A 'Ghetto' of One's Own: Communal Violence, Residential Segregation and Group Education Outcomes in India

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October 2021

Abstract

How does ethnic violence and subsequent segregation shape children's lives? Using exogenous variation in communal violence due to a Hindu nationalist campaign tour across India, I show that violence displaces Muslims to segregated neighbourhoods. Surprisingly, I find that post-event, Muslim primary education levels are higher in cities that were more susceptible to violence. For cohorts enrolling after the riots, the probability of attaining primary education decreases by 2.3% every 100 kilometres away from the campaign route. I exploit differences in the planned and actual route to show that this is due to residential segregation of communities threatened by violence.

Keywords: Conflict, Education, Inter-group Inequality.

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I am grateful to Andrew Foster, Bryce Steinberg, Jesse Bruhn, John Friedman, Jesse Shapiro, Patrick Heller, David Weil, Daniel Bjorkegren, Stelios Michalopoulos, Alisa Tazhitdinova, Martin Mattsson, Naveen Bharathi, Diane Coffey and seminar participants at Brown University for invaluable comments. I thank Naman Garg, Jose Belmar, Marcela Mello, Joao Garcia, Giulia Buccione, Aastha Tyagi, Suvaid Yaseen, Harkirat Singh, and Saket Tiwari for their support. All remaining errors are my own.

1 Introduction

Ethnic inequality has serious economic ramifications across the Developing world (Alesina et al., 2016, 2020). Analyzing inter-group inequality in India is especially important, as religious identity has gained a bigger role in expressions of conflict as well as inequality. Despite recent poverty reductions, Indian Muslims earn less than Hindu Dalits[1], where the latter group is counted among the most historically disadvantaged communities in India. Muslim per capita incomes, on average, comprise only 68% of Dalit per capita incomes in Haryana, 69% in Gujarat, and 87% in Maharashtra (NSSO, 2012). This paints a grim picture of the relative deprivation of this social group. Furthermore, only 14% of the Muslim population in India was likely to complete college education as opposed to 37% of Hindu upper castes (Jaffrelot and Kalaiyarasan, 2019).

My work investigates how religious conflict impacts educational outcomes of Muslims. Communal conflict not only exacerbates between-group income disparities (Esteban and Ray, 2011), it also leads to discriminatory practices and policies against the dominated group. This paper gauges the long run education choices that members of a threatened community make in the aftermath of violence. I do this by using plausibly exogenous variation in communal violence due to a Hindu nationalist campaign tour across Indian states.

Identifying causal impact of communal violence is difficult because riots are more likely to occur in areas with rising Muslim incomes relative to Hindu incomes (Mitra and Ray, 2014). This is likely to generate omitted variable bias in the OLS relationship between communal riots and education outcomes due to selection. To overcome this challenge, I leverage plausibly exogenous variation in riots that emerges from the planned path of a series of anti-Muslim campaign events. Known as the *Ram Rath Yatra* (henceforth, *Yatra*), this controversial campaign trail across several Indian states[2], was organized by the Bhartiya Janata Party (henceforth, BJP) in 1990. During this campaign, many prominent leaders of the BJP planned to travel 10,000 kilometres (Mishra, 2015) to mobilize support for the demolition of a 16^{th} century Mosque in India[3]. This led to communal riots, majority of which took place on or very close to the campaign route (Engineer, 1991).

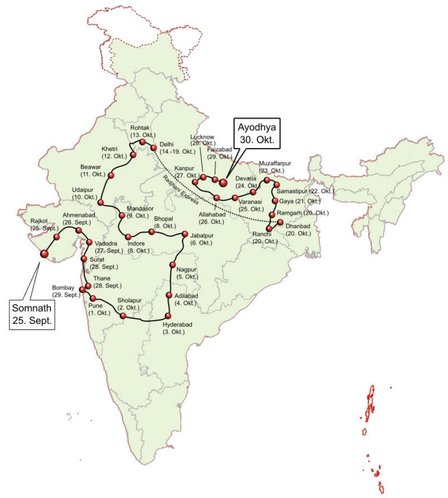
I find that communal violence is associated with *improvement* in early education outcomes of Muslim children who begin schooling after violent events. Muslim cohorts starting school after the event in 1990 attain higher levels of primary education in cities that were more susceptible to rioting (and were closer to the campaign route). In particular, for Muslims in cohorts born between 1986-90, the probability of attaining primary education fell by 0.23%, with every 10 kilometres away from the campaign route. The education gap between Muslims and non-Muslims (as measured in various rounds of the NSS Employment-Unemployment Surveys between 1987 and 2012) became narrower in cities with a history of Hindu-Muslim conflict[4].

[1]The preferred political term for people belonging to the lowest caste in India, earlier characterised as "untouchable": `https://en.wikipedia.org/wiki/Dalit`.

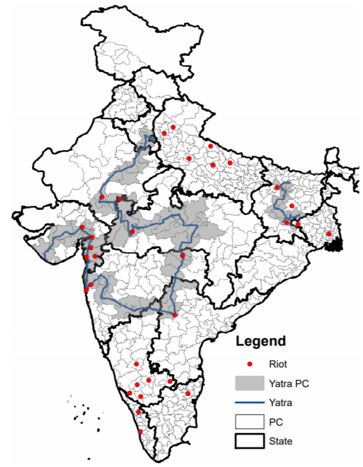
[2]Figure 1a shows the planned route of the campaign trail.

[3]The campaign commenced at the site of a famous temple in Somnath (Gujarat), and was to conclude in Ayodhya, the *purported* site of another temple allegedly destroyed by Mughal rulers.

[4]This is important because inter-group inequality and inter-generational economic mobility in India is best measured using data on education attainment (Asher et al., 2018), in the presence of measurement error in income data collected using survey methods.



(a) Plan of campaign route, *Rath Yatra* (Source: David Ludden, *India and South Asia: A Short History*).



(b) Actual campaign route, *Rath Yatra* (Source: Blakeslee (2018))

Figure 1: Planned versus Actual campaign route

I provide descriptive evidence that stronger within-community ties, built on the aftermath of violence, may be driving this very surprising result. In lieu of micro-measures for within-community ties, I show high degree of association between cities segregated along religious lines, and cities exposed to communal tensions.

I construct placebo tests by using differences' in the planned and actual route of the *Yatra*. These differences are granted by an unexpected impediment in the path of the campaign, when the leader of the movement (LK Advani) was arrested in the state of Bihar, before entering the destination state of Uttar Pradesh[5]. Due to this arrest, there are portions of the planned route where the *Yatra* never entered. Along the placebo routes, I find no association between the primary education outcomes of Muslim children (post-*Yatra*), residential segregation, and distance from the planned route.

I strengthen the reduced form results by demonstrating that *Yatra* indeed provides quasi-random variation in communal violence. I do not find differential patterns in the distribution of Muslim consumption expenditures, or in education infrastructure, pre and post-*Yatra*, across cities along the campaign route. However, I cannot rule out that the campaign was more likely to go to more populous cities. Therefore, I measure the effect of being closer to the *Yatra* route among cities with similar observable characteristics. I estimate the effect of violence on education by matching propensity scores at the individual level with continuous treatment (Hirano and Imbens, 2004). The treatment effect on early education remains statistically significant and negative for cohorts starting school after 1990.

I interrogate if residential segregation is a channel that led to improved early education outcomes among Muslim children who started school after violent events. I find a strong

[5]See Section 2.1 for background details.

first stage relationship by way of negative correlation between distance from *Yatra* route and residential segregation. The dissimilarity index in 2013 falls by 0.2% with every 10 kilometres away from the route.

To check the validity of this correlation, I then look for treatment effects on 'untreated' groups. Displacement on account of communal riots between Hindus and Muslims should not affect residential patterns of the Scheduled Caste (henceforth, SC) population, which has also been documented to live in segregated neighbourhoods in Indian cities (Bharathi et al., 2018). Therefore, SCs are being held as the untreated group. I am able to demonstrate that the continuous treatment, by way of distance from the *Yatra* route, is uncorrelated with segregation of SC populations. I also check for treatment effects in states that were not exposed to the campaign (controlling for spillovers), and do not find any correlation of the treatment with segregation of Muslims in these states.

I follow Abdulkadiroğlu et al. (2014) in order to estimate the causal effect of residential neighbourhoods on early education outcomes of Muslims. I impose weak assumptions on the direction of bias induced by factors (other than segregation induced by displacement), that relate to communal violence and potentially bias estimates of the neighbourhood effects. I demonstrate that neighbourhood effects on Muslim education outcomes continue to remain positive, when other violence related factors in the education production function are argued to impact Muslim education outcomes negatively.

Finally, I offer a discussion of possible mechanisms that may be driving the surprising effect of communal violence and segregation on Muslim education outcomes. I hypothesize that *strong ties* (Granovetter, 1973) within a community provide role models and resources to pursue primary education. This hypothesis is supported by qualitative evidence in Jaffrelot and Gayer (2012), which shows that members of Muslim enclaves, who were segregated by communal violence in Gujarat, eventually achieved higher socio-economic status. This was true despite the conspicuously absent public provision of welfare for this community, and because communities are very tightly knit in the aftermath of violence, as members support each other to rebuild businesses and incomes.

This paper speaks to four strands of literature in Economics: one deals with measurement, causes and effects of ethnic or inter-group inequality; while the other relates to the role of ethnic violence in determining socio-economic outcomes. In interrogating channels through which inter-group violence impacts education outcomes, my work is, thirdly, related to literature investigating the relationship between residential segregation of historically disadvantaged groups and their economic outcomes. Finally, this paper picks up on the recent literature on human capital formation among Muslim communities.

First, this paper extends the vast and rapidly growing literature on inter-group inequality and inter-generational economic mobility (Chetty et al., 2014) to developing countries. Alesina et al. (2016) measure consequences of ethnic inequality across African countries, whereas Sethi and Somanathan (2010) investigate the relationship between group identities and mobility for the Scheduled Castes and Scheduled Tribes in India. In particular, my work is especially relevant in contexts where religion is key in the process of identity formation, and is closely tied with the work of Alesina et al. (2020) and Asher et al. (2018), who study the relationship of Muslim identity with educational mobility in Africa and India, respectively. This paper contributes to this literature by investigating the causes for differences in educational attainment across religious groups, and the role of conflict herein.

Second, this work fits into the literature on the economics of ethnic conflict. Mitra and Ray (2014) interrogate the correlates of communal riots and verify the hypothesis that a relative increase in Muslim income or consumption is associated with an increase in the probability of a riot. Iyer and Shrivastava (2015) as well as Blakeslee (2018) use different Instrumental Variable approaches to show that Hindu-Muslim riots increase the vote share of the BJP in elections that follow such rioting. I borrow Blakeslee's (2018) assignment of electoral wards to the *Yatra* route (in Figure 1b) to construct my continuous treatment, i.e. distance of a city from the *Yatra* route. Field et al. (2008) consider the case of communal violence in Ahemdabad (Gujarat), and find that riots were more likely to break out in more integrated neighbourhoods. They also hypothesized, albeit did not test, that residential segregation was likely to increase after the riots. I bring evidence to test this hypothesis, while looking at the impact of violence on a broader range of outcome variables.

Third, this analysis deals with the literature on residential segregation and socio-economic outcomes of marginalized groups in the Indian context. Chetty and Hendren (2018) analyze the effect of neighbourhoods on child development across races in the US. This is done, in part, by exploiting exogenous displacement shocks, and therefore, ties closely with my empirical strategy. Susewind (2017) provides a description of segregation across religious lines in India using electorate rolls at the ward level for 11 cities[6]. However, Bharathi et al. (2018) demonstrates that ward level variation does not fully capture concentration of social groups in various parts of a city, and therefore, document segregation along caste lines at neighbourhood levels.

The literature on residential segregation in India has mainly focused on segregation along caste, and not religious lines[7]. Most recently, Adukia et al. (2019) use neighbourhood and religious identifiers in the Economic Census to provide descriptive evidence of residential segregation along communal lines. Mine is the first attempt to provide causal estimates of neighbourhood effects on long-term education outcomes among Muslims.

Finally, there is sprouting interest in the positive effects of ethnic enclaves in Developing Countries. Despite lower wealth, consumption, educational attainment, and access to state services (Jaffrelot and Kalaiyarasan, 2019), Muslims exhibit higher human capital accumulation in early childhood (Bhalotra et al., 2010). Geruso and Spears (2018) link religious composition of neighbourhoods with lower infant mortality among Muslims, through the channel of sanitation. Meyersson (2014) shows that women's political participation as well as secular high school education rates increased in Turkish municipalities where Islamic parties came to power, due to strong within-community ties. This paper links segregated Muslim enclaves with higher early education attainment for Muslims in India.

The rest of the paper is organized as follows: Section 2 describes the data sources along

[6]A ward is a local administrative unit of a city, with an average population of 1,500 to 6,000 for small statutory towns, and 30,000 to 200,000 for larger metropolitan cities (Prasad, 2006). It is therefore, much larger than an enumeration block (which measures a neighbourhood in this paper) with population of 650 to 700 (MRD and MHUPA, 2011).

[7]This is primarily due to the fact that most government sources of data do not share information at the neighbourhood level, to ensure safety of historically marginalised groups. Neighbourhood level data (electoral rolls, for instance) on Muslims is documented to make the community more vulnerable in the event of a riot (Jaffrelot and Gayer, 2012).

with a brief background of the *Yatra*, Section 3 describes the Empirical Strategy, Section 4 describes the reduced form results and explores the channels driving them, Section 5 demonstrates a range of robustness checks, Section 6 discusses the results with necessary caveats and concludes.

2 Data

In this section, I set the socio-political context and provide a broad overview of the background in which I study the effect of communal violence on educational outcomes of Indian Muslims. I also provide a detailed description of the employed data sources.

2.1 *Ram Rath Yatra* Route

Background

The *Ram Rath Yatra* rally was part of the *Ram Janmabhoomi* ('birthplace of *Ram*') movement. The goal of this movement was the construction of a temple for the mythical king *Ram* at his legendary birthplace in Ayodhaya, Uttar Pradesh[8]. According to supporters of this campaign, the original temple at his birthplace was destroyed by the Mughal ruler Babur, who in its place built a mosque, *Babri Masjid* in 1527 (Rashid and Venkataraman, 2019). These claims are made based on archaeological evidence that has been challenged by independent historians and archaeologists (Gopal et al., 1990).

This movement gained importance on the Indian political landscape due to various factors including,

- The breaking out of an armed rebellion against the Indian state in Muslim-majority region of Kashmir, claiming more than tens of thousands of lives[9]
- The Mandal Commission award of affirmative action to backward castes threatened the position of upper-caste Hindus (Balagopal, 1990). In 1980, the Socially and Economically Backward Classes Commission's report recommended that members of Other Backward Classes (OBC) be granted reservations in 27% of jobs under PSUs
- The Shah Bano case in the Supreme Court of India, which was a controversial maintenance lawsuit where the Court delivered a judgment favouring maintenance given to an aggrieved divorced Muslim woman. The then ruling government's actions solidified the perception that the ruling party favoured Muslims (Pathak and Rajan, 1989)

These political factors culminated into hostility between the two communities, and led to Hindus feeling threatened in a country where they constitute the majority population. The objective of the campaign was to promote a unitary Hindu identity transcending the

[8]This goal was achieved on August 5, 2020 when Prime Minister Narendra Modi laid the foundation stone of the *Ram* Temple after a long legal battle. `https://www.bbc.com/news/world-asia-india-53577942`.

[9]`https://www.bbc.com/news/10537286`.

divisive caste system. This identity was perceived to be threatened on account of the Muslim community and upward mobility of backward castes (Jaffrelot, 2010).

The *Yatra* commenced on September 25, 1990, at the site of a famous temple in Somnath (Gujarat), and was to conclude in Ayodhya (Uttar Pradesh), the *purported* site of another temple allegedly destroyed by Mughal rulers. Political rallies and religious processions were held along the path of the route, with Hindu activists greeting and cheering the campaign wherever it went[10].

The Route

Blakeslee (2018) constructed data on the route of the *Yatra* using daily accounts from the *The Times of India*, one of the major national newspapers in India. Using these journalistic accounts along with GIS maps of Parliamentary constituencies, the road network and built-up areas, he determines the constituencies through which the *Yatra* had actually passed.

I use Figure 1b to plot the *Yatra* route on GIS maps and then compute the distance of the route from sample cities, in kilometres. This is done for six states where the *Yatra* was planned to enter: namely Gujarat, Maharashtra, Andhra Pradesh, Bihar, Jharkhand, Madhya Pradesh, Chhattisgarh, Rajasthan, Haryana, Delhi, and Uttar Pradesh.

Note that the route does not enter Uttar Pradesh (UP), whereas Ayodhya (the destination of the route) is in UP. This was on account of Bihar government's decision to arrest LK Advani, the leader of this campaign, before they entered UP[11]. This is key for my identification strategy as I calibrate distance of a city in UP from *planned* route, using Figure 1a and GIS maps, to perform placebo tests.

2.2 Riots

A number of Hindu-Muslim riots erupted along the route of the *Yatra*. On November 1, 1990 (a day after the planned culmination of *Yatra*), riots broke out in a number of places, and curfew was enacted in at least 30 districts (Engineer, 1991). Of the 64 Hindu-Muslim riots which took place between 1990-91, 35 occurred during the 6 weeks surrounding the *Yatra*, 11 of which were in constituencies through which it passed (Blakeslee, 2018).

In Figure 2, I use Varshney and Wilkinson (2006) data set on communal violence in India, to plot the count of riots (in the aftermath of the *Yatra*) as a function of distance from *Yatra* route. It is clearly demonstrated that cities closer to the route were much more susceptible to rioting, than cities that were farther away.

[10] In his film *In the name of God*, director Anand Patwardhan captured the site of thousands of people assembling to welcome the arrival of the *Yatra* in various cities, thus deeply polarizing the political environment.

[11] On October 23, LK Advani, one of the most powerful leaders of the BJP who led the *Yatra*, was arrested in Bihar's Samastipur district on the orders of then Chief Minister Lalu Yadav to prevent him from proceeding to Ayodhya.

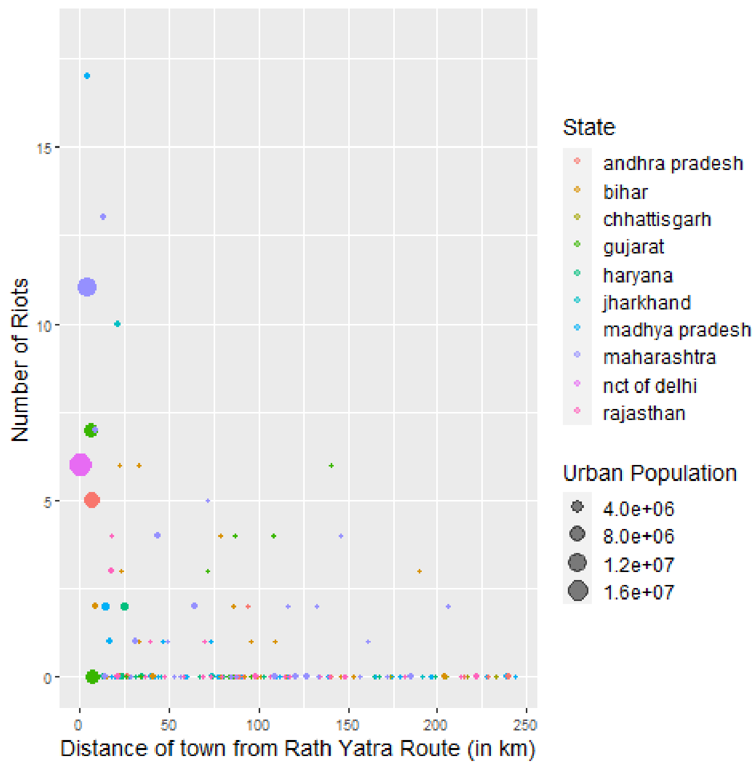


Figure 2: Number of Riots (1990-93) by Distance from *Yatra* route

These data provide comprehensive information on all Hindu-Muslim riots reported in a newspaper of record (*The Times of India*, Bombay edition), from January 1950 through December 1995. The data consists of information about the location as well as injuries and deaths. Of particular interest are the riots between 1990 (start of the *Yatra* campaign) until 1993 (after the ultimate demolition of Babri Masjid in December 1992). I geocode this data[12] to match with town level indices constructed using the Economic and Population Censuses of India and SEDAC (2011).

2.3 Education Outcomes

I access demographic information of individuals in urban households–level of education, gender and age– from the National Sample Survey Organization's Employment Unemployment Surveys (NSS-EUS, thick rounds between 43 (NSSO, 1988) to 68 (NSSO, 2012)). I obtain household level attributes like religion, social group, and consumption level from the same data source. I normalize current consumption and wages (in Rupees), setting 1983 as the base year, using disinflation factors from Consumer Price Index for Industrial Workers series (Reserve Bank of India, 2018).

[12] courtesy, Naman Garg for geocoding cities in Varshney and Wilkinson (2006).

I link urban centres of districts in the NSS data with cities in the Economic and Population Censuses, and use district urban averages as proxies for city-level averages, following Greenstone and Hanna (2014). Since district boundaries have changed over time (Somanathan and Kumar, 2009), I use district keys from NSS-EUS round 43, held in 1987[13] to harmonize administrative boundaries over the sample period [14].

I divide the sample of Muslim and Non-Muslim individuals (after dropping members of Scheduled Castes (SC) and Scheduled Tribes (ST)) into six cohorts: (1) Born before 1950, (2) Born 1951-60, (3) Born 1961-70, (4) Born 1971-80, (5) Born 1981-90 and (6) Born between 1991-96. Figure 3 shows trends in educational attainment within the two groups of interest: Muslims and Non-Muslims.

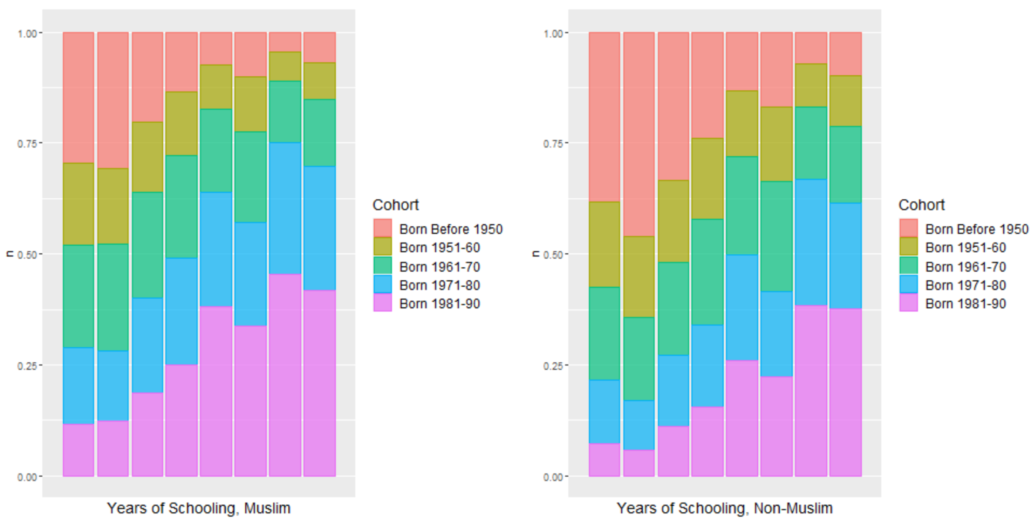


Figure 3: Within group variation in levels of education, by cohort

I describe the education gap between Hindus and Muslims in sample states in Table 1. It is worth noting that mean levels of education are systematically lower among Muslims for all the cohorts.

Cohort Born	Before 1950		1951-60		1961-70		1971-80		1981-90	
Whether Muslim	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Illiterate	30.02864	50.79830	18.09566	37.71874	14.84766	31.27104	10.33762	22.08000	5.42100	13.48787
Primary School	58.19958	33.95423	74.38935	49.07731	78.89624	55.72391	84.90503	67.36000	91.14260	78.31379
Middle School	45.62408	21.78020	62.85213	33.88482	67.63743	39.39394	75.87518	51.18000	83.33450	63.22384
Secondary School	35.22632	14.18308	48.84306	21.36494	51.97007	24.10564	58.68956	31.00000	63.68395	36.68666
College	14.539067	4.350718	21.766857	6.745148	21.591673	7.070707	26.033095	8.640000	21.041856	7.106417

Table 1: Across group variation in level of schooling, by cohort

Furthermore, I slice the cohorts constructed by birth years into two-year intervals, to highlight the correlation between year of birth relative to the year 1990, and educational at-

[13]courtesy Bryce Steinberg, for sharing district mappings across NSS rounds

[14]The resulting imprecision in matching leads to considerable noise. See Harari (2020) for a detailed treatment of matching Indian cities in Census and NSS data

tainment. I expect educational outcomes for Muslims born between 1976 and 1983 to be directly disrupted by *Yatra*, as this cohort was still in school at the time of violence. Of particular interest are the education outcomes of the children born between 1984 and 1991, as they enrolled in school after the violence and subsequent displacement.

2.4 Segregation

I measure segregation using the dissimilarity index (Massey, 1990). Residential segregation, for some city c, is measured as:

$$d_c = \frac{1}{2} \sum_i \left| \frac{m_{ic}}{M_c} - \frac{h_{ic}}{H_c} \right| \tag{1}$$

where, m_{ic} is the Muslim population in city c in neighbourhood i, M_c is the total population of Muslims in city c, h_{ic} is the non-Muslim population in city c in neighbourhood i, and H_c is the total population of non-Muslims in city c[15].

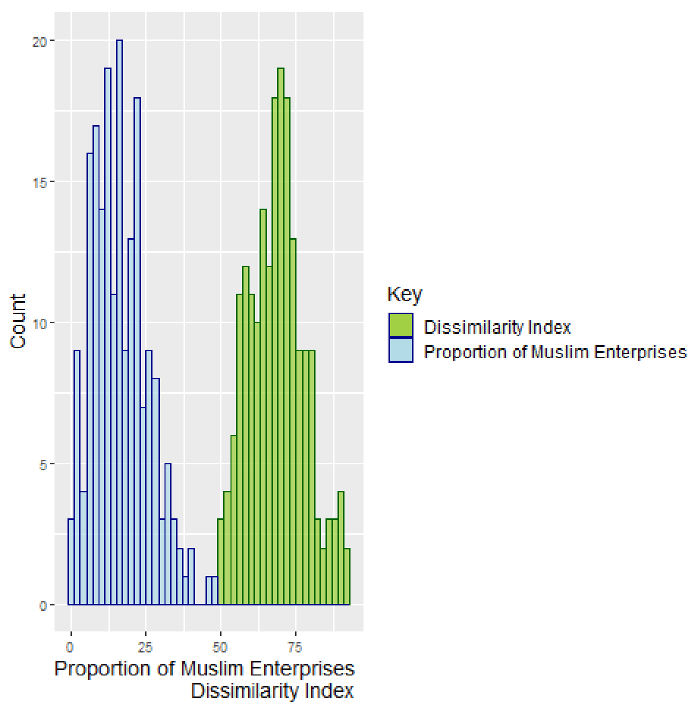


Figure 4: Distribution of dissimilarity indices, and Proportion of Muslim Enterprises across Indian cities

I construct this measure using religion indicators in the Sixth Economic Census conducted by the Ministry of Statistics and Program Implementation, Government of India (MoSPI, 2013)

[15]See Appendix C for a detailed discussion.

for about 25 million residential and residential-cum-commercial enterprises all over India. These data are available at the level of enumeration block (henceforth, EB), where every EB contains about 80 to 200 households. Figure 4 shows the distribution of the segregation measure and proportion of Muslim owned enterprises across Indian cities computed using this data.

2.5 Town Level Attributes

I link the Dissimilarity Indices (from (9)), constructed using Economic Census, to Town Directories of the Population Census (1990, 2000 and 2010), compiled by the Ministry of Statistics and Program Implementation, Government of India (MoSPI, 2011). The linking of the Economic Census and Population Census was done using Socio-Economic High Resolution Rural-Urban Geographic (SHRUG) Platform for India (Asher et al., 2019).

I use the Town Directories from the Population Census to get city level attributes like urban population, number of primary and secondary schools (both public and private), and number of colleges and universities, over time. I summarize the variables of interest in Table B.2.

3 Empirical Strategy

I analyze the effect of communal violence on education attainment for Muslims in India. Mitra and Ray (2014) demonstrate that rioting is correlated with relative income levels (measured by consumption expenditures) of Hindus and Muslims. This, in turn, is correlated with schooling choices, thus biasing the OLS estimates due to omitted variables. Therefore, I exploit quasi-random exposure of cities to communal violence. I obtain this variation in riots by tracing the path of the Ram Rath Yatra (1990). I check that distance from *Yatra* is in fact uncorrelated with relative Muslim incomes over time[16].

I propose my preferred instrument for communal violence: the distance of a city from the *Yatra* route, as it actualized in reality. I have already demonstrated that a disproportionate share of riots in 1990-93 occurred on or very close to the *Yatra* route in Figure 2, delivering a credible first stage. Thus, I employ a difference-in-difference research design, with continuous treatment, to measure differences in education gap (between Muslims and non-Muslims) for various cohorts as a function of differences in distance of cities from the campaign route.

In effect, I am comparing education levels of Muslims across two similar cities, where one city is closer to route and the other is farther away. I make this comparison before and after the violence. As an example, consider the urban centres of two districts: Thane and Buldana in the state of Maharashtra. While Thane is very close to the campaign route, Buldana is approximately 200 kilometres away. I compare the trend in education gap[17] in Thane against the trend in Buldana in Table 2.

[16]This helps to address valid issues raised by Mitra and Ray (2014). See Figure 9.

[17]The education gap is measured by the difference in proportion of non-Muslims completing primary education and the proportion of Muslims having done the same.

	Primary Education Gap	
Cohort:	1976-80	1986-90
Thane	0.145	0.115
Buldana	0.307	0.08

Table 2: Education Gap (proportion of non-Muslims completing primary education minus proportion of Muslims completing primary education) between Thane and Buldana, pre and post-*Yatra*

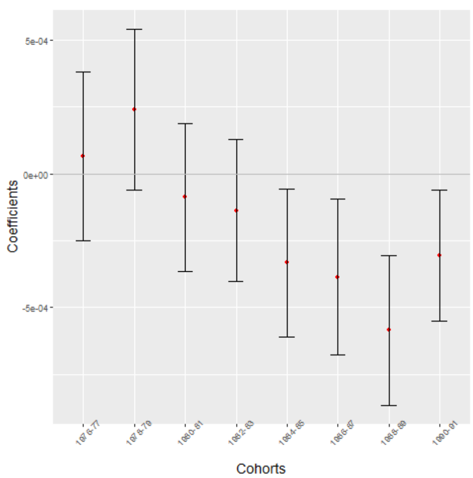
I compare the education gap in the two towns between cohorts that were already in school in 1990 (born between 1976 and 1984), and cohorts that started school after the *Yatra* (born 1984-91). This example illustrates that violence *improves* Muslim education outcomes as the primary education gap narrows by 73.9% in Buldana, as against 20.7% in Thane. Thus, my empirical strategy identifies the causal effect of communal violence if the education trends in towns that are farther away, form an accurate counterfactual for the trends in towns that are closer.

On account of the timing of the event (September-October, 1990), I expect different cohorts across religious lines to respond to the event differently. In particular, cohorts born after 1984 begin schooling after facing displacement on account of *Yatra* riots. Cohorts born between 1976 and 1984 are assumed to already be in school by 1990. Cohorts born before 1976 would have completed schooling by this time. Then, the probability for Muslims of cohort t to have attained education level e is

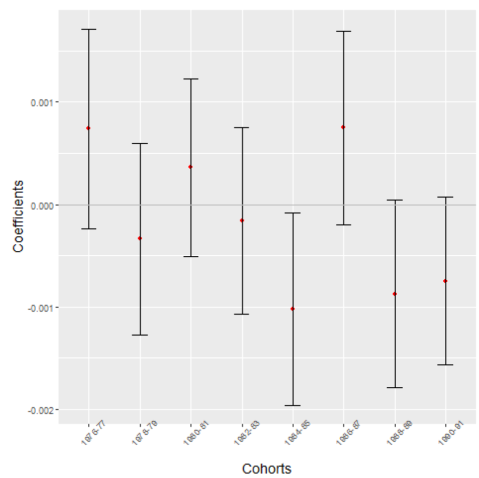
$$\begin{aligned} \sigma^e_{ihct} = \alpha + d_{ihc} + \gamma z_c + \sum_t \delta_t I(born = t) + \\ \sum_t \beta_t z_c \cdot I(born = t) \cdot d_{ihc} + \\ \xi_c + \varepsilon_{ihct} \end{aligned} \tag{2}$$

where, $\sigma^e_{ihct} \in \{0, 1\}$ indicates whether individual i in household h, city c and cohort t attained education level $e \in \{\text{Primary, Secondary or College}\}$, d_{ihc} is dummy coded indicating whether household is Muslim, $t \in \{\text{Before } 1976, 1976-83, 1984-91\}$ depicts the cohort an individual was born into and $I(\cdot)$ is an indicator function. The three cohorts refer to individuals who had finished schooling, were already in school, or had not yet begun schooling as of October 1990, respectively. z_c denotes distance of city c from *Yatra* route in kilometres, ξ_c are city fixed effects, ε_{ihct} is the random error term. The pairs of cross-products from the triple difference term were dropped from (2) for brevity.

In order to correctly estimate (2), I want to ensure that the education levels of cohorts (both Muslims and Non-Muslims) attending school prior to 1976, are not affected differently along the *Yatra* route. That is, I expect education levels of Muslims born after 1984 (aged between 0 to 6 in 1990) to be differently impacted by distance of their city to *Yatra* route. In Figure 5a, I do not find pre-trends in the probability that Muslims and Non-Muslims complete Primary School with respect to the treatment. Also, A.1b demonstrates that the linear probability model approximates the trends in primary education attainment very well.



(a) $\hat{\beta}$'s estimated from (2), for completion of primary school.



(b) $\hat{\beta}$'s with respect to *placebo Yatra*, for completion of primary school.

The empirical strategy is granted further credibility by Figure 5b, as there are no discernible effect on primary education outcomes with respect to the placebo. The placebo route is generated due to an unforeseen obstruction in the path of the campaign route. On October 23, 1990, the state government of Bihar issued orders to arrest prominent BJP leader, LK Advani, who was spearheading the *Ram Janmabhoomi* campaign (Yadav, 2017). This brought the *Ram Rath Yatra* to an unexpected halt, a week before it was supposed to reach it's final destination in Ayodhya, UP.

I exploit exogeneity in the timing (and therefore, the location) of this arrest, to construct a placebo treatment. This is done by charting out the *planned* route which the campaign was unable to tread due to this arrest. I find no correlation between the placebo route and the number of riots occurring in a city until the ultimate demolition of *Babri Masjid* in December, 1992.

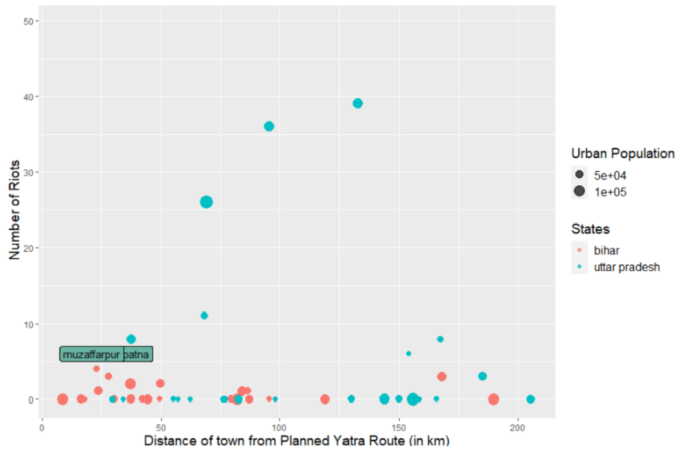


Figure 6: Number of riots (1990-93) by distance from *placebo Yatra* route.

Figure 6 presents a contrasting picture with respect to Figure 2, where I saw a strong and negative relationship between rioting and the continuous treatment. It is worth mentioning that rioting occurred in the labelled districts of Patna and Muzaffarpur, that are close to the placebo route. However, note that both these districts are right next to Samastipur district, where LK Advani was in fact arrested. It is therefore, more likely, that rioting here was on account of the treatment, and not the placebo.

Then, I estimate the reduced-form equation (2) again, this time with respect to distance from the placebo *Yatra*

$$\begin{aligned} \sigma^{e}_{ihct} = \alpha + d_{ihc} + \gamma z^{P}_{c} + \sum_{t} \delta_t I(born = t) + \\ \sum_{t} \beta_t z^{P}_{c} \cdot I(born = t) \cdot d_{ihc} + \\ \xi_c + \varepsilon_{ihct} \end{aligned} \tag{3}$$

where, z^{P}_{c} denotes the placebo *Yatra* route, and the remaining notation remains the same as above. I will show that this test is successful as Muslim educational outcomes are not correlated with the placebo route in any distinguishable way.

Finally, I replace city fixed effects with random effects as the fixed effects specification is known to be inefficient. I perform Hausman test comparing the two estimators and cannot reject the hypothesis that the specification employing random effects is the appropriate one.

3.1 Identifying Conditions

The causal effect in (2) is identified under the assumption that differences in post-*Yatra* Muslim education outcomes are driven by exposure of the city to communal violence only.

That is to say, there are no time-varying unobservable characteristics of a city that vary with the distance from the campaign route and that affect Muslim education outcomes differently. In order to check the validity of these assumptions (and in the absence of unobservable city characteristics), I check how various time-varying observable characteristics of a city relate with z_c.

Figure 7 demonstrates that cities closer to the route are more populous (and larger), and in this way the identifying condition may be violated. I further divide the sample of cities within 250 kilometres of the *Yatra* route into five subsamples, in intervals of 50 kilometres each. In Table B.1, I find that cities closest to the route (0 to 50 kilometres) are more populous in all three Census years under consideration. The remaining four groups, however, are indistinguishable in terms of urban population.

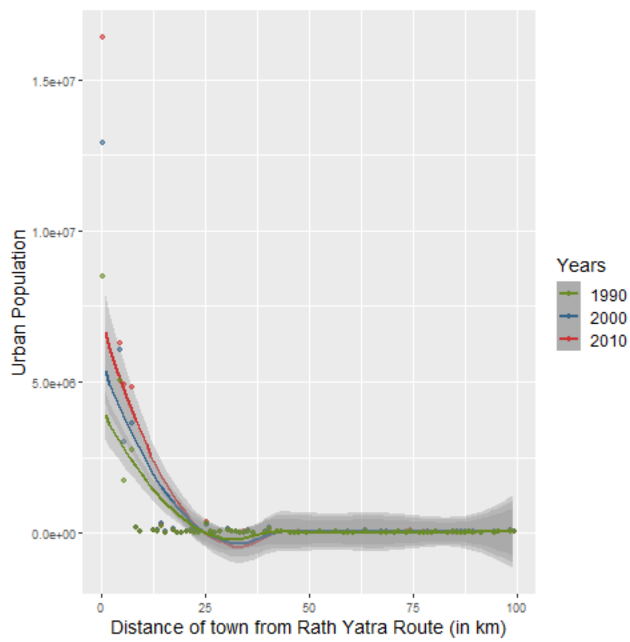


Figure 7: Total urban population as a function of Distance from *Yatra*.

Table 3, on the other hand, demonstrates that the most populous cities (which are closer to the *Yatra* route) are not different from all other cities in terms of supply of school and college infrastructure, over the years. Therefore, the distribution of school and college infrastructure seems to be balanced along the continuous treatment.

Distance from Yatra (in km)	0 to 50 (N = 63)	50 to 100 (N = 52)	100 to 150 (N = 30)	150 to 200 (N = 21)	200-250 (N = 16)
Schools	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Primary					
2010	4.64 (2.27)	5.81 (2.38)	5.43 (2.02)	5.73 (2.48)	6.01 (1.95)
2000	4.14 (2.33)	3.94 (1.22)	3.80 (1.31)	4.18 (1.66)	5.09 (5.05)
1990	3.47 (1.28)	3.70 (0.98)	3.38 (1.07)	4.16 (1.83)	3.77 (1.80)
Secondary					
2010	1.62 (0.88)	2.19 (1.18)	2.05 (1.00)	1.84 (0.73)	1.96 (0.71)
2000	1.35 (0.71)	1.34 (0.60)	1.27 (0.81)	1.58 (0.68)	1.30 (0.61)
1990	1.15 (0.51)	1.17 (0.45)	1.07 (0.42)	1.20 (0.37)	0.95 (0.46)
College					
2010	0.79 (0.46)	1.06 (0.62)	1.04 (0.71)	0.67 (0.41)	0.97 (0.71)
2000	0.41 (0.40)	0.45 (0.20)	0.41 (0.29)	0.37 (0.18)	0.39 (0.15)
1990	0.65 (0.41)	0.82 (0.48)	0.83 (1.04)	0.80 (0.42)	0.67 (0.42)

Table 3: Balance of school and college infrastructure, per 10,000 urban residents (by distance from *Yatra*).

I also check if urban Muslim population varies with distance of city from *Yatra* over time. In Figure 8, I plot the percentage share of Muslims sampled in the NSS surveys over the years, as well as percentage of Muslim enterprises from the Economic Census in 2013.

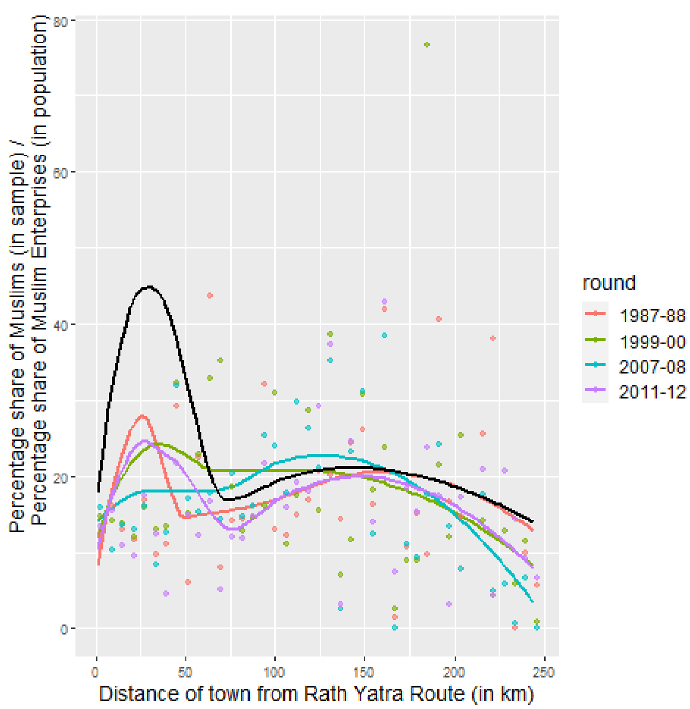


Figure 8: Proportion of Muslim households in NSSO samples over various round, and Muslim enterprises in Economic Census (black solid line) in 2013, by distance from *Yatra* route.

From Figure 8, I cannot conclude that Muslim population is distributed differently by distance, pre- and post-1990. It is, however, clear that the odds of finding Muslim-owned enterprises are higher within the first 50 kilometres of the *Yatra* route.

Another important household characteristic is income, which I proxy with Monthly Per Capita Expenditures (hencefoth, MPCE). Figure 9 shows that there is no relationship between the instrument and MPCE (normalized to 1983 Rupees), over the years.

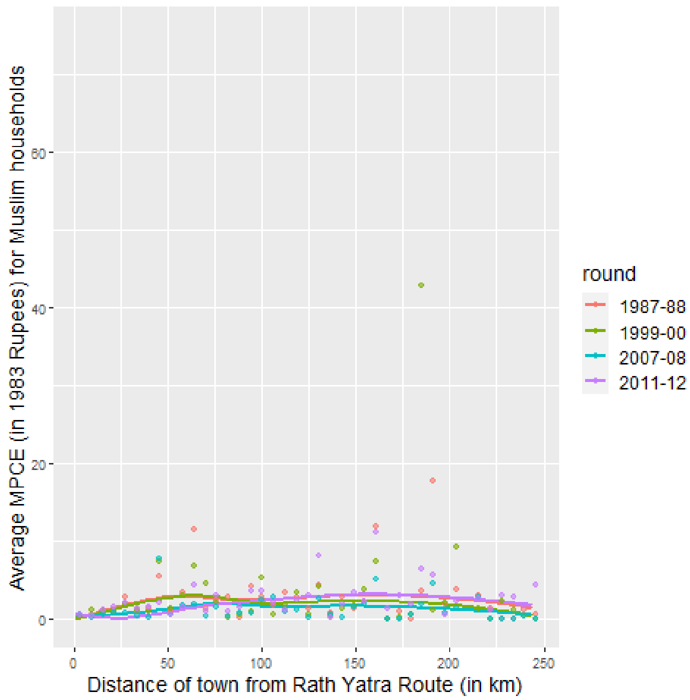


Figure 9: Muslim Monthly Per Capita Consumption (in 1983 Rupees) relative to non-Muslim MPCE, as a function of distance from *Yatra* route.

This assuages the threat to identification raised by Mitra and Ray (2014), who found that higher Muslim MPCEs relative to Hindu MPCEs, were correlated with the higher probability of communal violence. This is because, in my sample, the probability of a riot is higher closer to the campaign route, but average Muslim MPCEs (relative to non-Muslim MPCEs) seem to be uniformly distributed in the sample, both pre- and post-*Yatra*.

I describe strategies to correct for bias on account of differences in city size and in presence of Muslim owned-enterprises (by continuous treatment). I lay out a matching design to correct for selection on observable city characteristics.

3.2 Propensity Score Matching with Continuous Treatment

Using observable characteristics of the sample cities, I demonstrated above that cities closer to the *Yatra* route are more populous (and bigger), than cities away from it. I also showed that it was more likely to find a Muslim-owned enterprise in the first 50 kilometres of the *Yatra* route, in the post-treatment period. Therefore, I estimate the model in (2) by matching propensity scores calibrated for continuous treatment (Hirano and Imbens, 2004).

To remain consistent with standard notation in the literature, I denote potential outcomes as $Y_{ihtc}(z_c)$ (education outcomes, previously denoted as σ^e_{ihtc}), for $z \in Z$, where Z is continuous treatment (distance from *Yatra* route) in interval $[z_0, z_1]$. Dropping subscripts, the main assumption is described as

$$Y(z) \perp Z|X \text{ for all } z \in Z \tag{A4}$$

where, X denotes various city level controls. Then under (A4), I define the generalized propensity score (henceforth, GPS), $R = r(Z, X)$ as the conditional density of the treatment conditional on covariates, i.e.

$$r(z, x) = f_{Z|X}(z|x) \tag{4}$$

Then, $X \perp 1\{Z = z\}|r(z, X)$, which implies that assignment to treatment is not confounded, given the GPS. That is,

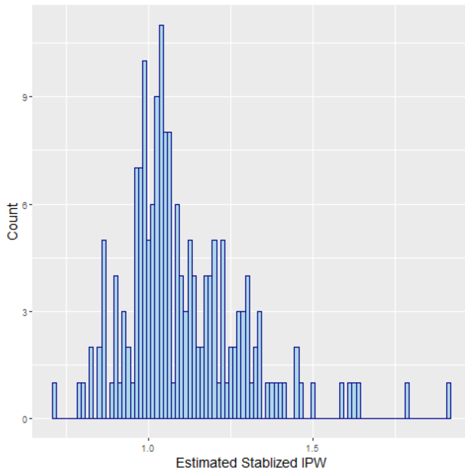
$$f_Z(z|r(z, X), Y(z)) = f_Z(z|r(z, X))$$

Following Robins et al. (2000), I calculate stabilized inverse probability weights (henceforth, IPW) as:

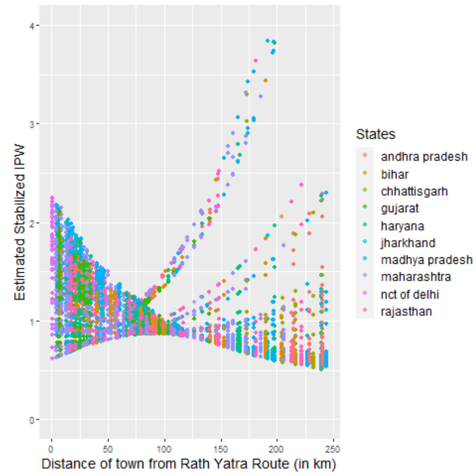
$$\iota^s = \frac{f_Z^{(z)}}{f_{Z|X}^{(z|x)}} \tag{5}$$

Assuming $Z_c|X_c \sim N(\beta_0 + \beta_1' X_c, \sigma^2)$, with X_c being city level attributes like urban population in 1990, I estimate stabilized IPWs [18]. Furthermore, I plot the density of ι^s in Figure 10a, and find that it is centred around 1.

[18] I fit the distribution of continuous treatment with a function that is piece-wise linear in urban population of 1990, by estimating a spline regression.



(a) Sample distribution of estimated Stabilized Inverse Probability Weights (IPW).



(b) Correlation between estimated Stabilized IPWs and distance from *Yatra* route

Figure 10: Properties of Estimated Inverse Probability Weights

I check if stabilized IPWs are systematically related with the treatment. In Figure 10b, I do not find that the weights co-vary with the continuous treatment in an identifiable way. This grants greater credibility to the research design.

3.3 Channels: Neighbourhood Effects

In light of the reduced form results obtained from the specifications above, I explore if segregation of communities along religious lines drives the effects of violence on Muslim educational outcomes. I estimate the static relationship between segregation (θ_c) and distance from the route (z_c), while controlling for city level observables (X_c) as:

$$\theta_c = \alpha + \tau z_c + \psi X_c + \mu_c \tag{6}$$

I also estimate the relationship between segregation and the placebo treatment, that is the distance from the planned *Yatra* route. This lends greater credibility to the stipulated channel. Additionally, I verify that there is no treatment effect on untreated groups like Scheduled Castes, as I test the hypothesis that distance of a city from *Yatra* route is uncorrelated with segregation along caste lines.

I hypothesize that segregation of Muslim and Hindu neighbourhoods in Indian cities drives better early education outcomes for Muslims in locations that were more susceptible to communal conflict. The ideal experiment to uncover causal effects of neighbourhood characteristics on individual education outcomes, would assign the opportunity to live in segregated or integrated neighbourhoods randomly. However, in addition to manipulating the composition of neighbourhoods, communal violence along the campaign trail may change the political environment in a way that changes the likelihood of going to school, for different groups differently.

Following Abdulkadiroğlu et al. (2014), I postulate a vector m_{ic} assumed to contain education inputs like composition of neighbourhood, parental income, measures of school quality, and political environment or discriminatory attitudes towards Muslims. Then, the education production function, dropping cohort and household subscripts, is given by

$$\sigma^e_{ic} = \pi' m_{ic} + \eta_{ic}$$

where, η_{ic} is the randomness in potential outcomes revealed under alternative assignments of the input bundle m_{ic} for individual i, residing in city c. Furthermore, I can partition m_{ic} into observed segregation levels for a Muslim individual i in city c, θ_{ic}, as well as unobserved inputs w_{ic}. Then, the structural education production function can be written as:

$$\sigma^e_{ic} = \delta' \theta_{ic} + \lambda' w_{ic} + \eta_{ic} \tag{7}$$

where, θ_{ic} is dissimilarity index (θ_c) interacted with a dummy variable that is equal to 1 for Muslims (d_{ic}). Then, the instrumental variable, z_{ic}, is given by distance of a city from *Yatra* route (z_c) interacted with the Muslim dummy. Here, I assume that the instrument is independent of potential outcomes, i.e. independent of η_{ic}. However, the instrument does not meet the exclusion restriction as not only does *Yatra* change the composition of neighbourhoods, it also changes unobserved inputs in (7). That is,

$$\theta_{ic} = \omega_1' z_{ic} + \nu_{1c}$$

$$w_{ic} = \omega_2' z_{ic} + \nu_{2c}$$

With this structure, the 2SLS estimate using z_{ic} as an instrument for θ_{ic}, omitting w_{ic} identifies $\delta + u'\lambda$, where u is the population 2SLS coefficient vector from a regression of w_{ic} on θ_{ic}, using z_{ic} as instrument[19]. Then, suppose if u and λ are positive, 2SLS estimates of neighbourhood effects, omitting w_{ic}, tend to be too big.

Building on the existing literature, I will provide evidence that λ is in fact negative, while maintaining the assumption that $u > 0$. That is to say, while the unobservables co-vary with the segregation measure, w_{ic} impacts education outcomes negatively. Therefore, under weaker assumptions on the bias induced by unobserved inputs in the education production function, I will show in Section 4, that the direction of neighbourhood effects on Muslim education attainment is positive.

I empirically interrogate the effect of neighbourhood as a channel for improved education outcomes for Muslim children who started school after the violent events in 1990. I estimate the following structural relationship

[19] see Proposition 1 in Abdulkadiroğlu et al. (2014).

$$\sigma^e_{ihct} = \alpha + d_{ihc} + \gamma\hat{\theta}_c + \sum_t \delta_t I(born = t) + \sum_t \beta_t \hat{\theta}_c \cdot I(born = t) \cdot d_{ihc} + \phi X_{hct} + \xi_{ct} + \varepsilon_{ihct} \qquad (\star\star)$$

where $\hat{\theta}_c$ is estimated in (6). First, I assume that the only channel through which distance from *Yatra* influences educational outcomes for Muslims, is through segregation. Then, I relax this assumption in line with (7), and show that the neighbourhood effects are positive.

4 Results

First, I estimate (2), and present estimates corresponding to completion of primary school in Table 4. For cohorts born after 1984, I find a significantly negative relationship between distance from *Yatra* and probability of attaining primary education, for Muslims. The corresponding results for illiteracy, and attainment of secondary and college education are presented Figures A.2, A.3, and A.4 respectively.

	(1) Born 1976-83	(2) 1984-91
Yatra Distance	-0.000108	-0.0000162
	(0.0000948)	(0.0000872)
Muslim	-0.143***	-0.121***
	(0.00600)	(0.00410)
Yatra Distance × Muslim	-0.0000139	-0.000198***
	(0.0000648)	(0.0000441)
N	45856	84798

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Probability of attaining primary education among Muslims by cohort and distance from *Yatra* route

I find that Muslims born in cities away from the campaign route, after the year 1984, face a disadvantage in terms of early education. That is to say, Muslim children starting school after 1990 performed better in terms of early education attainment in cities that were more susceptible to violence. I do not find any such patterns for secondary and tertiary education attainment.

4.1 Placebo *Yatra*

I use the arrest of BJP leader, LK Advani, and the subsequent suspension of the campaign to identify parts of the planned route that did not actually see the campaign. This enables me to construct a placebo test, wherein I check if the same reduced form relationships hold in (3) for the *planned*, as opposed to the actual *Yatra* route.

I check if the relationship between the placebo route, and education attainment level of Muslims from various cohorts follows the case when the treatment was actually administered (See Figures A.1a and 5b). I find that the trends in primary education attainment with respect to the placebo route are parallel for Muslims and non-Muslims, both prior and post-*Yatra* (Figure A.8a).

	(1) Born 1976-83	(2) 1984-91
placebo Yatra Distance	-0.000116	-0.000178
	(0.000230)	(0.000189)
Muslim	-0.288***	-0.246***
	(0.0239)	(0.0162)
placebo Yatra Distance × Muslim	0.0000698	-0.000101
	(0.000201)	(0.000137)
N	6732	13441

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Probability of attaining primary education among Muslims by cohort and distance from *placebo Yatra* route

Table 5 shows that the placebo route has a statistically insignificant on Muslim education outcomes. I plot the corresponding coefficients for illiteracy, secondary and college education in Figures A.5, A.6, and A.7, respectively. Therefore, I have shown that the placebo route has no effects on the outcome variable of interest. This strengthens the reduced form results that were presented above.

4.2 Matching Propensity Scores with Continuous Treatment

I implement the research design laid out in Section 3.2 by reweighting the reduced form equation in (2) using the Stabilized Inverse Probability Weights (IPWs) in (10a), computed from the Generalized Propensity Scores (GPS). The sample distribution of estimated IPWs is given in Figure 10a, and the absence of correlation between the weights and the treatment is demonstrated in Figure 10b.

Born	(1) 1976-77	(2) 1978-79	(3) 1980-81	(4) 1982-83	(5) 1984-85	(6) 1986-87	1988-89	1990-91
Illiteracy Level								
Yatra distance × Muslim	0.0000989	-0.0000254	0.0003121	0.0002854	0.0001841	0.0001959	.0003949***	.0001748
	(0.0001392)	(0.0001324)	(0.0001166)	(0.0001121)	(0.0001166)	(0..0001232)	(0.0001135)	(0.0000999)
Primary School								
	0.0000134	0.0003131*	-0.00000619	-0.0000454	-0.000187	-0.000361*	-0.000389**	-0.000220
	(0.0001635)	(0.0001559)	(0.000141)	(0.000136)	(0.000143)	(0.000152)	(0.000145)	(0.000128)
Secondary School								
	0.000218	0.0000440	-0.0000140	0.000342	-0.000319	0.000274	-0.000271	0.000219
	(0.000236)	(0.000230)	(0.000218)	(0.000232)	(0.000244)	(0.000232)	(0.000235)	(0.000227)
College								
	-0.000311	0.000763*	0.000185	-0.000219	0.000199	-0.00000851	0.000244	0.000436*
	(0.000345)	(0.000335)	(0.000313)	(0.000351)	(0.000364)	(0.000346)	(0.000323)	(0.000211)

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Relationship between distance from *Yatra* route (in km) and education attainment levels for Muslims, weighted by stabilized IPW.

In Table 6, I summarize the relationship between education levels of Muslims from various cohorts with respect to the continuous treatment, weighted by IPWs. I find that all the estimates for early education (illiteracy and primary education attainment levels) retain the same signs as above. Furthermore, primary education attainment for Muslim cohorts born between 1986 and 1990 is significant and negatively related to continuous treatment. This strengthens the result that Muslim children starting school post-*Yatra* in cities that were more susceptible to violence attained higher levels of primary education.

4.3 Channels

I argue that segregation along religious lines is a channel driving the very surprising and positive effects of communal violence on early education outcomes for Muslims. First, I estimate (6) and analyze the relationship between the continuous treatment and my measure of segregation in Figure 11. In Table B.3, I provide estimates from OLS regressions without city level controls (in column 1), and while controlling for population and proportion of Muslim owned enterprises (column 2). In column 3, I regress the dissimilarity index on log of *Yatra* distance. In all three cases, I find that the coefficient on the treatment is statistically significant, and negative. Therefore, I can reject the null hypothesis of zero linear relationship between segregation and distance of city from campaign route.

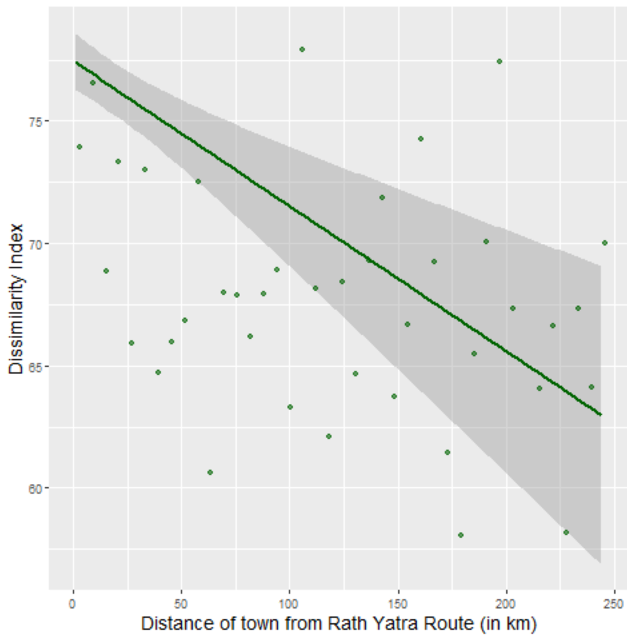


Figure 11: Segregation measure (Dissimilarity Index $\times 100$) by Distance from *Yatra* route.

Second, I demonstrate that the same relationship does not hold with respect to the placebo route. Figure 12 shows that in Uttar Pradesh, the segregation measure is in fact positively correlated with distance from the placebo route. This is not surprising, as Figure 6 shows that in UP, displacement on account of riots was not likely to occur closer to the *planned* route.

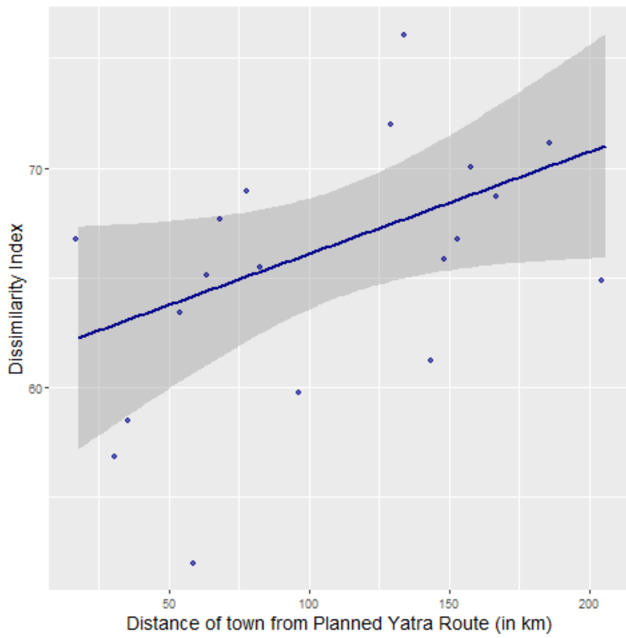


Figure 12: Segregation measure (Dissimilarity Index × 100) by Distance from *placebo Yatra* route.

Finally, I verify that there is no treatment effect on untreated groups. I have used quasi-random variation in communal violence to measure community (or religion) based segregation, as this is a displacement shock that affects Muslims only. In order to verify this, I check if the events of 1990 have any import on residential segregation of Scheduled Castes, who have also been documented to live in segregated enclaves (Bharathi et al., 2018). This group is not expected to get displaced due to communal conflicts. I construct caste based dissimilarity indices, using residential and residential-cum-commercial enterprises owned by Scheduled Castes in the Sixth Economic Census (2013).

In Figure A.15, I do not find a statistically significant relationship between caste-based segregation and distance from *Yatra*. This lends credence to the story that displacement shocks due to *Yatra* affected Muslims differently.

Furthermore, I perform robustness checks in Section 5 to show that there is no evidence that education outcomes of the untreated group were differently affected by quasi-random exposure to violence. This is evidenced by the absence of any statistically significant reduced form relationship between distance from *Yatra* and differences in primary education attainment between Scheduled Castes and other groups in Figure A.16a.

Taking stock of the reduced form estimates in this Section, I now estimate the structural relationship described in ($\star\star$), where I instrument the segregation measure with distance from *Yatra* route (according to the first-stage relationship in (6)).

Table B.3 makes a convincing case for a strong first-stage relationship. In (6), I assume that $\mathrm{E}[\mu_c|z_c, X_c] = 0$. That is to say, conditional on city characteristics, the only channel through which the treatment impacts education levels is that of segregation. I will later relax this exclusion restriction and show that results remain consistent.

Figure 13 shows that increased segregation leads to increased probability that Muslims born after 1984 attain primary education. Figures A.9, A.10, and A.11 provide the relationship between dissimilarity index and illiteracy levels, completion of secondary and tertiary education for Muslims of various cohorts, respectively.

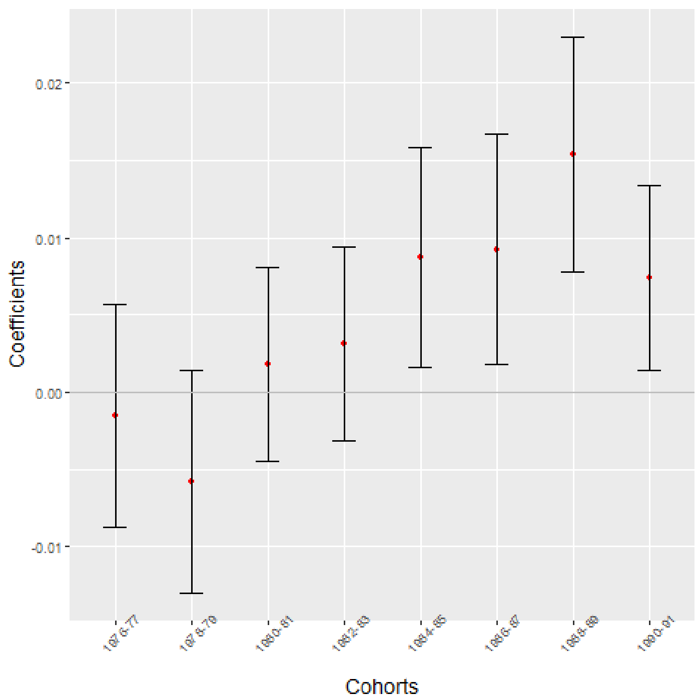


Figure 13: IV estimates of $\hat{\beta}$'s from ($\star\star$), for completion of primary school

I find that these results are consistent with the reduced form estimates above, wherein communal violence closer to the *Yatra* route may have displaced Muslims into ethnic enclaves, where Muslim children going to school after 1990 also achieved higher levels of primary education. This may be due to presence of role models from the same community in these ethnic enclaves. These role models in the neighbourhood may have enabled Muslims to achieve higher levels of early education. However, as described in Section 3.3, the exclusion restriction may be violated in the presence of unobservable inputs in the education production function if such inputs are also determined by distance of the city from *Yatra*.

I build upon previous work in the literature to argue that unobservable inputs (like political environment) in the education production function (those that are correlated with the instrument) have an effect that is diametrically opposed to that of segregation. First, notice that Blakeslee (2018) finds significant electoral gains for the BJP in constituencies through which the *Yatra* passed. He finds a 6 percentage point increase in the party's vote share, and an 11 percentage point increase in their probability of victory in elections. Similarly, Iyer and Shrivastava (2015) demonstrates a 5 percentage points increase in the vote share of the BJP associated with exogenous variation in Hindu-Muslim riots. So, it is clear that the instrument is associated with electoral gains for the Hindu nationalist BJP.

While communal riots cause much damage to life and property of Muslims, my second observation deals with worsening (or lack of improvement) in employment, consumption and education outcomes of Muslims in BJP ruled constituencies (Mitul, 2020). Farooqui (2020) contends that the rise of BJP in the Indian political arena has grossly aggravated the under-representation of Muslims in politics, and this may be associated with worsening provision of public goods for this community. That is to say, unobservable political climate closer to the *Yatra* route must necessarily correlate with Muslim education outcomes negatively, as the communal riots in these areas may have caused greater harm to Muslim life and property (direct channel) or led to electoral gains for BJP (indirect channel), which could not have improved Muslim education outcomes.

Additionally, I have provided evidence in Figure 9 that household consumption for Muslims is not significantly different by treatment, pre or post- *Yatra.* Then it must be the case that $\lambda \leq 0$. However, notice in Figure 13 IV estimates for attainment of primary education, for cohorts born between 1981-90, imply that

$$\delta + u' \cdot \lambda > 0 \tag{8}$$

This discussion suggests that since $u > 0$ and $\lambda \leq 0$, (8) implies that $\delta > 0$. That is to say, I can draw upon existing literature to make assumptions that enable me to conclude that the causal effect of segregated neighbourhoods on early education outcomes of Muslims is positive. In this way, I have correctly identified the direction of the causal effect of segregation on education outcomes, while relaxing the exclusion restriction.

Segregated Muslim neighbourhoods may be correlated with higher community spending on primary schools, in ways that improve school quality. This may be true, as more segregated cities also harbour a higher number of Muslim enterprises. However, this does not explain why I only see the effects in early education outcomes. A more convincing story, that is harder to test empirically, is given by the formation of *Strong Ties* within the community (Granovetter, 1973), as opposed to a larger number of *Weak Ties* outside the community.

These results, therefore, build on previous work evaluating positive neighbourhood effects on human capital formation of Muslims, who are likely to live in enclaves populated by other Muslim households (Geruso and Spears, 2018) in Indian cities. Strong within-community ties seem like the most plausible explanation of my results, which are very surprising and antithetical to research that finds negative effects of racial segregation on long term outcomes of Black men in the US (Chetty and Hendren, 2018).

This is supported by qualitative evidence in Jaffrelot and Gayer (2012), who claim that residents of Muslim enclaves, segregated by communal violence in Gujarat, eventually achieved higher socio-economic status, despite the conspicuously absent public provision of welfare for Muslims. This is because community links are very tightly knit in the aftermath of violence, as members support each other to rebuild businesses and incomes. While violence targeted at a community may strengthen already existing ties, the long term effects on the strength of these ties as well as identity formation is an open empirical question.

An investigation into the persistence of these ties across generations is beyond the scope of this paper, but it is plausible that Muslims may be investing in forming *Strong Ties*, from a sense of fear and loss. At the same time, they may not be formulating as many *Weak Ties* outside their community, which may be required to secure a position in institutions of higher learning. The absence of *Weak Ties* is a story that is consistent with under-representation of Muslims in political institutions, which further weakens their position to lobby for affirmative action in higher education and government jobs (Alam, 2010). Hence, this may explain the statistically insignificant neighbourhood effects on college education attainment for Muslims.

5 Additional Robustness Checks

In this Section, I perform some additional robustness checks to strengthen my analysis of Muslim education outcomes with respect to distance of cities from *Yatra* route, as well as segregation along religious lines.

5.1 Treatment Effects on Untreated Groups

5.1.1 *Yatra* and Scheduled Castes

Adukia et al. (2019) and Bharathi et al. (2018) document caste-based residential segregation in various Indian cities. I check if the treatment affects education outcomes of Scheduled Castes differently, where this group is considered to be untreated. This is because communal events that target Muslims should not affect other social groups differently. In Figure A.16a, I observe parallel trends in SC and non-SC primary education attainment, with respect to distance from *Yatra* route, across various cohorts.

Taken together, Figures A.15 and A.16a provide evidence that segregation is the channel associated with better early education outcomes for Muslims in India.

5.1.2 States unaffected by *Yatra*

As a final placebo test, I check if the first stage relationship between distance from *Yatra* route and dissimilarity index (measuring community/ religion based segregation) holds for states where the *Ram Rath Yatra* campaign did not enter. In Figure 14, I do not find statistically significant correlation between the treatment and segregation in these states.

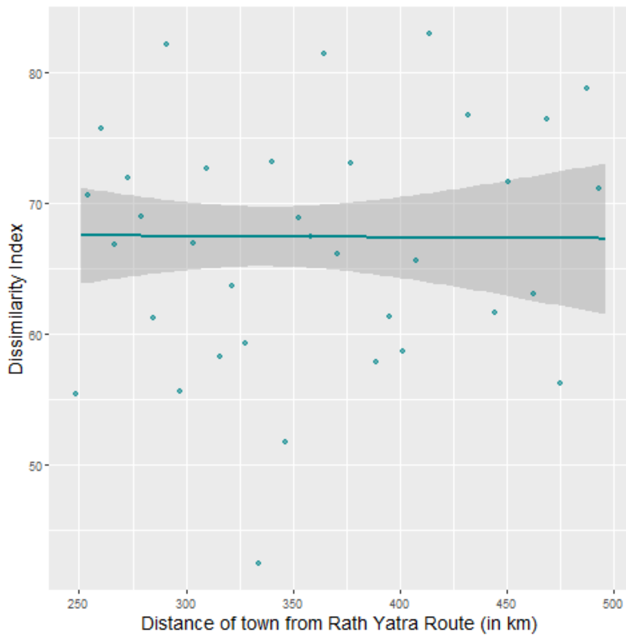


Figure 14: Relationship between segregation measure (dissimilarity index $\times$ 100) and distance from *Yatra* route in kilometres, for states that did not witness the *Yatra* campaign.

Therefore, I do not find any spurious correlation in states that were not exposed to this campaign. I only consider cities within 250 to 500 kilometre range in these states, to avoid capturing spillovers to cities close to the *Yatra* states.

5.2 Heterogeneous Effects of Muslim Population Share

I seek to understand how the treatment effects differ by proportion of Muslim population. That is to say, I estimate the heterogeneous effects of Muslim population in a city on the education outcomes of Muslims, by *Yatra* distance. I proxy for Muslim share of population by the proportion of Muslim owned enterprises in a city, calculated using the Sixth Economic Census (2013).

The treatment effects for early education (Table B.4) are still significant and have the same sign as in Table 4. Additionally, it seems that cities with a high share of Muslim owned enterprises seem to improve primary as well as tertiary education outcomes for Muslims. This could be on account of presence of role models, or higher community funding for private primary schools in cities with a higher share of Muslim enterprises. At any rate, this provides some evidence of positive neighbourhood effects for Muslims, consistent with the conclusions in Geruso and Spears (2018).

5.3 Migration

It is conceivable that in the aftermath of the violence, some Muslim families migrated to different cities, instead of safer locations in the same city. I therefore, take into account migration by controlling for difference in Muslim population in the cities between 1987 and 2012.

I estimate (2) controlling for difference in Muslim population, and find that the relationship between Muslim education attainment and the treatment still remains statistically significant for the relevant cohort, with the same signs as before. Figure 15 demonstrates this for primary education attainment.

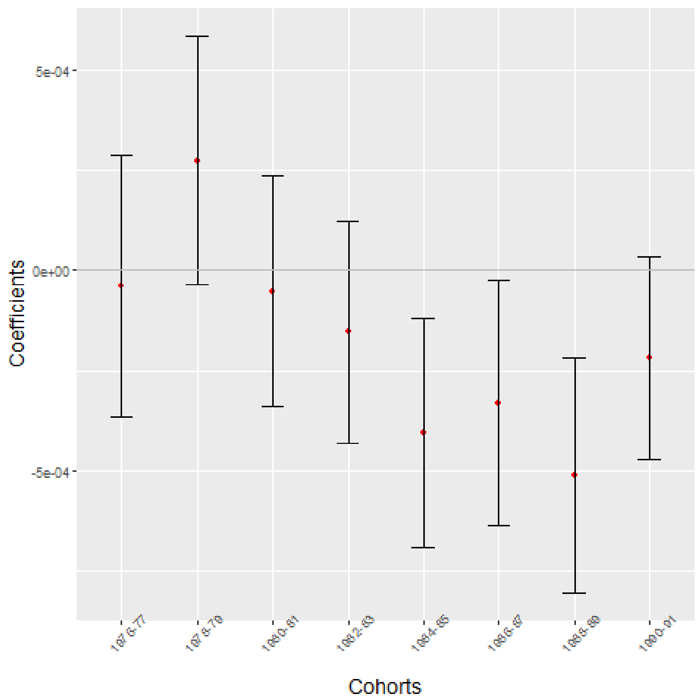


Figure 15: $\hat{\beta}$'s estimated from (2), for completion of primary school after controlling for difference in Muslim population.

I provide corresponding estimates for illiteracy levels, secondary and college education in Figures A.12, A.13, and A.14, respectively. I draw the same conclusions as above.

6 Conclusion

In this paper, I evaluated the long-term impacts of a violent event of great political significance in contemporary India. Surprisingly, I found that Muslims perform better in cities that were more susceptible to communal violence in terms of early education outcomes, whereas overall educational mobility of Muslims has been declining in the country Asher et al. (2018). I also found that cities that were closer to the route of the *Ram Rath Yatra* are more segregated than cities farther away.

Based on the reduced form estimates, I isolated the causal impact of neighbourhood effects on Muslim education outcomes, using distance from *Yatra* route as an instrument. I found that higher dissimilarity index leads to better early education outcomes for Muslims. I also demonstrated that the direction of the effect remains unchanged under less restrictive assumptions on the validity of the instrument.

I discuss various mechanisms through which neighbourhood effects could improve education outcomes. I stipulate that Muslim enclaves may witness *stronger ties* within the community in the aftermath of communal violence. In this process, Muslims may not be forming *weak ties* outside the community. This affects their ability to bargain for positions in higher education institutions.

My findings open various avenues for future research. Firstly, it is worth enquiring how Muslim parents make schooling decisions for their children in cities, as we need a better understanding of school choice mechanisms in urban India. Secondly, the available data limits our understanding of inputs that produce quality education in urban schools. This has serious policy implications in Developing countries.

Thirdly, I have only employed cross-sectional variation in segregation. Temporal variation in residential segregation would enable estimation of neighbourhood effects on education outcomes for various disadvantaged social groups. Adukia et al. (2019) also highlight that research employing better measures of residential segregation, than the one employed here, would also bring valuable insights.

Finally, research backed by data on social networks, in segregated and integrated neighbourhoods, would provide a concrete understanding of the mechanisms through which the choice of residential neighbourhoods impacts educational outcomes of historically disadvantaged social groups.

This paper contributes to existing literature on inter-group inequality in Developing countries by demonstrating the effect of communal violence on education outcomes of historically disadvantaged groups. I have also highlighted channels through which communal violence drives group education outcomes, and a deeper investigation into these mechanisms is a subject for future research.

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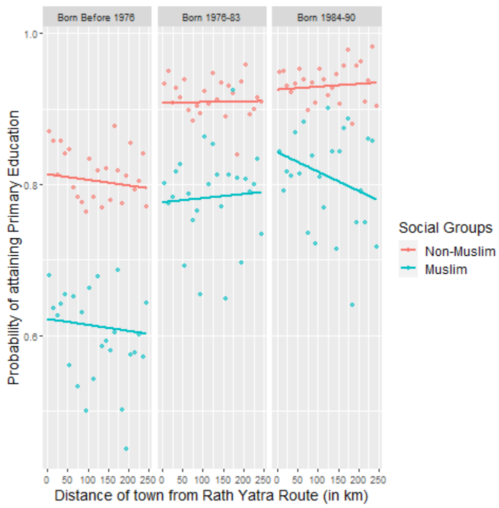
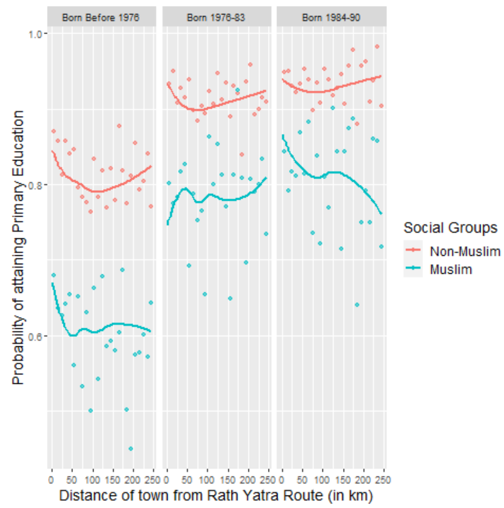
Appendix A: Graphs



(a) Linear Model



(b) Loess Model

Figure A.1: Probability of attaining primary education by religion, cohort, and distance from *Yatra*

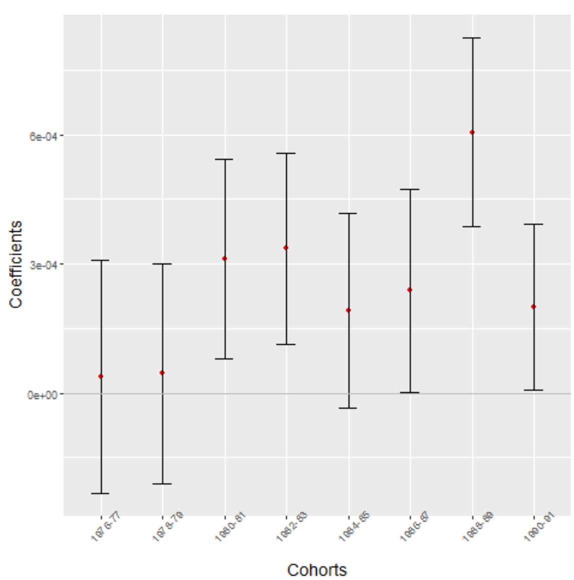


Figure A.2: $\hat{\beta}$'s estimated from (2), for illiteracy levels

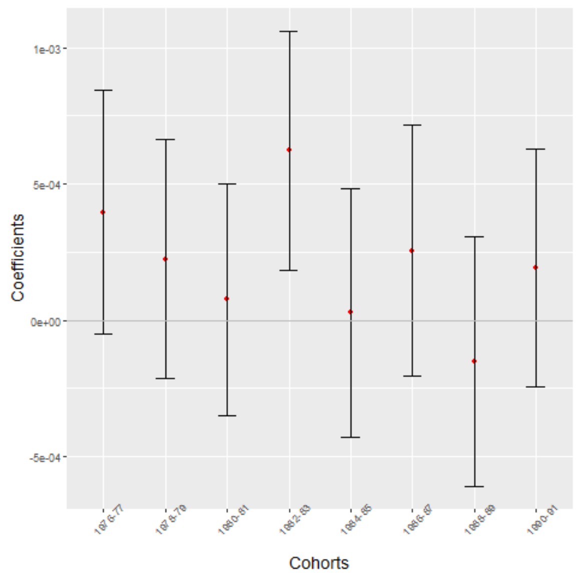


Figure A.3: $\hat{\beta}$'s estimated from (2), for completion of secondary school

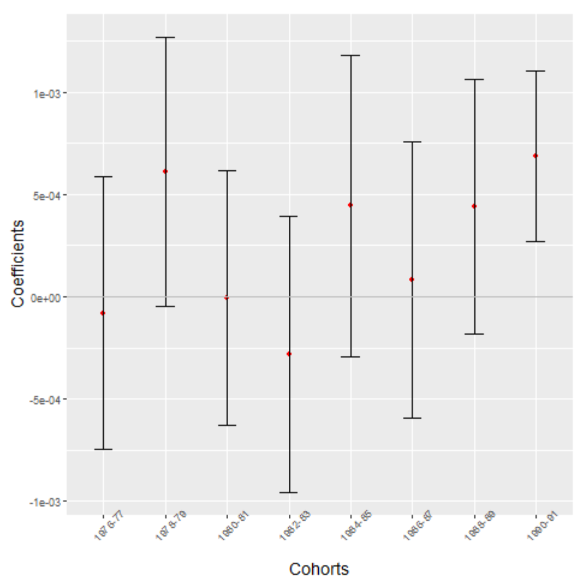


Figure A.4: $\hat{\beta}$'s estimated from (2), for attainment of college education

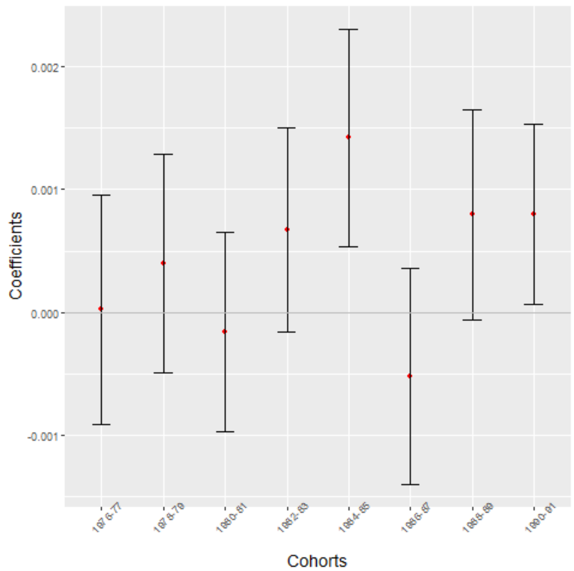


Figure A.5: $\hat{\beta}$'s estimated from (3) with respect to *placebo Yatra*, for illiteracy levels

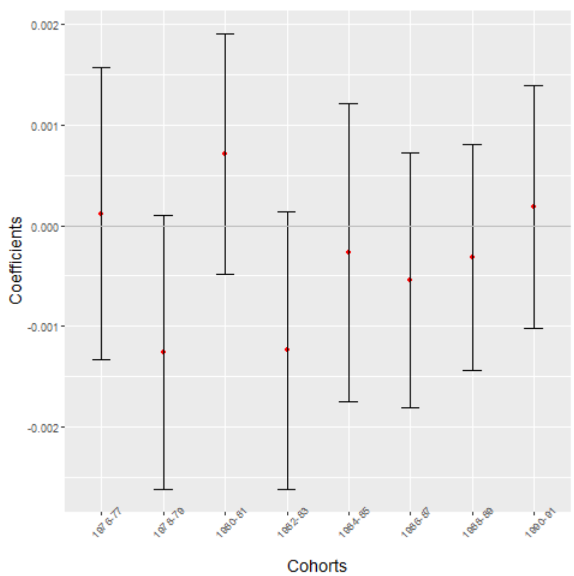


Figure A.6: $\hat{\beta}$'s estimated from (3) with respect to *placebo Yatra*, for illiteracy levels

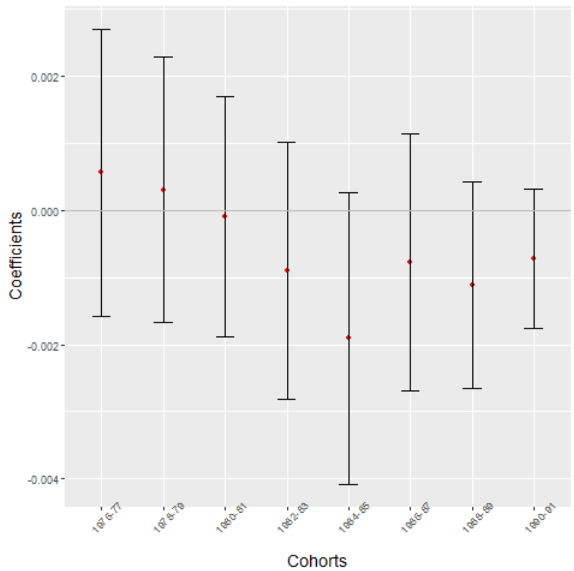
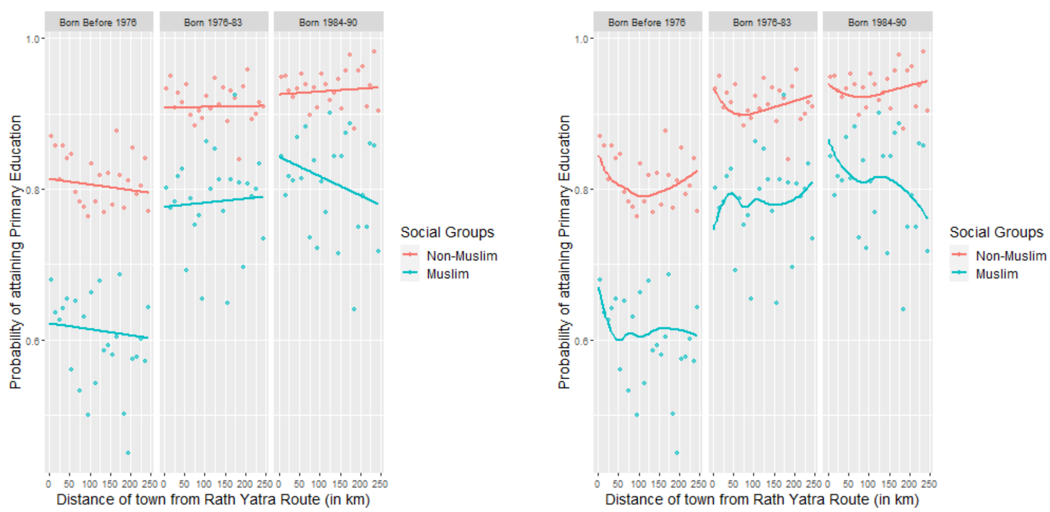


Figure A.7: $\hat{\beta}$'s estimated from (3) with respect to *placebo Yatra*, for illiteracy levels



(a) Linear Model

(b) Loess Model

Figure A.8: Probability of attaining primary education by religion, cohort, and distance from placebo *Yatra*

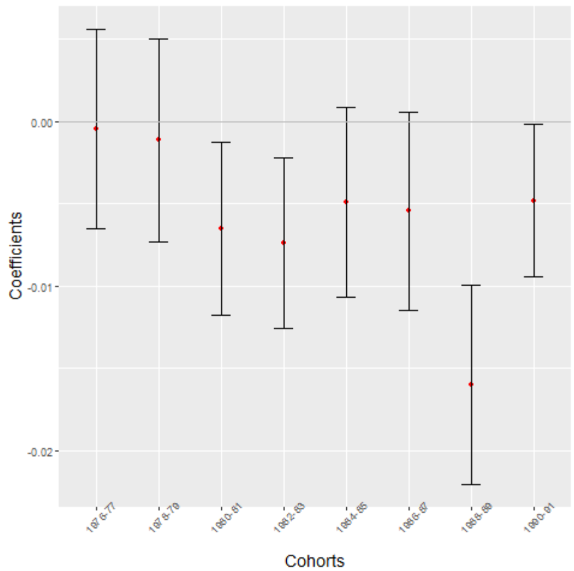


Figure A.9: IV estimates of $\hat{\beta}$'s from ($\star\star$), for illiteracy levels.

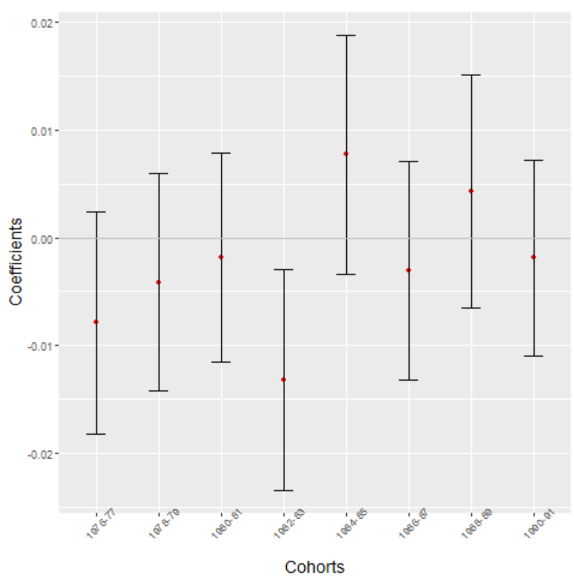


Figure A.10: IV estimates of $\hat{\beta}$'s from ($\star\star$), for completion of secondary school.

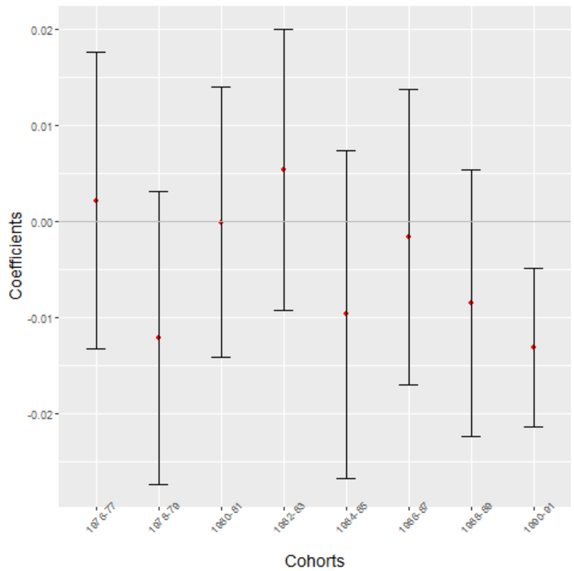


Figure A.11: IV estimates of $\hat{\beta}$'s from ($\star\star$), for completion of college

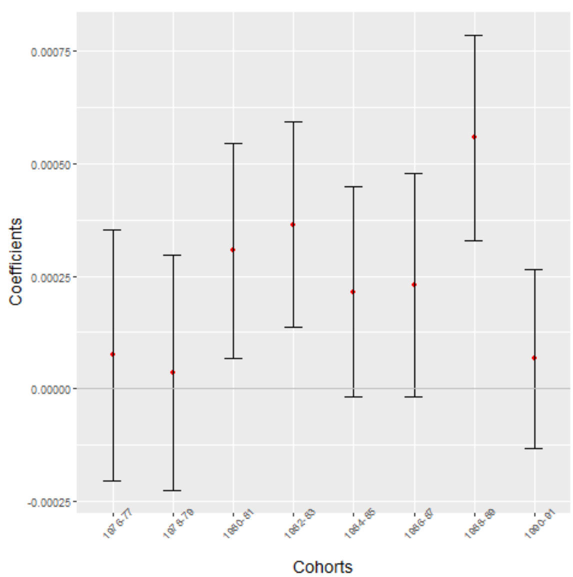


Figure A.12: $\hat{\beta}$'s estimated from (2), for illiteracy levels after controlling for difference in Muslim population.

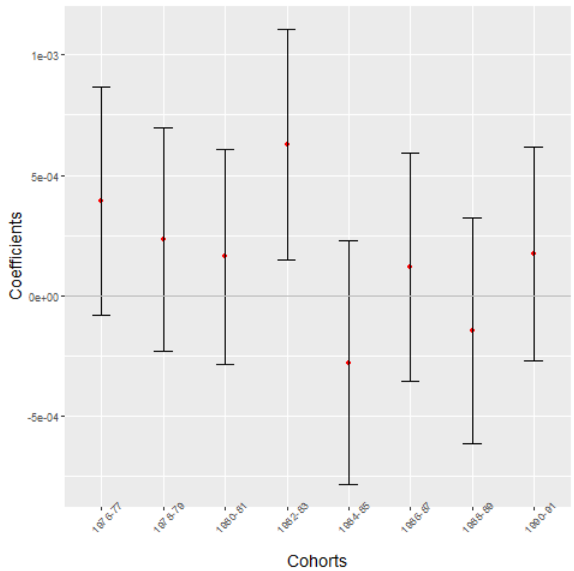


Figure A.13: $\hat{\beta}$'s estimated from (2), for completion of secondary school after controlling for difference in Muslim population.

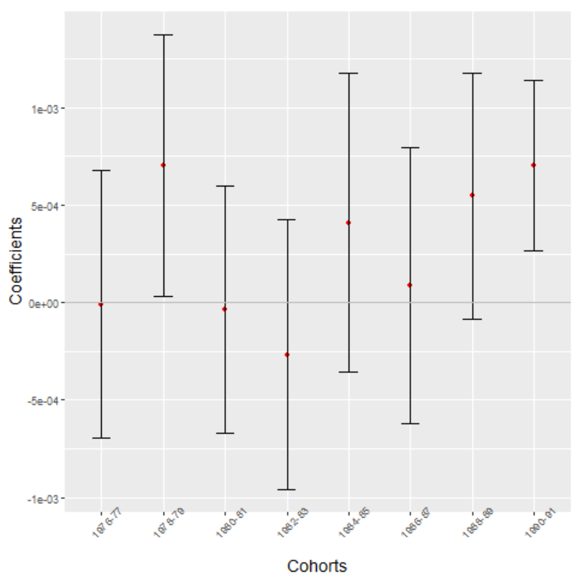


Figure A.14: $\hat{\beta}$'s estimated from (2), for college education after controlling for difference in Muslim population.

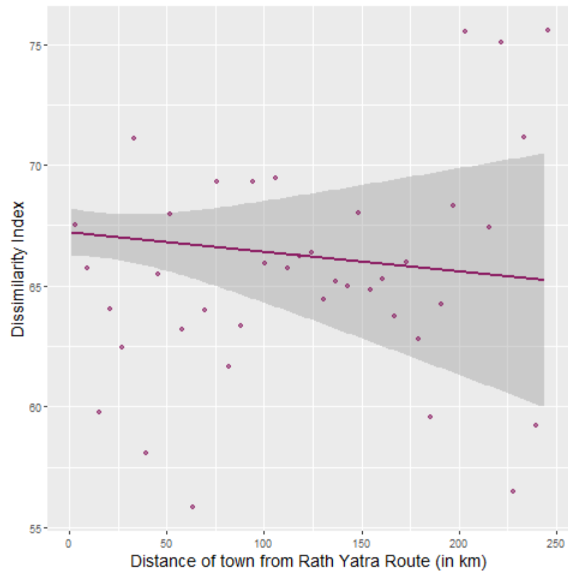
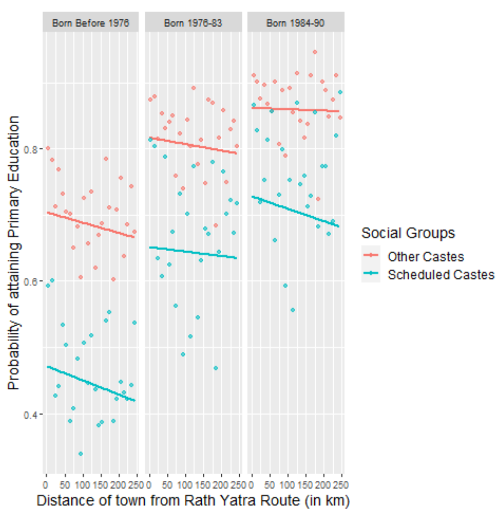
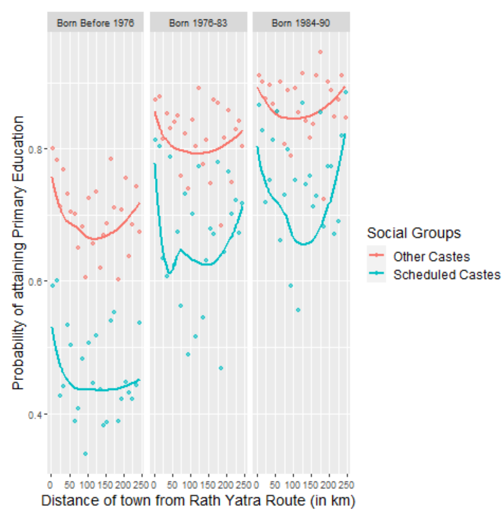


Figure A.15: Relationship between caste-based segregation measure (dissimilarity index for scheduled castes × 100) and distance from *Yatra* route in kilometres.



(a) Linear Model



(b) Loess Model

Figure A.16: Probability of attaining primary education by religion, cohort, and distance from *Yatra* for untreated groups (Scheduled Caste)

Appendix B: Tables

Distance from Yatra (in km)	0 to 50 (N = 63)	50 to 100 (N = 52)	100 to 150 (N = 30)	150 to 200 (N = 21)	200 to 250 (N = 16)
Urban Population	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
2010	860,772 (2,891,720)	42,831 (21,389)	51,719 (23,873)	46,532 (22,279)	61,608 (40,284)
2000	711,609 (2,392,860)	44,498 (18,465)	52,610 (23,772)	47,561 (21,078)	69,836 (45,792)
1990	528,602 (1,772,673)	34,963 (12,855)	41,373 (18,612)	39,743 (22,183)	51,743 (35,830)

Table B.1: Balance of Urban Population across groups (by distance from *Yatra*).

Variable Name	Notation	Across City	Across Households	Time/ Cohort
Distance from *Yatra* Route	z_c	Yes	No	No
Segregation Measure	θ_c	Yes	No	No
Violence	ρ_c	Yes	No	Yes
Whether attained Education Level e	σ^e_{ihct}	Yes	Yes	Yes
Whether Muslim	d_{ihc}	Yes	Yes	No
City Controls	X_{ct}	Yes	No	Yes
Household Controls	X_{hct}	Yes	Yes	Yes

Table B.2: Variation in variables on interest

	(1)	(2)	(3)
Distance from *Yatra* (in km)	-.023836*	-0.0206*	
	(0.0105)	(0.0104)	
Log of Distance from *Yatra*			-1.832*
			(0.706)
Population Controls	NO	YES	NO
Muslim Enterprise Controls	NO	YES	NO
N	177	177	177
F	4.181	5.112	6.729

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.3: OLS estimates from regression of Segregation Measure (Dissimilarity Index $\times 100$) on Distance from *Yatra*, with city level controls

	(1) Born 1981-85	(2) Born 1986-90
Illiteracy Level		
Yatra distance × Muslim × Muslim Enterprise Share	0.0000161*	0.0000198*
	(0.00000765)	(0.00000771)
Muslim × Muslim Enterprise Share	-0.00310***	-0.00340***
	(0.000872)	(0.000894)
Primary School		
Yatra distance × Muslim × Muslim Enterprise Share	-0.0000243**	-0.0000201*
	(0.00000924)	(0.00000962)
Muslim × Muslim Enterprise Share	0.00365***	0.00350**
	(0.00105)	(0.00112)
Secondary School		
Yatra distance × Muslim × Muslim Enterprise Share	-0.00000724	-0.0000153
	(0.0000152)	(0.0000153)
Muslim × Muslim Enterprise Share	0.000679	-0.000213
	(0.00176)	(0.00175)
College		
Yatra distance × Muslim × Muslim Enterprise Share	-0.0000297	-0.0000642**
	(0.0000224)	(0.0000220)
Muslim × Muslim Enterprise Share	0.00298	0.00574*
	(0.00256)	(0.00246)

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.4: Heterogeneous treatment effects by share of Muslim enterprises in a city.

Section C: Concepts and Definitions

Dissimilarity Index

Residential segregation, for some city c, is measured with the Dissimilarity Index (Massey, 1990):

$$d_c = \frac{1}{2}\sum_i \Big|\frac{m_{ic}}{M_c} - \frac{h_{ic}}{H_c}\Big| \tag{9}$$

where, m_{ic} is the Muslim population in city c in EB i, M_c is the total population of Muslims in city c, h_{ic} is the non-Muslim population in city c in EB i, and H_c is the total population of non-Muslims in city c.

Essentially, the dissimilarity index captures evenness in the distribution of the two social groups across neighbourhoods that constitute a city. Its value ranges between 0 and 1, where the value of d_c attains the maximum value when each neighbourhood has residents from only one group. On the other hand d_c is at its miniumum when $\frac{m_{ic}/T_c}{M_c/T_c} = \frac{h_{ic}/T_c}{H_c/T_c}$ in all neighbourhoods i in city c where T_c is the total population in the city. In other words, $d_c = 0$ when proportion of each group in each neighbourhood equals the proportion of each group in the city, i.e.

$$\frac{m_{ic}/T_c}{h_{ic}/T_c} = \frac{M_c/T_c}{H_c/T_c}$$