.013)	(0.079)	(0.171)	(0.034)
BJP seatshare	*2.925	**2.379	***7.609	***7.070	***4.702	**0.683
	(0.098)	(0.043)	(0.000)	(0.000)	(0.000)	(0.049)
Gini: Hindu per-capita expenditure	13.064	17.659	12.337	13.347	13.734	3.034
	(0.596)	(0.314)	(0.330)	(0.290)	(0.294)	(0.270)
Gini: Muslim per-capita expenditure	-25.417	-21.483	-11.341	-14.343	-7.823	-1.970
	(0.290)	(0.302)	(0.280)	(0.304)	(0.538)	(0.450)
Past Casualties			-0.116	-0.107		
			(0.369)	(0.416)		
Past Killed					-0.091	
					(0.460)	
Past Outbreaks						***0.307
						(0.009)
Overall fit, P$>chi^2$	0.000	0.000	0.000	0.000	0.000	0.000
Hansen J test, P$>chi^2$			0.661	0.537	0.368	0.526
Observations	129	129	86	86	84	88

TABLE 10. The Effect of Hindu and Muslim Expenditures on Regional Conflict: Linear Dynamic Panel, 2-step GMM estimation. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th, 43rd and 50th rounds. All counts over a five-year period starting immediately after the expenditure data. Robust standard errors corrected according to Windmeijer (2005); corresponding p-values in parentheses. Muslim Index, Hindu Index and ratio of the Muslim to Hindu indices have been introduced in the set of exogenous instruments for Muslim expenditures, Hindu expenditures and Muslim/Hindu expenditure ratio, respectively. Time dummies included in all regressions. Past conflict corresponds to years 1979-83 for the 38th round (1983), 1984-88 for the 43rd round (1987-8) and 1989-93 for the 50th round (1993-4). *significant at 10% **significant at 5% ***significant at 1%

In all the columns of Table 10, we note that the coefficient on Muslim expenditures is positive and significant, in line with our previous findings. This bolsters our confidence in our prior results as we note that even after explicitly controlling for a separate channel from past conflict to expenditures, we find that the effect of Muslim expenditures on conflict is positive and significant. The effect of Hindu expenditures is sometimes significant, and generally negative.

In particular, the possibility that past conflict affects movement across occupational sectors should be adequately addressed by the GMM approach, and should serve to assuage any (remaining) concerns about this instrument failing the exclusion restriction. If the *causal* relationship between conflict and Muslim expenditures runs from conflict to expenditures and not the other way round, then including past conflict as a control in our regressions must eliminate any significant coefficient on Muslim expenditures. But that is not what we observe in our regressions; the coefficient on Muslim expenditures continues to be positive and significant when previous conflict is included as a control.

5.2. **Religious Rioting Versus General Rioting.** It might be argued that a rise in Muslim expenditure (controlling for Hindu expenditure), or more generally a rise in the ratio of Muslim to Hindu expenditures, is just a proxy for overall Hindu stagnation, which could be associated with an increase in social unrest quite generally, and not just in Hindu-Muslim conflict. This argument would maintain that a concomitant rise in Hindu-Muslim conflict is just a by-product of this overall uptick in social unease, and could therefore not be interpretable as *directed* violence against a specific community.

One can test this hypothesis in many ways. We do so by using the Government of India dataset on Crime in India, which has data on "all riots". That would presumably include but not be limited to Hindu-Muslim riots.[36] Though Hindu-Muslim riots must form an important component of "all riots", it is by no means the dominant component. There are numerous sources of unrest in a country as culturally diverse as India. Examples include caste conflict (between upper and lower caste Hindus), Maoist insurgencies (often taking the form of a class struggle), separatist uprisings (in the forms of ethnic groups demanding an "autonomous state": Bodoland, Gurkhaland, Telengana, etc.), conflicts over land, and all sorts of political clashes.

We run a variety of specifications to evaluate this argument. Throughout, we use a full set of controls, and we run separate regressions for absolute levels of expenditure and their ratios. For each of these, we use the Poisson, negative binomial and OLS specifications, just as in the baseline Table 3. The results are reported in Table 11. There is no significant effect of Muslim expenditures on "all riots". In particular, a rise in Muslim expenditure (controlling for Hindu expenditure), or an increase in the ratio of Muslim to Hindu expenditures, is *not* associated with a increase in overall social unrest. If anything, it appears that a rise in Hindu expenditures is associated with *increased* overall rioting, which is in stark contrast with the results obtained in the specific case of religious violence.

5.3. **A Question of Interpretation.** We use the theory in Section 3 to interpret the empirical findings. Recall that we describe a two-group model in which aggressors in each group can initiate a conflict with victims in the other group. We've argued that a balanced increase in the incomes of a group should lead to unambiguously higher levels of attacks being perpetrated against them. (There

[36] It is important to note that this dataset does not have specific information regarding Hindu-Muslim violence.

	[1] Poisson	[2] Poisson	[3] Neg. Bin.	[4] Neg. Bin.	[5] OLS	[6] OLS
Hindu per-capita expenditure	***0.754		-0.525		0.374	
	(0.007)		(0.448)		(0.467)	
Muslim per-capita expenditure	-0.189		-0.117		-0.123	
	(0.301)		(0.607)		(0.617)	
Muslim-Hindu exp. ratio		-0.233		-0.087		-0.121
		(0.202)		(0.702)		(0.642)
Per-capita expenditure		*0.519		-0.677		0.394
		(0.072)		(0.243)		(0.287)
Population	0.057	0.056	0.497	0.519	0.734	0.704
	(0.910)	(0.912)	(0.221)	(0.149)	(0.314)	(0.336)
Religious Polarization	*-0.641	*-0.623	0.199	0.171	0.118	0.135
	(0.051)	(0.056)	(0.721)	(0.744)	(0.839)	(0.815)
Literacy rate	-0.000	-0.000	-0.000	-0.000	0.003	0.003
	(0.942)	(0.930)	(0.978)	(0.961)	(0.261)	(0.222)
Urbanization rate	***0.013	***0.012	0.002	0.002	0.001	0.001
	(0.001)	(0.001)	(0.726)	(0.731)	(0.869)	(0.877)
Gini: Hindu per-capita exp.	**-1.632	*-1.562	0.846	0.842	0.190	0.138
	(0.046)	(0.058)	(0.594)	(0.562)	(0.902)	(0.928)
Gini: Muslim per-capita exp.	-0.735	-0.764	0.345	0.355	0.606	0.545
	(0.307)	(0.293)	(0.717)	(0.671)	(0.441)	(0.495)
Log-Likelihood	-14,754.24	-14,840.02	-910.85	-910.82		
Adjusted R^2					0.032	0.036
Observations	165	165	165	165	165	165

TABLE 11. The Effect of Hindu and Muslim Expenditures on All Regional Riots: FE regressions with Poisson, Negative Binomial and OLS, respectively. *Sources and Notes. National Sample Survey* 38th, 43rd and 50th rounds; Govt. of India dataset on crime. Conflict is measured by regional aggregates of casualties (killed + injured) over a five-year period starting immediately after the expenditure data. Standard errors clustered by region; corresponding p-values in parentheses. Time dummies included in all regressions. *significant at 10% **significant at 5% ***significant at 1%

is more to loot, or greater urgency to exclude, or more reasons to hate.) In contrast, an increase in incomes reduces attacks perpetrated *by* that group. (The opportunity cost of violence increases.) The theory therefore permits the following interpretation of our empirical results. The fact that Muslim expenditures display a strong and positive connection with conflict, while Hindu incomes have just the opposite effect, allows us to suggest that Hindu groups have been more responsive to economic considerations in precipitating Hindu-Muslim violence in post-Independence India.

This interpretation is in line with the Indian experience. Section 2 refers to several case studies which describe a Hindu reaction to what they perceive as the invasion of their occupational or entrepreneurial turf by Muslims. It is possible that in other situations the opposite may have been true, but as we've already noted, Muslims come off far worse, on average, in conflictual encounters.

This is not surprising, as Indian Muslims constitute a minority. At the same time, it is impossible to be sure from the case studies alone that one group has engaged in a more systematic perpetration of economic violence than the other. Most incidents follow earlier incidents in a cycle of retribution, and no incident can be viewed in isolation. The question of whether one group has engaged "more systematically" in violence cannot be answered through case studies alone, though these provide valuable indicative information. We view our results as adding to this body of evidence.

One counterargument to our interpretation is that the positive impact of Muslim expenditures on violence stems from *Muslim*, not Hindu aggression. Specifically, rising Muslim incomes make it easier to fund conflict, outweighing the negative opportunity cost effects of direct participation. That, too, would generate a positive relationship between the income of an aggressor group and subsequent conflict.

While hard empirical information on the funding of religious conflict does not appear to be available, it is reasonable that financial resources play a role in organized violence. The question is whether it is this phenomenon that lies behind the correlations we do observe. The fact that the Hindu coefficient is negative means that if "funding effects" are responsible for what we see, they are somehow observed only for Muslim groups while the effect is entirely obliterated and reversed for Hindu groups. That is possible, but in light of the fact that Muslims are by far the larger losers in outbreaks of violence, the simultaneous conjunction of all these explanations — Muslims fund conflict, Hindus don't, Hindus are on average richer, Muslims are by far the bigger per-capita losers — appears unlikely.

Next, there are good reasons to suppose that "funding effects" will not heighten conflict in situations of balanced growth in group incomes, which is precisely the focus of our empirical exercise. To see this, recall the analysis from Section 3, and reconstruct the attack function to include two alternatives: direct participation in violence, and the funding of violence. These two alternatives are affected very differently by an increase in income. Direct participation is reduced (the opportunity cost of time goes up), while the funding of violence is increased (the opportunity cost of financial contributions is lowered).[37] To capture the latter effect we now endow the attacker with a strictly concave utility function u, which we take to have constant elasticity: $u(c) = c^{1-\sigma}/(1-\sigma)$ for $\sigma > 0$.[38]

As before, suppose that a potential aggressor with income z must decide whether or not to inflict violence on an individual with income y. He can do so via direct participation, which involves a time opportunity cost of t. Or he can fund an equivalent amount of violence by paying for the opportunity cost of someone else's time at the rate of f. The net return to "participatory violence" is then

$$d(z) = (1-p)u([1-t]z) + pu([1-t]z + \lambda y),$$

while the net return to "funded violence" is given by

$$m(z) = (1-p)u(z-f) + pu(z-f+\lambda y),$$

where, just as we had earlier, λy stands for the economic equivalent of the spoils from a victim with income y in the event of a successful attack.

[37]Esteban and Ray (2011) develop these observations to connect within-group inequality to violence.

[38]When $\sigma = 1$, u is logarithmic.

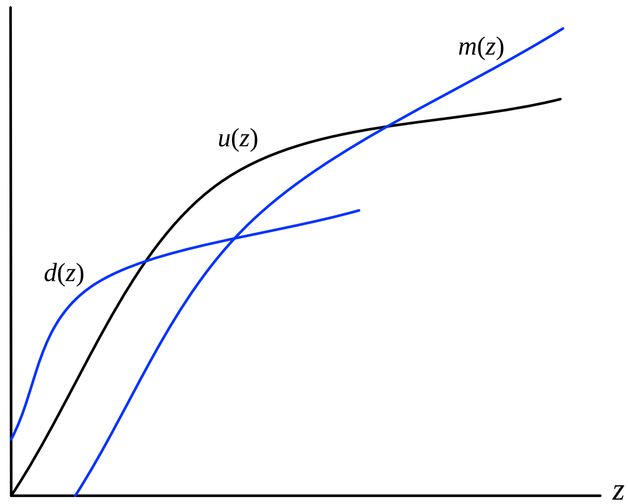

FIGURE 5. PARTICIPATION AND FUNDING: LOW AND HIGH INCOMES

The question is what a balanced growth in group incomes does to the funding requirement f. If violence is principally carried out by *individuals from the same group as the aggressor*, then f must rise in roughly proportional fashion to z. We then have

PROPOSITION **2.** *A balanced increase in group incomes that causes both the funding requirement f and aggressor income z to rise in equal proportion, must reduce attacks perpetrated by members of that group.*

An increase in z raises the opportunity cost of time, thereby reducing participatory violence. As for funded violence, a proportionate increase of f with z keeps the opportunity cost of violence constant as a proportion of z. At the same time, the looting of a victim with the same income y appears less attractive in relative terms. So in both cases, violence perpetrated *by* the group must decline. (The formal arguments rely on the constant-elasticity formulation of utility; see Appendix.)

Faced with Proposition 2, it is still possible to assert either that paid attackers are not from the same religious group, or that funding pays for non-human inputs into violence.[39] In either case f will not change (or won't change in proportion) when z does. We would actually contend that same-group human input *is*, in fact, central to funded violence, but we can go a step further by considering an implication of this argument: that the funding effect must be stronger at higher levels of income.

PROPOSITION **3.** *Suppose that both funding and direct participation can be used to generate a violent attack, and that f does not change with z. Then:*

(a) *If funded violence is preferred to participatory violence at income z, the same preference is maintained for all $z' > z$.*

(b) *If funded violence is preferred to peace at income z, then it is preferred for all higher incomes.*

(c) *If participatory violence is preferred to peace at income z, then it is preferred for all lower incomes.*

[39]The latter argument is analogous to that carried out for the protection function in Section 3.

	OLS			Poisson		
	[1] All	[2] Non-Low M/H	[3] Non-High M/H	[4] All	[5] Non-Low M/H	[6] Non-High M/H
Hindu per-capita expenditure	*-8.462	**-10.057	*-10.213	***-6.824	**-5.132	***-7.180
	(0.085)	(0.037)	(0.061)	(0.003)	(0.019)	(0.003)
Muslim per-capita expenditure	***9.523	***10.549	**9.152	***4.670	**3.312	***4.798
	(0.009)	(0.004)	(0.021)	(0.001)	(0.015)	(0.001)
Population	-1.230	-3.468	-2.254	3.914	-4.329	3.620
	(0.877)	(0.630)	(0.784)	(0.496)	(0.118)	(0.538)
Religious Polarization	6.680	5.602	5.788	*5.566	1.831	*5.427
	(0.408)	(0.588)	(0.505)	(0.056)	(0.366)	(0.071)
Literacy rate	-0.043	-0.016	-0.025	0.023	0.025	0.021
	(0.552)	(0.834)	(0.736)	(0.242)	(0.258)	(0.285)
Urbanization rate	-0.055	-0.078	-0.069	-0.017	*-0.037	-0.010
	(0.371)	(0.287)	(0.322)	(0.354)	(0.055)	(0.576)
Gini: Hindu per-capita exp.	-14.473	-16.791	-13.936	-5.426	2.010	-5.656
	(0.342)	(0.328)	(0.388)	(0.317)	(0.719)	(0.295)
Gini: Muslim per-capita exp.	-11.073	-17.319	-9.558	3.399	5.466	3.950
	(0.451)	(0.250)	(0.549)	(0.497)	(0.222)	(0.429)
Adjusted R^2	0.348	0.398	0.343			
Log-Likelihood				-3 357.29	-1 680.59	-3 247.35
Observations	129	90	120	129	90	120

TABLE 12. The Effect of Hindu and Muslim Expenditures on Conflict in Regions with Varying Ratios of Muslim to Hindu Expenditure: OLS and Poisson FE regressions. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th, 43rd and 50th rounds. All counts over a five-year period starting immediately after the expenditure data. Dependent variable is regional casualties (killed+injured). Robust standard errors clustered by region; corresponding p-values in parentheses. Time dummies included in all regressions. Column 1 reports the result for all regions. Column 2 pertains to all regions minus those regions which have Muslim/Hindu expenditure ratios that are systematically *lower* than the national average in each of the 3 periods (hence termed “Non-Low M/H” regions). Column 3 pertains to all regions minus those regions which have Muslim/Hindu expenditure ratios that are systematically *higher* than the national average in each of the 3 periods (hence termed “Non-High M/H” regions). Columns 4, 5 and 6 have regions analogously defined for the Poisson regressions. *significant at 10% **significant at 5% ***significant at 1%

Figure 5 illustrates the proposition, which we prove formally in the Appendix. Payoff from participatory violence dominates all other payoffs at low incomes, while payoffs from funded violence dominate at high incomes. This makes perfect sense once we consider the opportunity costs of time and money. It follows that when the distribution of income is concentrated on relatively low incomes, an increase in aggressor income is likely to result in a decline in violence, as individuals move from the first zone of participatory violence into the second zone of peace. The opposite effect holds when incomes are relatively high: individuals will move from the peace zone into the funded violence zone.

That suggests a test: if funded violence drives the positive relationship between Muslim expenditures and subsequent conflict, then Proposition 3 applies, and *the positive effect should be stronger in richer regions*. Table 12 provides one way of examining this relationship. We define a region to be "low" if Muslim/Hindu expenditure ratios in that region are systematically lower than the national average for each of the three time periods. Removing the low regions gives us the *non-low* regions. In similar fashion, we may define the *non-high* regions.[40]

Table 12 shows that there is no discernible difference in the strength of the Muslim coefficient across the two sets of regions. If we believe the funding argument, we must also accept the conclusion that the poorer regions are funding conflict to the same degree as the richer regions, even after controlling for inequality, an observation that runs up against Proposition 3.

6. Summary and Concluding Remarks

Our empirical investigations yield a central result, which we explore from a number of angles. An increase in Muslim well-being, measured by per-capita Muslim expenditures, leads to a *large and significant* increase in religious conflict in the short to medium run; specifically, in a five-year aggregate of conflict starting from the very next year (relative to the expenditure year). In contrast, an increase in Hindu well-being has either an insignificant or a negative effect on future conflict. We obtain this finding using a three-period Indian panel with region and time effects employed throughout.

This result is robust along a number of dimensions. It is robust to different measures of religious conflict: numbers killed, numbers killed+injured, or coarser outcomes such as the number of riots. It persists whether we use the absolute values of Hindu and Muslim expenditures, or their ratio. It is robust to the inclusion of several controls, such as literacy rates and the degree of urbanization. It is robust to the inclusion of political variables, such as the share of the BJP in total Lok Sabha seats.

Our finding is also robust to the use of alternative lag structures, as long as we focus on conflict *following* the change in expenditure. That isn't surprising, as the contemporaneous relationship between conflict and Muslim per-capita expenditure is negative, a phenomenon well-documented in studies that show that Muslims suffer disproportionately from religious violence in India. In the light of this fact, it is remarkable that the association between Muslim per-capita expenditures

[40]We take this approach so as to preserve a relatively large number of regions in each subcase, otherwise we quickly run out of statistical power. We have tried several alternative specifications with no essential difference in the results. Indeed, in some specifications the results support our contention to a greater degree, but Table 12 uses the data in closest conformity to the rest of the text.

and *subsequent* conflict is precisely reversed, and turns positive. Indeed, a rise in Muslim per-capita expenditures seems to increase conflict starting from the very next year and for some years onwards.

Relatedly, we discuss the question of endogeneity in some detail. We instrument for the ratio of Muslim to Hindu expenditures using an index that we construct from data on occupational structure. Next, we use two-stage GMM estimation with lagged expenditure and our occupational income indices as joint instruments. In addition, we introduce controls for current conflict. In all of these exercises, our results persist.

As a final check, we show that a parallel investigation for *all* riots in India — which include but are by no means restricted to Hindu-Muslim riots — show no systematic relationship between Muslim per-capita expenditures and conflict. The relationship we uncover is specific to riots between two religious groups, and not conflict in general.

We haven't run a randomized experiment, not even a natural one. Therefore it is entirely possible (especially with some stretching of the creative imagination) to offer up a variety of explanations that are compatible with this finding. Alternatively, one might choose to treat these results as a curiosum of interest in its own right, and leave it at that without resorting to interpretation.

Our preferred interpretation is based on the theory outlined in Section 3. But that theory can be placed in a context. There are many case studies in which attacks on the Muslim community can be traced to various forms of Muslim economic empowerment; see the references in Section 2. Moreover, we've shown in Section 5.3 that alternative theories, such as conflict created by the funding of Muslim groups, are not consistent with the available empirical evidence.

An ongoing (and not entirely coolheaded) conversation is invariably present in India over which side is largely "at fault" when religious violence breaks out. This debate, as one might imagine, is politically and emotionally charged, and the "evidence" offered up in one reading is predictably challenged by another. Some incidents, such as the demolition of the Babri Masjid in 1992 or the attacks in Mumbai in 2008, are relatively clear-cut in the immediate identity of their initiators, though — to be sure — their antecedents may go back a long way. Other incidents can be traced to still earlier incidents along the well-worn trail of revenge and retribution, and there is no clear-cut perpetrator.

To some, the question of whether there is systematic perpetration by one group is a politically loaded question to which only an ideological answer is possible. No incident can be viewed in isolation, and it is easy enough to argue that a particular episode has roots that have been conveniently ignored by the ethnographer. Perhaps there is no such thing anyway as systematic perpetration "by one side".

But — while important — it is unclear that all conflict is driven by chicken-and-egg-like processes, with their original roots entirely lost in time. Those familiar with particular histories, such as the modern history of India, will know that there are group leaders on either side of the Hindu-Muslim conflict that have *systematically* attempted to take advantage of inter-group tensions. To understand whether one group has done so "more systematically" than the other is not just important from a policy perspective, it is crucial to our intellectual understanding of the politics of a society, and to the policies that one must adopt. It is also important to note that we uncover an asymmetry in the sensitivity or response of violence to economic change. It is indeed possible that such an asymmetry can be compatible with a symmetric level of "baseline" violence perpetrated by both groups.

Finally, we do not believe that a particular religious group is intrinsically more predisposed to the use of violence. Our personal opinion is that religious fundamentalists are of the same ilk everywhere. Yet particular histories do condition subsequent events. In the Hindu-Muslim case under discussion, the Partition of India may provide a useful clue. It has been argued that Muslims in India, far from being acknowledged as showing their greater loyalty to India by staying, are constantly under pressure to demonstrate their "Indianness". While extremist Islamic groups are undoubtedly active, the majority of Muslims constantly live under the pressure to prove their loyalty, and go out of their way to maintain communal harmony. Hindu fundamentalist groups face no such constraint. That, coupled with the sheer realities of demography, might explain the results we obtain. In a parallel universe, with a different history and a different demography, the outcomes may well have been very different.

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APPENDIX

Proof of Observation 1. First we show that the protection function is downward-sloping. Recall that d is chosen to minimize

$$\alpha(\mu-\beta)p(d)+[c(d)/y],$$

where $\mu-\beta>0$. Pick two values of α, call them α_1 and α_2, with $\alpha_2>\alpha_1$. Let d_1 and d_2 be two corresponding minima. Certainly,

$$\alpha_1(\mu-\beta)p(d_1)+[c(d_1)/y]\leq\alpha_1(\mu-\beta)p(d_2)+[c(d_2)/y],$$

while at the same time,

$$\alpha_2(\mu-\beta)p(d_2)+[c(d_2)/y]\leq\alpha_2(\mu-\beta)p(d_1)+[c(d_1)/y],$$

Combining these two inequalities, we must conclude that

$$(\alpha_2 - \alpha_1)[p(d_2) - p(d_1)] \leq 0.$$

It follows that $p(d_2) \leq p(d_1)$, as required.

The fact that the attack function is (weakly) increasing is an immediate consequence of (3). It will be strictly increasing when the cdf F is strictly increasing everywhere.

Finally, the graphs of both functions can be made continuous by spreading individuals in different proportions over their optimal actions (in case the best-response is multi-valued somewhere). Moreover, the relevant endpoint conditions are met. So a unique equilibrium exists. ■

Proof of Proposition 2. Note that the condition for participatory violence is given by

$$(1-p)\frac{([1-t]z)^{1-\sigma}}{1-\sigma} + p\frac{([1-t]z + \lambda y)^{1-\sigma}}{1-\sigma} \geq \frac{z^{1-\sigma}}{1-\sigma},$$

and dividing through by $[(1-t)z]^{1-\sigma}$ on both sides, we get

$$\frac{1-p}{1-\sigma} + p\frac{\left[1 + \frac{\lambda y}{(1-t)z}\right]^{1-\sigma}}{1-\sigma} \geq \frac{1}{(1-t)^{1-\sigma}}. \tag{4}$$

We see immediately from (4) that there exists a unique threshold value z^* such that participatory violence is preferable to peace for $z < z^*$, while the opposite is true when $z > z^*$.

When f is proportional to z, exactly the same observation goes through for funded violence. Simply define $t \equiv f/z$, and apply the previous argument. ■

Proof of Proposition 3. (a) Funded violence uses a payment of f to achieve the same probabilistic result that participatory violence achieves for a payment of tz. It follows that the former will be preferred to the latter if and only if $z > f/t$.

(b) The proposition asserts that if funded violence dominates peace for some z, then it does so for all $z' > z$. This is equivalent to the condition of nonincreasing global risk aversion; see Axiom V in Yaari (1969). By Remark 7 and the subsequent discussion on pp. 326–327, we see that decreasing absolute risk aversion (DARA) in the sense of Arrow implies Axiom V. But our utility function satisfies DARA.

(c) Follows from the same argument as in the proof of Proposition 2.