Erasmus School of Economics

Master Economics and Business Economics
Track: International Economics

Trade Liberalization and Political Violence in India

Abstract

Between 1985 and 1995, many countries throughout the developing world adopted trade liberalization policies. India's sudden and externally imposed export policy (1992-1997) provides for an interesting context to the study of unintended effects of trade. Building on the analysis of Topalova (2010), this thesis explores whether Indian districts that, by their industrial composition before the reforms, were more exposed to trade, have experienced more violent conflict. I study 431 Indian districts before and after the liberalization period. Conflict data is retrieved from the Global Database of Events Language and Tone. Estimating a Poisson pseudo-maximum likelihood regression as well as a negative binomial regression, I find no conclusive and significant effect of trade liberalization on the conflict count of districts that are relatively more exposed to trade. While results are often insignificant and not robust to adding controls, in general the signs of the coefficient do show a pattern: I find that a decline in the average tariff in a district is often associated with an increase in violent events in the short term. This effect reverses after 1997.

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I. Introduction

The global economy is organized around on the notion, predicted by standard economic theory, that trade liberalization leads to an increase in overall welfare. During the late 1980s and early 1990s, the ideological dominance of economic liberalism and lending programmes conditional on strong trade reforms incentivized many developing countries to move from a protectionist economy to a liberal economy (Edwards, 1997).

Critics of globalization have warned against potential adverse effects of trade liberalization, above all against the potential within-country distributional effects of trade liberalization. Numerous studies have found empirical evidence of trade openness disproportionally affecting certain populations within a country.[1] This is especially problematic for developing countries, as these countries are often characterized by large vulnerable populations, weak institutions, and inadequate social safety nets to mitigate these effects, aggravating the risk of increasing social and political instability.

The effects of sudden loss of trade protection may extend further than the labor market. There exists much anecdotal evidence of countries finding themselves in dire economic, political, and social situations following economic liberalizations, that have in some instances lead to civil unrest and violent protests. A recent example are the ongoing protests in Haiti. For Haiti, liberalization entailed the destruction of previously protected industries. The wages of the exporting garment industry that was set up to participate in the global economy are too low to secure a sufficient standard of living. In Indonesia in 1998 and Argentina in 2002, protesters expressed similar grievances (Stiglitz, 2002).

It is important to continuously assess the effects of trade, in particular any unintended effects. Influential institutions operate from a perspective that inherently links trade to economic growth and development. This study empirically considers whether a short and/or medium-term increase or decrease of political violence can be perceived after the 1991 trade reforms in India. The research question is as follows:

[1] See e.g. (Hanson & Harrison, 1999) for Mexico; (Galliani and Sanguinetti, 2002) for Argentina; Colombia (Attanasio et al, 2001) for Colombia.

Did India’s trade liberalization episode of 1992-1997 result in more violence in Indian districts that were exposed to more trade as compared to districts that were less exposed to trade?

This paper does not assess the role of trade liberalization in influencing aggregate trends of Indian conflict, but rather assesses the relative impact on areas that experienced more or less liberalization. While literature has found some general predictors of conflict, the phenomenon remains complex and rooted in history or conjunctural events that are unrelated to economic liberalization. This paper does not draw a conclusion about the general relationship between trade and political conflict. Moreover, this study only assesses the impact of trade liberalization through the labor market. Other effects of trade liberalization, such as decreases in consumer prices, are not included.

1. Results

Controlling for local district characteristics and nation-wide yearly shocks, and in some specifications unobservable variables that vary, I find no conclusive evidence of tariff liberalization affecting the violence count of districts that are more exposed to trade. I do find that the signs of the coefficients of violence right after tariffs start decreasing and violence after the trade liberalization policy ended in 1997 differ. In the short term, average tariff exposure and violence are negatively related, while in the medium-term average tariff exposure and violence count are positively related.

2. Literary landscape

This paper is in line with other studies that study the Indian trade liberalization episode. Topalova (2005), Hasan, Mitra & Ural (2006) and Topalova (2010) study how the policy change has affected poverty. Edmonds, Topalova & Pavcnik (2009) studied the effects on human capital investment. Topalova & Khandelwal (2011) studied the effect on firm productivity. Prasad (2012) and Iyer & Topalova (2014) studied the effect on crime. Anukriti & Kumler (2019) look at the relationship between trade, female income and fertility in India.

More generally, it fits in the strand of literature that assesses the (unintended) consequences of trade (for democracy see (Hayam, 2022), for crime (Dix-Carneiro et al., 2018)). Specifically, the research that takes an interest in the relationship between trade and conflict and uses a within-country analysis to do so, comparable to Dube & Vargas (2013) and

Berman & Couttenier (2015). Methodologically, the present research builds on Topalova (2010) and draws inspiration from Kovak (2013) and Dix-Carneiro & Kovak (2017). To my knowledge, there is no research that links the Indian trade liberalization episode to the incidence of violence through the effects of trade on the labor market.

3. Outline

The paper proceeds as follows. Chapter 2 will outline the context in which the relationship is assessed, by firstly discussing the Indian liberalization episode and presenting a brief summary of the history of political violence in India. Chapter 3 is the literature review. Chapter 4 discusses the data employed in this thesis. In chapter 5 I will describe the methodology. In chapter 6 the results are discussed. Chapter 6 consists of the discussion and concludes.

II. Context

1. India's Trade Liberalization

The identification strategy builds on Topalova (2007, 2010). Topalova (2007) takes an innovative approach on one of these liberalization episodes, namely the liberalization of the Indian economy between 1992 and 1997. After independence, India's development strategy was inward looking and highly interventionist (Cerra & Saxena, 2000). In 1991 India experienced a large balance-of-payment crisis. This crisis is largely attributed to a spike in oil prices due to the Gulf War in 1990, a drop in remittances from middle Eastern Indian migrant workers and a declining demand of trading partners (Cerra & Saxena, 2000). India requested a standby agreement from the International Monetary Fund (IMF) in August 1991. Support from the IMF was conditional on structural reforms. In the area of trade policy, reforms consisted of reduction in the level and dispersion of tariffs, as well as levitating quantitative restrictions. This resulted into the 1992-1997 export-import policy plan Topalova (2005).

This policy change is well-suited for causal interpretation of results because of the following reasons. Firstly, the change in policy was comprehensive and large. In the span of several years, the tariffs of most sectors, including agriculture with the exception of cereals and oilseeds, were significantly reduced.[2] The average tariff declined from 117% to 39% and the

[2] Figure 1, panel C in Topalova (2005) shows that average tariff declined between 1997 and 1987 in cereals and oilseeds, agriculture (other than cereals and oilseeds), and manufacturing and mining over time.

share of imports covered by NTBs fell from 82% to 17% between 1990-1991 and 1999-2000 (Anukriti & Kumler, 2019). The standard deviation of tariffs was also reduced, meaning that tariff rates across sectors lie closer together. The decline in tariffs resulted in a significant increase in trade flows: within approximately 10 years, imports increased from 13% of India's GDP to 19% precent Topalova (2010).

Importantly, Topalova (2010) discusses several possible sources of endogeneity of the methodology and mitigates the concerns. Firstly, she argues that the sudden and externally imposed nature of the policy change suggests that Indian households did not have the opportunity to alter decisions regarding employment, consumption and production anticipating trade liberalization. The policy change was not preceded by political discussion.

Secondly, Topalova (2004, 2005) argues that there is no evidence that tariff changes are related to the various products and industry in systematic ways.[3] She substantiates this by looking for a correlation between firm productivity levels and productivity growth preceding liberalization and the tariff reduction assigned to the industries. She finds that no correlation exists for the period 1989-1996, but after 1997 future tariffs are negatively correlated with current productivity. Therefore, Topalova (2010) as well as subsequent studies employing the same identification strategy in Indian context such as Anukriti & Kumler (2019) focus their studies on the period before 1997.

The liberalization episode consisted of significantly reducing tariff and non-tariff barriers (NTB) to trade, the latter often took on the form of import licenses. These NTBs were a significant part of Indian trade policy and gradually declined over the sample period, albeit more slowly. Following earlier studies (Anukriti & Kumler, 2019; Edmonds et al., 2010), I choose to not take NTBs into account and focus solely on tariff reduction. Firstly, NTBs are difficult to capture in data and difficult to compare over time. Moreover, NTB data is not easily available at detailed level. Secondly, excluding NTBs in the analysis is not likely to affect the analysis as NTBs and tariffs move similarly over the sample period, namely downwards. Topalova (2010), in response to Hasan, Mitra and Ural (2007), who ascribe their differing results to Topalova (2005) to the exclusion of NTBs, included NTBs in her original analysis and found statistically insignificant results. To the extent that the two types of trade barriers

[3] Table 1 in Topalova (2005) shows that industry tariff declines are not correlated with industry log wage, industry skill-intensity (measured by the share of nonproduction workers in industry employment), industry capital intensity (measured by capital-labor ratio), log output, average factory size, log employment, pre-reform output growth, and pre-reform employment growth. In addition, Topalova (2004) shows that tariff changes between 1987 and 1997 were not correlated with firm-level productivity.

are positively related, some portion of the effect of tariff reduction may be caused by NTBs. Furthermore, the coefficient of NTBs cannot be causally interpreted, as they were implemented more slowly (thus theoretically enabling households to alter decisions in anticipation) and more deliberately across different industries (Topalova, 2010).

2. Violence in India

India's post-independence history has been marked by a persistent backdrop of political violence, providing for an especially useful context to assess the trade-violence nexus. India displays many characteristics that are traditionally associated with civil conflict in economic literature: a large population, low-income levels, political instability, and war-prone and undemocratic neighbors (Hegre & Sambanis, 2006).

Conflicts in India are often an amalgamation of political, religious, ethnic, tribal, demographic and socio-economic factors (UCDP, 2022). The partition of India in 1947 into India and Pakistan following British colonial rule has fueled the long standing and ongoing conflict between India and Pakistan over the Jammu and Kashmir region, politicizing religious and cultural identities. Hindu-Muslim riots throughout the country have resulted in significant loss of life and property, with more than 7,000 killed between 1950 and 1995 (Bohlken & Sergenti, 2010).

In the 1980s and early 1990s, the Sikh separatist movement in Punjab aimed for an independent Sikh state, operating by violent clashes and terrorist attacks. A significant event was Operation Blue Star in 1984, a large-scale military counterinsurgency intervention by Indian armed forces in Amritsar, Punjab (UCDP, 2022). Several states in North-East India, such as Assam and Nagaland, also witnessed insurgency movements. Demographic shifts caused by immigration exacerbated existing economic problems such as land-alienation, poverty, and unemployment, reinforcing a sense of marginalization among tribal groups (UCDP, 2022).

In the 1990s, India was also confronted by the resurgence of leftist guerrillas, the Naxalite movement, previously separated from communist political parties. These guerillas operated in rural areas of states including Andhra Pradesh and Orissa, aiming to undermine the administrative structures of government authorities. Through land redistribution and measures aiming to increase agricultural wages, boosting popularity among impoverished communities. Through fragmentation of private militant groups, this conflict also was fought along the dimension of castes (UCDP, 2022).

Preceding India's trade openness policy, the country experienced rising political uncertainty as the ruling coalition became entangled in caste and religious disputes, resulting in nation-wide riots. Violence peaked in 1991 with the assassination of a former Indian prime minister during election campaigns (Cerra & Saxena, 2000). India's tumultuous history of political violence highlight the intricate nature of socio-political tensions in India. Notably, in most conflicts, whether religiously, culturally, or politically motivated, a sense of economic deprivation and unmet demands seem to be a significant underlying factor. It is in this context that the trade-violence relationship is studied.

III. Literature Review

In this section I will present the theoretical literature on the trade – conflict link, as well as studies that assess this relationship empirically. I will describe the two most common intermediate causal channels in literature: income and political grievances. Therefore, studies that focus on the link between trade and income as well studies focusing on the relationship between ethnicity and conflict will be discussed. The aim of this chapter is to discuss common and contradictory findings on the subject, as well as situating this paper in the existing literature.

1. Scope

Civil conflict may arise because of (a combination of) many different social, political, and psychological phenomena. This is reflected in many studies across different disciplines, linking the incidence, onset, or severity of civil war to numerous variables, ranging from social (ethnicity, history) to geographic (ruggedness, closeness) and political factors (democracy). At the beginning of the century, conflict captured the interests of empirical economists. Earlier influential studies focus on civil wars, which is defined as internal conflicts that have resulted in more than 1,000 battle deaths Blattman & Miguel (2010) While the present study is not limited to large-scale conflicts, I will discuss economic literature of civil war as many subsequent studies of violence on smaller scales are built on the theoretical frameworks developed studies of civil war. Findings of studies focusing on riots and smaller scale violent events are closely related to the findings resulting from the civil war literature, see e.g. (BOHLKEN & SERGENTI, 2010). Moreover, I will also include research that study crime.

Theories of economic analysis of crime (Becker, 1968) have been applied to conflict by e.g. Hirshleifer (1995).

Earlier studies typically either do not find any statistically significant relationship between trade and conflict, or find evidence that trade reduces conflict. Most of these studies work with a larger cross-country sample and employ variables that proxy trade openness, usually trade as a fraction of GDP, resulting in correlates that might suffer from omitted variable bias or reverse causality (Bussman et al., 2006; Bussmann M & Schneider G, 2007; Magee & Massoud, 2011; Barbieri & Reuveny, 2005).

2. *Trade and income*

Trade may affect political violence through several channels. The most straightforward and most extensively researched channel is income. With theoretical foundations in the Heckscher-Ohlin model, it is widely accepted that trade is associated with an increase in national income and many studies depart from that assumption.[4] Frankel & Romer (1999) found early empirical evidence of this relationship, relying on a geographic instrumental variable to supposedly ensure exogeneity. Feyrer (2009), employing the closing of the Suez Canal as a natural experiment, shows that 17% of the cross-country income variation can be explained by differences in predicted trade growth. Building on this assumption, the following paragraphs describe studies that link income to violence.[5]

3. *Income and violence*

The most influential theory that links income to violence is the "contest model", which highlights the importance of opportunity costs. Rational agents decide whether to produce or to appropriate goods by weighing relative returns of production and conflict. This theory is empirically backed by influential papers, such as Collier & Hoeffler (2004) and Miguel et al. (2004), both employing a cross-country study of sub-Sahara Africa and using rainfall as a proxy for income. While these two studies focus on the simple income-violence relationship, thus not necessarily focusing on trade, the underlying hypothesis has been extended to trade-induced income shocks. The rationale is that agricultural crops are more closely linked to household

[4] Widely accepted certainly does not imply consensus, see e.g. Rodrik, D., & Rodriguez, A. (1999). Trade policy and economic growth: A sceptic's guide to the cross-national evidence. Centre for Economic Policy Research Discussion Paper: 2143. Still, this relationship is the basis of many policies of (inter)national institutions.

[5] Due to time constraints, I did not verify if this assumption holds on the district level in India.

income, as compared to state revenue. Rising (export) prices of agricultural goods increases income, and therefore lowers the risk of conflict. Income and conflict are inversely related.

The state capacity theory also assumes that income and conflict are inversely related. This theory entails that states are better able to suppress insurgency and conflict if price shocks result in an increase in state revenue. Richer states have stronger executive forces, better infrastructure, and more efficient administration, resulting in stronger central control (Fearon & Laitin, 2003).

Another theory that incorporates state revenues comes to an opposite conclusion. Contrary to the first two theories, income and conflict are assumed to be positively related. In the state-as-the-prize (rapacity) theory, states are regarded as a prize that may be captured. As rising export prices raises state revenue, the incentives for violence are stronger. This theory may be particularly relevant in countries with weak institutions. This theory is often linked to extractive commodities such as mining and petroleum. Often very valuable and easily taxable, extractive commodity shocks are argued to disproportionally affect state revenues (Bazzi & Blattman, 2014).

Besley & Persson (2011) develop a theoretical model in which the incumbent and opposition can choose to engage in one sided violence or conflict to obtain revenue. The outcomes are peace, repression by the incumbent or civil war. It is argued that insurgencies are contained by consensual political institutions, as a proportional electoral system can ensure minority protection. Thus, sound political institutions may mitigate the workings of the state-as-the-prize theory.

Martin, Thoenig & Mayer (2008) study all countries between 1945 and 2001 and find evidence for competing theories by differentiating between intense civil wars and lower scale conflicts. They find that international trade may discourage severe civil wars through the opportunity cost mechanism. However, international trade may also increase the risk of lower-scale conflicts. This is explained by another mechanism: international trade substitutes internal trade, lowering within-country dependency. As dependency is said to foster peace, the risk of smaller conflicts is increased.

Other studies try to find empirical evidence for conflicting theories by disaggregating export price shocks between different types of commodities associated with either household income or state revenue. Dube & Vargas (2013) study this relationship in Colombia, finding that a rise of coffee prices – a labor intensive good – lowers the incidence of violence in coffee-exporting municipalities, congruent with the opportunity cost effect as wages also increased. In contrast, rising oil prices export prices increase municipality revenue and increases violence in oil-exporting municipalities, in accordance with the state rapacity theory.

However, Bazzi & Blattman (2014) come to a different conclusion with regards to the state rapacity theory studying 65 developing countries between 1957 and 2007. They find that commodity export price shocks do not affect the outbreaks of new conflict and, in fact, decreases severity and length of intense episodic type of conflicts. This effect is found also in countries with weak institutions. Moreover, they find a that rising mineral and oil prices are associated with less conflict, leading them to tentatively conclude that the state prize effect is not an empirically important motive for insurgencies.

While Berman & Couttenier (2015) do not distinguish between the effects of different types of commodities, they do distinguish between short-run and long-run trade shocks. They examine 13 African countries and find that incidence, intensity, and onset of sub-national conflict are significantly related to local variations in income. They make the link with trade by arguing that this relationship is less pronounced in regions that lie further away from the nearest port – thus less exposed to trade. Also at the national level, conflict probability is only affected in the most open regions. Furthermore, they extend the coverage of price shocks by also constructing a variable that reflects a longer lasting demand shock in international trade, namely the number of banking crises in countries to which the most exports are directed. They find significant negative effects of longer lasting shocks as well.

Amodio et al. (2020) focus on the 1995-2010 period and study the effects of Preferential Trade Agreements between 27 countries in the Global South and major countries in the North. Utilizing the variation in agricultural tariffs an agro-climatic conditions, they find that liberalization agreements induce political violence and instability in grid cells that are suitable for the production of exported products.

The studies that most closely resemble the methodology employed in this thesis are conducted in the field of crime. Dix-Carneiro et al. (2018) employ regional tariff changes to assess the effects of changes in economic conditions brought on by trade liberalization on crime in Brazilian context. They find that regions with a relatively larger tariff-reduction show more crime for the following eight years after the liberalization episode. This crime increased was accompanied, thus arguably caused, by a deterioration in labor market conditions that followed a similar pattern as the dynamics of crime after liberalization. They also found a decline of government revenues, implying less public goods provision, which persisted longer than the effects in the labor market. Interestingly, in the context of rural Indian districts exposed to trade, Edmonds, Pavcnik & Topalova (2010) find that tariff reduction has resulted in a relative worsening of education returns, consistent with the argument trade liberalization affecting public goods.

Iyer & Topalova (2014) utilize the data on the Indian tariff liberalization episode to estimate the effects on crime and find that reductions in tariff led to an increased incidence in violent crimes and property crimes. The relationship between rainfall and crime follows a similar pattern, suggesting that the income channel is an important determinant of crime. This is contrary to prior evidence by Prasad (2012), who finds that the Indian trade liberalization did not lead to more homicides. This analysis was conducted at the state-level. Prasad (2012) interpreted his results by arguing that economic liberalization would reduce the incentives for illegal trade which is often associated with violent crime.

Following this strand of literature, I define the following hypothesis:

Hypothesis 1: Indian districts that are more exposed to trade liberalization will experience more instances of violence.

4. *Trade and inequality*

The Stolper/Samuelsen theorem extends the Heckscher-Ohlin model and explains how trade may exacerbate income inequality, at least in the short run. As the relative price of a good increases, the return to the factor that is used intensively in the production of that good also increases, while the return to the other factor decreases. This would imply that wages in developing nations in labor-intensive industries should be increasing, as developing nations often export labor-intensive goods. However, empirical evidence in developing countries often suggests the opposite.[6] Banerjee & Newman (2003) suggest that the theory does not take slow adjustment in capital and labor into account, disproportionally burdening the poor.

[6] See e.g. Barro (2000) and Kapstein & Milanovic (2002).

Several papers employing a similar methodology as the current paper have found fairly consistent results with regards to the disruptive effects of trade liberalization on the local income markets. The magnitude of the effect is generally also significant. In Brazilian context, Kovak (2013) found that a 10-percentage point liberalization-induced price decline led to 4 percentage point large wage decline.

Relevant to the current context, Topalova (2010) find that districts in rural areas that are more exposed to trade liberalization experienced a slower decline in poverty and a slower growth of consumption in the 1987 – 1997 period. She refutes prior contrary evidence of Hasan, Mitra & Ural (2007) who found the opposite result, which they explain to be due to the inclusion of non-tariff barriers. Banerjee and Piketty (2003) show that the post-liberalization period (1992 onwards) was associated with a very rapid increase in the incomes of the country's richest top 0.1%.

5. *(Ethnic) inequality and violence*

Another channel through which trade affects conflict can be grouped under the denominator "grievances". The previous paragraph argues that trade may affect inequality – importantly, there is empirical evidence hereof for the rural subsample of the dataset employed in this thesis. Income inequality may manifest itself along ethnical lines. Chua (2002) hypothesizes that a greater role of market forces in a democratic setting may sometimes result in ethnic or class conflict. Bezemer & Jong-A-Pin (2013) provide cross-country empirical evidence in support of this argument in sub-Saharan Africa.

Early, influential papers finding evidence for the opportunity cost effect, often find little explanatory power for variables proxying political grievances. Earlier papers measured grievances by variables capturing, inter alia, the extent of democracy or the level of ethno-linguistic fragmentation (Collier & Hoeffler, 2004;Fearon & Laitin, 2003; Miguel et al., 2004).

Later work assigned this conclusion to an incorrect conceptualization and measurement of inequality. Østby (2008) argues that the multidimensional nature of inequality is not sufficiently considered. Many studies look at 'vertical inequality', which captures income inequality between individuals. Case studies suggest that 'horizontal inequalities' might be more relevant, the concept linking economic inequality to ethnic polarization. This makes sense theoretically, as civil wars typically occur between groups and not between individuals.

In her study, Østby (2008) considers 36 developing countries in the period 1986 – 2004. She defines horizontal social inequality as the inequality between the two largest ethnic groups along the lines of ownership of household assets (the economic dimension) and educational opportunities (the social dimension). The indicators are constructed on a national level. She finds that social polarization and horizontal social inequality affect conflict outbreak positively but finds weaker evidence for the economic dimension.

Østby et al. (2009) disaggregate the data on a first-level administrative unit in each country, arguing that this is level of aggregation is meaningful as it may reflect regional affiliation and belonging. Again, a positive relationship between intra-regional socioeconomic inequalities and conflict is found.

Cederman et al. (2011) assesses the relationship globally on a group-level and make use of a new dataset that geographically maps ethnic groups as well as regional wealth. They find that ethnic groups below and above the average wealth in a country are overrepresented in civil war data.

This literature leads to the following hypothesis:

Hypothesis 2: The effect of trade liberalization is more pronounced in regions with large between-group ethnic inequalities.

6. *Sub-conclusion and contribution to literature*

The literature on trade, income and political violence is vast and the different conceptualization and operationalization of the dependent and independent variables render comparison difficult. Nevertheless, a relationship between trade and income, income and violence and trade and violence has been found in several different settings. Recently, strides have been made in the economic literature linking trade to violence. Recent studies have bypassed concerns regarding causal inference by exploiting exogenous shocks, often either by exploiting differences in exogeneous agroclimatic conditions to proxy local vulnerability to changes in import and export policies and exploiting global commodity prices, that are exogenously determined by world supply and demand.

Studies that proxy trade by using export price shocks (Berman & Couttenier, 2015; Bazzi & Blattman, 2014) essentially test for the income – conflict mechanism. However, these methodologies directly measure income shocks. The methodology of the current study derives part of the variation of the independent variable directly from a decrease in tariffs and part of the variation of the industrial composition of a district. This approximate trade's relationship to violence through the labor market more directly (Kovak, 2013). This paper contributes to the literature by assessing the trade-conflict relationship using tariff liberalization data to approximate export instead of price shocks that affect locations differentially based on agricultural conditions. To my knowledge, such research has not been conducted in the context of Indian trade liberalization.

Finally, much of the income shock mechanism in conflict literature is situated in the (sub-Saharan) African context. This paper contributes to this literature by shifting that geographical focus to India, which is a suitable country to assess as studies stress that this mechanism is stronger in countries with non-cohesive political institutions (Besley & Persson, 2011). Earlier literature employing similar methodologies have focused on the effects on crime.

IV. Data

The analysis is conducted at the district-level and the panel consists of 431 districts from 1987 to 2000. The sample is disaggregated at the district level because districts are generally similar with respect to their agroclimatic features and socioeconomic conditions. Moreover, it can be reasonably assumed that the district constitutes the appropriate labor market for households, which is an important consideration for the theoretical validity of the methodology (Edmonds et al., 2010). During the 1990s, the administrative division of India changed substantially. New districts were formed by splitting existing districts. I match all data to the districts used by Topalova (2010).

1. Dependent variable

For measures of political violence, I rely on the Global Database of Events Language and Tone (GDELT).[7] GDELT Events is a machine-coded georeferenced database that collects event data as reported in local and national news outlets. GDELT Data is available from January 1st, 1979 onwards. Each event is recorded once, thus subsequent reports on the same event are not treated

[7] The Uppsala Conflict Data Program (UCDP) database restricts itself to conflicts that have caused more than 25 deaths and only collects georeferenced data from 1989 onwards. This limits the scope of the paper, both conceptually and by number of total events recorded. More intense violence is concentrated in a few states – see Appendix A Figure A1. The Armed Conflict Location & Event Dataset (ACLED) contains no data coverage for India before 2016. Electoral Contention and Violence (ECAV) is a georeferenced dataset that lists incidence of violent and non-violent electoral contention from 1990 onwards on a global scale. Using this dataset results in a pre-period that is too short.

as a new event. Events are stored in CAMEO (Conflict Mediation Event Observations) format. This thesis focuses on events coded as FIGHT, defined as "all uses of conventional force and acts of war typically by organized groups". A more disaggregated account of constitutes a "FIGHT" event can be found in Appendix A Table A1.

I operationalize the dependent variable, *Violence count,* as follows:

Events are spatial-temporally matched to district-polygons and aggregated to the number of events per year. I leave out events that can only be assigned to the state or country level. Then, I account for the bias that GDELT records more events as time goes on by dividing by the number of yearly violent events in a district by total number of violent events in India in the corresponding year. This normalized variable allows comparison of relative proportions of events in each district and follows earlier research using GDELT (Data Saz-Carranza et al., 2021).

Then, I aggregate the data further to a pre-treatment period (1987 to 1991) and two after-treatment period: one reflecting conflict count in the period shortly after tariffs began decreasing (1993 to 1997), one reflecting violence count after several years of tariff reductions (1998 to 2001). Dix-Carneiro & Kovak (2017) find that in the Brazilian context the persistent effects of tariff reduction are gradually amplified. As the effects of tariff change work mostly through the labor market – the initial labor demand shock is amplified in the presence of imperfect interregional labor mobility, slow capital adjustments and agglomeration economies. I therefore test whether an amplified effect can be perceived in the period after 1997.

Figure 1: Violence in India

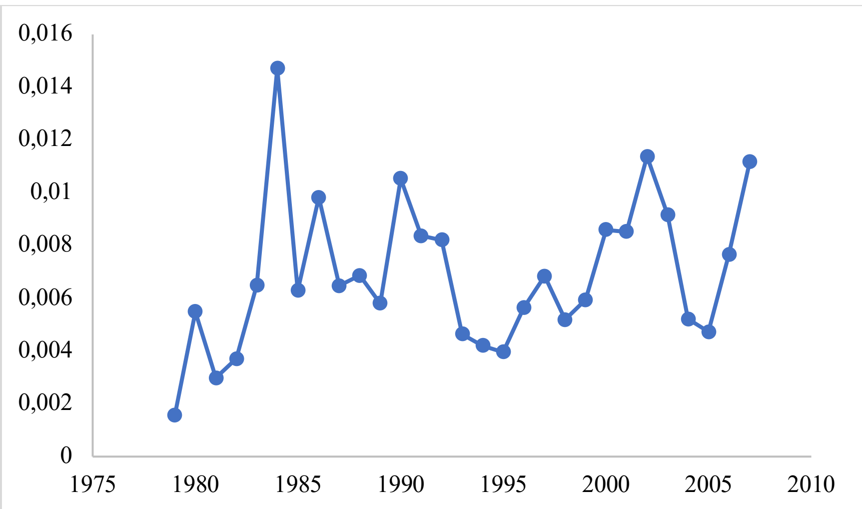


Figure 1 shows how violent events in India have evolved over time. A large outlier is 1984, which coincides with the Sikh insurgency. The following uptick in violent events can be perceived in 1991, which coincides with the political instability in India preceding the assassination of former prime minister Rajiv Gandhi. India's 1992 – 1997 tariff decrease period starts off with a concurring decrease in violent events. However, in 1995 incidents increase again until 2001, which marks the end of the sample period of this study.

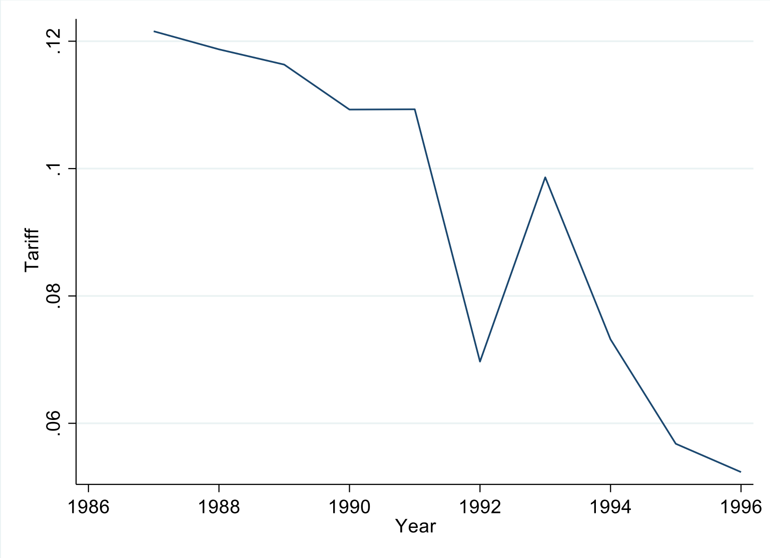
Violent events are recorded all throughout India over the sample period. Thus, there is sufficient geographic dispersion (Appendix A Figure A1). There are notable outliers. In the pre-period, the largest outlier state is Punjab. Outlier district in the post period lie mostly in the states Delhi, Maharashtra, and Jammu and Kashmir. Please refer to Appendix A Table A2 for a list of States and events. Moreover, the dataset contains many zero values. In the medium-term sample, 349 out of 862 observations are zero-values. In the short-term sample, there are 447 zero-observations.

2. *Independent variable*

Following Topalova (2010), the independent variable is constructed by interacting the initial composition of production sectors within a district with the tariff changes at the production sector level. The geographical spread of production sectors before the liberalization episode is retrieved from the Indian Census of 1991. The census publishes the industries of employment at the three-digit NIC code for each district in India. There are 450 industry codes, of which 190 are traded agricultural, mining or manufacturing industries. Important to the methodology is to underline that the industrial composition is determined prior to the reforms.

Detailed average sector-level tariffs are constructed by Topalova (2004), using data from the Indian Trade Classification Harmonized System for approximately 5,000 product lines that are matched to NIC codes using the concordance table of Debroy & Santhanam (1993).

Figure 2. Tariff Decline



Topalova (2010) and subsequent research based on the same dataset divide the dataset between urban and rural areas. Significant results are often perceived only in the rural subsample. As the GDELT Database and provides the landmark-centroid level longitude and latitude data for each event and does not distinguish between urban and rural events, I cannot divide the sample in a similar way. Therefore, I retrieve percentage of urban and rural workers from the 1991 Indian Census and calculate the average tariff level the entire district is exposed to. Figure 2 presents the tariff decline over the sample period. The nominal average tariff faced by districts in 1987 is the pre-period tariff. Tariffs in 1997 are assigned to the post period, as after this year Topalova (2005) finds that tariffs are correlated to industry measures. Tariff data is not available for Jammu and Kashmir because insurgencies in the region during the period of interest inhibited data collection. Jammu and Kashmir is an outlier state in the dataset.

Figure 3: Average tariff change between 1987 and 1997

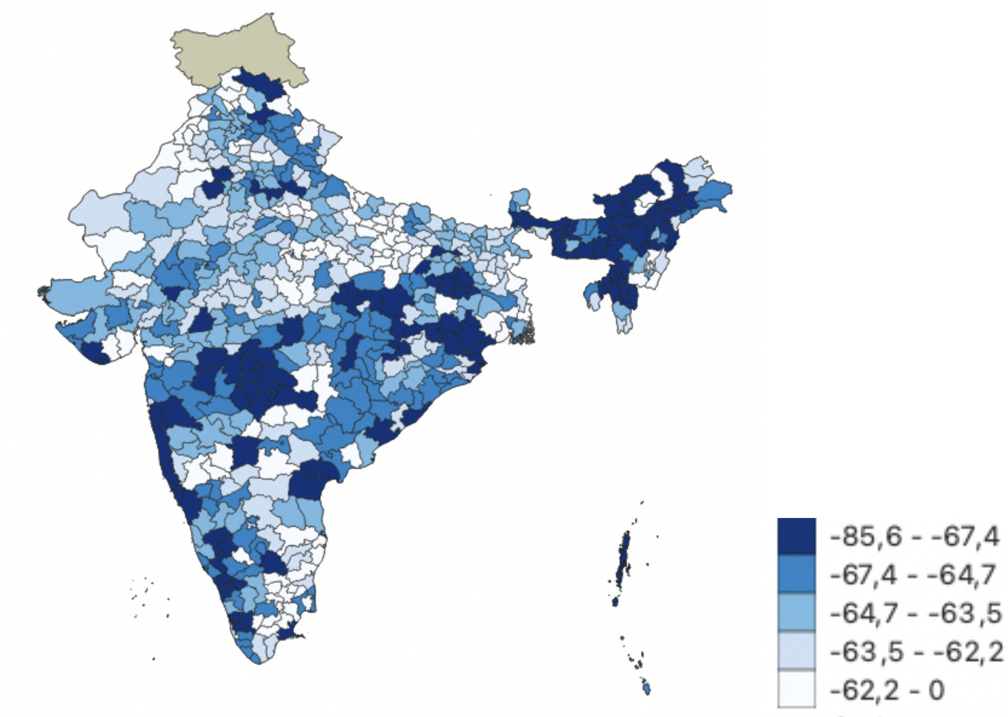
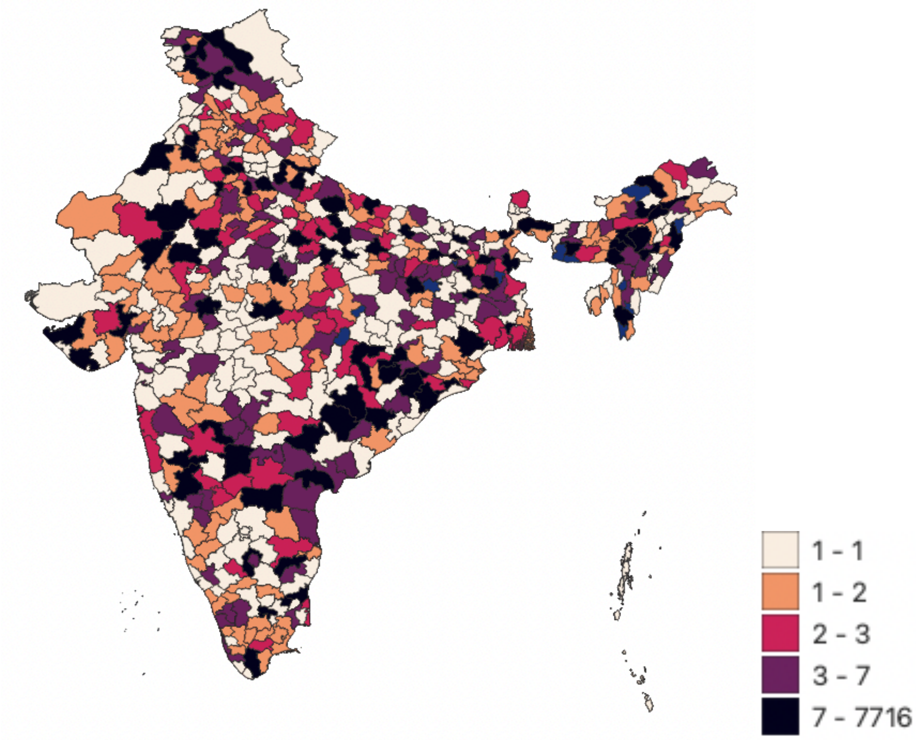


Figure 4: Prevalence of violence



3. *Other variables*

Several control variables are retrieved from India's National Sample Survey (NSS), in particular the 43rd round of 1987-1988 and the 55th round of 1999-2000. Relevant indicators include the share of literacy in district and the percentage of population belonging to Scheduled Castes and Scheduled Tribes. Moreover, I include data on share of workers in the following sectors: manufacturing, trade, transport, farming mining and service. These variables are more aggregated than the share of workers used to calculate the independent variable, as will be discussed in the following section. I aggregate data collected by Topalova to the district level by interacting the rural value with the rural extent of a district and vice versa. Unlike the construction of the independent variable, here I use the total rural/urban population as opposed to the number of workers in rural/urban areas. Control variables are also not available for Jammu and Kashmir.

Finally, I retrieve the variables *Linguistic fractionalization*, *Religious heterogeneity* and *Differences in Hindu-Muslim growth rate* from Urdal (2008). I assign the 1987 values of these variables to the pre-period to match the tariff rates and the 1997 values to the post period.

Table 1 lists the descriptive statistics for the pre, short-term and medium-term period separately. The mean conflict count does not vary greatly across time periods, but it does reflect relative decline of violent incidence in the mid-90s as perceived in Figure 1. The average district in the sample has the largest share of workers in the agricultural industry.

Table 1. Descriptive statistics

	Pre liberalization	Short term	Medium term
Period	1987 – 1991	1993 – 1997	1998 – 2001
A: Dependent Variable			
# of total events	412602	405602	402480
# of districts	431	431	431
Mean	978	942	983
Standard deviation	7138	6101	5352
Min - Max	0 – 110030	0 - 105875	0 - 110030
Districts with no incidents	244	196	105
B: Independent Variable			
Average tariff	0.095	0.032	0.032
Average tariff IV	0.853	0.305	0.305
C: Controls			
Share manufacturing	0.084		

Share trade	0.065		
Share transport	0.024		
Share farming	0.703		
Share mining	0.006		
Share service	0.098		
Share literate	0.406		
Share SC/ST	0.266		
D: Heterogeneity			
Religious heterogeneity	0.314	0.327	0.327
Hindu-Muslim growth diff.	-27.543	-12.120	-12.120
Linguistic frac.	0.335	0.204	0.204

Note: With the exception of Panel D, all variables are at measured the district level. Heterogeneity is measured at the state-level over time.

V. Methodology

1. Construction of the independent variable

Regional exposure to trade liberalization is measured by interacting the industrial composition of a district by the changes in tariffs. Districts can be more exposed to tariff changes depending on their industrial composition, as different industries face different changes in tariff. The independent variable is constructed as follows:

(1) $$Tariff_{d,t} = \sum_i tEmpshare_{i,d,1991} * \ tariff_{i,t}$$

Where $Tariff_{d,t}$ is the extent of tariff protection in district *i, $Empshare_{i,d}$* is the share of employment in industry *i* and district *d* in the year 1991 and $tariff_{i,t}$ is the tariff faced by an industry *i* in year *t*.

In this formula, nontraded industries are assigned a zero tariff for the entire period. Certain industries, such as cereal and construction, continued to be subject to trade protection. This means that a large part of the variation in this measure comes from variation in initial district composition. As argued in the Context section, the nominal tariff decline is arguably exogenous. However, the initial industrial landscape of a district can be related to conflict outcomes. Non-traded industries cultivation of cereals and oilseeds may largely employ poorer people – who may experience different trends in violence rates as compared to districts that are less poor.

Previous papers expressed concern that the inclusion of these industries lead to mechanically lower $Tariff_{d,t}$ for districts that have a large initial share in non-traded industries. To account for this, studies rely on a Traded Tariff measure to instrument tariffs (Topalova, 2010; Kovak, 2013; Dix-Carneiro et al., 2018; Anukriti & Kumler, 2019).

$$(2) \qquad TradedTariff_{d,t} = \sum_{i} trEmpshareTraded_{i,d}, 1990\ x\ tariff_{i,t}$$

Where $TradedTariff_{d,t}$ is the extent of tariff protection in district *i, $trEmpshare_{i,d}$* is the share of employment in traded industry *i* and district *d* in the year 1991 and $tariff_{i,t}$ is the tariff faced by an industry *i* in year *t*. This measure better captures the policy driven effect of tariff decreases. A valid instrument must be relevant, which entails that the traded tariff measure must be correlated with the endogenous variable. Moreover, the exclusion restriction must hold. The exclusion restriction states that $TradedTariff_{dt}$ must only affect the number of violent events through its effect on the endogenous variable. This assumption should hold, as construction of the instrumental variable is based solely on values of $Tariff_{d,t}$.

The following regression is estimated to test for relevance:

$$(3) \qquad Tariff_{dt} = \alpha + \beta TrTariff_{dt} + Post_t + \delta_d + \varepsilon_{dt}$$

The instrument is relevant, as shown by Table 2.

Table 2. First-stage regression

	Dependent variable: Average Tariff
Traded Tariff	0.335^{***}
	(5.05)
Post	0.119^{**}
	(3.34)
N	830
R^2	0.866

Standard errors are clustered at the state-year level and shown in parentheses. District fixed effects are included.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

2. *Main Regression Framework*

The baseline specification holds the following form:

$$(4) \qquad Violence_{dt} = \alpha + \beta Tariff_{dt} + Post_t + \rho X_{dt=0} * Post_t + \delta_d + \sigma_{st} + \varepsilon_{dt}$$

Where $Violence_{dt}$ is the dependent variable, which stands for the number of violent events in district d and time period t. Coefficient β is the coefficient of interest, and $Tariff_{dt}$ represents the average nominal tariff a district d is exposed to in time t. The district fixed effects are denotated by δ_d and control for time-invariant unobservable that may affect the dependent variable, such as initial district size. $Post_t$ is a dummy indicating the post-period. As the panel data sets only consists of two time periods, this dummy also controls for yearly shocks that affect India nationally. X_d controls for initial district characteristics. These characteristics can be divided into sectoral controls (the share of workers in agriculture, manufacturing, trade, transport, and services – share of construction workers is omitted) and socioeconomic controls (literacy rate and the population share of Scheduled Castes and Scheduled Tribes). These controls are interacted by the Post dummy to allow for differential treatment effects based on initial characteristics. σ_{st} captures the state-year fixed effects, which account for time-varying state-specific confounding factors.

The identifying assumption for β is that changes in the tariff measure are not correlated with district-specific unobserved time-varying shocks that influence the extent of violence in a district. An omitted variable must be correlated with both the initial industry composition of a district and with the changes in national industrial tariff rates to bias the estimates of β.

This methodology estimates the effect of trade openness on violence through the labor market channel. Any affect that trade would have through changes in commodity prices, or changes in import prices for factories is not encapsulated in the outcome. The constructed variable could be subject to measurement error if the district level is not the appropriate aggregation level for the labor market (Edmonds et al., 2010). In this case, the coefficient of interest would be biased downwards. There is however a large micro-empirical literature situated in India that views the district as the correct unit of analysis. Another potential threat to the methodology related to this measure could be labor reallocation. Topalova (2005, 2010) verified that there is little mobility across districts during the sample period.

3. *Estimation*

To estimate the regression, I have considered several models. The dependent variable is a nonnegative count variable with a high proportion of zeroes, which renders an ordinary least squares estimation unfit. Count dependent variables are often estimated using a Poisson model. However, the dataset shows significant outliers and the variance of the number of violent incidents is significantly larger than the mean. There are competing views with regards to the estimation model. Studies with a comparable set up employ the negative binomial model (Bohlken & Sergenti, 2010;Bulutgil & Prasad, 2022; Urdal, 2008; De Juan & Bank, 2015), the latter in particular using GDELT event data.

However, Woolridge (2001) argues that negative binomial models can be inconsistent and may suffer from the incidental parameter problem. He suggests estimating a Poisson Fixed Effect model as this model does not require a specific variance-mean relationship. Taking this into account, I estimate a Poisson pseudo-maximum likelihood regression with multi-way fixed effects, which allows for clustered standard errors (Correia et al., 2020). A benefit of the PPML estimation is that state-year fixed effects can be included. I cluster standard errors at the state-year level to allow for spatial correlation. If anything, clustering standard errors at a higher level should lead to more conservative standard errors. I compare the results to a negative binomial regression, as is usual in literature dealing with count data.

Moreover, employing the instrumental variable is also rendered difficult by the dependent variable. Estimating an instrumental variable is not possible with the negative binomial model. While an instrumental variable can be used in a Poisson estimation, the Instrumental Variable Poisson model does not allow for fixed effects.

4. *Heterogeneity*

The coefficient produced by specification (4) may obscure policy-relevant sources of heterogeneity, notably the extent of inequality within a district. I introduce an interaction term to the baseline regression, resulting in the following specification:

$$(5)\quad Violence_{dt} = \alpha + \beta Tariff_{dt} + Post_t + \rho X_{dt=0} * Post_t + \pi\psi_{st} * Post + \delta_d + \sigma_{st} + \varepsilon_{dt}$$

Where definition of variables is equal to estimation (4). The added element is ψ_{st}, which are variables that vary on the state level throughout time. I use three measures to account for difference dimensions of possible inequality: linguistic fractionalization, religious heterogeneity, and differences in Hindu-Muslim growth rates.

VI. Results

1. Main results

Table 3 presents the effects of trade liberalization on violent incidents in the 1993 – 1997 period. The interpretation of Poisson regression coefficients differs from coefficients estimated with an OLS model. The coefficient represents the multiplicative change in the expected count of violent events within a district associated with a 1 percentage point change in the tariff levels. The exponential transformation of the coefficient yields a more meaningful interpretation of the results and is computed as follows: 100%*((exp(β)-1).

Column (1) shows that, without any additional controls, tariff reductions are positively but insignificantly related to the count of violent incidents. So, if tariff rates decrease, the number of conflicts decrease as well. The magnitude is very large: the coefficient implies that, ceteris paribus, the expected number of conflicts is 4506% lower following a percentage point decrease in nominal tariffs. Column (2) presents the results of the instrumenting for Tariff with the *TradedTariff* instrument, as suggested by literature. Inclusion of the instrumental variable increases the magnitude of the effect even further. This could imply that the instrumental variable captures some source of variation that leads to a downward bias of the endogenous regressor. A district with a higher share of workers in traded industries, thus in industries that are more industrialized, will experience a larger drop in the *Tariff* measure compared to districts with a smaller share of traded industries. The difference between the IV estimate and the estimate in Column (1) could be explained by the IV accounting for extent of industrialization of a district being negatively related to the violence count.

From estimation (4) onwards district fixed effects are included. The inclusion of non-time varying unobservable characteristics of districts in the model has a non-trivial effect. The coefficient turns negative and significant. A decrease of a percentage point of the independent variable leads to an increase of the expected violence counts of 99,86%. The coefficients of the models controlling for sectoral shares are the largest and the most significant. However, these models also have the highest Akaike Information Criteria. In general, a lower AIC value indicates a model that strikes a better balance between model fit and parsimony.

Interestingly, controlling for states that have flexible labor laws turns the effect of tariff reductions somewhat smaller compared to similar regressions – see column (3) and (8) and (6) and (7), which is congruent with the theory that free movement of labor mitigate the possible adverse negative effects of opening up to trade.

Table 3: PPML regression - Short Term Effects of Trade Liberalization on Violence

	Dependent variable: Violence							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tariff	2.600	6.407	-6.578^{*}	-16.117^{***}	-9.482	-18.209^{**}	-18.322^{**}	-7.286^{*}
	(3.033)	(0.661)	(0.023)	(0.000)	(0.184)	(0.002)	(0.002)	(0.019)
Sectoral shares	No	Yes	No	Yes	No	Yes	Yes	No
Literacy	No	Yes	No	No	Yes	Yes	Yes	No
SC/ST	No	Yes	No	No	Yes	Yes	Yes	No
Labor law	No	Yes	No	No	No	No	Yes	Yes
Model	PPML	IV Poisson	PPML	PPML	PPML	PPML	PPML	PPML
District FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	720	720	524	474	474	474	474	484
AIC	3697486	NA	251084	144679	222641	142352	142325	235870
Pseudo R2	0.315	NA	0.934	0.959	0.937	0.960	0.960	0.937

Note: All estimations are estimated with the Poisson pseudo-maximum likelihood model, except for (2), which is a Poisson instrumental variable. Standard errors (in parentheses) are clustered at the state-year level for all specifications except for model (2), which has robust standard errors. The total population of a district is the exposure variable in all regressions. Sectoral shares, socioeconomic controls and flexible labor laws at the state-level are interacted with a post-treatment dummy. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.00$

Table 4: PPML Regression - Short Term Effects with state-year fixed effects

	Dependent variable: Violence			
	(1)	(2)	(3)	(4)
Tariff	-4.354^{**}	-2.023	1.700	-3.565
	(0.001)	(0.537)	(0.504)	(0.343)
Sectoral Shares	No	Yes	No	Yes
Literacy	No	No	Yes	Yes
SC/ST	No	No	Yes	Yes
District FE	Yes	Yes	Yes	Yes
State-year FE	Yes	Yes	Yes	Yes
N	508	468	468	468
AIC	78751.546	70237.061	70165.104	67633.215

State-year clustered standard errors are in parentheses. Each regression includes a Post dummy and initial districts characteristics are interacted with the post dummy. The total population of a district is the exposure variable in all regressions. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2 introduces a state-year fixed effect to see whether the large values in Table 1 are caused by unobservable variables that vary between states over time. The magnitude of the coefficient varies less and is less large. The inclusion of share of literate people in a district and share of people belonging to a scheduled caste or scheduled tribe changed the sign of the main coefficient, but this effect is not stable. Because of the inclusion of state-year fixed effects, the effect of state law favoring the employer cannot be assessed.

Table 5: Negative Binomial Regression - Short Term Effects

	(1)	(2)	(3)	(4)	(5)
Tariff	-0.180	-0.677	-0.727	-0.718***	-0.695
	(0.912)	(0.691)	(0.601)	(0.631)	(0.660)
Sector	No	Yes	No	Yes	Yes
Literacy	No	No	Yes	Yes	Yes
SC/ST	No	No	Yes	Yes	Yes
Law	No	No	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Obs.	524	474	474	474	474
AIC	2367	2164	2156	2164	2166

Bootstrap standard errors are in parentheses. Each regression includes a Post dummy and initial districts characteristics are interacted with the post dummy. The total population of a district is the exposure variable in all regressions. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5 shows the negative binomial estimation of the tariff reduction and violence relationship. The coefficients of this model are more consistent as compared to the PPML regression. Comparing column (1) with the subsequent columns show that excluding initial district characteristics from the estimation understates the importance of average tariff rates in explaining the conflict count. Controlling for all initial district characteristics renders the coefficient of estimation (4) highly coefficient. The coefficients can be transformed to the Incidence Rate Ratio (IRR) by taking the exponential of the coefficients, on the basis of which an approximate percentage change can be calculated. Column (1) has the smallest effect and an IRR of 0.84. A one unit decrease of the average tariff is associated with an increase in expected violence counts of 16.5%. The significant coefficient in column (3) corresponds to an IRR of 0.48, or a 51.2% expected increase in violent incidents per percentage point decrease of tariffs.

While it cannot be ruled out that the observed findings are due chance, comparing various estimations of both Poisson pseudo maximum likelihood model and the negative binomial regression show that overall, there is a negative correlation between tariff decreases and incidence of violence. The magnitude of this effect is unclear and inconsistent across estimations, although the results of the negative binomial regression make more intuitive sense.

Table 6: PPML Regression - Medium Term Effects of Trade Liberalization on Violence

	Dependent variable:					
	(1)	(2)	(3)	(4)	(5)	(6)
Tariff	-4.077	-4.446	2.889*	1.487	6.727	0.880
	(0.230)	(0.224)	(0.012)	(0.574)	(0.060)	(0.760)
Sectoral shares	No	No	No	Yes	No	Yes
Controls	No	No	No	No	Yes	Yes
Law	No	Yes	No	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
State-year FE	No	No	Yes	Yes	Yes	Yes
N	654	600	628	580	580	580
AIC	260690	252645	78355	60262	69142	59690

State-year clustered standard errors are in parentheses. Each regression includes a Post dummy and initial districts characteristics are interacted with the post dummy. The variable "Controls" consists of the literacy rate as well as the share of Scheduled Caste / Scheduled Tribes within a district. The total population of a district is the exposure variable in all regressions. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In Table 6 the effects of trade liberalization on violence after 1997 are stated. The first two columns are insignificant, and the sign is the same as the sign in the short-term regression. This changes however when state-year fixed effects are introduced from Column (3) onwards. The sign reverses and becomes weakly significant, and this is robust, albeit insignificant, for controlling for a differential effect of initial district characteristics. The significant coefficient implies a decrease of expected violence with 1797% for each percentage point decrease in tariffs. The percentual change associated with this coefficient is still unexpectedly large, except for the coefficient in Column 6. This coefficient implies that a percentage point decrease in tariffs leads to an increase in the expected count of conflicts of 141%.

Table 7: Negative Binomial Regression – Medium Term Effects

	(1)	(2)	(3)	(4)	(5)
Tariff	1.607	1.605	1.935*	-1.018***	1.751
	(0.168)	(0.162)	(0.046)	(0.000)	(0.067)
Sectoral shares	No	Yes	No	Yes	Yes

Literacy	No	No	Yes	Yes	Yes
SC/ST	No	No	Yes	Yes	Yes
Law	No	No	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Obs.	795	716	716	716	716
AIC	7044	6412	6433	2322325	6403

Bootstrap standard errors are in parentheses. Each regression includes a Post dummy and initial districts characteristics are interacted with the post dummy. The total population of a district is the exposure variable in all regressions. *P*-values in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

To compare models, I again estimate the relationship with a negative binomial regression. The results show a similar pattern compared to the PPML regression: trade liberalization leads to a decrease of political violence in the long run. The exception to this is regression (4), where the sign is reversed when one controls for all initial district characteristics. This large change of coefficients could imply that there the control variables are correlated.

2. *Robustness*

The findings in the previous section are inconclusive but suggest a pattern: in the short term, trade liberalization is negatively associated with violence count, but the effect referces in the medium term. The first robustness check involves assessing how excluding outliers influences results. Statistically, removing outliers can lead to more stable and reliable estimates. Removing outliers can also show whether results are driven by these outlier values.

From the Indian census, I retrieve the 10 largest cities in 1993, the middle of the sample period. The second criterium is that the urban extent of the district must exceed 50%. This results in the exclusion of 7 districts: Mumbai, Delhi, Kolkata, Chennai, Bangalore, Hyderabad and Ahmedabad. Exclusion of these districts can also be theoretically motivated. There exists a strand of literature that focuses on urban violence, arguing that these conflicts merit seperat attention because of the distinct characteristics (Thomson et al., 2023; Urdal & Hoelscher, 2012).

Table 8 below shows the results. All coefficients are now insignificant. The sign of the effect of trade in the short term is also less consistent compared to previous tables, as now half of the regressions estimate a positive relationship between trade and violence in the short term. The coefficients for the medium term have the same sign as previous regression models, but the magnitude of the effects is still very influenced by the inclusion of control variables.

Table 8: Urban cities

	Short				Medium			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tariff	0.983	-3.309	1.705	-4.649	4.343	1.079	6.588	0.584
	(0.717)	(0.341)	(0.520)	(0.235)	(0.122)	(0.699)	(0.082)	(0.845)
Sector	No	Yes	No	Yes	No	Yes	No	Yes
Lit. SC/ST	No	No	Yes	Yes	No	No	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	496	464	464	464	616	576	576	576
AIC	72963	69097	69963	66824	74742	60009	69090	59525
chi2	0.131	29.602	20.342	102.960	2.386	25.866	10.436	46.275

State-year clustered standard errors are in parentheses. Each regression includes a Post dummy and initial districts characteristics are interacted with the post dummy. The total population of a district is the exposure variable in all regressions.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: PPML Endogenous borders

	Short				Medium			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tariff	0.977	-1.379	1.115	-2.021	7.520	3.992	9.609	5.153
	(0.787)	(0.686)	(0.729)	(0.618)	(0.169)	(0.272)	(0.146)	(0.176)
Sector shares	No	Yes	No	Yes	No	Yes	No	Yes
Literacy and SC/ST	No	No	Yes	Yes	No	No	Yes	Yes
State-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	322	306	306	306	400	378	378	378
AIC	34813	30015	33659	29390	41870	27357	37555	26725

State-year clustered standard errors are in parentheses. Each regression includes a Post dummy and initial districts characteristics are interacted with the post dummy. The total population of a district is the exposure variable in all regressions.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A second robustness test is to additionally check for the effect of the changes in administrative borders of districts throughout the sample period. It is plausible that the border change is endogenous, either resulting from conflict or driving the conflict counts in districts (Dube &

Vargas, 2013). Excluding districts that have split during the sample period results in dropping 122 districts.

Excluding these districts does not influence the significance of any coefficient. The signs stay the same as in Table 8 across all specifications. It seems that excluding these districts with border changes make the effects as compared to Table 9 somewhat smaller in the short-term, but larger in the medium term.

To conclude, both robustness tests provide little additional information about the relationship between trade and violence in Indian districts.

3. *Heterogeneity*

Table 10: PPML – Heterogeneity

	Short			Medium		
	(1)	(2)	(3)	(6)	(7)	(8)
Tariff	-4.813	-7.374	-2.899	-0.051	-3.310	10.454
	(0.377)	(0.838)	(0.701)	(0.995)	(0.424)	(0.176)
Linguistic	-22.471			2.521		
	(0.050)			(0.866)		
H-M growth		-0.529			-0.506	
		(0.073)			(0.171)	
Religious het.			-3.450			-36.120
			(0.843)			(0.062)
Sector shares	Yes	Yes	Yes	Yes	No	Yes
Literacy and SC/ST	Yes	Yes	Yes	Yes	Yes	Yes
State-year FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
N	468	452	452	580	562	562
AIC	67618	64679	65678	59685	57164	57027

State-year clustered standard errors are in parentheses. Each regression includes a Post dummy and initial districts characteristics are interacted with the post dummy. The total population of a district is the exposure variable in all regressions. The variables Linguistic fractionalization, differences in Hindu-Muslim growth rates and religious heterogeneity are interacted with the independent variable.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10 introduces heterogeneity in the specification. The interpretation of interaction terms in Poisson regressions are not straightforward, so will not be discussed. In the short term, the signs of the effect of tariff remains the same as perceived in previous table. The coefficients of the interaction term denoting linguistic fractionalization, differences in Hindu-Muslim growth and religious heterogeneity are all negative. This is consistent with the intuition: effects of trade liberalization are more pronounced in areas in which inequalities along ethnic or religious lines are present. In the medium term, the sign of the coefficient of interest reverses in the case of linguistic fractionalization and differences in Hindu-Muslim growth rates. The signs of the interaction effect are consistent with theory except in the case of linguistic fractionalization, which in specification (6) implies that districts with a high degree of fractionalization the negative effect of tariff reduction is mitigated. Still, it is important to keep in mind that all results of these specifications may be due to chance.

VII. Conclusion and discussion

In general, the empirical findings of this paper are insignificant and inconclusive. What is interesting is the relatively stable difference in signs of coefficient when the short-term effect is compared to the medium-term effect. My findings suggest that districts that lose relatively more trade protection are also more peaceful after an initial adjustment period where violence rises. In many (sometimes insignificant) specifications, labor laws reduce the magnitude of the hypothesized negative effect of trade. According to theory, labor laws should mitigate the vulnerability of districts to tariff reductions, as labor can be more easily efficiently allocated. My findings are thus not consistent with the findings of Dix-Carneiro & Kovak (2017) in Brazil, who find that trade liberalization increases crime and that this effect is more pronounced in later periods due to the labor market shock being persistent. It is important to reiterate that the estimates found by this study only encompass the effect of tariff reduction through the labor market.

In the present research I have also not been able to distinguish between intermediate causal channels of the trade-violence relationship. Topalova (2010) finds that districts more exposed to tariff liberalization experience a slower decrease in poverty. It cannot be assumed that the same relationship holds for the current sample, as I aggregate rural and urban areas. This might be one of the factors impeding finding meaningful results. Conflict in rural and urban areas may operate differently. Literature studying the trade liberalization episode in India finds significant effects only in rural areas. Prasad (2012) finds a decrease of violence on the Indian state level, while Iyer & Topalova (2014) find preliminary evidence for an increase in crime over the same period, but on the rural district level.

Apart from the aggregation level, another element that might affect the findings of this thesis are measurement errors. Measurement errors in the dependent variable does not bias the estimates of the coefficients unless it is related to the average tariff a district is exposed to. The dependent variable uses machine news reports as a proxy for the true number of conflicts. As GDELT events is based on journalism – less events in rural areas. This could be mitigated by controlling for distance to infrastructure.

Measurement error in the independent variable may also be problematic and can lead to a bias of the coefficient towards zero. This entails that the coefficients estimated by the model are consistently estimated closer to zero than the true values. It is possible that this is a concern in the present research. Due to the sampling strategy of the NSS, survey data is representative at the region level, not representative at the urban district level. However, I aggregated average tariffs to the district level.

Another issue is finding an appropriate estimation model that fits the data. To my knowledge, estimating count models with outliers is not straightforward if one wants to include instrumental variables as well as fixed effects. Kovak (2013) constructed a similar measure of regional trade exposure measuring the effect of trade liberalization on labor demand as is used in the present research. Non-tradable sectors are accounted for, and the magnitude of the trade-induced regional shock does not depend on the non-tradable sector as is currently the case. Further research could re-estimate this relationship while using the measure as proposed by Kovak (2013), so endogeneity concerns of a large non-trade sector can be mitigated while unobservable confounders can be accounted for by the estimation model.

In further research, the ethnical inequality mechanism could be further explored. Bohlken & Sergenti (2010) refers to two examples in which ethnic riots were exploited to drive out economic competitors. In Jabalpur, Muslims largely control the cigarette industry. Competition over the industry resulted in riots. In Aligargh, Hindu businessmen engaged in riots to forcefully evict Muslims, so that they could collect land. It is a plausible hypothesis that industries more affected by trade are industries that overrepresent a segment of the Indian population, leading to greater ethnic competition and tensions.

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Appendix A: Data

Table A1. CAMEO Codes

19	FIGHT
190	Use conventional military force, not specified below
191	Impose blockade, restrict movement
192	Occupy territory
193	Fight with small arms and light weapons
194	Fight with artillery and tanks
195	Employ aerial weapons, not specified below
1951	Employ precision-guided aerial munitions
1951	Employ remotely piloted aerial munitions
196	Violate ceasefire

Figure A1. Left: Violent incidents in GDELT. Right: Violent events in UCDP,

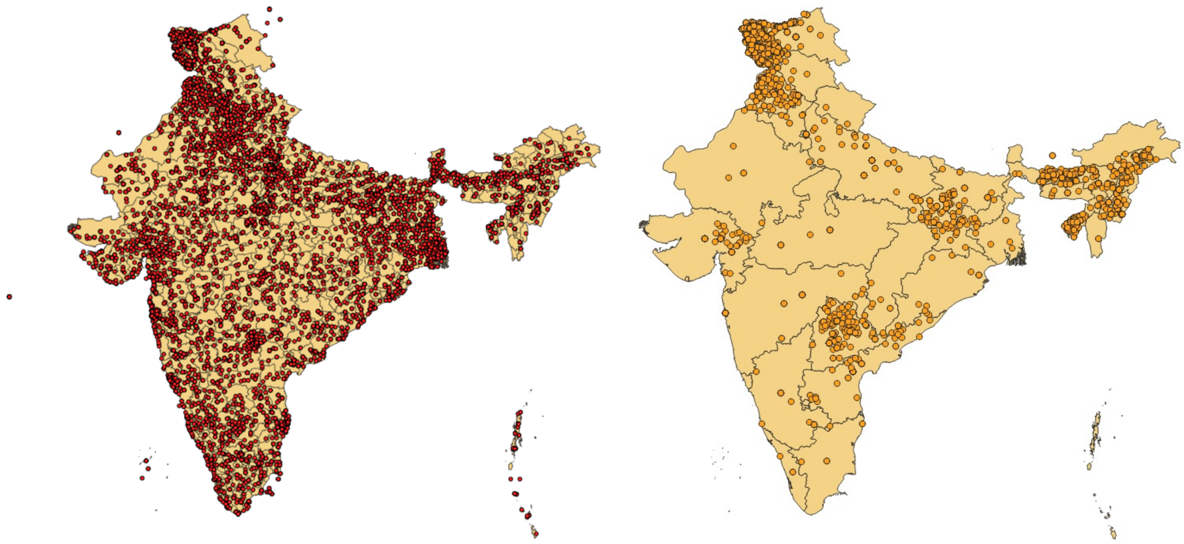

Table A2. Events per state

State	Pre	Post-short	Post-medium
Andaman and Nicobar Islands	0	49	111
Andhra Pradesh	10,41	10,314	8,519
Arunachal Pradesh	0	783	251
Assam	2,228	11,5	14,685
Bihar	4,159	6,339	7,612
Chandigarh	0	0	30
Dadra and Nagar Haveli	163	0	0
Delhi	89,804	105,875	88,388
Goa, Daman and Diu	118	241	145

Gujarat	6,739	5,466	15,241
Haryana	15,202	2,742	3,222
Himachal Pradesh	11,462	28,542	20,136
Jammu &Kashmir	24,151	64,509	118,531
Karnataka	3,223	8,187	3,84
Kerala	710	1,087	291
Lakshadweep	0	0	0
Madhya Pradesh	11,883	4,755	12,563
Maharashtra	48,304	77,773	56,728
Manipur	82	4,712	2,44
Meghalaya	0	0	45
Mizoram	0	0	73
Nagaland	364	2,138	829
Orissa	2,77	6,505	3,739
Pondicherry	0	0	0
Punjab	144,827	10,115	3,685
Rajasthan	4,909	7,638	5,584
Sikkim	154	157	11
Tamil Nadu	8,994	4,779	3,59
Tripura	720	1,599	1,269
Uttar Pradesh	23,93	30,159	21,639
West Bengal	6,297	9,902	9,283
Total	421,603	405,866	402,48