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 correlations are 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 well-being has no significant effect. This robust empirical finding, combined with the theory, suggests that Hindu groups have been primarily responsible for Hindu-Muslim violence in post-Independence India.

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1. INTRODUCTION

We study Hindu-Muslim conflict in post-Independence India through the lens of economics. We allow for two formally 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, primordial inter-group hatreds are sparked off 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 conflict. We examine these predictions empirically.

The recurrent episodes of Hindu-Muslim violence 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 1950–1995. While our study does not use data after this period, the situation may not have changed much since: witness, for instance, the violence unleashed in Gujarat in 2002. It may be argued that these numbers are small relative to the overall population of India. From a

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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 Fearon and Laitin have called 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 "primordially" 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., André and Platteau (1998), Collier and Hoeffler (1998, 2004), Dube and Vargas (2009), Field *et al.* (2009), Iyer and Do (2009) and the recent survey by Blattman and Miguel (2009)). This economic perspective is no contradiction to the use of noneconomic markers (such as religion) in conflict. 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.

In this paper, we take the economic approach to conflict seriously, and apply it to the empirics of 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, to be interpreted in the sequel as Hindus and Muslims. Depending on the circumstances, members of either group can be aggressors or victims in an inter-religious conflictual encounter. We assume that such violence is entirely decentralized, though it takes place against a backdrop of religious antagonism.

Specifically, within either group, there are elites that fund "conflict infrastructure". That infrastructure creates a backdrop for decentralized encounters across religious groups. What we mean by "encounters" are simply day-to-day interactions: an accident, a confrontation, a provocation that 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 (more on this below).

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 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 is the substance of Propositions 1 and 2. The former states the result for fixed elite investment, and the latter endogenizes elite investment to argue that the main assertion remains unaltered. This proposition — that a positive correlation between group incomes and subsequent violence is an indicator of victimization of the group — forms the basis of our empirical exercise.

We use a unique dataset on Hindu-Muslim violence between 1950 and 1995, compiled by Ashutosh Varshney and Steve Wilkinson. 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 round we can use is the 38th (1983), and the next is the 43rd (1987–1988). Both rounds contain information on the religious affiliation of the household, or more precisely, the head of the household. This enables us to compute average per capita monthly expenditure of Hindu and Muslim households, and at some sacrifice of disaggregation (see below), we obtain a panel at the regional level.

Table 2 contains the basic results. Total "casualties" (killed + injured) is used as the dependent variable of interest. In five different panel specifications with or without controls, Hindu expenditures have an insignificant effect on conflict (measured by total casualties; killed + injured), while the coefficient on Muslim expenditures is significant and positive.

The coefficient is also large. A one percent increase in Muslim expenditures is predicted to increase casualties — after three years — by over 3% in the fixed effects model.[2] Compare this to the population effect, which should (and roughly does) predict a unit elastic response to population increase. We conclude that an increase in Muslim prosperity is positively associated with greater religious fatalities in the near future. The remainder of the paper subjects this basic finding to a number of different checks.

As we argue throughout, our preferred explanation for this strong and curious relationship rests on the theory outlined in Section 2. The fact that Muslim expenditures display a strong and positive connection with later conflict, while Hindu expenditures have none, suggests that (statistically speaking) members of Hindu groups have largely been the aggressors in Hindu-Muslim violence in India.

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 entirely left to the reader. That said, we must emphasize that the theory does not arise from thin air. 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; see, for instance, Thakore (1993) and Das (2000) on

[2]The results for a random effects variant are smaller but still sizable: around 2%.

riots in Bombay and Calcutta, Rajgopal (1987) and Khan (1992) on riots in Bhiwandi and Meerut, Engineer (1994) and Khan (1991) on violence in Jabbalpur, Kanpur, and Moradabad, Wilkinson (2004) on Varanasi violence, and the work of Bagchi (1990), Engineer (1984), Wilkinson (2004) on the economic aspects of Hindu-Muslim conflict more generally. These studies are supportive of our main approach, but to our knowledge no one has pointed out the general statistical relationships we observe here.

2. THEORY

The underlying theory is simple. It is based on the assumption that religion is — at least in part — a marker for economic conflict. The marker could be made salient by elites in society that lay down the infrastructure for inter-religional conflict. The components of such an infrastructure will range spreading of rumors or incendiary speech all the way to the direct financing of militias, but with space in between for targeted violence, riots, or the organization of religious processions.

Against this general backdrop, there are decentralized, everyday encounters across individuals or small groups. Several of these could be confrontational: an accident, a crime, or a provocation that *could* boil over into something bigger. Such "provocations" happen all the time and may have no ethnic or religious significance. But they can be manipulated by the affected member(s) of the relevant aggressor group. An individual confrontation might trigger off a deliberate and targeted riot.

Our basic presumption is that members of each of the two groups are willing and able to use violence as a way to extort, exclude or simply lash out at the other group, and that such willingness (or ability) responds to economic circumstances. In principle, then, either group could be an "aggressor", and either group a "victim", depending on the circumstances of a particular situation. We are interested in two questions. First, does economics affect group violence? Second, do the observed patterns help us discern which group is more likely to be the aggressor?

For an aggressor who participates in or instigates a violent outcome, an opportunity cost must be borne, typically measured in units of time that would be lost for economically productive work. The gain is the prospect of excluding, exploiting, or hurting the victim in some way. It could be indirect, measured in terms of greater access to the victim's business revenues (as in the case of a rival businessman) or job (as in the case of a rival worker), or it could be direct looting of the victim's monetary wealth or assets. Finally, it could be simply be the satisfaction of seeing an enemy humiliated or hurt, though in this last case we would insist that that satisfaction be heightened if the victim is economically more prosperous.

This "decentralized" aspect of conflict that we emphasize is much in the spirit of Kalyvas (2003), André and Platteau (1998), and others. There are elites, who have an agenda of their own. And there are day-to-day encounters at the individual or small-group level, where the ambient climate of conflict may allow not just primordial hatreds to flourish, but also permit the "undercover" achievement of other goals.

2.1. **A Model.** Let us formalize this a little more. Begin with decentralized encounters or provocations, which the relevant individuals — in their role as aggressors — decide whether or not to take further. At the same time, members of either group — in their role as potential victims — buy security against the possibility of such attacks. We reiterate that an individual could be an aggressor or a victim (or both).

An individual is characterized by his income or wealth, which we denote by y. As a potential victim, the individual may fear that he will be attacked. Let α be the perceived probability of attack. Assume that the individual can buy protection against attack (think of this as "defence" d) at some unit cost w. Defence lowers the probability that the attack will be "successful" from the aggressor's point of view. Write this probability as $p = p(d)$, with p decreasing in d, with $p(d) = 0$ for all d exceeding some threshold $\bar{d}$.

A potential victim with income y chooses 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 cost of defence d, 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.[3] 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

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

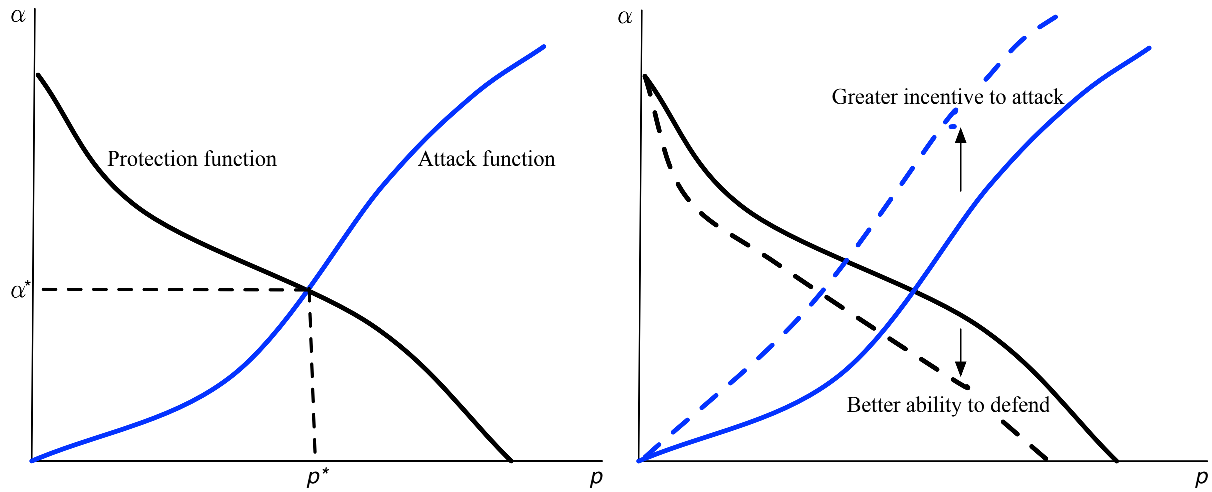
where the first term details the extra loss that will accrue from a successful attack, and the second term is the cost of lowering the success probability.

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*. Below, we impose a bit more structure on 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 there are decentralized "encounters" or "provocations" that might occur. Conditional on such an encounter, a potential aggressor with income y' can choose whether to take advantage of the situation. This involves an opportunity cost, typically incurred in time units t which could have been spent in productive work. The income loss is therefore $y't$. 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

$$(1-p)[1-t]y' + p([1-t]y' + \lambda y) > y'.$$

[3]It makes for easier exposition (but it is by no means necessary) to collapse the defence 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 defence 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 1. EQUILIBRIUM

Rearranging, we may rewrite this condition as

$$y > (t/\lambda p)y', \tag{2}$$

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

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

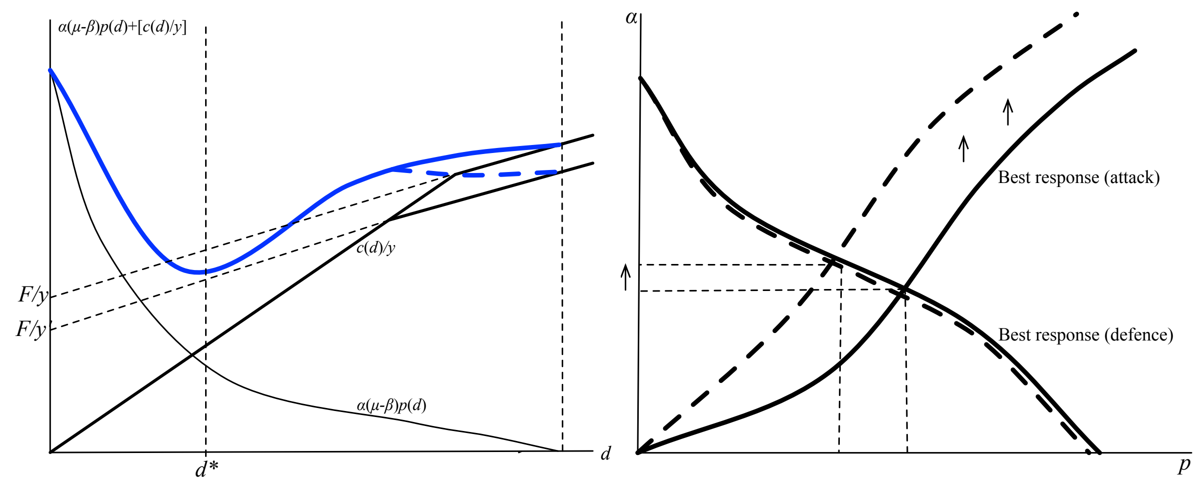
$$\alpha = \pi A(\lambda p y/t), \tag{3}$$

where π is the probability of a cross-religious encounter and A is the cumulative distribution function of aggressor incomes. Call this the *attack function*.[4]

2.2. **Equilibrium.** We may now formalize an equilibrium notion for conflict. This is a collection of attack and success probabilities, α^* and p^*, 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^*.

The discussion so far easily yields

[4]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 defence, 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 y' of the aggressor, and haven't adjusted it for his attack or defence activities elsewhere. It is easy enough to adjust the model to take these endogenous adjustments into account. There is no difference in the results, but the resulting model is just more complicated in terms of exposition.



(A) The Defence Response

(B) Equilibrium

FIGURE 2. THE EFFECT OF A CHANGE IN GROUP FORTUNES: LOW INCOME

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 1 summarizes an equilibrium. The upward-sloping line is the attack function that generates α as a function of p.[5] The downward-sloping line is the protection function[6] that connects attack probabilities to investment in protection and so to success probabilities. 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, 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.

2.3. **Income and Conflict.**

This model, elementary though it is, can in principle be used to address a variety of different questions. For the purposes of the present exercise, we are interested only in one of them: the effects of income changes (within any group) on the likelihood of conflict α.

Imagine drawing a variety of attack and protection functions for different values of the income of a potential victim. It is quite 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 will invest more on protection. The net impact of victim wealth on the

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

[6] It is actually weakly downward-sloping, and it may have jumps, but we can use indifferences 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.

probability of attack can, therefore, go in either direction. Panel B of Figure 1 summarizes this situation.

However, the effect of a change in *group* incomes can be very different. Consider a group which experiences an across-the-board increase in incomes, and a potential victim in that group. There are two components to the protection that he can acquire. The first component is human: protection provided by other individuals in the same group. The unit cost of such protection must then be viewed as compensation and *would therefore be proportional to incomes in the victim group*. To be sure, some compatriots would spontaneously defend a potential victim, but by and large contributions have to be made to the community, or obligations incurred, and these will be proportional to the opportunity cost of providing protection services, which in turn is related to the average of group incomes to which our victim belongs. We therefore expect that the cost of "human protection" would 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.

Certainly, such capital-intensive protection options are available only to the rich, and indeed, our use of fixed costs will be tantamount to presuming that this is indeed the case. Specifically, we suppose that

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

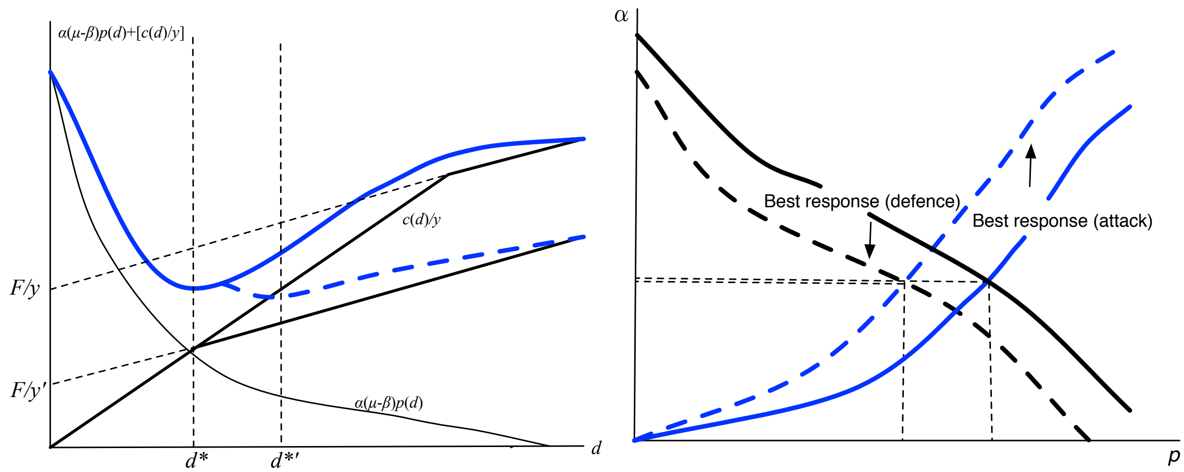
where the first entry represents a protection technology with a dominant human component, and the second a technology with a dominant physical component, with the potential advantage that it has lower variable costs. That is, $w > w^* \geq 0$. The important assumption that we make is that the variable 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 linearly related 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.*

To understand how this proposition works, consult Figure 2. Consider a typical member of that group — 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 in the event of an attack that's met with defence resources d. The piecewise linear segment in that panel is the function



(A) The Defence Response (B) Equilibrium

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

$c(d) = \min\{wd, F^* + w^*d\}$, 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 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 figure, 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 defence expenditure of that individual.

Moving over to Panel B with this information, we see that when incomes are low, the variable cost of defence 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.[7]

[7]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).

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 attack probabilities must climb as well. The ambiguity arises from individuals whose incomes cross the threshold. Figure 3 shows what happens with incomes that rely on the pure-variable-cost technology before the change, but move into the fixed-cost technology after the change. Now it is possible for there to be a sharp upward jump in defence expenditures. Panel A of this diagram captures this phenomenon.[8] 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, if the group is largely composed of aggressors and not victims, violence is expected to decline with the economic fortunes of that group.

Our basic theory, then, has the following implications:

(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 group is more likely to consist of potential victims rather than aggressors.

(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.

2.4. **Elite Investment in Violence.** It is not at all difficult to add elite investment to this model. One particularly tractable (and relevant) way to do this is to add an earlier stage to the theory, in which the elites of each group choose infrastructural investments. The "earlier stage" embodies the property that the elites make these choices with our earlier equilibrium as a "subgame outcome", which will be influenced by elite decisions.

We view elite infrastructure as a device to lower an aggressor's opportunity cost of engaging in conflict. One can interpret this reduction in opportunity cost in several ways, ranging from direct compensation for attackers, to the provision of information or weaponry, or to the provision of militants to assist in violence. It is possible that conflict infrastructure has independent effects on the probability of an attack being successful, or (for instance) on the extent of wealth that is seized following a successful attack. These alternative specifications will not affect our main results in any way, and we simply settle on one description for the sake of concreteness.

[8]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.

We take the monetary cost of elite investment to be a decreasing, strictly convex, smooth function $C(t)$: to lower the opportunity cost for aggressors, more infrastructure is needed, and successive reductions are progressively harder to achieve. Given a particular choice of t, the "subgame" interaction determines an attack probability as well as a success probability for every level of victim income. We presume that the elite seek to maximize some weighted combination of aggregate payoffs to aggressors in their own group and the losses borne by victims in the other group, net of their own costs of infrastructural setup.[9]

To write this a bit more formally, note that in a violent attack by an aggressor with income y' on a victim with income y, with success probability $p(y)$, the aggressor's expected gain is

$$p(y)\lambda y - ty'.$$

Meanwhile, the victim's expected loss conditional on the attack is

$$(p(y)\mu + [1 - p(y)]\beta)\, y,$$

so that if the elite places a weight of ω on aggressor gains and $1 - \omega$ on victim losses, its *unconditional* expected payoff is

$$\pi \int_y \int_0^{p(y)\lambda y/t} \left\{\omega[p(y)\lambda y - ty'] + (1-\omega)\,[p(y)\mu + (1 - p(y))\beta]\, y\right\} dA(y')dV(y) - C(t),$$

where V is the distribution of victim incomes, and where we've included the probability of a confrontation π as well as the attack condition $ty' < p(y)\lambda y$. We can now state

PROPOSITION **2.** *Endogenous elite investment cannot reverse Proposition 1. Under the conditions of that Proposition, 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.*

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

The proposition shows that the main insights of Proposition 1 are unaffected by a consideration of elite investment. We first show why the proposition is true, and then discuss qualifications to it.

Write $y = mz$ and $y' = m'z'$, where m is the mean income among victims in the "other" group, m' is the mean income among aggressors in "this" group, and z and z' are normalized income variables with cumulative distributions fixed at V and A respectively. With some abuse of notation, write $p(y)$ as $p(z)$. Then the attack condition is

$$z' < p(z)Tz, \text{ where } T \equiv \lambda m/tm'.$$

It follows that the expected payoff to the elite is given by

$$\pi m \int_z \int_0^{p(z)Tz} [L(z) - \omega m' t z'/m] dA(z')dV(z) - C(t), \tag{4}$$

[9] Little discipline is imposed on the theory if we allow for arbitrary objective functions on the part of the elite with no regard to individual decision-making at the decentralized stage. The "utilitarian" benchmark we adopt has the virtue of linking elite objectives firmly to the goals of individual aggressors in the group.

where

$$L(z) \equiv \omega p(z)\lambda z + (1-\omega)[p(z)\mu + (1-p(z))\beta]z$$

is a weighted compendium of various gains and losses. This specification restates the elite problem as one of choosing the value T rather than opportunity cost t. Using $T = \lambda m/tm'$, we may rewrite (4) as

$$m\Psi(T) - C\left(\frac{\lambda m}{Tm'}\right) \tag{5}$$

where

$$\Psi(T, \mathbf{p}) \equiv \pi \int_z \int_0^{p(z)Tz} [L(z) - \omega\lambda z'/T] dA(z') dV(z).$$

where the notation $\mathbf{p}$ refers to the dependence of Ψ on the functional form $p(z)$.

Now consider two values of m, say m_1 and m_2, with $m_1 < m_2$ and corresponding equilibrium outcomes are $\{T_1, p_1(z)\}$ and $\{T_2, p_2(z)\}$. The crucial observation is that if all incomes are below the threshold y^* before and after the change, the elite can feasibly generate the subgame outcome $\{T_2, p_2(z)\}$ under m_1 and the outcome $\{T_1, p_1(z)\}$ under m_2.

To see why, note that an individual with normalized income z has actual income $m_1 z$ to begin with ("situation 1"), and $m_2 z$ after the increase in group incomes ("situation 2"). Suppose the elite chooses T_1 in situation 2. Because $m_2 z < y^*$, the effective variable cost of protection (w normalized by mz) is unchanged. So is the opportunity cost t relative to m_1.[10] Therefore, the same subgame (with all payoffs to individuals scaled) as in situation 1 is induced, leading to the equilibrium success probability function $p_1(z)$. Exactly the same argument holds for implementing $\{T_2, p_2(z)\}$ in situation 1.

Therefore, given that $\{T_j, p_j(z)\}$ is feasible in situation i, we may use (elite) optimization, along with (5), to record the two inequalities

$$\Psi(T_1, \mathbf{p}_1) - \frac{1}{m_1} C\left(\frac{\lambda m_1}{T_1 m'}\right) \geq \Psi(T_2, \mathbf{p}_2) - \frac{1}{m_1} C\left(\frac{\lambda m_1}{T_2 m'}\right),$$

and

$$\Psi(T_2, \mathbf{p}_2) - \frac{1}{m_2} C\left(\frac{\lambda m_2}{T_2 m'}\right) \geq \Psi(T_1, \mathbf{p}_1) - \frac{1}{m_2} C\left(\frac{\lambda m_2}{T_1 m'}\right).$$

Adding these inequalities and removing common terms, we see that

$$\frac{1}{m_2}\left[C\left(\frac{\lambda m_2}{T_1 m'}\right) - C\left(\frac{\lambda m_2}{T_2 m'}\right)\right] \geq \frac{1}{m_1}\left[C\left(\frac{\lambda m_1}{T_1 m'}\right) - C\left(\frac{\lambda m_1}{T_2 m'}\right)\right]. \tag{6}$$

We claim that $T_2 \geq T_1$. If the assertion is false, then $T_2 < T_1$. But this contradicts (6), because

$$\frac{1}{x}\left[C\left(\frac{x}{y_1}\right) - C\left(\frac{x}{y_2}\right)\right]$$

[10] If T_1 is chosen in situation 2, then the corresponding opportunity cost t is given by $T_1 = \lambda m_2/tm'$. But it is also true that $T_1 \equiv \lambda 1 m_1/1m'$.

is a decreasing function of x whenever $y_1 > y_2$.[11]

So the claim is true, and $T_2 \geq T_1$. Now return to the subgame. For any victim income indexed by z before and after, the attack function shifts upward at every level of p, because the threshold is given by

$$z' \leq pT_i z$$

for $i = 1, 2$. The defence function is unchanged for every α, by the fact that w/m is unchanged. It follows, as in our argument for the first part of Proposition 1, that equilibrium attack probabilities must (weakly) increase at (every) z.

Now consider the second part of the Proposition. Think of the change as increasing m' from m'_1 to m'_2, while m is held constant. Once again, it is true that the elite can feasibly generate the subgame outcome $\{T_2, p_2(z)\}$ under situation 1 and the outcome $\{T_1, p_1(z)\}$ in situation 2. But now the argument needs no qualification at all on group incomes, because the victim income distribution is unchanged. We may, therefore, repeat all the steps leading to (6), this time with m' changing instead of m. As before, elite optimization tells us that

$$m\Psi(T_1, \mathbf{p}_1) - C\left(\frac{\lambda m}{T_1 m'_1}\right) \geq m\Psi(T_2, \mathbf{p}_2) - C\left(\frac{\lambda m}{T_2 m'_1}\right),$$

and

$$m\Psi(T_2, \mathbf{p}_2) - C\left(\frac{\lambda m}{T_2 m'_2}\right) \geq m\Psi(T_1, \mathbf{p}_1) - C\left(\frac{\lambda m}{T_1 m'_2}\right),$$

so that

$$C\left(\frac{\lambda m}{T_1 m'_2}\right) - C\left(\frac{\lambda m}{T_2 m'_2}\right) \geq C\left(\frac{\lambda m}{T_1 m'_1}\right) - C\left(\frac{\lambda m}{T_2 m'_1}\right). \tag{7}$$

Now we claim that $T_2 \leq T_1$. Suppose not; then $T_2 > T_1$. But this contradicts (7), because

$$C\left(\frac{x_1}{m}\right) - C\left(\frac{x_2}{m}\right)$$

is a decreasing function of m whenever $x_1 > x_2$.[12] So the claim is true, and its translation into a declining attack probability is made exactly as in the first part. This completes the proof of the Proposition.

While some of the effects are ambiguous, the theory is quite clear in asserting that a *positive* correlation between group incomes and violence must be connected to violence perpetrated *against* that group. This is the fundamental interpretation we take to the data.

In short: if a group's mean income is positively related to later outbreaks of conflict, while the other group's income is negatively related or not at all, *then it is the second group which is, "in the net", the group with more aggressors*. We reiterate that the theory will have to be "believed" to interpret the empirical exercise in this way. Thus the empirical study that

[11] The derivative with respect to x is $(1/x^2)\left\{[(x/y_1)C'(x/y_1) - C(x/y_1)] - [(x/y_2)C'(x/y_2) - C(x/y_2)]\right\}$, which is negative if $y_1 > y_2$. The latter assertion is true because the derivative of $(x/y)C'(x/y) - C(x/y)$ with respect to y is $-(x^2/y^3)C''(x/y)$, which is negative since C is strictly convex.

[12] The derivative of this expression with respect to m is given by $-[C'(x_1/m) - C'(x_2/m)]/m^2$, which is negative because C is strictly convex.

follows is not a test of this theory. The theory is a device to make sense of the empirical observations.

3. Empirical Analysis

3.1. **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.[13] It summarizes reports from *The Times of India* 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.

Some of these incidents are directly linked to an outside event. That is, explicit mention is made, in the dataset, of this event being connected to contemporary violence elsewhere. In our analysis, we drop all cases of violence which had some reported link to outside events.[14] We do this as we are interested in cases of violence which originate in the region *per se* (or at least are not described to be otherwise) and do not occur as a direct aftermath of some previous violence in the region or outside. From the standpoint of the empirical model (discussed in detail below) including such linked cases is tantamount to a direct violation of the standard assumption that event counts (the dependent variable) in the different areas, conditional on the observables of that area and area-specific effects, are independently and identically distributed.

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. Religious conflict appears to have sharpened significantly as we move from 1950–81 to 1982–95. In a country the size of India, these numbers may not appear to be very high, but the spillover effects (in terms of fear, insecurity, and the erosion of general well-being) are enormous.

In this paper, we primarily utilize the Varshney-Wilkinson data from 1982 to 1993. 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

[13]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.

[14]Linked cases form a very small fraction of all the reported events in the dataset. By dropping the incidents which have been reported to be linked to other cases, we lose very few observations.

State	Conflict						Expenditures					
	1986–89			1990–1993			1983			1987–1988		
	Cas	Kill	Out	Cas	Kill	Out	H/M	Min	Max	H/M	Min	Max
Andhra Pradesh	7	0	2	181	133	5	0.99	0.96	1.09	0.99	0.92	1.17
Bihar	508	434	12	145	49	9	0.98	0.88	1.12	1.07	1.02	1.12
Gujarat	1159	176	70	364	57	20	1.02	0.89	1.19	0.98	0.78	1.14
Haryana	0	0	0	6	4	2	1.20	1.07	1.53	0.96	0.85	1.05
Karnataka	180	22	10	371	64	15	0.98	0.84	1.19	1.00	0.83	1.07
Kerala	0	0	1	42	5	3	1.10	1.07	1.19	1.15	1.15	1.16
Madhya Pradesh	178	29	8	25	5	1	0.92	0.78	1.38	0.86	0.71	1.04
Maharashtra	655	74	42	122	13	10	1.04	0.97	1.25	1.04	0.74	1.29
Orissa	0	0	0	0	0	2	0.69	0.36	1.04	0.85	0.58	0.93
Punjab	0	0	0	0	0	0	0.86	0.75	1.15	1.21	1.19	1.22
Rajasthan	168	26	12	20	1	2	0.97	0.43	1.18	1.02	0.46	1.19
Tamil Nadu	35	4	2	82	7	3	1.06	0.82	1.44	0.88	0.80	0.94
Uttar Pradesh	869	202	28	363	123	17	1.12	1.01	1.23	1.11	0.95	1.54
West Bengal	80	18	5	78	12	4	1.18	1.05	1.26	1.21	1.05	1.31

TABLE 1. Descriptive Statistics: Conflict & Economic Data. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th and 43rd rounds. "Conflict" is measured by regional aggregates of casualties, killed and outbreaks over a four-year period. Cas = Casualties (killed + injured), Kill = Killed, Out = Outbreak. H-M ratio= Hindu per-capita expenditure/ Muslim per-capita expenditure. For the states, the entry for H-M ratio denotes the average value for the state. The range for the state comes from the constituent regions of the state.

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"), killed, the number of riot outbreaks, or just the plain binary incidence of *some* riot over the period. In all cases, we take aggregates over a four 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. It also appears to be the case that these riots predominantly occur in urban areas. This predominance cannot be simply explained by a (perfectly reasonable) presumption of urban bias in news reporting: the rural-urban disparity is huge. There are many possible explanations. One is that in urban areas the degree of residential segregation is limited whereas for the most part, villages in India are either almost entirely populated by Hindus or by Muslims. Apart from such high levels of segregation, the traditional roles of the different communities in villages are well-defined as are the norms regarding social interactions. This may serve to minimize sudden provocations or acts of violence. In any case, it is useful to bear in mind that religious violence in India is primarily an urban phenomenon.

The "expenditure" columns of Table 1 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 1 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 1.54 in a region in Uttar Pradesh in 1987–88.

Because there is so much regional variation, it is important to exploit a panel structure with regional fixed effects. We do so, but 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. [15]

We seek spatially disaggregated economic information by religion. The earliest "thick" round that permits us to do this is the 38th (1983), and the next is the 43rd (1987–1988).[16] For both 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 round, which does not permit identification of the surveyed households all the way down to the district level. To use both the rounds (and thereby exploit the panel structure), we must aggregate the Varshney-Wilkinson dataset up to the regional level in India,

[15]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.

[16]We cannot take on the next thick round as the economic data needs to be lagged relative to the conflict data by three years (see below for details).

"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.[17]

3.2. **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. Our basic empirical framework views these counts as a Poisson process with parameter $\lambda = f(\mathbf{X}, \epsilon)$, where $\mathbf{X}$ is a vector of observed parameters and ϵ is unobserved noise specific to the incident in question.

The observables 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. Propositions 1 and 2 tell us, additionally, that attack data will depend on average incomes in each group. Taken together, this motivates a Poisson specification in which the parameter depends on all these variables, with possibly additional region- and time-specific variation. Because the variances of all observed counts significantly exceeds their means, this motivates the negative binomial specification that we use:

$$\mathbb{E}(\text{Count}_{i,t+1} | \mathbf{X}_{it}, \gamma_i) = \gamma_i \exp(\mathbf{X}'_{it} \beta + \tau_t)$$

where we add in region effects γ_i as well as time effects τ_t in the panel regressions below.

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.[18] 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 could vary with

[17] 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.

[18] Because these expenditures are always introduced in logarithmic form, the ratio specification is essentially equivalent to the restriction that the coefficients on Muslim and Hindu per-capita expenditures are of equal magnitude and opposing sign.

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).[19] 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.[20]

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 variables on Hindu-Muslim conflict starting three years later. Lag specifications and issues of endogeneity are discussed in some detail below.

We present specifications that utilize both regional fixed effects as well as random effects. Fixed effects is a preferred specification for well-known reasons of flexibility. Yet the more restrictive random effects model is also employed in some of the regressions. This is because we are also interested in including a lagged dependent variable to control for the effect of past conflicts on the present. A short panel that includes a lagged dependent variable in the presence of fixed effects is typically inconsistent. In any case, when we do use random effects the Hausman test does not reject the specification, and the results with the two approaches are qualitatively similar.

Dependent variables are described in the tables. A quick glossary for all independent variables is included here: H pce = (log of) Hindu per-capita expenditures, M pce = (log of) Muslim per-capita expenditures, pce = (log of) overall per-capita expenditure, M/H = log (Muslim per-capita exp./Hindu per-capita exp.), Pop = (log of) population, Mus % = percentage of Muslims in the population, RelPol = religious polarization, Lit = literacy rate, Primary Edu = primary education completion rate, Urb = urbanization rate, H Gini = Gini calculated for Hindu households, M Gini = Gini calculated for Muslim households, Curr = "current casualties", contemporaneous with expenditure data and lagged three years behind dependent variable, BJP = share of regional Lok Sabha seats won by the BJP, BJP89 = BJP for the year 1989, Fixed = fixed effects, Rand = random effects, Time = time dummies.

3.3. **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 2 contains the main results. Each of the five columns uses fixed effects.

We display two columns with minimal controls (only population and two versions of Muslim presence are used), and then a third column which controls in addition for literacy and urbanization. The fourth column further includes measures of within-Hindu and

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[19] 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.

[20] In areas such as Punjab, there are other religious groups, so that Muslim percentage and Hindu-Muslim polarization measure different things. But these cases are exceptions rather than the rule.

	[1]	[2]	[3]	[4]	[5]
H pce	-1.55	-1.54	-1.24	-1.54	-1.81
	(1.12)	(1.12)	(0.79)	(0.94)	(0.96)
M pce	***3.45	***3.44	***3.43	***3.31	***4.56
	(3.15)	(3.16)	(3.05)	(2.88)	(3.27)
Pop	**0.83	**0.83	**0.83	*0.81	0.16
	(1.99)	(1.98)	(2.02)	(1.80)	(0.34)
Mus %	-0.00				
	(0.02)				
RelPol		-0.12	0.36	0.84	-0.14
		(0.07)	(0.20)	(0.46)	(0.07)
Lit			-0.02	-0.02	**-0.05
			(1.20)	(0.93)	(2.05)
Urb			0.01	-0.00	0.02
			(0.69)	(0.01)	(0.62)
H Gini				6.85	6.66
				(0.87)	(0.79)
M Gini				3.01	5.97
				(0.72)	(1.16)
BJP					***-3.57
					(3.24)
Time	yes	yes	yes	yes	yes
Log likelihood	-121.3	-121.3	-120.5	-119.6	-114.1
Observations	110	110	110	110	110

TABLE 2. The Effect of Hindu and Muslim Expenditures on Regional Conflict; Negative Binomial with Fixed Effects. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th and 43rd rounds. 110 observations, each specification."Conflict" is measured by regional aggregates of casualties (killed or injured) over a four-year period starting three years after the expenditure data. Test statistic (absolute) values in parentheses.

within-Muslim inequalities. Finally, the fifth column takes into account political considerations by using the regional share of BJP-occupied seats in the Indian Parliament as an added control (more on this in the sub-section "Politics"). In all five panel specifications with or without controls, Hindu expenditures have no significant effect on conflict (measured by total casualties; killed + injured), while the coefficient on Muslim expenditures is significant and positive.

The coefficient is also large. A one percent increase in Muslim expenditures is predicted to increase casualties — after three years — by over 3% in the fixed effects model. A good benchmark to situate this coefficient is the population effect, which should (and roughly does) predict a unit elastic response to population increase. 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. But there is little doubt that the effect is significant and big, and we conclude that an increase in Muslim prosperity is positively associated with greater religious fatalities in the near future.

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 4 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.[21] 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.

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

3.4.1. *Other Dependent Variables.* The use of alternative count variables generate the same results. The dependent variable used in Table 2 is total casualties, but we can move to progressively coarser indicators: the number killed in riots, the number of outbreaks, or just an indicator variable for whether there was any riot at all in a particular year.

The results are also robust to the use of Muslim-Hindu expenditure *ratios*: a higher ratio of Muslim to Hindu income, controlling for overall per-capita income, is positively and significantly associated with greater conflict three years later.

Table 3 displays some of these results. Column 1 runs the exercise for all killed, while column 2 counts the number of outbreaks of rioting. Column 3 revisits the baseline dependent variable of "all casualties" using the ratio of Muslim to Hindu expenditures (and includes overall per-capita expenditure as a separate variable), while columns 4 and 5 do the same for all killed and for the number of outbreaks respectively.

All these variants consistently report that an increase in Muslim per-capita expenditure is positively and substantially correlated with conflict three years down the road.

3.4.2. *Previous Violence.* So far we have not included any control for previous levels of violence in our fixed effects regressions. Yet some regions do exhibit violence more persistently over time than others, and besides, there is truth to the aphorism that "violence

[21]Unbalanced changes in group incomes can affect conflict. For example, if victim incomes change in a manner that bring 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 (2009). We do not, however, focus on these changes here.

	[1] Kill	[2] Out	[3] Cas	[4] Kill	[5] Out
H pce	-1.76	-0.09			
	(0.81)	(0.06)			
M pce	***4.43	***3.37			
	(2.85)	(3.61)			
M/H			***3.01	***4.02	***2.79
			(2.78)	(2.59)	(3.05)
pce			1.69	2.70	**3.25
			(0.03)	(1.53)	(2.48)
Pop	0.26	*-1.60	*0.80	0.23	*-1.71
	(0.53)	(1.84)	(1.79)	(0.47)	(1.93)
RelPol	1.92	-1.48	0.78	1.87	-1.69
	(1.01)	(1.15)	(0.42)	(0.98)	(1.29)
Lit	0.02	-0.01	-0.02	0.03	-0.00
	(0.80)	(0.25)	(0.92)	(0.90)	(0.21)
Urb	-0.03	-0.03	-0.00	-0.04	-0.03
	(1.01)	(1.41)	(0.03)	(1.10)	(1.40)
H Gini	4.68	1.35	6.93	4.77	1.75
	(0.56)	(0.19)	(0.88)	(0.58)	(0.25)
M Gini	7.86	**6.65	3.15	8.38	**6.82
	(1.33)	(2.19)	(0.75)	(1.36)	(2.26)
Time	yes	yes	yes	yes	yes
Log likelihood	-74.1	-53.5	-119.6	-74.1	-53.6
Observations	110	110	110	110	110

TABLE 3. The Effect of Hindu and Muslim Expenditures on Regional Conflict: Variations with Fixed Effects. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th and 43rd rounds. All counts over a four-year period starting three years after the expenditure data. 110 observations, each specification. Cas = Casualties (killed + injured), Kill = Killed, Out = Outbreak. Test statistic (absolute) values in parentheses.

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 lagged conflict is not generally time-invariant.

To check that the positive relationship between Muslim expenditures and future conflict observed so far is robust to inclusion of levels of current violence in the region, we add lagged dependent variables as an added control. Table 4 uses random effects — the Hausman test rejects none of the specifications reported here — so that we are able to include

	[1] Cas	[2] Cas	[3] Kill	[4] Kill	[5] RiotY	[6] RiotY
H pce	0.01	-0.01	0.24	0.27	0.57	0.45
	(0.02)	(0.01)	(0.25)	(0.27)	(0.71)	(0.56)
M pce	*1.54	*1.55	*1.94	*1.92	**2.08	**2.09
	(1.65)	(1.65)	(1.91)	(1.90)	(2.55)	(2.57)
Pop	**0.55	*0.50	***0.93	***0.94	***0.61	**0.50
	(2.06)	(1.72)	(3.19)	(3.20)	(2.75)	(2.05)
RelPol	***2.61	**2.56	***3.20	***3.24	***2.60	***2.60
	(2.61)	(2.57)	(2.95)	(2.95)	(2.73)	(2.74)
Lit	-0.01	-0.01	-0.02	-0.02	-0.02	*-0.02
	(1.05)	(1.11)	(1.31)	(1.24)	(1.56)	(1.72)
Urb	0.01	0.01	0.01	0.01	0.00	0.00
	(0.62)	(0.65)	(0.38)	(0.35)	(0.03)	(0.25)
BJP		-0.40		0.29		-0.64
		(0.48)		(0.32)		(0.95)
Curr	***0.00	***0.00	-0.00	-0.00	***0.53	***0.56
	(3.29)	(3.26)	(0.26)	(0.26)	(4.93)	(5.01)
H gini, M gini	yes	yes	yes	yes	yes	yes
Time	yes	yes	yes	yes	yes	yes
Log likelihood	-371.2	-371.1	-258.3	-258.2	-116.3	-115.8
Observations	110	110	110	110	110	110

TABLE 4. The Effect of Hindu and Muslim Expenditures on Regional Conflict: Random Effects with lagged Conflict. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th and 43rd rounds. 110 observations, each specification. All counts over a four-year period starting three years after the expenditure data. Cas = Casualties (killed + injured), Kill = Killed, RiotY = Number of years (out of 4) in which at least one riot occurred. Test statistic (absolute values) in parentheses.

lagged dependent variables for conflict.[22] The dependent variable for columns 1 and 2 is "all casualties" (killed + injured); for columns 3 and 4 it is "all killed". Column 5 introduces yet another dependent variable: the number of years (out of four, starting three years later) for which there was at least one riot. For this last variable, which takes integer values from zero to four, we run random effects ordered probit regressions.

[22]It is well-known that a short panel that includes a lagged dependent variable in the presence of fixed effects is inconsistent. Hence, we employ a random effects negative binomial model to allow for the inclusion of previous violence.

Table 4 makes it clear that the random effects specifications with lagged conflict do not do much damage to the results. Muslim per-capita expenditures continues to be positively and significantly related to conflict, though the both the magnitude and significance of the coefficients are somewhat reduced relative to the fixed effects model. Furthermore, "current casualties" is indeed significantly related to future conflict, though the size of the effect is small. It appears that — in addition to the fixed effects that capture the violence-proneness of a region — that time-varying violence has some effect on future violence.

The survival of our results in the random effects regressions with lagged dependent variables tells us something else. These results help us to rule out "mean-reversion" arguments of the following nature. Suppose one starts with the premise that conflict is serially correlated over time and also that current conflict tends to depress current Muslim expenditures.[23] Suppose a region experiences high levels of conflict today. This would reduce Muslim expenditures today. However if that high conflict is not endemic, Muslim incomes would recover over time (i.e., revert to its mean value). At the same time, more conflict may well occur in the future in this region (owing to serial correlation). This "mean-reversion" argument would then imply that Muslim expenditures are positively correlated with future conflict.

There are two reasons why this argument is problematic. The first comes from the random effects regressions with lagged conflict, some of which we've reported in Table 4. 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 random effects regressions; the coefficient on Muslim expenditures continues to be positive and significant when previous conflict is included as a control.

The second problem with the mean-reversion argument is conceptual. For expenditures to revert, conflict must be temporary. Yet for expenditures to be correlated with (without casually influencing) *future* conflict, current and future conflict must be correlated. But the greater this correlation, the less space there is for the mean-reversion to occur to begin with. Thus the mean-reversion argument, even without the empirical indictment that we present here, rests on a delicate foundation.

An interesting byproduct of the random effects specification is that Hindu-Muslim polarization is positively correlated with conflict. (This is not true of the fixed effects model.) The theory we use does not emphasize the role of polarization, but it is easy enough to incorporate it: even allowing for partial segmentation in commercial and social dealings, the number of cross-matches should be related to the extent of polarization across the two groups. However, as Esteban and Ray (2007) have argued, the relationship between polarization and conflict could be complex, once the question of conflict initiation is also accounted for. Conditional on the existence of conflict in the first place, polarization should be positively related to it, but conflict in highly polarized societies could be very costly to the aggressor, and therefore not be initiated to start with. With a low Muslim share,

[23] In fact, we do find evidence that current conflict depresses current Muslim expenditures while Hindu expenditures are largely unaffected; more on this later.

however, one would expect this latter effect to be relatively weak, and consequently for polarization to be positively connected with conflict, in line with the random effects findings. These observations are also in agreement with other findings that link polarization to conflict (see, e.g., Montalvo and Reynal-Querol (2005) on civil wars).

3.4.3. *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 "conflict infrastructure". One way to get at this is to use the ambient political climate as a proxy. 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.[24] 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 to shed light on two issues. First, we can check if the effect of Muslim expenditure on conflict which we have uncovered so far is not merely a reflection of the effect that the BJP's presence in a region has on regional conflict. After all, it is well-documented that politics has a major role in determining the extent of Hindu-Muslim rioting in India (see, e.g., Wilkinson (2004)).

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.[25]

In Table 5, in columns 1, 2, 4 and 5 we include BJP share as an additional (time-varying) control. We report results for fixed and random effects under alternative specifications of the dependent variable. Column 1 is simply replicated from Table 2 to facilitate comparison. We look at Hindu and Muslim expenditures separately, but entering them as ratios, as in Table 3, yields very similar results.

The basic finding that Muslim expenditures significantly and positively affect conflict, while Hindu expenditures do not, remains entirely unaltered. At the same time, the fixed and random effects specifications differ on the significance attached to the BJP control itself: the former reports this as reducing the effect on conflict, while the latter renders this insignificant. While it may be interesting to speculate on the differences between these two specifications, we do not do so here as the intercept BJP control is not of great

[24]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".

[25]Wilkinson (2004) argues that religious dominance in politics at the state level can have one kind of effect, which may be reversed at smaller levels such as a municipality or 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.

	[1] Cas	[2] Kill	[3] Cas	[4] Cas	[5] Kill	[6] Cas
H pce	-1.81	-1.48	-1.81	-0.01	0.27	0.37
	(0.96)	(0.65)	(1.09)	(0.01)	(0.27)	(0.39)
M pce	***4.56	***4.44	**2.98	*1.55	*1.92	1.03
	(3.27)	(2.86)	(2.54)	(1.65)	(1.90)	(1.13)
Pop	0.16	-0.10	*1.24	*0.50	***0.94	***1.10
	(0.34)	(0.18)	(1.91)	(1.72)	(3.20)	(3.67)
RelPol	-0.14	0.93	0.70	**2.56	***3.24	**2.55
	(0.07)	(0.48)	(0.37)	(2.57)	(2.95)	(2.45)
BJP	***-3.57	**-2.46		-0.40	0.29	
	(3.24)	(2.07)		(0.48)	(0.32)	
M pce* BJP89			0.16			***0.19
			(0.99)			(3.38)
Lit	**-0.05	-0.00	-0.00	-0.01	-0.02	-0.00
	(2.05)	(0.02)	(0.20)	(1.11)	(1.24)	(0.18)
Urb	0.02	-0.01	-0.00	0.01	0.01	0.01
	(0.62)	(0.28)	(0.09)	(0.65)	(0.35)	(0.80)
Curr				***0.00	-0.00	***0.00
				(3.26)	(0.26)	(2.62)
H Gini, M Gini	yes	yes	yes	yes	yes	yes
Time	yes	yes	yes	yes	yes	yes
Fix/Rand	F	F	F	R	R	R
Log likelihood	-114.1	-71.8	-119.1	-371.1	-258.2	-365.7
Observations	110	110	110	110	110	110

TABLE 5. The Effect of Hindu and Muslim Expenditures on Regional Conflict: Fixed and Random Effects with BJP Controls. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th and 43rd rounds, *Election Commission of India*. All counts over a four-year period starting three years after the expenditure data. 110 observations, each specification. Cas = Casualties (killed + injured), Kill = Killed, Out = Outbreak, RiotY = Number of years (out of 4) in which at least one riot occurred. Test statistic (absolute values) in parentheses.

intrinsic interest to us. As we've mentioned, conceptually the results could go either way. The results appear to suggest that BJP presence is a sign of Hindu dominance, where gains from conflict are perhaps too insignificant for Hindus to initiate such violent exchanges. But we are unwilling to take this interpretation much further.

We could use BJP share in just a single year (in our case, 1989) as a limited way of allowing regions to exhibit variations in the *response* of conflict to *changes* in victim well-being. That is, one might include the BJP 1989 share as an interaction, to see whether the response to an increase in Muslim per-capita expenditure is heightened by a stronger BJP presence. Now an interesting difference between the fixed and random specifications arises; columns 3 and 6 in Table 5 highlight the main difference. In the fixed effects case (column 3), the coefficient on Muslim expenditures remain positive and significant and the coefficient on M pce* BJP89 is not significant. On the other hand, for the random effects model (column 6) Muslim expenditures on their own lose significance, but the M pce* BJP89 term is positive and highly significant. It appears from the random effects model that Muslim expenditures do matter, as they did before, but a significant portion of this effect is driven by BJP share. In regions with a large BJP share, the effect of increased Muslim expenditure on conflict is correspondingly larger.

It is important, however, to reiterate that our main result is entirely robust to the inclusion of political considerations.

3.4.4. *Urban Conflict.* It is true that Hindu-Muslim riots are primarily an urban phenomenon; rural India is witness to very few cases of religious riots. 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 6. The results are in line with what we have obtained earlier, as the several specifications in that table, with or without political controls, show.[26]

However, it is worthwhile to remark that in our opinion these regressions, whilst important, 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 are just not there 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, better-defined social norms, etc.) they may not experience significant realizations of such conflict. Dropping them simply because they exhibit little or no conflict is ignoring relevant information.

3.4.5. *Different Lags.* Our main specification relates Muslim and Hindu expenditures "today" to conflict three years later, or more precisely, to a four-year aggregate of conflict starting three years later. No theory can pin down this choice of lag, though 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 3.5. At the same time, it is a safe presumption that our effect should die out with very long lags.

[26]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 6.

	[1] Cas	[2] Cas	[3] Kill	[4] Kill	[5] Out	[6] Out
H pce	-1.08	-1.09	0.41	0.70	-0.37	0.50
	(1.18)	(1.19)	(0.43	(0.68)	(0.30)	(0.49
M pce	**2.49	***3.35	***2.85	***5.59	**2.34	***2.59
	(2.49)	(2.81)	(2.58)	(3.98)	(2.54)	(2.97)
Pop	*0.58	0.48	0.16	-0.93	0.02	-0.59
	(1.74)	(1.32)	(0.35)	(1.47)	(0.01)	(0.80)
RelPol	0.55	-1.48	1.18	-0.92	0.57	-0.39
	(0.37)	(0.81)	(0.85)	(0.54)	(0.45)	(0.30)
BJP share		***-3.30		***-4.82		***-2.78
		(2.78)		(3.46)		(3.37)
Primary Educ	-2.91	**-7.51	0.57	-3.30	5.61	4.05
	(0.98)	(2.03)	(0.16)	(0.81)	(1.31)	(1.23)
H Gini, M Gini	yes	yes	yes	yes	yes	yes
Time	yes	yes	yes	yes	yes	yes
Fixed/Rand	F	F	F	F	F	F
Log likelihood	-122.6	-117.8	-76.4	-70.9	-60.1	-53.4
Observations	110	110	110	110	110	110

TABLE 6. The Effect of Hindu and Muslim Expenditures on Regional Conflict (Urban Households only); Negative Binomial with Fixed Effects. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th and 43rd rounds, *Election Commission of India*. All counts over a four-year period starting three years after the expenditure data. 110 observations, each specification. Cas = Casualties (killed + injured), Kill = Killed, Out = Outbreak, RiotY = Number of years (out of 4) in which at least one riot occured. Test statistic (absolute values) in parentheses.

It is easy enough to explore the effects of different lag structures on our regressions. That is, we match the two expenditures rounds with different 4-year periods of conflict that are centered n years into the future. A summary of the results is to be found in Table 7, which reports on $n = 0, 1, 2, 3, 4$ (our baseline case), and 5.

Observe that "contemporaneous conflict" (column 1) 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 four-year averages of conflict that are centered a year ahead (column 2), two years ahead (column 3), three years ahead (column 4) and four years ahead (column 5). For lags larger than the ones we have chosen, the positive relationship diminishes and then any association between the

	[1] Cas +0	[2] Cas +1	[3] Cas +2	[4] Cas +3	[5] Cas +4	[6] Cas +5
H pce	0.25	0.28	1.77	0.71	-1.81	0.05
	(0.15)	(0.18)	(1.05)	(0.42)	(0.96)	(0.02)
M pce	-0.77	*2.04	*2.01	**2.57	***4.56	1.83
	(0.66)	(1.91)	(1.73)	(2.26)	(3.27)	(1.42)
Pop	0.64	**1.47	*0.96	**1.06	0.16	**1.16
	(1.21)	(2.57)	(1.65)	(2.07)	(0.34)	(2.19)
RelPol	3.14	2.08	1.70	0.66	-0.14	0.06
	(1.51)	(1.28)	(1.53)	(0.43)	(0.07)	(0.04)
BJP share	***2.69	*1.62	1.32	0.85	***-3.57	**-2.49
	(2.95)	(1.78)	(1.47)	(0.81)	(3.24)	(2.06)
Lit	0.01	0.01	-0.02	-0.02	**-0.05	-0.02
	(0.64)	(0.33)	(0.87)	(0.88)	(2.05)	(0.69)
Urb	0.02	-0.03	-0.03	-0.01	0.02	0.01
	(1.01)	(1.47)	(1.16)	(0.39)	(0.62)	(0.43)
H Gini, M Gini	yes	yes	yes	yes	yes	yes
Time	yes	yes	yes	yes	yes	yes
Log likelihood	-120.5	-132.1	-133.7	-122.9	-114.1	-111.0
Observations	110	110	110	110	110	110

TABLE 7. The Effect of Hindu and Muslim Expenditures on Regional Conflict; Different lags: Negative Binomial with Fixed Effects. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th and 43rd rounds. 110 observations, each specification. "Conflict" is measured by regional aggregates of casualties (killed or injured) over a four-year period. Cas + n means that the 38th round expenditures are matched with conflict during $(1982 + n)$–$(1985 + n)$ and the 43rd round with conflict during $(1986 + n)$–$(1989 + n)$. Test statistic (absolute) values in parentheses.

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.

3.5. **Concerns.** We raise two concerns, and describe what we do to alleviate them. First, even though we've lagged expenditure data, and looked at conflict three years later, there are potential issues of reverse causation. Conflict may be correlated over time, and conflict could affect expenditures rather than the other way around. One resolution to this issue is to control for current levels of conflict, which we do anyway in every one of the random effects specifications.

	[1] MEx	[2] MEx	[3] MEx	[4] MEx	[5] HEx	[6] HEx	[7] HEx	[8] HEx
Cas/100	-0.02				-0.00			
	(1.52)				(0.56)			
Kill/100		***-0.07				*-0.02		
		(4.58)				(1.76)		
Out/10			**-0.14				-0.00	
			(2.66)				(0.27)	
RiotY/10				**-0.64				-0.13
				(2.61)				(0.79)
Urb	*-0.01	*-0.01	**-0.01	**-0.01	-0.00	-0.00	-0.00	-0.00
	(2.00)	(1.95)	(2.19)	(2.15)	(1.07)	(1.08)	(1.02)	(1.08)
Mus %	0.01	0.01	*0.01	0.01	-0.01	-0.01	-0.01	-0.01
	(1.30)	(1.22)	(1.70)	(1.39)	(1.43)	(1.49)	(1.53)	(1.44)
Pop, Lit, BJP	yes	yes	yes	yes	yes	yes	yes	yes
Time	yes	yes	yes	yes	yes	yes	yes	yes
Adjusted R2	0.61	0.62	0.64	0.64	0.77	0.77	0.77	0.77
Observations	110	110	110	110	110	110	110	110

TABLE 8. The Effect of Regional Conflict on Muslim and Hindu Expenditures. *Sources and Notes.* Varshney-Wilkinson dataset on religious riots, *National Sample Survey* 38th and 43rd rounds, *Election Commission of India*. All counts over a four-year period around the expenditure data. 110 observations, each specification. Test statistic (absolute) values in parentheses.

But one can go further. Several case studies (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)) suggest that if anything, conflict has a *negative* impact on Muslim well-being; in particular driving their incomes down with heightened exclusion and segregation. This is not very surprising as Muslims are a minority and happen to be poorer on average than their Hindu counterparts. In the event of a religious riot it is quite conceivable that the Muslims might come off second-best most of the time. Their more unfortunate economic circumstances also imply that they would be less able to protect their lives and property in the event of a communal riot. However, the lagged relationship we obtain in every one of the tables so far *is just the opposite*.

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

To back up this argument we regress Muslim and Hindu per-capita expenditures on current levels of conflict. We employ a fixed effects linear regression model where we include a region fixed effect and a time fixed effect. The results are unambiguously consistent with the case studies: current levels of conflict are significantly correlated with *lower* Muslim expenditures, and current conflict is uncorrelated with Hindu expenditures (see Table 8).

Second, one might argue that an omitted variable is driving both Muslim expenditures and conflict. We have already dealt with the "reversion to the mean" argument by the inclusion of lagged conflict. General time trends in the funding of conflict, from other countries or regions, are picked up in the time effects. There is finally the possibility that elites in the very groups that enjoy an income increase also fund a higher level of conflict. To this last concern our defence is primarily theoretical: we have tried to argue, in the context of a well-specified model, that this effect does not dominate the opportunity cost effect (Proposition 2).

Next, 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.[27] 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 the same regressions as in Tables 2 and 4; the results are reported in Table 9.

Notice that there is no 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. The last four columns, which uses Muslim-Hindu expenditure ratios, along with overall per-capita expenditure support the general observation that per-capita incomes are negatively correlated with overall conflict. In these regressions with Muslim-Hindu ratios, per-capita expenditure enters significantly with a negative sign, a result that mirrors the cross-country findings in Fearon and Laitin (2003), Collier and Hoffler (2004), or Miguel *et al.* (2004). Unlike the specific case of directed religious violence, it does seem that overall rioting is significantly reduced by an improvement in economic conditions.

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

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
H pce	0.03	0.00	-0.38	-0.37				
	(0.07)	(0.01)	(1.51)	(1.46)				
M pce	-0.02	0.01	-0.23	-0.27				
	(0.08)	(0.03)	(0.91)	(1.45)				
M/H					-0.03	0.00	-0.16	-0.19
					(0.11)	(0.01)	(0.69)	(0.82)
pce					-0.21	-0.22	***-0.71	***-0.74
					(0.62)	(0.64)	(2.58)	(2.72)
Pop	0.22	0.21	***0.35	***0.35	0.23	0.23	***0.35	***0.35
	(1.25)	(1.19)	(3.91)	(3.92)	(1.32)	(1.28)	(3.87)	(3.89)
Mus %	*0.01		**0.02		0.01		**0.02	
	(1.74)		(2.37)		(1.60)		(2.23)	
RelPol		0.42		**0.56		0.38		**0.53
		(1.56)		(2.14)		(1.42)		(2.06)
Curr			***0.00	***0.00			***0.00	***0.00
			(11.75)	(11.55)			(11.82)	(11.64)
Lit	yes	yes	yes	yes	yes	yes	yes	yes
Urb	yes	yes	yes	yes	yes	yes	yes	yes
BJP	yes	yes	yes	yes	yes	yes	yes	yes
H Gini, M Gini	yes	yes	yes	yes	yes	yes	yes	yes
Time	yes	yes	yes	yes	yes	yes	yes	yes
Fixed/Rand	F	F	R	R	F	F	R	R
Log likelihood	-421.9	-422.1	-968.1	-968.5	-421.7	-421.9	-967.2	-967.5
Observations	110	110	110	110	110	110	110	110

TABLE 9. The Effect of Hindu and Muslim Expenditures on All Regional Riots: Fixed and Random Effects. *Sources and Notes.* Govt. of India dataset on crime, *National Sample Survey* 38th and 43rd rounds. "Conflict" is measured by regional aggregates of casualties (killed or injured) over a four-year period starting three years after the expenditure data. 110 observations, each specification. Test statistic (absolute) values in parentheses.

4. Summary and Concluding Remarks

Our empirical investigations yield a central result, which we have tried to 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 four-year average of conflict starting three years into the future. In contrast, an increase in Hindu well-being has no significant effect on future conflict. We obtain this finding using a two-period Indian panel with region and time effects employed throughout (in both fixed- and random-effects variants).

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 or binary riot incidence. 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, both entered as a direct control and as a variable that might effect the responsiveness of conflict to changes in Muslim expenditure. It is robust to the use of alternative lag structures.

While endogeneity is (in principle) a concern, we obtain our results using controls for current conflict, and show in addition that the contemporaneous relationship between conflict and Muslim per-capita expenditure is negative. This negative effect is well-documented in particular case 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, and conflict a few years later, is precisely reversed, and turns positive. Indeed, as discussed in the body of the paper, a rise in Muslim per-capita expenditures seems to increase conflict starting from the very next year and for some years onwards.

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 relationship between Muslim per-capita expenditures and conflict. The relationship we uncover is specific to riots between these two religious groups.

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 2. Recall that we describe a two-group model in which "aggressors" in each group can engage in decentralized conflict with "victims" in the other group. We argue in some detail that a balanced increase in the incomes of a group should lead to unambiguously higher levels of attacks being perpetrated against them. There is more to loot, or greater reason to exclude, or more to hate, and even if higher incomes permit greater security, we show that the former effect must outweigh the latter when individuals have low income to start with. In contrast, the same increase in incomes reduces attacks perpetrated *by* that group. The opportunity cost of violence declines. We show that these two arguments are robust to the inclusion of an aggressive elite in either group, who can fund "conflict infrastructure".

The theory therefore permits the following interpretation. The fact that Muslim incomes (or expenditures) display a strong and positive connection with conflict, while Hindu incomes have none, allows us to suggest that (statistically speaking) Hindu groups have been the "net aggressor" in Hindu-Muslim violence in post-Independence India. We reiterate that a conclusion of this sort relies not only on our empirical findings, but a joint reliance on those findings *together* with the theory we propose.

For instance, one might argue that the positive impact of Muslim expenditures on violence stems from *Muslim*, not Hindu aggression. Such an interpretation would presumably be based on the assertion that rising Muslim incomes make it easier to fund conflict. But that assertion will need to address Proposition 2, which shows that the opportunity cost effect, which serves to dampen conflict, must exist even in the presence of elite infrastructural investment. To be sure, that Proposition is based on assumptions that we cannot directly verify, and one might press ahead to argue that the positive income effects enjoyed by the elite could outweigh the opportunity cost effect. But even in that case, the first parts of Propositions 1 and 2 will need to be confronted. If most of the aggressors are Muslims, the victims are among the Hindus, and according to the theory, a rise in Hindu expenditures must then have a positive impact on later conflict. We just do not see this for Hindu expenditures, however hard we try.

It is in this sense that our interpretation is conditioned by a theory. At the same time, the theory does not arise from a vacuum. We can read many case studies in which attacks on the Muslim community can be traced to various forms of Muslim economic empowerment; see the references in the Introduction.

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.

We do uncover an asymmetry in the Indian context. But 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 another culture, with a different history and a different demography, the outcomes may well be 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. ■