For the love of God? Proselytization, Religious Restrictions and Social Conflicts in India*

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August 28, 2026

Keywords: Religious Restrictions; Religious Conversions; Proselytism; Riots; India

JEL: Z12; N35; J15; D74; K42

Abstract

I study the social effects of religious restrictions in the context of proselytism, which constitutes an important tenet of certain religions. To establish causality, I exploit plausibly exogenous variation from 'Freedom of Religion' laws that restrict coercive conversions and were enacted across several Indian states between 1961 and 2011. Using administrative data on riots and employing a heterogeneity-robust difference-in-differences estimator, I find that these laws lead to reductions in social conflict. Riots decline by 6.3% for each year the law is in force. Reduced inter-religious competition, driven by the signalling impact of the law, is likely to be the primary channel underlying this effect. I also document suggestive shifts in minority community outcomes but find no significant impact on enacting states' religious composition. Overall, the findings suggest that while religious regulation can reduce social conflict in religiously diverse settings, it may also entail costs for minority communities, pointing to potentially ambiguous welfare implications.

*__Acknowledgements__: I thank the editor, Adam Chilton, an anonymous editor, and an anonymous referee for their insightful suggestions and comments, which have helped shape the paper in its current form. I am also grateful to Eliana La Ferrara for extensive discussions and valuable input that have significantly improved the paper. I also thank Sourav Bhattacharya, Somdeep Chatterjee, Shreyasee Das, Kabir Dasgupta, Christopher Erwin, Edward Glaeser, Juliana de Castro Galvão, Tarun Jain, Ondřej Krčál, Lisa Meehan, Stepan Mikula, Gail Pacheco, Hrushikesh Panda, Kalpit Sharma, Rostislav Stanek, Zdeněk Tomeš, and Laura Zimmermann for useful suggestions and inputs. I also thank the participants at the Annual RES Conference, the Annual ASREC Conference, and the Econometric Society European Winter Meeting for their valuable feedback. I also thank Vijaya Singh for her assistance with the data. This work was supported by Research England under the Enhancing Research Culture funding stream. This publication arises from research funded by the John Fell Oxford University Press Research Fund.

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1 Introduction

Religious restrictions around the world are on the rise.[1] Recent data from Pew Research Center suggests that the number of countries imposing significant levels of restrictions on religion has gone up from 40 in 2007 to 59 in 2022 (Majumdar, 2024). There is, however, considerable regional heterogeneity, with the Asia-Pacific and Middle East-North Africa regions having the highest numbers of such countries, i.e., 25 and 17, respectively. These increasing restrictions on religion are a potential cause of concern as limited empirical evidence on the subject suggests a positive relationship between government restrictions on religion and conflict (Grim and Finke, 2010; Finke and Martin, 2012).

A particularly prominent form of religious regulation that has expanded in recent decades consists of laws restricting religious conversion (Majumdar, 2024). Currently, 46 countries have some form of national legislation regulating conversion, with 29 countries also having a law to regulate proselytization (USCIRF, 2023). While some of these laws, such as apostasy statutes, are widely regarded as incompatible with international legal standards, others have received a more mixed response and remain controversial due to their nature, scope, and implementation (Nowak, 1993). The tension surrounding such laws stems in part from provisions of the International Covenant on Civil and Political Rights (ICCPR), which protects the right to adopt (or convert to) a religion while simultaneously prohibiting coercive or forced religious conversion (UNGA et al., 1966). As a result, national laws that restrict such forced conversions are not in direct contravention of international legal provisions. Proponents of such laws, particularly legislators, argue that these laws can safeguard vulnerable communities from coercive conversion and can potentially reduce intergroup tensions as well as flashpoint issues that may escalate into conflict (Ackerman, 2018; Hertzberg, 2020; Jenkins, 2008; Niyogi, 1956).[2] Critics, however, contend that these laws often impose disproportionate costs on proselytizing religious communities due to their subjective interpretation of terms including 'forced' or 'coercive' (Teater and Jenkins, 2019; Jenkins, 2008;

[1] https://www.pewresearch.org/religion/2024/12/18/government-restrictions-on-religion-stayed-at-peak-levels-globally-in-2022/

[2] www.timesofindia.indiatimes.com/city/jaipur/anti-conversion-bill-passed-raj-recorded-no-love-jihad-case-ever/articleshow/123873392.cms

Coleman, 2008; Adcock, 2014; Fischer, 2018). Currently, laws limiting coercive conversions are prevalent in South Asia, with countries including India, Bhutan, and Myanmar having legislation restricting such conversions, and others deliberating over similar measures (Fischer, 2018). Some scholars suggest that the growing prominence of Pentecostal Christianity, with its emphasis on the supernatural, has contributed to the emergence of this regulatory trend in South Asia (Mallampalli, 2023; Freston, 2001).

In this paper, I take the case of India and try to understand the impact that religious regulation in the form of laws that restrict coercive conversions can have on societal conflict. To study this, I exploit plausibly exogenous variation generated by 'Freedom of Religion' laws that were enacted in several Indian states between 1961 and 2011. The primary aim of these laws was to limit religious conversions that were carried out through forced or fraudulent means, or that relied on allurement or inducement to encourage a person to convert.[3] For instance, under these laws, converting someone on the pretext of material benefit or under threat of divine displeasure would constitute a criminal offence. The provisions of the law make such unjust conversions punishable by fines and/or imprisonment. Considering that the majority Hindu religion in India is traditionally non-proselytizing, the laws can be thought of as restricting the proselytizing activities of minority religions, including Christianity and Islam. While both the minority religions have featured in national debates on the subject, the laws were primarily a response to Christian missionary activity in tribal areas. Furthermore, while the drivers of Hindu-Muslim conflict are historically multi-faceted and structural in nature, Hindu-Christian friction in the post-independence era has been centred predominantly on the issue of conversion, which remains the primary catalyst for communal tension between these two groups.

To estimate the impact of these laws, I leverage state-time variation in their enactment using the difference-in-differences estimator proposed by De Chaisemartin and d'Haultfoeuille (2024) that is able to account for both heterogeneous and dynamic treatment effects. The results from my analysis, based on this novel estimator, suggest that laws restricting coer-

[3] https://www.loc.gov/law/help/anti-conversion-laws/india.php

cive religious conversions can potentially lead to lower social conflict, as measured by riots. Estimates suggest that riots are reduced by 6.3% for each year the law is in force, which translates into a 0.075 standard deviation reduction in riots. I show that this result remains robust to alternative methods and specifications, as well as a variety of other sensitivity checks. I also provide suggestive evidence that, given the limited number of arrests and convictions, the primary channel for this effect is likely the law's signalling impact. By clarifying the government's stance on the issue, the legislation reduces inter-religious competition even in the absence of rigorous enforcement. The enactment of these laws is also positively associated with an increase in the share of Hindus and a decline in the share of Muslims and Christians in the enacting states, at least in the short run. However, the estimated effects are small and not statistically significant, and may potentially be confounded by the selective outmigration of minorities, suggesting that any reduction in coercive conversion due to the law is likely to play a limited role in reducing conflict here. I also show that the laws are associated with higher religiosity, lower perceived freedom of choice, adverse labour market outcomes, and changes in inter-group contact preferences among a relatively small sample of Christians, patterns that may also reflect a reduction in violence through a potential chilling-effect channel.

The study makes three unique contributions to the existing economic literature. First, the paper provides one of the first causal evidences for the impact of religious restrictions on social conflicts. While there are studies that look at the association between the two and have contributed a lot to our understanding, causal analysis on this issue has remained largely missing (Finke and Martin, 2014; Finke, 2013). The paper exploits plausibly exogenous variation in state-level religious regulations and employs a novel heterogeneity robust difference-in-differences estimator (De Chaisemartin and d'Haultfoeuille, 2024) to show impact on conflict, thereby providing one of the first causal estimates for this empirical question. The paper also adds to the limited literature on the impact of religious issues (and differences) on societal conflicts, in general (Esteban, Mayoral and Ray, 2012; Galindo-Silva and Tchuente, 2021; Mayoral and Esteban, 2024; Esteban and Mayoral, 2011). Second, the study

contributes to the scant economic literature on religious conversions (Barro, Hwang and McCleary, 2010; Botticini and Eckstein, 2007; Nunn, 2010; Rubin, 2014). The discussion on the issue of proselytism and its role as a flashpoint in conflict between communities has remained largely limited in the economics literature. Third, the paper highlights a potential tension in the welfare implications of religious regulations (Jacobs and Arora, 2018; Hofvander, 2002; McConnell and Posner, 1989; Barkey, 2008). While laws limiting coercive religious conversions are linked with reductions in social conflict, they are also associated with changes in outcomes for minority communities, including lower perceived freedom and shifts in intergroup preferences. The study, therefore, raises important questions about the trade-offs between public order and minority well-being (McConnell and Posner, 1989; Barkey, 2008). In addition to the above three contributions, the study also adds to the scant economic literature on religion in developing countries. Most of the existing research on the issue of religion and conflict has been conducted in a developed country context. Only recently have the researchers been able to utilise new data sources, including field experiments, to try and understand these and related issues in a developing country scenario (Iyer, 2016; Gupta et al., 2018; Maiti et al., 2022). Thus, the study also contributes to this global effort for examining questions related to religious restrictions and conflict in a developing country context.

2 Background

In this section, I first outline the existing literature that explores the relationship between religion and conflict. I then discuss the religious context of India, followed by details on the religious conversion laws enacted by Indian states.

2.1 Existing Literature

In the context of the relationship between religion and conflict, studies have shown that competition on the economic front or for control of resources can induce conflict between

members of different religious communities (Field et al., 2008; Esteban and Ray, 2008; Jha, 2014; Finley and Koyama, 2018; Becker and Pascali, 2019). Settings in which religious communities can benefit from exchange and complementarities or have incentives to cooperate are likely to produce higher levels of tolerance (Jha, 2013; Abramitzky, 2008). Economic and weather shocks can also create scenarios that can lead to conflict between different religious communities (Chaney, 2013; Ticku, Shrivastava and Iyer, 2018; Anderson, Johnson and Koyama, 2017; Grosfeld, Sakalli and Zhuravskaya, 2020). Studies have also highlighted the role of historical factors in religious violence as well as their tendency to persist (Iyigun, 2008; Voigtländer and Voth, 2012). Political actors and leaders could also play an important role in establishing tolerance or stoking conflict between religious communities (Glaeser, 2005; Jha, 2014; Iyer and Shrivastava, 2018; Saleh and Tirole, 2021). Furthermore, cultural and ideological aspects of different religions can also give rise to violence (Huntington, 1993; Morgan, 2003; Reinders, 2004). The opportunities for inter-group contact can, however, reduce prejudice and potentially increase tolerance among members of different religious communities (Hewstone and Brown, 1986; Scacco and Warren, 2018; Maiti et al., 2022).

Government restrictions on religion can play an important role when it comes to conflict. Governments possess the capacity to restrict the supply of religion (Iannaccone, Finke and Stark, 1997). They can also favour particular religions in terms of economic opportunities, for instance, based on the religious affiliation of the head of the state, which might alienate other religious groups and potentially lead to conflict (Johnson and Koyama, 2019; Coşgel, Miceli and Yıldırım, 2023; Iyer, 2022; Chaudhary and Rubin, 2016). Evidence suggests that restrictions on minority religions can increase when the government forms an alliance with specific (or majority) religious groups (Gill, 2008; Finke, 2013). Strong social and cultural pressures coupled with a perception of being dangerous to public welfare result in government restrictions on minority religious groups (Finke, 2013). These restrictions, by curtailing the activities of certain religions, can result in higher levels of religious persecution and conflict (Grim and Finke, 2010; Finke, 2013; Grim and Finke, 2007). Such restrictions, in general, also have a strong positive correlation with social conflicts (Finke and Martin, 2014;

Finke, 2013; MacCulloch and Pezzini, 2010). However, research also suggests that a regime's experience with religious conflict can make it more repressive of the minorities (Henne and Klocek, 2019). Although the evolving literature in the field has studied the relationship between government restrictions and persecution and conflict, establishing the direction of causality and identifying causal effects has largely remained an empirical challenge (Arena and Joyce, 2015; Ritter and Conrad, 2016).

The particular issue of religious conversions has received only limited attention in the economics literature. Barro and McCleary (2024) and Barro, Hwang and McCleary (2010) compute religious conversion rates for different countries and suggest that the rates are positively related to education levels and religious pluralism and negatively related to the religious conversion restrictions imposed by the governments. They further suggest that the level of conversions depends on how costly it is to switch as well as the cost of staying in the 'wrong' or unsuitable religion (Barro, Hwang and McCleary, 2010). While conversion through proselytization is one way, intermarriage is also often associated with a change in one's religion. Research suggests that, within this context, conversions are more likely to occur into stricter religions, and liberal religions are likely to lose members (Stark and Finke, 2000; Barro, Hwang and McCleary, 2010). Some evidence also suggests that religious norms of a religion can sometimes result in voluntary conversions away from it (Botticini and Eckstein, 2007). Nunn (2010) has also highlighted the long-term impact of missionary activities on religious conversions in Africa.

2.2 Religious context in India

India is a democratic, secular and religiously diverse country without an official state religion. The country is the birthplace of several important religions, namely Hinduism, Buddhism, Sikhism, and Jainism (*Census of India*, 2001). While Hindus form the majority of the population (around 80% as per (*Census of India*, 2011)), the country also has one of the largest populations of Muslims around the world at around 170 million. In terms of share of religious minorities, Muslims account for 14.23%, Christians for 2.30%, Sikhs for 1.72%,

and Buddhists and Jains combined for 1% of the total population (*Census of India*, 2011).[4] The country provides for 'Right to Freedom of Religion' through specific provisions (Article 25-28) enshrined in its Constitution. Specifically, citizens have the 'freedom of conscience and the right to freely profess, practice, and propagate religion.'[5]

Despite the religious equality in the country, studies have found differential outcomes for different religious communities in the spheres of education (Basant, 2007; Niaz Asadullah, Kambhampati and Lopez Boo, 2013; Asher, Novosad and Rafkin, 2022), health (Borooah, 2010*a*; Bhalotra, Valente and Soest, 2010; Vyas, Hathi and Gupta, 2022) and labour market (Thorat and Attewell, 2007; Basant, 2007; Borooah, 2010*b*; Desai and Temsah, 2014). Most of the studies show that outcomes for minority religious groups are worse when compared to the majority religious community, i.e. Hindus. However, it should be noted that much of the work has only focused on Muslims as the minority community.

India has also witnessed numerous instances of religious conflicts or riots since independence mainly between the Hindu and Muslim communities (Varshney, 2003; Field et al., 2008; Jha, 2014; Mishra et al., 2014; Muralidharan, 2014; Mitra and Ray, 2019).[6] [7] [8] For instance, evidence shows that the likelihood of riots increase when there is an increase in per capita expenditure of Muslims, or when economic growth is low, in general (Bohlken and Sergenti, 2010; Mitra and Ray, 2014; Iyer and Shrivastava, 2018). The inter-relationship between religious conflicts and electoral politics has also been explored in a number of studies (Wilkinson, 2004; Varshney and Gubler, 2012; Jha, 2014; Nellis, Weaver and Rosenzweig, 2015; Iyer and Shrivastava, 2018). When it comes to conflict between Hindus and Christians, some anecdotal and descriptive evidence suggests that the issue of religious conversion has often been a central flashpoint between these communities (Bauman, 2020).

The recently conducted study by Pew Research Center provides some fascinating insights on religious aspects in India (Sahgal et al., 2021). According to the survey, around 90% of

[4] www.pib.gov.in
[5] www.constitution.org/cons/india/p03025.html
[6] www.foxnews.com/world/troops-deployed-to-quell-deadly-communal-clashes-between-hindus-muslims-in-north-india
[7] www.bbc.com/news/world-asia-india-51639856
[8] www.nytimes.com/2002/03/02/world/more-than-200-die-in-3-days-of-riots-in-western-india.html

Hindus, Muslims, and Christians believe that they are very free to practice their religion, with around 80% also believing that respecting all religions is very important to being Indian (Sahgal et al., 2021). This is despite the fact that the majority of individuals from these religions responded that they are very different from each other, and a significant proportion preferred religious segregation. When it comes to religious conversions, the study points to largely stable numbers across religions, with little movement between them (Sahgal et al., 2021). For instance, 0.4% of adults respond as being raised Hindu but now practise Christianity. However, over 60% of these respondents report having come from marginalised sections (Scheduled Castes and Scheduled Tribes) (Sahgal et al., 2021).

2.3 Religious Conversion Laws

The Religious Conversion Acts or 'Freedom of Religion' laws, as more commonly (and officially) known, are state-level laws in India that seek to restrict coercive or unjust religious conversions (Jenkins, 2019). These laws primarily aim to curtail religious conversions that are forced or fraudulent in nature or are done through allurement or inducement. For instance, under these laws, religious conversion carried out under threat (physical or psychological) or misrepresentation, or through the offer of a gift or gratification (material or otherwise), is considered a legal offence (Hertzberg, 2020). Currently, 12 states in India have laws/ rules that restrict such unjust conversions. [9] [10] Figure 1 shows the states in which these laws have been enacted at least once till the year 2025. Although the modalities of these laws differ slightly based on the state, the primary aim is to restrict religious conversions which are not of a voluntary nature. Under the law, such illegal conversions are punishable by monetary fines and/or imprisonment, depending on the state in question. The laws typically prescribe double the punishment if coercive conversion is in the context of vulnerable sections, including minors, women, Scheduled Castes or Scheduled Tribes. Existing literature suggests that religious conversions in India have often been concentrated among marginalised groups, particularly Scheduled Castes and Scheduled Tribes, which may help explain the law's em-

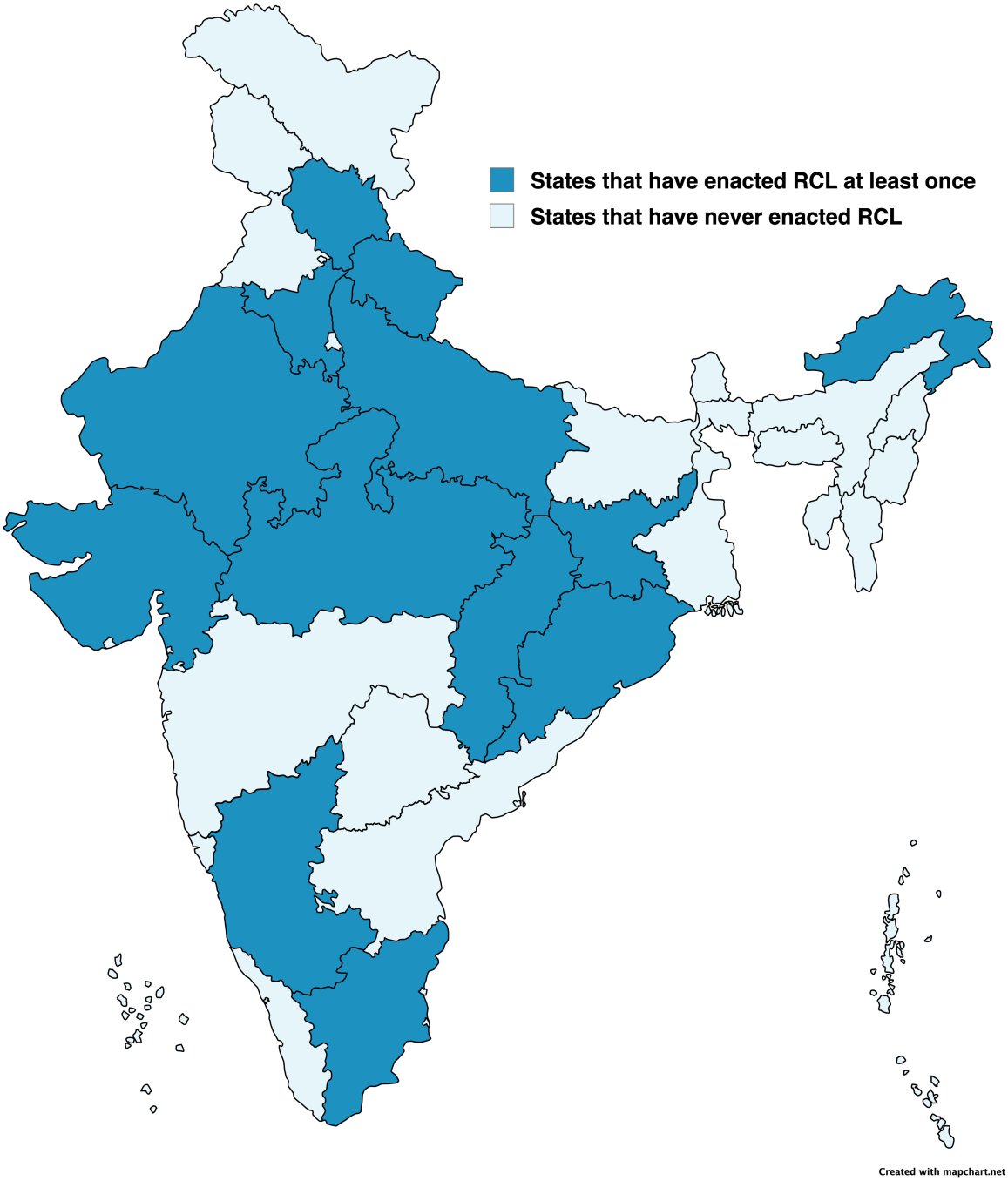
[9] www.uscirf.gov/sites/default/files/2023%20India%20Apostasy%20Issue%20Update.pdf
[10] https://www.uscirf.gov/sites/default/files/2025-04/India%202025%20USCIRF%20Annual%20Report.pdf

phasis on these communities (Kling, 2020; Mallampalli, 2023). Some recent state laws also exempt reconversion to an 'ancestral religion' or a 'religion prior to conversion' from the ambit of regulation. This distinction is also reflected in the discourse surrounding the *ghar wapsi* (homecoming) movement, which has gained prominence since 2014 and frames reconversion or readmission to Hinduism as a return to one's ancestral faith rather than as a new conversion, thereby often placing it outside the scope of such laws (Rajeshwar and Amore, 2019; Katju, 2015; Mallampalli, 2023). Overall, the nature and scope of religious conversion laws were such that they may have imposed costs on minority religious groups, particularly Christians and Muslims, by regulating proselytizing and conversion activities that form an important component of these traditions and that could potentially be construed as coercive under state-level legislation.[11] At the same time, these laws may be considered as more beneficial to the majority community, Hindus, by restricting coercive religious conversions out of it. While both minority religious communities - Christians and Muslims - have featured in national debates on religious conversion, these laws were primarily framed in response to Christian missionary activity among tribal and marginalised groups (Kim et al., 2003). The drivers of Hindu–Muslim conflict are more diverse and structural in nature and, in the context of conversion, have often focused on interfaith marriages (starting around 2009). This contrasts with Hindu–Christian tensions, which have been driven primarily by concerns over religious conversion and missionary proselytization in the post-independence era (Sahoo, 2018; Bauman, 2020; Sahoo, 2020). In addition to these inter-religious dynamics, the laws may also have implications for the ability of marginalised communities, particularly Scheduled Castes, to convert out of Hinduism as a way to escape potential caste-based discrimination, given their historically lower status within the traditional caste hierarchy among Hindus in India (Kling, 2020; Mallampalli, 2023).

The first discussions regarding restricting religious conversions carried out through 'co-

[11] While proselytization is often considered more institutionally central to Christianity, the da'wah tradition in Islam also encourages religious outreach and invitation (Kuiper, 2021).

Figure 1: States in India that have enacted Religious Conversion Law at least once



Notes: The map above depicts the Indian states where a Religious Conversion Law has been enacted at least once. States where the law currently exists include Arunachal Pradesh, Chhattisgarh, Gujarat, Odisha, Madhya Pradesh, Himachal Pradesh, Haryana, Karnataka, Uttarakhand, Uttar Pradesh, Jharkhand, and Rajasthan. In Tamil Nadu, the law was repealed in 2014. Map created using MapChart.

ercion and undue influence' in independent India (1947) took place during the drafting of the Indian Constitution (Kim et al., 2003; Teater and Jenkins, 2019). The Constituent Assembly's Advisory Committee on Fundamental Rights, however, decided against the same and entrusted future lawmakers to decide on this issue (Granville, 1966; Teater and Jenkins, 2019). In 1954, the 'Indian Conversion (Regulation and Registration) Bill' was introduced in Parliament to regulate religious conversions at the national level. The provisions of the bill sought to implement the licensing of missionaries along with the registration of conversions with the government.[12] The bill, however, did not become law due to a lack of requisite support in Parliament. In 1956, the *Report of the Christian Missionary Activities Enquiry Committee* also recommended regulating religious conversions. The report contained details as well as testimonies of individuals who were suspected to have been converted through 'allurement' or 'inducement'. It also highlighted that a large number of religious conversions were carried out using undue influence and misrepresentation, with marginalised sections (Scheduled Castes and Scheduled Tribes) being the primary groups affected. Furthermore, it noted that places such as schools, hospitals, and orphanages, as well as activities like money lending, were used as means to facilitate proselytization.[13] Two more bills - the Backward Communities (Religious Protection) Bill (1960), and the Freedom of Religion Bill (1979) - were introduced in Parliament to regulate religious conversions at the national level. However, like the first bill, these two bills also failed to gather the requisite support in the Parliament. More recently, in 2015, the Ministry of Law and Justice of the country stated that such a law at the national level would fail judicial scrutiny, as it is a state subject matter.[14] [15]

At the sub-national level, Orissa became the first state to pass a religious conversion law with its 'Orissa Freedom of Religion Act, 1967.' Forced, fraudulent, and induced conversions were punishable under the law with a fine of up to Rs 5000 and/or imprisonment of up to one year. The magnitude of both the fine and the term of imprisonment was doubled in

[12] www.uscirf.gov
[13] www.voiceofdharma.org/books/ncr/8vipiv.htm
[14] www.tile.loc.gov/storage-services/service/ll/llglrd/2018298841/2018298841.pdf
[15] www.deccanherald.com/india/national-anti-conversion-law-not-2123432

cases where the forced conversion was committed against a minor, a woman, or an individual belonging to marginalised groups, i.e., the Scheduled Castes and the Scheduled Tribes. Madhya Pradesh followed Orissa by enacting the 'Madhya Pradesh Freedom of Religion Act' in the year 1968. The provisions of the law were similar to those in Orissa. Under the law, the person performing the conversion ceremony was required to provide notice to the District Magistrate within seven days of the date of the conversion ceremony.

The 'Orissa Freedom of Religion Act' was initially declared unconstitutional in 1973 by the Orissa High Court on the grounds that religious conversion (or proselytizing activities) was part of the Christian religion and hence was covered under 'right to propagation of religion' guaranteed by the Constitution under Article 25(1). This decision, however, was overturned by the Supreme Court of India in 1977 on the grounds that the right to propagate is limited only to the spread of the tenets of a religion and that conversion impinges upon the freedom of conscience guaranteed by the Constitution. Effectively, the right to propagate included only the right to *transmit* one's religion, not the right to *convert* a person to one's religion (Jenkins, 2019; Hertzberg, 2020). The Supreme Court also stated that the maintenance of public order may also act as a basis for limitation of religious freedom, as forcible conversions can potentially create a situation of public disorder in states. The Acts were subsequently passed by Arunachal Pradesh in 1978, Chhattisgarh in 2000, Tamil Nadu in 2002, Gujarat in 2003, and Himachal Pradesh in 2006.[16] In Tamil Nadu, the law was repealed in 2004 by the state government. Table 1 provides a brief summary of the states where the religious conversion laws were enacted during the period 1961-2011. In recent years, similar religious conversion laws have been enacted in other Indian states.[17] Critics of the laws have argued that these legislations are potentially discriminatory and paternalistic in nature and provide discretionary powers to the state (Teater and Jenkins, 2019; Jenkins, 2008; Coleman, 2008; Adcock, 2014; Fischer, 2018).

When it comes to the political dimension, the first round of these sub-national laws in

[16]The law was retained in Chattisgarh when it was carved out of Madhya Pradesh in November 2000. For the purpose of analysis, the year of enactment of the Act has been taken as 2001 in Chhattisgarh.

[17]States like Haryana, Karnataka, Uttarakhand, Uttar Pradesh, Jharkhand, and Rajasthan have come up with laws/ rules in this regard in recent years.

Table 1: States that have enacted a Religious Conversion Law (1961-2011)

State	First Enactment	Out of Force	In Force
Orissa	1967	1973	1977
Madhya Pradesh	1968	×	×
Arunachal Pradesh	1978	×	×
Chhattisgarh	2001	×	×
Tamil Nadu	2002	2004	×
Gujarat	2003	×	×
Himachal Pradesh	2006	×	×

Notes: The table shows states that enacted a Religious Conversion Law between 1961 and 2011. In Orissa, the law was enacted in 1967 but was put out of force in 1973 due to the decision of the Orissa High Court declaring it as unconstitutional. However, the act was again put into force in 1977, following the Supreme Court's reversal of the Orissa High Court's judgment. In Tamil Nadu, the law was repealed in 2004.

Orissa and Madhya Pradesh coincided with the rise of new political parties or coalitions at the state level that posed competition to the Congress Party.[18] In recent years, most of these laws have been enacted by the Bharatiya Janata Party state governments.[19] However, other major political parties, such as the Congress Party, have also enacted these laws during their tenure in state governments (Teater and Jenkins, 2019; Jenkins, 2008). The laws also seem to play an important role in political advertisements as well as in electoral considerations (Figure A.1).[20] Some of the state level laws have also been challenged in the Indian courts (Teater and Jenkins, 2019).[21] In 2022, the Supreme Court of India remarked that forced conversions is a serious issue and could potentially affect national security.[22]

When we look at the on-ground implementation of these laws, we find that they have

[18] www.hindustantimes.com/columns/why-jharkhand-s-anti-conversion-bill-is-against-constitution-and-not-necessary/story-FIhGsnxuqIItvniVAoiLQO.html

[19] www.indianexpress.com/article/cities/chandigarh/haryana-implement-anti-conversion-law-proving-innocence-accused-8333201/

[20] www.indianexpress.com/article/political-pulse/political-pulse-budget-session-karnataka-govt-not-to-table-bill-to-repeal-law-on-anti-conversion-8844984/

[21] www.thehindu.com/news/national/muslim-body-challenges-religious-conversion-laws-of-five-states-in-supreme-court/article66343446.ece

[22] www.indianexpress.com/article/india/forced-religious-conversion-very-serious-matter-supreme-court-8267897/

resulted in limited arrests and no convictions.[23] [24] Jenkins (2008) suggests that the laws primarily play a symbolic role and increase the cost of conversion for potential converts. This also highlights the potential signalling impact of the law, conveying to the majority Hindu population that their religious identity is secure. In public surveys conducted in 2004 and 2009 by the Centre for the Study of Developing Societies (CSDS), around 50% of respondents supported the idea of legislative bans on religious conversions (Sahgal et al., 2021).[25] [26] According to some scholars, the contention surrounding religious conversions and proselytism largely stems from the different attributes that members of traditionally non-proselytizing religions, such as Hindus, and members of traditionally proselytizing religions, such as Christians or Muslims, assign to religion (Claerhout and De Roover, 2005).

3 Empirical Framework

3.1 Identification Strategy

To understand the causal impact of religious restrictions, in the form of religious conversion laws, on social conflicts, I use the new difference-in-differences estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). The estimator allows for estimation of treatment effects when the treatment timing differs across groups and can also accommodate a non-absorbing treatment design, i.e. where the treatment can switch on and off. Allowing for such treatment reversals is crucial from the perspective of my study, as I observe instances where the religious conversion laws were repealed in certain states, only to be implemented again at a later stage. For instance, as Table 1 shows, the religious conversion law was introduced in Orissa in 1967 but went out of force in 1973 due to a High Court ruling before being reinstated in 1977 following the Supreme Court decision. In Tamil Nadu, too, the law was introduced in 2002 but went out of force in 2004. Employing the difference-in-difference estimator proposed by De Chaisemartin and d'Haultfoeuille (2024) helps to account for

[23] www.loc.gov/law/help/anti-conversion-laws/india.php
[24] www.dnaindia.com/india/report-the-law-vs-freedom-of-religion-2782408
[25] www.lokniti.org/media/PDF-upload/1536129143_52791100_download_report.pdf
[26] www.lokniti.org/media/PDF-upload/1536218444_21623000_download_report.pdf

these cases. This heterogeneity-robust DID estimator also serves as an improvement over the traditional two-way fixed effects design that can produce biased estimates when there is heterogeneity in the treatment effects across groups or over time (De Chaisemartin and d'Haultfoeuille, 2024; Roth et al., 2023). [27] The dynamic difference-in-difference estimator also allows for past treatments (lagged treatments) to affect current outcomes. It essentially compares the outcome evolution of the first-time switchers and not yet switchers from the last period before the first-time switchers' treatment changes. I describe the estimator in more detail below.

Using the notation of De Chaisemartin and d'Haultfoeuille (2024), let $Y_{g,t}(d_1, \ldots, d_t)$ denote Riots (per 1000 people) in state g at time t, when it's treatment from period 1 to t is $(d_1, \ldots, d_t)$. Let F_g denote the first time period at which state g's treatment changes, i.e. it introduces a Religious Conversion Law. Then,

$$\delta_{g,\ell} = \mathbb{E}\big[Y_{g,F_g-1+\ell} - Y_{g,F_g-1+\ell}(D_{g,1}, \ldots, D_{g,1})\big] \tag{1}$$

denotes the expected difference between state g's actual Riots (per 1,000 people) at $F_g - 1+\ell$ and the counterfactual Riots (per 1,000 people) if its treatment had remained equal to its period-one value from period one to period $F_g - 1 + \ell$ (De Chaisemartin and d'Haultfoeuille, 2024). Here, ℓ denotes the number of periods since the last period when state g was treated for the first time. In designs where treatment reversals are possible, i.e. treatment can switch on and off, Equation 1 would take the following form (Bjørkås and Rickertsen, 2026):

$$\delta_{g,\ell} = \mathbb{E}\big[Y_{g,F_g-1+\ell} - Y_{g,F_g-1+\ell}(D_{g,1}, \ldots, D_{g,t})\big] \tag{2}$$

Parameter $\delta_{g,\ell}$ is then estimated using,

[27] de Chaisemartin and D'Haultfœuille (2020) and De Chaisemartin and d'Haultfoeuille (2024) show that the two-way fixed effects model are weighted averages of individualistic treatment effects. Furthermore, these weights can potentially be negative and can produce an estimate where the overall average treatment effect is negative even though the individualistic average treatment effects are positive (Liu, Wang and Xu, 2022; Mensah, 2021); thereby, resulting in biased estimates.

$$\hat{\delta}_{g,\ell} = DID_{g,\ell} = Y_{g,F_g-1+\ell} - Y_{g,F_g-1} - \frac{1}{N^g_{F_g-1+\ell}} \sum_{\substack{g': D_{g',1}=D_{g,1} \\ F_{g'}>F_g-1+\ell}} \left(Y_{g',F_g-1+\ell} - Y_{g',F_g-1}\right) \quad (3)$$

where N^g_t refers to the number of states g' with the same treatment as g in period 1, and that have not changed their treatment status throughout period 1 to t. Here, the DID estimator compares the outcome evolution of state g to that of states that have the same baseline treatment and an unchanged treatment status from period 1 to $F_g - 1 + \ell$ (not yet treated units). Averaging $\hat{\delta}_{g,\ell}$ effects over treated states, we get:

$$\hat{\delta}_\ell = \frac{1}{N_\ell} \sum_{g:\, F_g-1+\ell \leq T_g} S_g \, \hat{\delta}_{g,\ell} \quad (1)$$

where T_g represents the last period where there is still a state with the same treatment as state g in period 1 and whose treatment status has remained unchanged. S_g denotes if state g has switched it's treatment status over time and N_ℓ represents the number of states for which $\hat{\delta}_{g,\ell}$ is being estimated (Bjørkås and Rickertsen, 2026). $\hat{\delta}_\ell$ then represents event study estimates for treated states. $\hat{\delta}_\ell$ can be further aggregated to $\hat{\delta}$ that represents the average treatment effect on the treated and calculates the average effect of being treated for $t - F_g + 1$ periods in a setting with dynamic effects (De Chaisemartin and d'Haultfoeuille, 2024; Bjørkås and Rickertsen, 2026).

I rely on STATA package *did_multiplegt_dyn* to estimate event study effects as well as the average treatment effect as calculated in De Chaisemartin and d'Haultfoeuille (2024) (de Chaisemartin et al., 2025).[28] For the purpose of my study, $D_{g,t} = 1$ if religious conversion law was passed or was in force in a state g at time t, and zero, otherwise. $Y_{g,t}$ measures social conflicts using Riots (per 1,000 people) in state g at time t. I include state- and time-fixed effects in my model and cluster the standard errors at the state level to account for possible correlated shocks to state-level riots over time (Iyer et al., 2012).

I also include controls for a wide range of factors that may potentially impact the num-

[28] Version as on March 10, 2026

ber of riots in a state. Some of these include demographic and economic controls, such as urbanisation, sex ratio, literacy rate and per capita incomes that have been well identified in the literature to affect the general crime rate in a state (Soares, 2004; Iyer et al., 2012). I also control for the share of the Scheduled Castes and the Scheduled Tribes population in a state, as anecdotal evidence suggests that these marginalised groups are likely to be more susceptible to coercive religious conversions. In addition, I include time-varying controls for the share of the Muslim population and the share of the Christian population in the state. Controls for political factors such as the year of state election and the presence of specific political parties in the state government - Congress party (INC) or the Bharatiya Janata Party (BJP) - have also been included in the regressions.[29] This inclusion is motivated by the evidence that electoral competition can influence the incidence of riots in India (Chaturvedi, 2005; Wilkinson, 2004; Vadlamannati, 2009; Iyer and Shrivastava, 2018). Furthermore, certain political parties in the country's multi-party political system may be more likely than others to enact the Religious Conversion Laws and accounting for their presence in the state government helps ensure that the results are not biased. I also control for the per capita strength of police force due to their potential impact on frequency of riots in a state.[30]

The dynamic DID estimator introduced by De Chaisemartin and d'Haultfoeuille (2024) is valid under the assumptions of no anticipation and parallel trends, which can be tested using placebo estimators. My identification relies on 5 states in the data that act as switchers and have changed their RCL status at least once within the sample period.[31] I compute my main effects for 10 years post-RCL enactment and placebo estimates for 6 years prior to RCL enactment, as both the number of switchers and the number of observations fall considerably beyond this interval.

[29] I also account for the presence of Bharatiya Jan Sangh (BJS) and the Janata Party (JP) as the roots of BJP trace back to these parties.

[30] Considering the possibility that controlling for some of these factors may also potentially give rise to the problem of *bad controls*, I also estimate my effects without including these control variables in the Appendix with very similar results.

[31] While Chhattisgarh and Arunachal Pradesh are included in the sample, they do not contribute to identification because their treatment status does not change within the study period. Chhattisgarh was created in 2001 and inherited the religious conversion law from the erstwhile state of Madhya Pradesh, leaving no pre-treatment period within the sample. Arunachal Pradesh introduced the law in 1978 while it was still a Union Territory governed by the central government. Although archival records suggest that the central government monitored religious conversions in the Union Territory, Arunachal Pradesh enters the study sample only in 1987, when its status changed to that of a state. This distinction is important because Union Territories, which are directly governed by the central government, may exhibit different conflict-related dynamics. Accordingly, the study's analysis focuses only on states.

3.2 Data

To understand the causal impact of religious restrictions on social conflicts, I collect data on state-level riots in India for the period 1961 - 2011. The data for the period 1961 - 2006 comes from the Center for Systemic Peace, which compiles this information from the 'Crime in India' reports published by the National Crime Records Bureau (NCRB), Government of India. I augment this data with information on state level riots between 2007-2011 directly from NCRB. I also use information from the NCRB to fill some data gaps in the CSP database. The data from NCRB has been extensively used to study crime in India and serves as the most popular source of administrative data on the matter in the country (Iyer et al., 2012; Andrés et al., 2014; Mishra et al., 2014; Vicente et al., 2020).[32] I use data from 25 Indian states to conduct my analysis.[33]

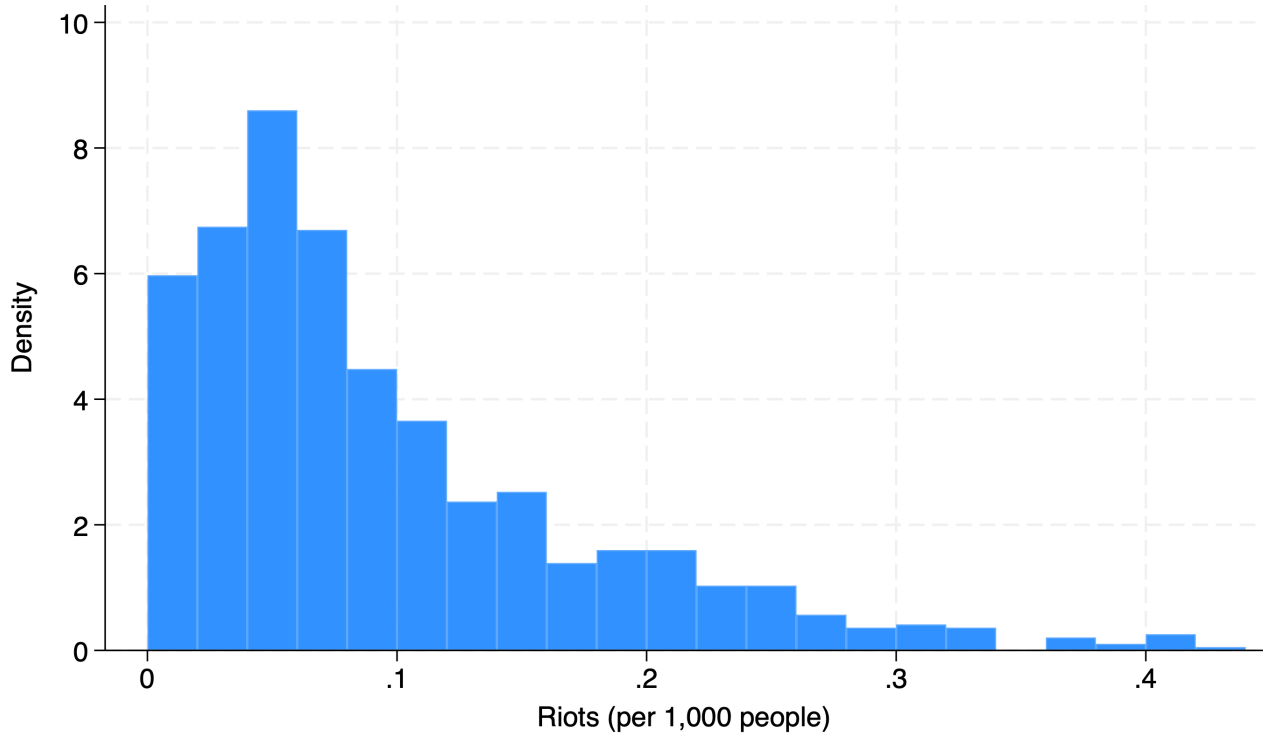
The Indian Penal Code (IPC) provides the following explanation to define riots: '*Whenever force or violence is used by an unlawful assembly, or by any member thereof, in prosecution of the common object of such assembly, every member of such assembly is guilty of the offence of rioting.*' Essentially, the use of force by an unlawful assembly of 5 or more people to achieve a common object is classified as a riot under IPC Section 146. The NCRB does not provide specific information on the type of riot for our sample period. However, the compiled riot measure includes instances of communal violence, along with instances of political and caste violence (SC/ST vs. Non-SC/ST), among others, which may, at least in part, be rooted in religious differences. For instance, research has shown that religious and social conflict can itself be driven by political actors (Froystad, 2009; Jha, 2014; Iyer and Shrivastava, 2018). Similarly, caste violence between Upper Castes and Scheduled Castes/Scheduled Tribes in

[32] I also consider other potential sources of conflict data for India such as from ACLED (Armed Conflict Location & Event Data) and UCDP (Uppsala Conflict Data Program), however, the coverage of these datasets both in terms of number of years and conflict events were limited when compared to the NCRB data. I also explore the Varshney-Wilkinson dataset on religious conflict; however, again, the coverage is limited to Hindu-Muslim conflicts and does not include conflict events related to Christian communities, as well as political and caste-based violence. Evidence also suggests that the data is likely to suffer from under-reporting (Iyer and Shrivastava, 2018). Also, since the conflict between Hindu and Muslim communities is more likely to be driven by factors other than conversion, I do not consider this dataset for my analysis. In the Indian context, issues surrounding conversion have tended to feature more prominently in Hindu–Christian tensions than in Hindu–Muslim conflict.

[33] The states forming a part of the sample include: Andhra Pradesh, Arunachal Pradesh, Assam, Bihar, Chattisgarh, Gujarat, Haryana, Himachal Pradesh, Jammu and Kashmir, Jharkhand, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Manipur, Meghalaya, Nagaland, Orissa, Punjab, Rajasthan, Tamil Nadu, Tripura, Uttar Pradesh, Uttarakhand and West Bengal. This is an unbalanced panel due to state bifurcations and also because some sub-national units changed their status from union territory to state during the study period.

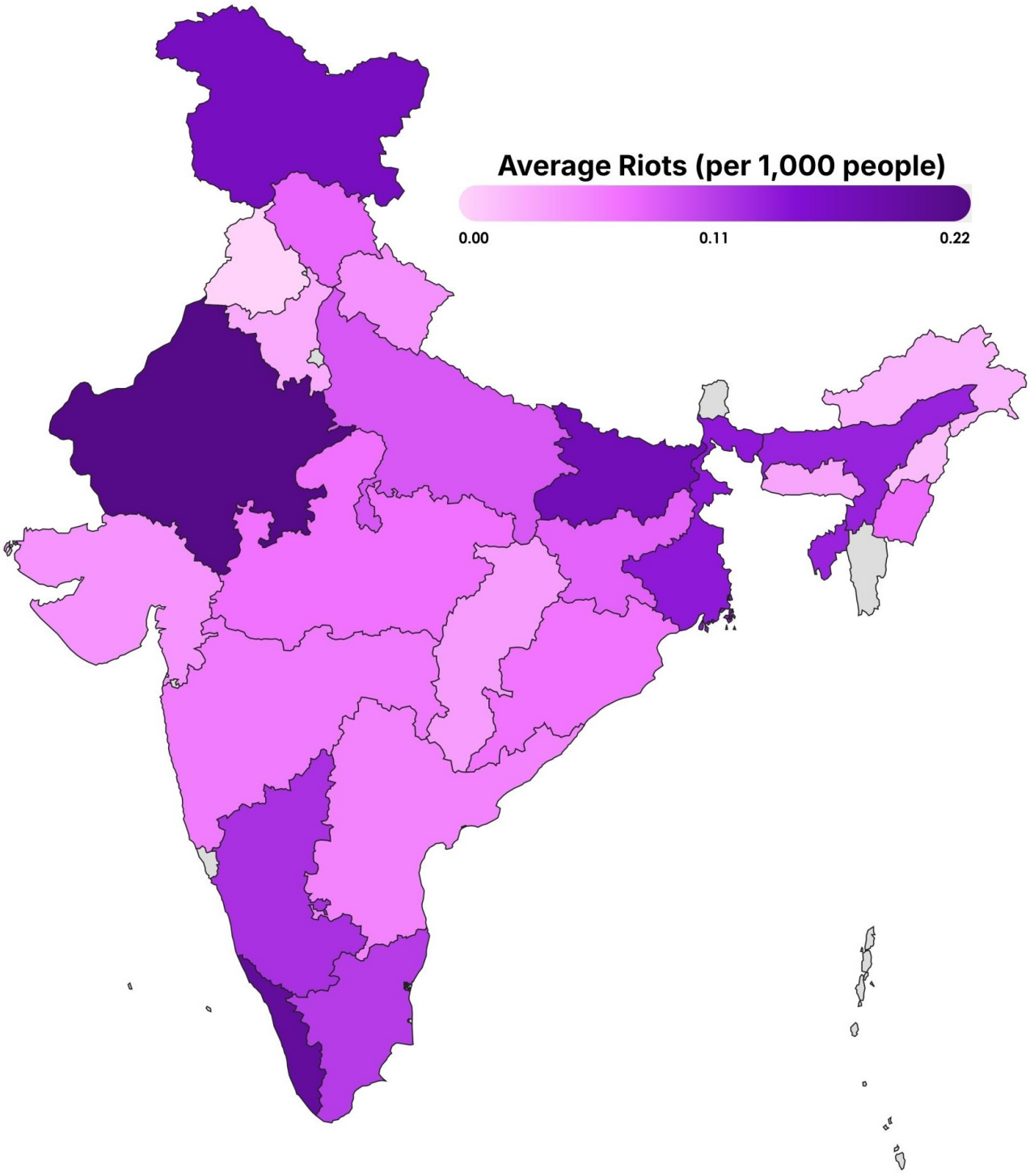
India may actually be driven by tensions around the religious conversion of the latter, as the Upper Castes may believe that conversion disturbs the traditional Hindu social order (USCIRF, 2001; Teltumbde, 2020; Sahoo, 2018; Jaffrelot, 2003). Furthermore, evidence also suggests that religious conflict is likely to be strongly positively correlated to social conflict (Finke and Martin, 2014; Finke, 2013; MacCulloch and Pezzini, 2010). Taken together, these considerations suggest that the NCRB's riot measure serves as an appropriate measure of social conflict for our analysis. Figure 2 presents the distribution of Riots (per 1,000 people) in our sample. Fewer than 0.5% of the observations take a value of zero, suggesting that the dependent variable is not dominated by zeros. I also show Mean Riots (per 1,000 people) as well as temporal variation in Riots (per 1,000 people) for different Indian states in Figure 3 and Figure 4, respectively. As we can observe, the figures highlight significant variation in our outcome variable both across states and over time.

Figure 2: Distribution of Riots (Per 1,000 people)



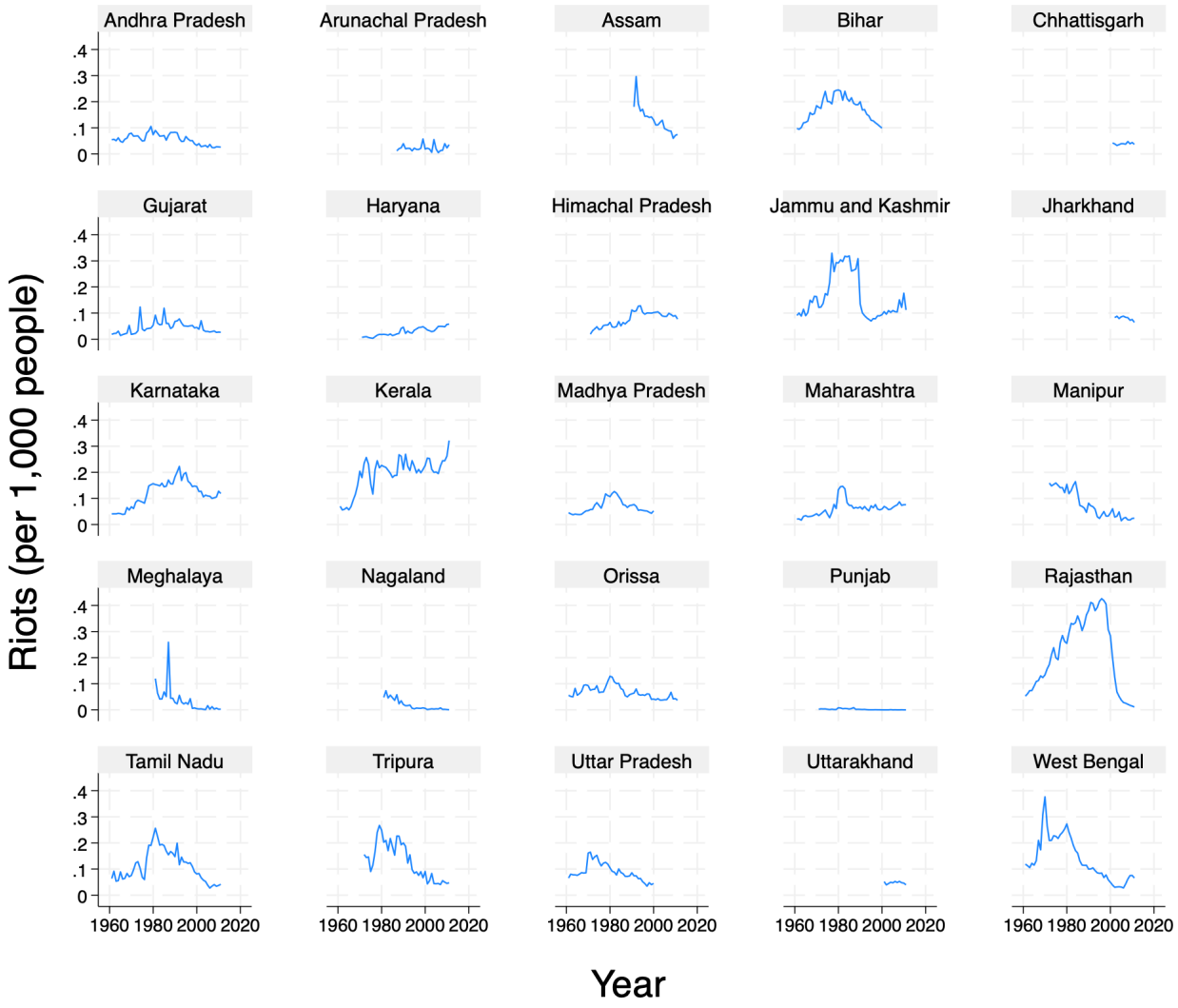
Notes: The histogram shows the distribution of riots (per 1,000 people) using annual NCRB data for the period 1961–2011.

Figure 3: Mean Riots (Per 1,000 people) at the State level



Notes: The map shows average riots (per 1,000 people) at the state level in India for the period 1961-2011 using data from NCRB. States in grey are not included in the sample due to limited data availability. Map created using iipmaps.

Figure 4: Riots (Per 1,000 people) in different states of India over time



Notes: The figure shows riots (per 1,000 people) in different states of India over time using data from NCRB.

To obtain the information on the enactment of the Religious Conversion Legislation in a state, I rely on official government documents.[34] [35] [36] [37] [38] [39] [40] I supplement the informa-

[34] Orissa: www.indiacode.nic.in/bitstream/123456789/5957/1/freedom_of_religion_act.pdf

[35] Madhya Pradesh: https://indiankanoon.org/doc/197298948/

[36] Arunachal Pradesh: www.indiacode.nic.in/bitstream/123456789/3921/1/AP%20freedom%20of%20Religion%201978.pdf

[37] Tamil Nadu: www.indiacode.nic.in/bitstream/123456789/20496/1/the-tamil-nadu-prohibition-of-forcible-conversion-of-religion-act-2002.pdf

[38] Gujarat: www.prsindia.org/files/bills_acts/acts_states/gujarat/2003/Act%20No.%2022%20of%202003%20Gujarat.pdf

[39] Himachal Pradesh: www.yumpu.com/en/document/read/37653014/himachal-pradesh-freedom-of-religion-act-2006-indian-christians

[40] A detailed summary of the laws is also present at https://tile.loc.gov/storage-services/service/ll/llglrd/

Table 2: Summary Statistics

	Observations	Mean	SD	Min	Max
Riots (per 1,000 people)	971	0.095	0.080	0	0.426
Females Per 1000 Males	973	940.911	43.482	859	1084
Literates Per 1000 People	973	450.099	154.887	110	842.237
% of ST population	973	16.302	22.388	0	88.90
% of SC population	973	13.717	7.485	0	31.936
% of Muslim population	973	12.225	14.514	0.84	68.314
% of Christian population	973	8.902	19.561	0.08	89.96
Per Capita Real State NDP	973	16457	9287.649	3182.969	59411.02
Proportion of urban population	973	0.232	0.088	0.063	0.483
BJS / JP / BJP in state govt.	972	0.191	0.394	0	1
INC in state govt.	972	0.580	0.493	0	1
Year of state election Dummy	973	0.219	0.414	0	1
Total Police strength (per 1,000 people)	973	2.169	2.102	0.381	15.996

Notes: The table provides descriptive statistics for both outcome and control variables using annual data from NCRB for the period 1961-2011.

tion on riots and legislations with data on state level control variables such as urbanization rate, literacy rate, sex ratio, state net domestic product (constant price), share of scheduled castes, share of scheduled tribes, share of christians, share of muslims, year of state elections and government formation, presence of Bharatiya Janata Party (BJP) (or its predecessor Bharatiya Jana Sangh (BJS) and Janata Party (JP)) in the state government, presence of Indian National Congress (INC) in the state government, per capita police strength from several sources. A list of these sources is provided in the Appendix. I also use data from the World Values Survey (Waves: 1990, 1995, 2001, 2006, and 2012) (Inglehart et al., 2014) and Employment Surveys of the National Sample Survey Organization (Rounds: 1983, 1987,

2018298841/2018298841.pdf

Table 3: Summary Statistics: Control vs Treated States

	All Periods		Before 1967	
	Control States (1)	Treated States (2)	Control States (3)	Treated States (4)
Riots (per 1,000 people)	0.105	0.068	0.073	0.047
	(0.088)	(0.042)	(0.031)	(0.021)
Females Per 1000 Males	934.932	956.479	938.555	968.687
	(46.118)	(30.739)	(47.826)	(25.606)
Literates Per 1000 People	450.254	449.694	253.411	266.043
	(160.297)	(140.104)	(108.606)	(64.594)
% of ST population	15.483	18.436	4.254	14.649
	(23.956)	(17.526)	(3.969)	(9.037)
% of SC population	13.549	14.155	13.643	13.334
	(7.671)	(6.971)	(5.480)	(4.293)
% of Muslim population	15.311	4.189	18.430	4.659
	(15.955)	(2.673)	(18.156)	(2.603)
% of Christian population	11.015	3.402	3.415	1.923
	(22.413)	(5.442)	(6.451)	(2.054)
Per Capita Real State NDP	16471.05	16420.41	8780.482	8347.148
	(9269.345)	(9352.308)	(2443.552)	(1945.67)
Proportion of urban population	0.235	0.221	0.184	0.189
	(0.076)	(0.112)	(0.058)	(0.087)
BJS / JP / BJP in state govt.	0.155	0.286	0	0
	(0.362)	(0.452)	(0.000)	(0.000)
INC in state govt.	0.586	0.565	0.907	1
	(0.493)	(0.496)	(0.292)	(0.000)
Year of state election Dummy	0.220	0.218	0.166	0.125
	(0.414)	(0.414)	(0.376)	(0.337)
Total Police strength (per 1,000 people)	2.371	1.643	1.173	1.173
	(2.357)	(1.037)	(0.600)	(0.352)

Notes: The table provides means for treated and control states separately using annual data from NCRB. In Column (1) and Column (2), these statistics are provided for all the periods (1961-2011), whereas in Column (3) and Column (4), these are provided for only the pre-law era, i.e. before 1967, when the religious conversion law was implemented for the first time in Orissa. Standard deviation is reported in parentheses.

1993, 1999, and 2004) (National Sample Survey Office, 1983, 1987, 1993, 1999, 2004) to explore potential mechanisms behind my effects. Table 2 provides the descriptive statistics for the primary outcome as well as control variables. In Table 3, I also present comparative summary statistics for states that enacted religious conversion laws relative to those that did not. As we can observe, in the pre-law era, i.e. before 1967, when the first Religious Conversion Law was enacted in Orissa, the treated and the control states looked similar across most of the demographic dimensions, with the exception of the share of Scheduled Tribes and the share of Muslims. While the difference in the share of Scheduled Tribes likely stems from their perceived vulnerability to coercive conversions, a primary justification for these legislations, the difference in the Muslim share likely reflects underlying regional demographic variation rather than systematic factors related to the enactment of the laws.

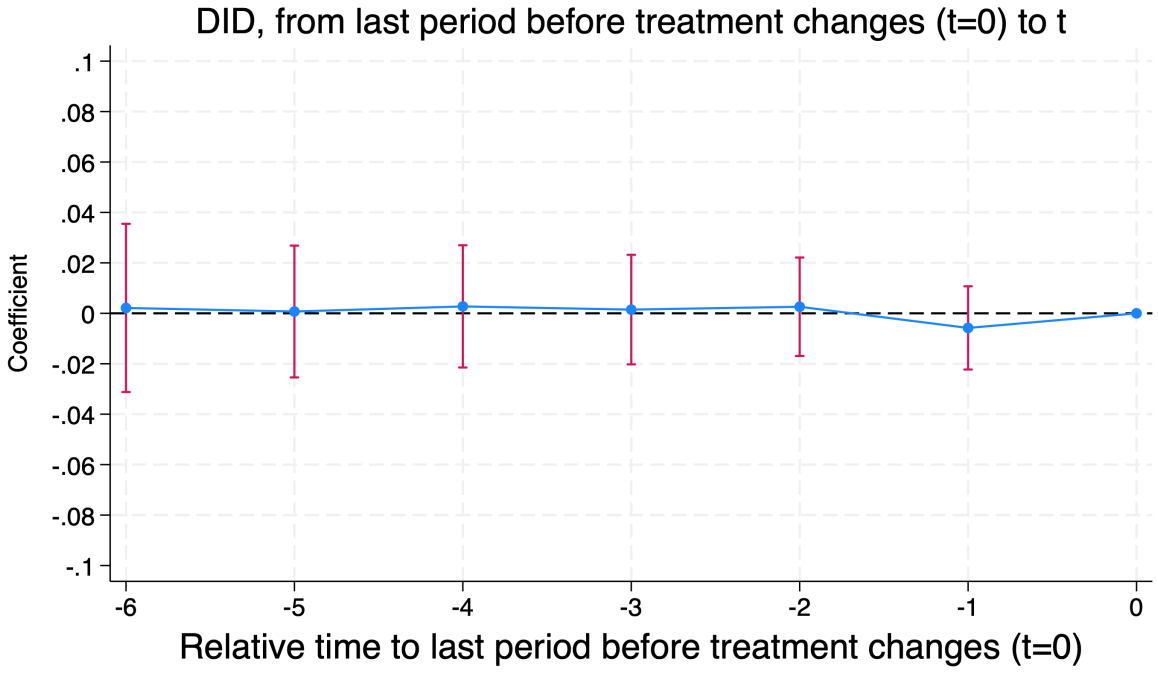
4 Results

In this section, I present the estimates for the impact of religious conversion laws on social conflicts in India using the dynamic difference-in-differences method introduced by De Chaisemartin and d'Haultfoeuille (2024). I first test the identifying assumptions behind the estimator and then report the main findings.

The results from the dynamic difference-in-difference estimator by De Chaisemartin and d'Haultfoeuille (2024) are valid under the assumptions of no anticipation and parallel trends. The no anticipation assumption would imply that the current level of riots in a state should not depend on its future RCL status, i.e., whether the religious conversion law is enacted or not enacted in the future. The parallel trends assumption, standard in the DID literature, suggests that the states switching their RCL status (or enacting a religious conversion law) would have experienced the same trend in riots when compared to the states that didn't switch their RCL status till then, in the absence of the switch. In other words, there should be the same expected evolution in riots for the switcher and not yet switcher states, where switcher and not yet switcher states had the same baseline RCL status, in the absence

of a switch in the RCL status for the switcher states. These assumptions can be tested using the placebo estimators introduced in De Chaisemartin and d'Haultfoeuille (2024). The placebo estimator, in line with the standard practice in the event study regressions, compares the number of Riots (per 1,000 people) across switcher and non yet switcher states during the years prior to the switch, relative to the last pre-switching year (De Chaisemartin and d'Haultfoeuille, 2024; Artunç and Saleh, 2025).

Figure 5: Placebo Effects (Pre-trends)



Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors are clustered at the state level.

Figure 5 plots the placebo estimates that suggest that the assumptions of no anticipation and that of parallel trends are supported by the data. Specifically, we do not observe any differential pre-trends in riots between states that switched RCL status and those that didn't. All the estimates are small and close to zero, with none of the effects coming out to

Table 4: Estimate for Placebo Effects (Pre-trends)

	Event Study Effects at time:					
	t-1	*t-2*	*t-3*	*t-4*	*t-5*	*t-6*
RCL	-0.006	0.003	0.001	0.003	0.001	0.002
	(0.010)	(0.012)	(0.013)	(0.015)	(0.016)	(0.020)
Switchers	5	5	5	5	5	4
N	76	73	73	71	68	56
P-value (F test for joint significance)			0.556			
P-value (Sup-t test)			0.849			

Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The table reports the dynamic effects for a given period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

be significant at conventional levels. Results from the conventional F-Test in Table 4 also suggest that the placebo estimates are jointly insignificant. Following the recommendation of De Chaisemartin and d'Haultfoeuille (2024), I also conduct a sup-t test that accounts for multiple testing and correlation among effects across time periods. The sup-t test also fails to reject the null hypothesis of parallel trends. This confirms that no single placebo period deviates significantly from baseline, providing further confidence in the absence of pre-treatment effects.

A potential threat to the validity of the difference-in-differences estimates can also come from contemporaneous policy changes at the state level that may coincide with the enactment of religious conversion laws. If this happens, the estimated treatment effect may be picking up the impact of these other policy interventions rather than the causal impact of the conversion laws themselves, thereby confounding the interpretation of the estimates. However, the staggered, time-varying nature of the state-level religious conversion laws helps to mitigate this particular concern to a large extent. Furthermore, to the best of my knowledge, there were no major policy-level shocks occurring in the same years as the enactment of these

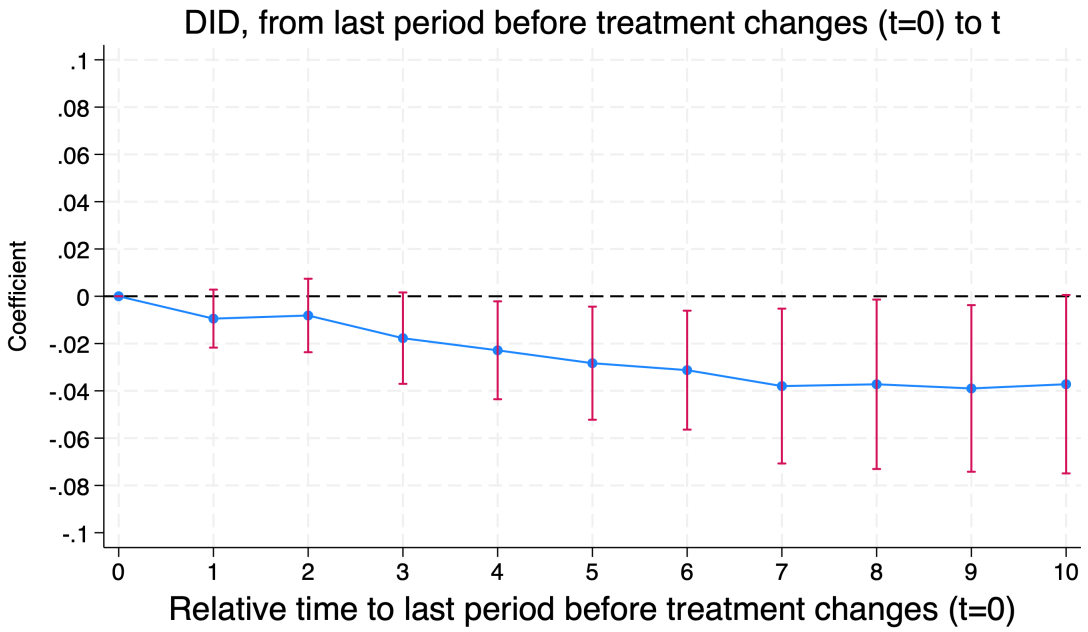
Table 5: Correlates of Religious Conversion Laws (Pre-1967)

	Dummy for RCL		
	(1)	(2)	(3)
Proportion of urban population	4.865	5.289	-1.797
	(5.502)	(12.610)	(6.580)
Proportion of literate population	-0.0003	0.0025	0.001
	(0.003)	(0.005)	(0.002)
Female-Male Ratio	0.010	0.006	-0.001
	(0.005)	(0.011)	(0.006)
Per Capita Real State NDP	0.000006	-0.00007	-0.00006
	(0.0001)	(0.0002)	(0.0001)
Proportion of SC population	0.057	0.054	0.020
	(0.034)	(0.107)	(0.053)
Proportion of ST population	0.067*	0.043	0.076
	(0.029)	(0.040)	(0.056)
Proportion of Muslim population	0.017	0.045	0.077
	((0.017)	(0.022)	(0.056)
Proportion of Christian population	-0.018	-0.037	0.080
	(0.072)	(0.081)	(0.117)
INC in state govt.		-7.148	5.993
		(12.829)	(7.784)
Year of state election Dummy		-2.191	-10.081
		(8.632)	(5.034)
Riots (per 1,000 people)			-16.491
			(5.960)
R^2	0.77	0.85	0.98
Observations	13	13	13
State Fixed Effect	No	No	No
Time Fixed Effect	No	No	No

Notes: The table shows correlates for the enactment of the religious conversion laws using pre-1967 data at the state level. Standard errors are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

religious conversion laws that would plausibly explain the observed patterns in social conflict. I report results from several robustness tests to check the sensitivity of the estimates in the next section.

Figure 6: Effect of treatment change after t periods: De Chaisemartin and d'Haultfoeuille (2024)



Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors are clustered at the state level.

We may also be concerned about potential endogenous adoption of religious conversion laws by the states. For instance, it is possible that states with a high level of social conflict were more likely to enact these laws when compared to the states that didn't. I, therefore, try to test whether certain state-level factors in the pre-law era (before 1967) serve as correlates for the eventual implementation of the religious conversion laws. The results of this exercise are presented in Table 5. We observe that only the share of Scheduled Tribes (before 1967) is likely to be positively correlated with the eventual implementation of the law. These results are consistent with the legislative intent, as Scheduled Tribes constituted part of the marginalised sections that states believed to be at a higher risk of forced conversion. Hence, it is possible that states with a higher share of Scheduled Tribe populations were more likely to enact these laws. This intent is also reflected in the provisions of most such

laws, which prescribe enhanced penalties, often double the standard punishment, for the forcible conversion of individuals belonging to marginalised groups. Importantly, however, we do not observe that social conflicts in the pre-law era predict the adoption of religious conversion laws in the future.

Table 6: Main Effects: For treatment at time t, dynamic effect using De Chaisemartin and d'Haultfoeuille (2024) at period -

	Event Study Effects at time:									
	t+1	*t+2*	*t+3*	*t+4*	*t+5*	*t+6*	*t+7*	*t+8*	*t+9*	*t+10*
RCL	-0.009	-0.008	-0.018	-0.023*	-0.028*	-0.031**	-0.038*	-0.037*	-0.039*	-0.037
	(0.007)	(0.009)	(0.012)	(0.013)	(0.015)	(0.015)	(0.020)	(0.022)	(0.021)	(0.023)
Switchers	5	5	5	5	5	5	4	4	4	3
N	78	76	76	75	74	74	57	57	57	40
Average Total Effect					-0.035*					
					(0.018)					
Average Time over which Treatment Effect Accumulates					5.61					
Switch × Periods					45					
N					331					

Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The table reports the dynamic effects for a given period along with the average total effect. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

I now report the main results for the impact of religious conversion laws on social conflicts, as measured by Riots (per 1,000 people). Figure 6 plots these estimates. As we can observe, results show that the religious conversion laws lead to a reduction in social conflict in the treated states. Each event study coefficient represents the average effect of being exposed to a weakly higher dose of the Religious Conversion Law for that specific time period. Since past exposure to the law can impact current riots, lagged effects contribute to the event study estimates for states where treatment potentially turns off (RCL gets repealed/stayed

in court) in certain time periods. The average total effect of the law per treatment unit, accumulated over current and future periods over which its effect persists, was -0.035 riots (per 1,000 people). This translates into an average effect of -0.006 (0.035/5.61) for being exposed to the religious conversion law for one additional year. In terms of percentage change, this is a reduction of around 6.3% each year the law is in force, which means going from an average of 0.095 riots (per 1,000 people) to 0.089 riots (per 1,000 people). To give a sense of the magnitude of effects, this amounts to a 0.075 standard deviation reduction in riots.

5 Robustness Checks

In this section, I undertake several sensitivity checks to ensure the robustness of my findings. This is important to ensure that the identification design is actually picking up the true causal effect of Religious Conversion Laws on social conflict and is not leading to a spurious result.

5.1 Alternative Identification Design

I first test the robustness of my findings using a different identification design. As Table 1 shows, Madhya Pradesh is the only treated state in the sample where the religious conversion law has been continuously in force since its enactment in 1968, and the data is available for a long period of time. I therefore use this uninterrupted implementation of the Religious Conversion Law in Madhya Pradesh to assess the robustness of my estimates using a synthetic controls design (Abadie, Diamond and Hainmueller, 2015; Abadie and Gardeazabal, 2003; Abadie, Diamond and Hainmueller, 2010). Under the method, the parallel trends assumption is relaxed, and the control unit is built by weighing the comparison group observations. The technique essentially creates a counterfactual that can be compared with the actual trend of the treated group (White and Raitzer, 2017). The reliability of the estimator depends on its ability to trace the trajectory of the outcome variable in the pre-treatment period (Abadie,

2021). For a sub-sample of 10 states, I use the method to build a counterfactual for Madhya Pradesh and compare the resulting trend.[41] I use the values of the outcome variable in each pre-treatment year as predictors for creating the synthetic control.

Table 7: Robustness Check: Summary Statistics for Synthetic control predictors

Predictors	MP	'Synthetic MP'	Full Sample (without MP)	Period
Riots (per 1,000 people)	0.045	0.040	0.068	1961
Riots (per 1,000 people)	0.040	0.040	0.069	1962
Riots (per 1,000 people)	0.037	0.040	0.068	1963
Riots (per 1,000 people)	0.040	0.042	0.078	1964
Riots (per 1,000 people)	0.038	0.041	0.074	1965
Riots (per 1,000 people)	0.038	0.038	0.081	1966
Riots (per 1,000 people)	0.039	0.039	0.104	1967

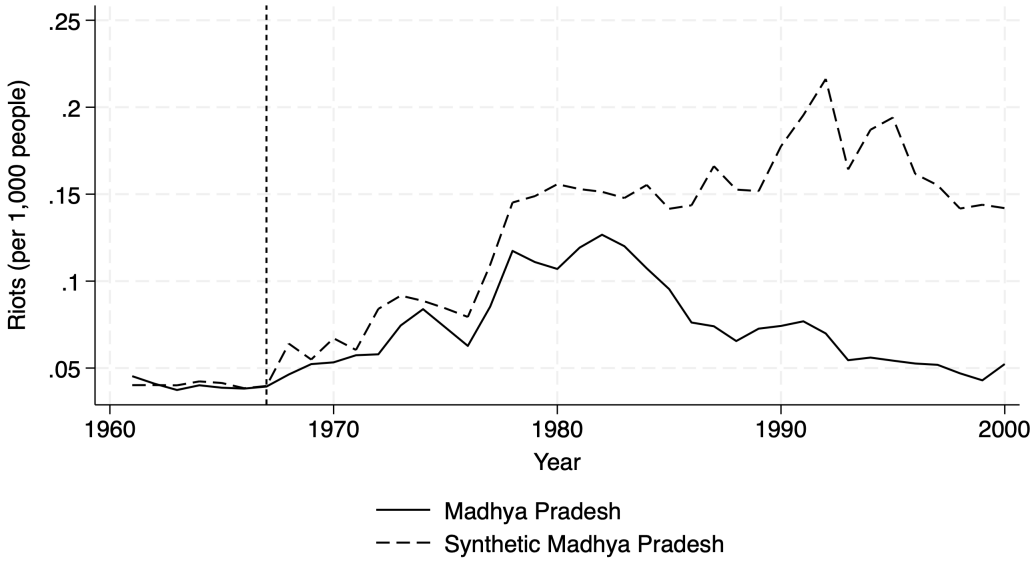
Note: The table shows descriptive statistics of predictors for Madhya Pradesh, Synthetic Madhya Pradesh and Full Sample (9 states after excluding Madhya Pradesh) for the period 1961-1967.

Table 7 shows the summary statistics for the predictors. We can observe that the mean values of the predictors for the state of Madhya Pradesh (MP) are roughly similar to those of the constructed 'Synthetic Madhya Pradesh (MP).' In contrast, the mean values for the predictors differ quite a lot when compared to the Full Sample. This shows that the estimation has succeeded in creating a reasonably good counterfactual for Madhya Pradesh. Figure 7 compares the trend for riots (per 1,000 people) for Madhya Pradesh (treated unit) and Synthetic Madhya Pradesh (synthetic control unit). We again observe that riots in 'Synthetic Madhya Pradesh' are higher when compared to Madhya Pradesh post-enactment of the Religious Conversion Law. The results are thus much in line with the estimates from our primary identification strategy, thereby providing us with confidence in our findings. In the Appendix Figure A.2, I also show results from the placebo exercise where I assign placebo treatment to different states and plot gaps in riots (per 1,000 people). The results broadly

[41] The implementation of the methodology using the *synth* package in STATA software requires a strongly balanced panel which leads to a reduction in the number of states that can be used for the analysis

support our findings in this section. I also run an alternative specification that excludes riots in 1967 as a predictor to account for potential endogenous adoption of the law and any anticipation effects. The results for this exercise, reported in Appendix Figure A.3, are similar to our main findings.

Figure 7: Robustness Check: Synthetic control estimates for effect on Riots (per 1,000 people) in MP



Notes: The figure compares the trend in riots (per 1,000 people) for Madhya Pradesh (with Religious Conversion Law) to Synthetic Madhya Pradesh (without Religious Conversion Law) using the Synthetic Controls methodology (*synth* command in Stata). The dotted red line represents 1967, the last pre-treatment year for Madhya Pradesh.

In addition to the synthetic controls design, I also use the counterfactual estimator developed by Liu, Wang and Xu (2022) to test the robustness of my findings. The imputation-based estimator can account for heterogeneous effects while allowing for treatment reversals. The results for this exercise are reported in Appendix Figure A.4 and are also in line with the main estimates.

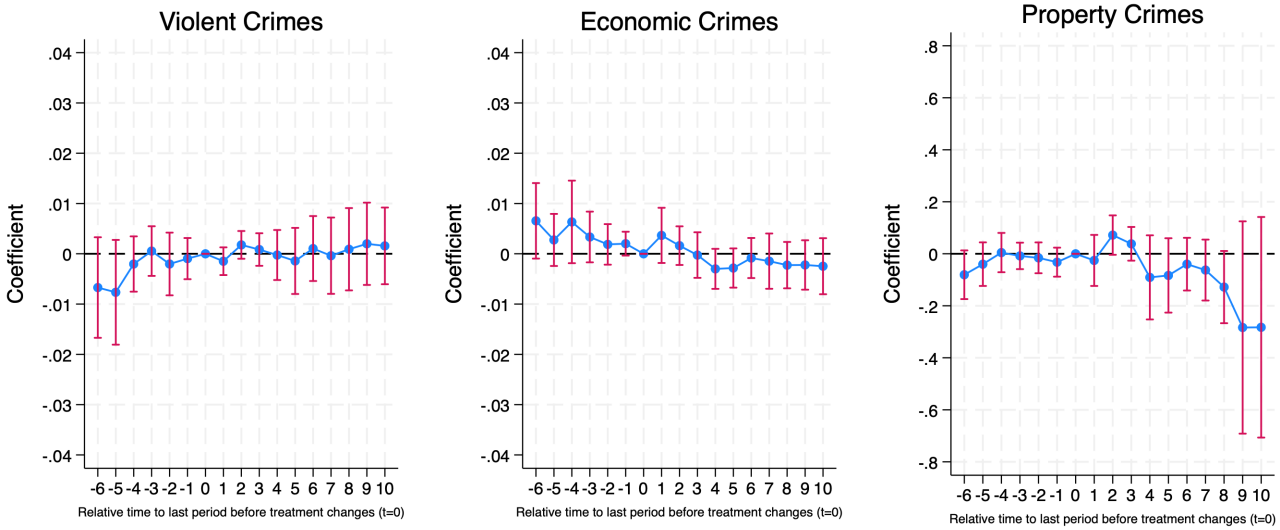
5.2 Impact on Other Crimes

Table 8: Robustness Check: Impact of RCL on other types of crime

	Violent Crimes	Economic Crimes	Property Crimes
Average Total Effect (ATE)	0.0005 (0.002)	-0.0010 (0.003)	-0.1128 (0.0952)
Average Time over which Treatment Effect Accumulates	5.61	5.61	5.61
Switch × Periods	45	45	43
N	331	324	329
P-value (F test for joint sig. of placebos)	0.657	0.563	0.503
P-value (Sup-t test for placebos)	0.497	0.374	0.451

Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The table reports the average total effect. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

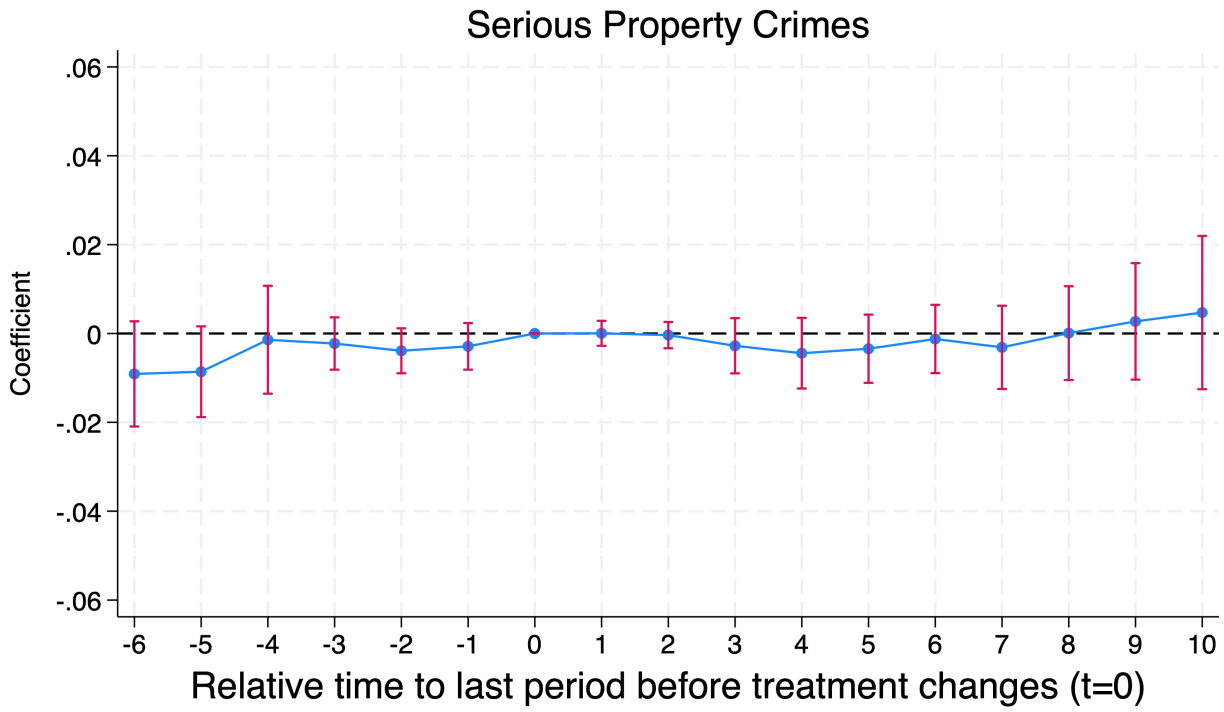
Figure 8: Robustness Check: Impact of RCL on other types of crime



Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors are clustered at the state level.

It is possible that the results for the impact of the religious conversion laws on social conflicts are potentially driven by a general decline in crime. If this happens, then our estimates are likely to be biased. In order to account for this concern, I try to check the impact of the law on other crimes in the state, including violent crime (murder and kidnapping), property crimes (theft, robbery, dacoity, and burglary), and economic crimes (counterfeiting and criminal breach of trust).[42] Ideally, the religious conversion law, which has religious and social dimensions, should not have a significant effect on these other crime categories that are more likely driven by personal enmity or economic motives.

Figure 9: Robustness Check: Impact of RCL on Serious Property Crimes



Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors are clustered at the state level.

[42] According to IPC, dacoity refers to a robbery committed by five or more people.

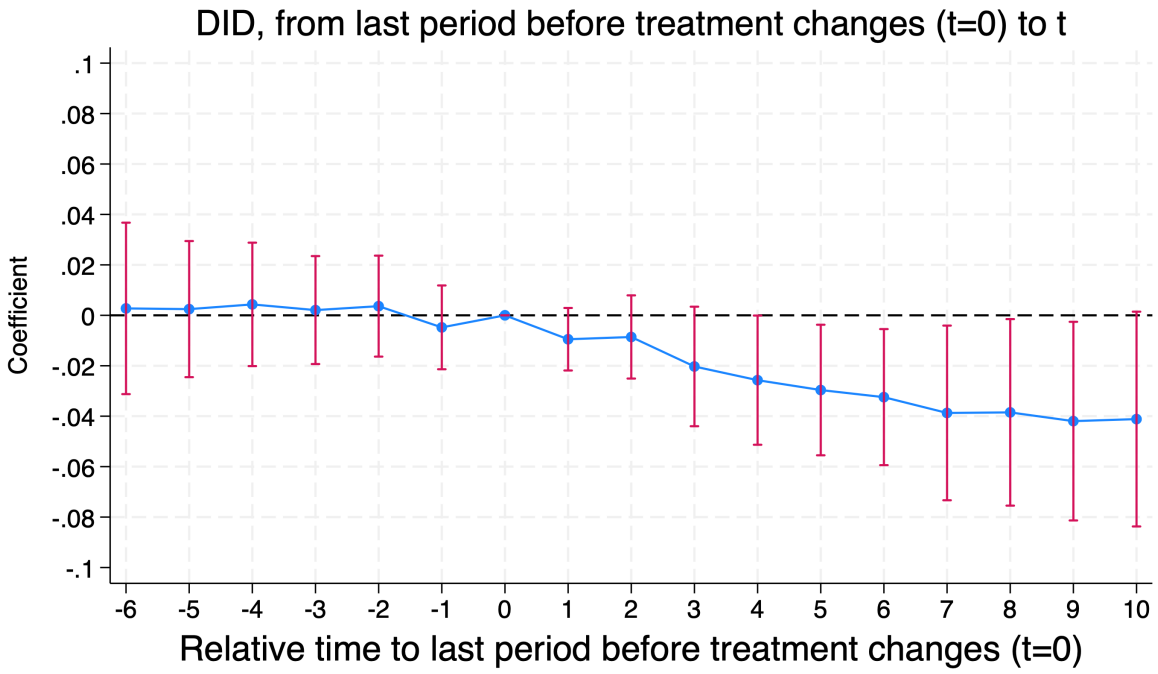
Figure 8 and Table 8 present the results for this exercise.[43] We find that other crimes, including violent crimes, property crimes, and economic crimes, remain largely unaffected by the enactment of the religious conversion laws and do not follow any monotonic pattern that is visible in riots. We do not observe any significant impact of the laws on these crimes across any event study period. Furthermore, the estimated coefficients are mostly close to zero, particularly for violent and economic crimes. The estimates for property crimes, while statistically insignificant for all time periods, are more noisy. This may be due to reporting variations inherent in lower-intensity property offences, such as petty theft or burglary, which are more susceptible to administrative discretion. I, therefore, perform an additional check and estimate effects for serious property crimes, specifically dacoity and robbery, which are less prone to under-reporting due to their severity. Figure 9 presents these results. We again do not observe any monotonic pattern in the effects, and the estimates remain close to zero. This suggests that the primary findings for Riots are not on account of a general secular decline in criminal activity, but rather reflect the impact of religious conversion law in the state. This is in line with our understanding that the religious conversion laws majorly affected conflicts born out of social tensions due to the inherent nature of such laws.

5.3 Never Switchers as Control Group

In order to estimate my effects, I rely on 'not yet treated' groups as a control group. This has the advantage of providing precision to the estimates as the control group in the 'not yet treated' case is likely to be larger when compared to the 'never treated' case (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021). However, some researchers also favour 'never treated' groups as a control group over 'not yet treated' groups, particularly when the number of 'never treated' units is large and where these units are similar to those units that eventually get treated (Callaway and Sant'Anna, 2021). This is because 'never

[43] Appendix Tables A.1 and A.2 presents event study estimates for these other crimes.

Figure 10: Robustness Check: Impact on Riots (per 1,000 people) using 'never treated' groups as control group



Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors are clustered at the state level.

treated' groups may provide a clean and stable control group for the comparisons (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). I, therefore, also test the robustness of my findings by using 'never treated' groups as a control group and present the findings in Figure 10 and Table 9. Results for the event study effects are provided in Appendix Table A.3 and A.4. As we can observe, using 'never treated' groups as a control group leads to a reduction in the number of observations that we can use to estimate our effects. However, even with never-treated units as a control group, the primary results remain largely unchanged, thereby providing confidence in my findings.

Table 9: Robustness Check: Impact on Riots (per 1,000 people) using 'never treated' groups as control group

	Riots
Average Total Effect (ATE)	-0.037*
	(0.020)
Average Time over which Treatment Effect Accumulates	5.61
Switch × Periods	45
N	303
P-value (F test for joint sig. of placebos)	0.629
P-value (Sup-t test for placebos)	0.896

Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The table reports the average total effect. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

5.4 Other Checks

I also undertake several additional checks to ensure the robustness of my findings. For instance, a potential issue with the analysis can be the presence of *'bad controls'* as certain variables, such as population shares of different groups, might be endogenous. To account for this concern, I conduct my analysis again, this time without including any controls and only with state and time fixed effects. The results for this analysis are presented in Appendix Figure A.5 and Table A.5 and are similar to our main findings.

Next, I also test the sensitivity of my estimates to alternative sample compositions. For instance, I exclude the state of Jammu & Kashmir from the sample as the state has experienced much higher levels of conflict on account of historical reasons when compared to other states in India. I also conduct similar analysis by removing smaller northeastern states, as well as states that were earlier union territories, from the sample. I also perform an additional check by excluding the state of Gujarat, which has experienced high levels

of Hindu-Muslim violence not directly related to the issue of religious conversion, from the sample.[44] The results for this analysis are presented in Appendix Figure A.6 and Table A.6 and are qualitatively similar to our main findings.

I also present placebo estimates for my main results for a longer period in Appendix Figure A.7. While the number of observations falls drastically beyond period $t-6$, i.e. from period $t-7$ to $t-10$, if the parallel trends hold for this extended pre-period, this would help ensure that my DID estimators for the period $t+7$ to $t+10$ are unbiased (De Chaisemartin and d'Haultfoeuille, 2024). As we can observe in figure A.7, we do not observe a violation of the parallel trends assumption for the periods $t-7$ to $t-10$, suggesting that the DID estimates for the period $t+7$ to $t+10$ remain unbiased. Here, placebo estimates for the periods $t-7$, $t-8$, and $t-9$ have been computed using 30 observations (2 switchers) and the placebo estimate for the period $t-10$ has been computed using 15 observations (1 switcher). This reflects a substantial reduction when compared to 56 observations (4 switchers) used to compute the placebo estimator for the period $t-6$.

I also use bootstraped standard errors to test the sensitivity of my estimates. Since, in my primary specification, I cluster standard errors at the state level (25 states), the estimates can potentially be biased due to a lower number of clusters (Cameron, Gelbach and Miller, 2008). I, therefore, bootstrap my standard errors by running 1000 iterations and present the results in Appendix Figure A.8. As we can observe, the results remain much in line with the main estimates.

In addition to the above checks, I also test the sensitivity of my results to non-linear models (Appendix Table A.7) and also run an alternative specification where I exclude Rajasthan from the sample post 2005, when the state government passed the religious conversion bill but didn't receive the assent for the same from the state governor (Appendix Table A.9). The results from these checks are qualitatively similar to our main findings.[45]

[44] Issue related to religious conversions has, however, indirectly contributed to such conflicts. For instance, research suggests that the 2002 violence in the state was also the first time Hindu - Muslim riots spread to the tribal belt of Gujarat where indigenous population (*adivasis*) played a major role in riots against Muslims (Jaffrelot, 2003,0) This was largely attributed to economic incentives as well as years of social and religious activism by Hindu right wing groups regarding religious conversion by Christians and Muslims that had led to several flashpoints and violent episodes in these areas prior to the 2002 riots (Jaffrelot, 2003; Narula, 1999; Jaffrelot, 2024).

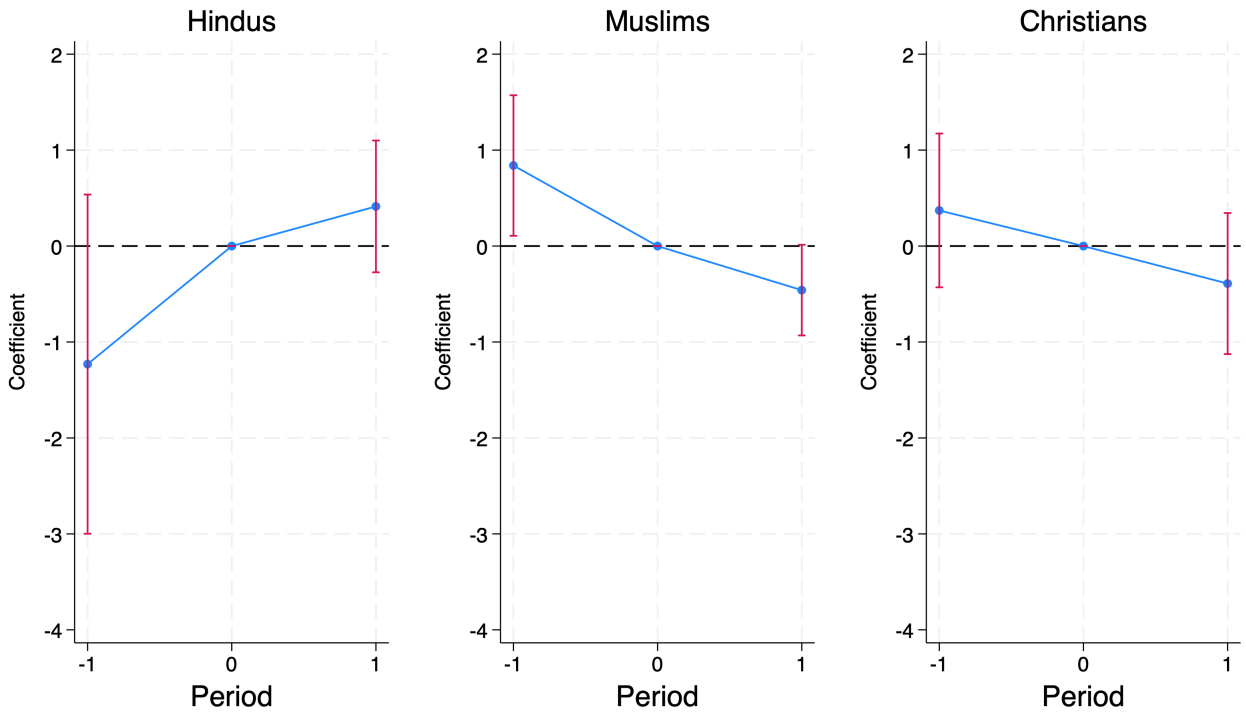
[45] Main results for all possible time periods are also presented in Appendix Figure A.10.

6 Potential Mechanisms

The empirical analysis in the preceding section tells us that the enactment of a Religious Conversion Law limiting coercive conversions can result in lower social conflict. In this section, I discuss three potential mechanisms through which such an effect may operate.

6.1 Increased cost of religious conversion

Figure 11: Impact of RCL on share of different religions in the state



Notes: Unconditional estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. Since the data on population of different religions is only available through the decadal Census of India, the primary independent variable here measures the number of years RCL has been in operation in a given state in a given decade. The main outcome variable is the share of individuals with a particular religious affiliation at the end of a decade. Regressions include state- and time-fixed effects. Standard errors are clustered at the state level. Coefficient estimates are also available in Appendix Table A.8.

The theoretical framework developed by Barro, Hwang and McCleary (2010) suggests that the cost of switching religion increases when the government has laws regulating it. It is, therefore, possible that the state-level religious conversion laws in India increase the

cost of conversion for *potential* converts as well as for the religious priest(s) performing the ceremony. While minor variations exist in different state-level legislations, a key feature is to intimate the government authorities, i.e. the district magistrate, within a specified period regarding the conversion. In some states, this has to be done prior to conversion and sometimes with the permission of the district magistrate.[46] This means that an immediate effect of the law is that it reduces coercive conversion by the way of dissuasion as well as the approval mechanism, in addition to the punishments imposed under the law. Even for those who wish to convert voluntarily and without any compulsion, the lengthy process still increases the cost of conversion by introducing significant bureaucratic hurdles (Azzi and Ehrenberg, 1975; Barro, Hwang and McCleary, 2010; Barro and McCleary, 2024; Jenkins, 2008). Furthermore, the religious conversion law subjects what would otherwise be a private decision to public scrutiny, which may itself serve as a deterrent to convert (Jenkins, 2008). Thus, by reducing the overall number of conversions, these laws may limit conversion-related flashpoints and thereby reduce the likelihood of conflict between different groups.

While there is no publicly available data on religious conversions in India, a crude way to test this mechanism could be to see if the law impacts the share of individuals belonging to different religious denominations within a state. I test this using data from the decadal census of India for the period 1961-2011. Here, I construct an explanatory variable that captures the number of years a religious conversion law was in force in a given state during a decade, and examine how it affects the share of individuals belonging to different religious affiliations. I restrict my analysis to only the decade during which the religious conversion law was implemented for the first time for a certain number of years in a particular state. This is because both the number of switchers and the number of observations decline sharply for subsequent periods, given that the data is at the decadal level and that the majority of the laws were enacted only in the last decade for my sample (2002-2011). Figure 11 presents the results for this analysis. The direction of the coefficients, atleast in the short run, is consistent with the conjecture that the laws reduced conversions (forced or otherwise) by

[46] https://tile.loc.gov/storage-services/service/ll/llglrd/2018298841/2018298841.pdf

increasing the costs associated with the process, as we observe a modest increase in the share of Hindus and a corresponding decline in the share of Muslims and Christians in states that adopted these laws. However, the estimated effects are small and not statistically significant at conventional levels. Furthermore, other factors, such as migration, may also influence these patterns and potentially inflate them. For instance, if the enactment of religious conversion laws signals to minority religious communities—Muslims and Christians—that the government is more likely to protect and support the religious identity of the majority community—Hindus—this could lead to selective outmigration of these minority groups, which would in turn be reflected in the religious composition at the state level. I discuss this mechanism in more detail in the next subsection. Taken together, the analysis in this section suggests that the laws had little, if any, measurable impact on religious conversions at the state level.

6.2 Reduced Competition between religions

The literature suggests that competition can often become a source of conflict when multiple parties seek the same prize (Simmel, 1955; Hall, 2003). This is especially true in the absence of a church-like hegemony that dictates social order (Hall, 2003). In such settings, the outcomes of conflict are very likely to be shaped by how defenders respond to challengers, particularly in the absence of third-party intervention (Kniss, 1997; Hall, 2003).

In the context of religious conversions in India, we observe strong parallels to this broader phenomenon of religious competition. Potential competition between religious communities — Hindus, Muslims, and Christians — to increase or retain members of their group can lead to conflict. This situation may be further exacerbated when other social or political groups compete for the same followers. Recent empirical evidence supports this view, showing that religious competition for followers plays an important role in social conflict, as groups may feel threatened by the prospect of declining membership due to competition (Galindo-Silva and Tchuente, 2021).

In the Indian setting, state-level religious conversion laws may function as a form of central

authority, limiting conflict and fostering social order by reducing uncertainty surrounding religious conversion. Importantly, the institutional and signalling role of such laws may be more consequential than their actual on-the-ground enforcement. This interpretation is consistent with the observation of Jenkins (2008), who document that religious conversion laws resulted in minimal arrests and no convictions (at least until 2011).[47] By their enactment alone, these laws can act as a signal for government's stance on religious conversion and may alter the behavior of communities and groups involved.

Table 10: Rajasthan: Signalling Impact

	Full Sample (1)	Northern States Sample (2)	Border States Sample (3)
RCL	-0.162***	-0.190***	-0.148
	(0.015)	(0.036)	(0.079)
R^2	0.76	0.81	0.94
Observations	701	235	173
Controls	×	×	×
State FE	×	×	×
Year FE	×	×	×

Notes: The estimates compare Rajasthan with other untreated states in India before and after the passage of the religious conversion bill in 2006, which was not enacted due to the governor's refusal to assent. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

I try to provide suggestive evidence for this conjecture by studying the case of Rajasthan, where the 'Freedom of Religion' bill that restricted coercive conversions was passed by the state legislature in 2006 but never became a law as it failed to get the Governor's assent (Jenkins, 2008). I use this instance to study the institutional and signalling effects of the law in a context where the government signalled its stance, but the legislation was not formally enacted or enforced. Table 10 presents the results of this exercise, where only Rajasthan

[47] https://tile.loc.gov/storage-services/service/ll/llglrd/2018298841/2018298841.pdf

has been included as a treated state, and other states with religious conversion laws have been excluded from the sample. I also run alternative specifications where only the northern states and the bordering states have been included as a control group. As we can observe, the placebo enactment of the law in Rajasthan is associated with a reduction in social conflicts, a pattern consistent with the possibility that the institutional and signalling effects of religious conversion laws may help reduce religious competition and social conflict even in the absence of enforcement. Taken together with anecdotal evidence of minimal arrests and convictions under these laws, the results suggest that the institutional and signalling channel may be an important mechanism underlying the observed effects on social conflict.

6.3 Impact on Religious Minorities

While religious conversion laws aim to curtail unjust conversions, scholars have argued that they could potentially have a disparate effect on religious minorities (Coleman, 2008; Jenkins, 2008; Teater and Jenkins, 2019; Fischer, 2018). This is because while the majority religion - Hinduism - does not proselytise, this religious practice constitutes an important tenet of minority religions, including Christianity and Islam. The laws may provide an improved sense of protection against coercive conversions to the majority Hindu community; however, this may come at a cost of a chilling effect for minorities who may consider laws as a religious restriction and a signal of government support for the majority religion. This would then have an impact on how religious minorities conduct their daily lives and could potentially have an impact on conflict as well.

Existing research in economics and sociology suggests that perceived external pressures on one's religious group may lead to increased religious identification and religious practice (Chen, 2010; Lim and Putnam, 2010; Immerzeel and Van Tubergen, 2013; Storm, 2025). Individuals may perceive threats against their religious group as personal threats, prompting a turn toward religion as a source of social and psychological support (McCoy and Major, 2003; Storm, 2025; Berman, 2000; Akerlof and Kranton, 2000). This effect may be particularly pronounced among minority groups who, when facing perceived prejudice from a

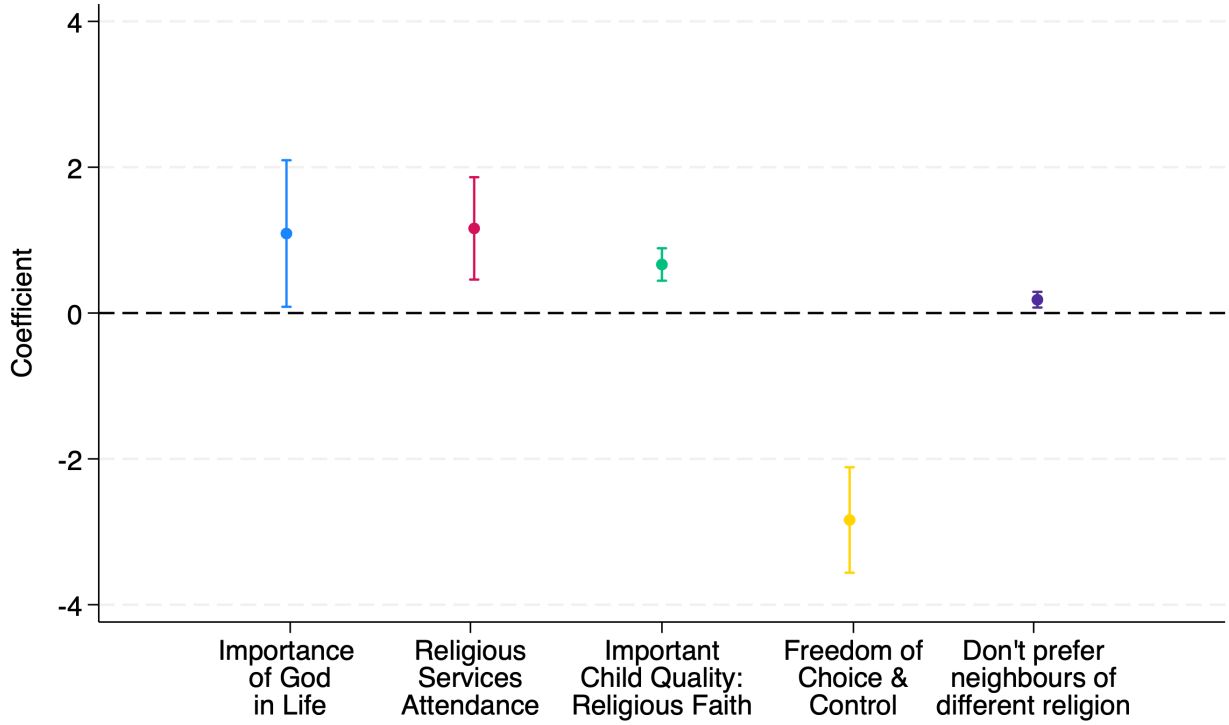
majority group, may increase in-group identification and commitment to in-group activities (Branscombe, Schmitt and Harvey, 1999; Stanley and Kay, 2022). In some contexts, this process can also result in reduced identification with other social groups and, in more extreme cases, can lead to outgroup prejudice and social distancing (Branscombe, Schmitt and Harvey, 1999; Fleischmann, Leszczensky and Pink, 2019; Storm, 2025). Reduced interaction between majority and minority communities may then lower opportunities for violence, though potentially at the cost of entrenched community stereotypes and adverse effects on minority well-being (Bhavnani et al., 2014).

I explore whether such a mechanism may potentially be at play in the case of religious conversion laws in India by examining attitudes and outcomes among Christians in Gujarat.[48] Using data from the World Values Survey, I examine how the enactment of the religious conversion law in the state relates to religiosity, well-being, and preferences among Christians. The results from this exercise, reported in Figure 12, suggest that religious service attendance and the reported importance of God are higher among Christians after the enactment of the law. We also observe that Christians are more likely to consider religious faith an important quality for their children to have. At the same time, Christians are less likely to report feeling freedom of choice and control over their lives, suggesting potential adverse implications for well-being. In addition, Christians are also less likely to express a preference for neighbours of a different religion. Taken together, these results provide suggestive evidence that the enactment of the religious conversion law in Gujarat coincides with higher religiosity, lower perceived freedom of choice, and changes in inter-group contact

[48]The primary reason I focus on Gujarat is on account of data limitations associated with the World Values Survey (WVS). I use data from five waves of the survey - 1990, 1995, 2001, 2006, and 2012 - for my analysis. This means that I cannot use it to understand effects for states that enacted these laws before 1990. The WVS also does not have data on Himachal Pradesh that enacted the law in 2006. Tamil nadu that enacted the law in 2002 and repealed it in 2004 also falls between two rounds leaving only Gujarat for possible analysis. I only focus on the outcomes of Christians in the state on account of clashes between the Hindu and Muslim communities in Gujarat in the year 2002. Literature suggests that while the Godhra incident served as an immediate trigger for the violence, deep-rooted hatred among some sections and historical grievances related to religious identity and demographic change, coupled with religious polarization served as important structural causes (Jaffrelot, 2003; Engineer, 2002; India Today, 2019; Jaffrelot, 2024). Research suggests that this was also the first time Hindu - Muslim riots spread to the tribal belt of Gujarat where indigenous population (*adivasis*) played a major role in riots against Muslims (Jaffrelot, 2003,0) This was largely attributed to economic incentives as well as years of social and religious activism by Hindu right wing groups regarding religious conversion by Christians and Muslims that had led to several flashpoints and violent episodes in these areas prior to the 2002 riots (Jaffrelot, 2003; Narula, 1999; Jaffrelot, 2024) However, since the riots were still predominantly between Hindu and Muslim communities whose attitudes may change on account of this significant event rather than the religious conversion law, I try to focus on understanding attitudes and outcomes among Christians in relation to the law. The results for other religious communities are, however, provided in the Appendix.

preferences among members of the Christian community. Additional results based on data from the National Sample Survey and reported in the Appendix Figure A.11 also indicate adverse associations for Christians in labour market outcomes. While these results, based on an analysis of a single treated state and a relatively small sample of Christians, should be interpreted with caution, they nevertheless remain broadly consistent with the possibility that the laws may potentially generate a chilling effect and have broader socio-economic implications for minority communities.

Figure 12: Impact of RCL on Christians



Notes: The estimates compare outcomes for Christians in Gujarat with those in other untreated states in India before and after the passage of the religious conversion law in the state in 2003, using data from the World Values Survey. The model includes individual-level controls for age, sex, marital status, and education, as well as state and time fixed effects. Standard errors are clustered at the state level. The figure shows 90% confidence interval.

7 Conclusion

In this paper, I provide empirical evidence on the impact of religious restrictions on social conflict in the context of a religiously diverse country, India. I examine this in the setting of religious conversion laws enacted by several sub-national governments to restrict coercive conversions, in response to Christian missionary activity in tribal areas. Since proselytism constitutes an important tenet of many religions, including Christianity and Islam, these laws can be viewed as a form of religious regulation on these groups. To estimate the effects of these laws, I exploit state-time variation in their enactment using the difference-in-differences estimator proposed by De Chaisemartin and d'Haultfoeuille (2024), which accounts for both heterogeneous and dynamic treatment effects. The results suggest that laws restricting coercive religious conversions—those deemed forced or fraudulent, or carried out through allurement or inducement—lead to a 6.3% reduction in social conflict, as measured by riots, for each year the law is in force. The analysis indicates that reduced inter-religious competition, driven by the signalling impact of the law, is likely to be the primary channel underlying this effect, particularly given limited on-ground arrests and convictions under these laws. The laws also had a small but statistically insignificant impact on the share of Hindus (positive), Muslims (negative), and Christians (negative) in the enacting states in the short run. In addition, the laws were associated with higher religiosity, lower perceived freedom of choice, adverse labour market outcomes, and changes in inter-group contact preferences for a relatively small sample of Christians, in line with patterns that may reflect some role of a potential chilling-effect channel in reducing conflict. The results of the analysis remain robust to a range of alternative specifications and sensitivity checks.

The study adds to the rather scant literature on the economic analysis of religious regulations and restrictions. It highlights the importance of considering not only the direct effects of such regulatory interventions but also their broader institutional and signalling implications for inter-group relations. More generally, the findings suggest potentially ambiguous welfare implications of such regulations, as they may lower social conflict but may also entail

costs for minority well-being.

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A Appendix

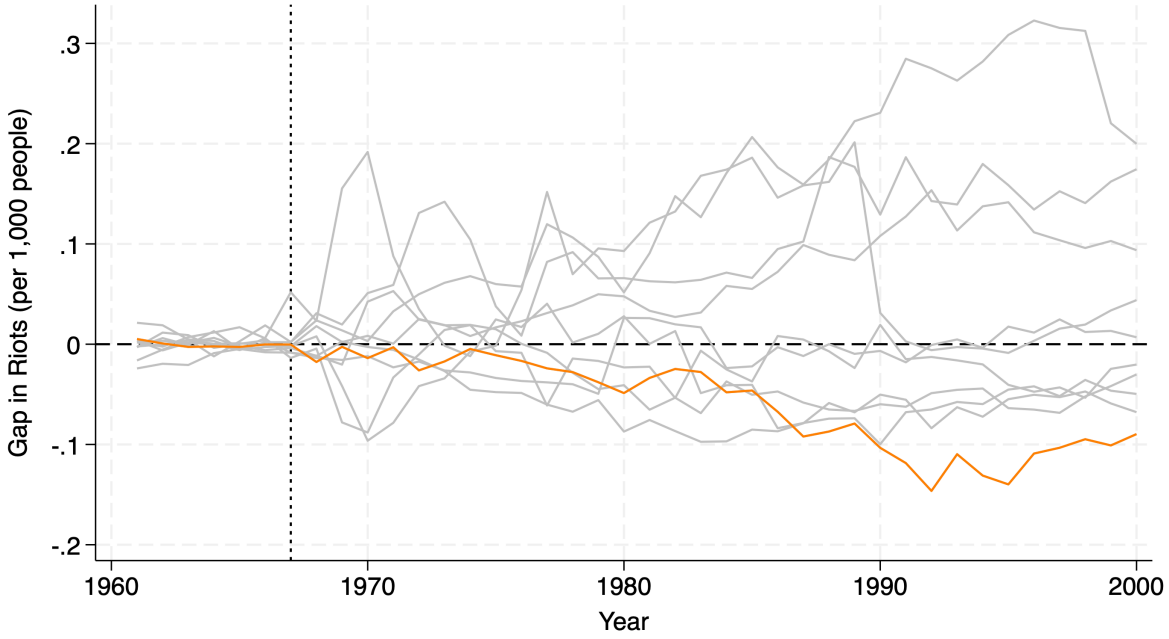
A.1 Data Sources for variables used as controls or for robustness checks

1. State-level data on violent, economic, and property crimes and total police strength is from the Centre for Systemic Peace and the National Crime Records Bureau.
2. State-level data on urbanisation, literacy, sex ratio, share of Scheduled Castes, share of Scheduled Tribes, share of Christians, and share of Muslims is from the Census of India (1961-2011). Since the Census is conducted once in 10 years, the values for the intervening years have been calculated using interpolation.
3. State Net Domestic Product at Constant Price data 1961-2011: Central Statistical Organization, Ministry of Statistics and Programme Implementation, Government of India, and Reserve Bank of India.
4. Data on State election years, results, and state government: Election commission of India, indiastatelections.com, and news articles.

Figure A.1: Advertisement of Religious Conversion Law in Uttarakhand

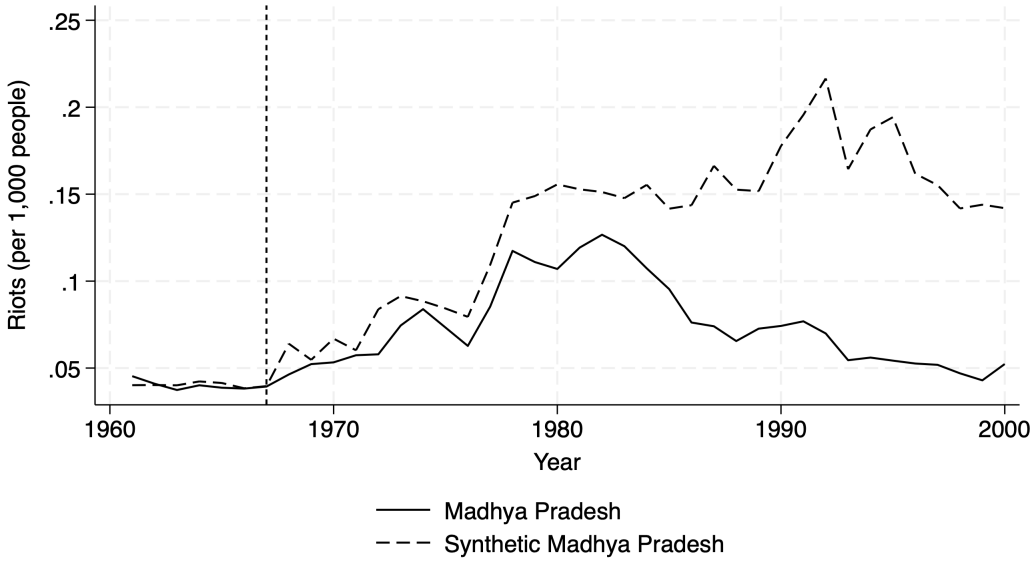


Figure A.2: Placebo effects for Synthetic Control design



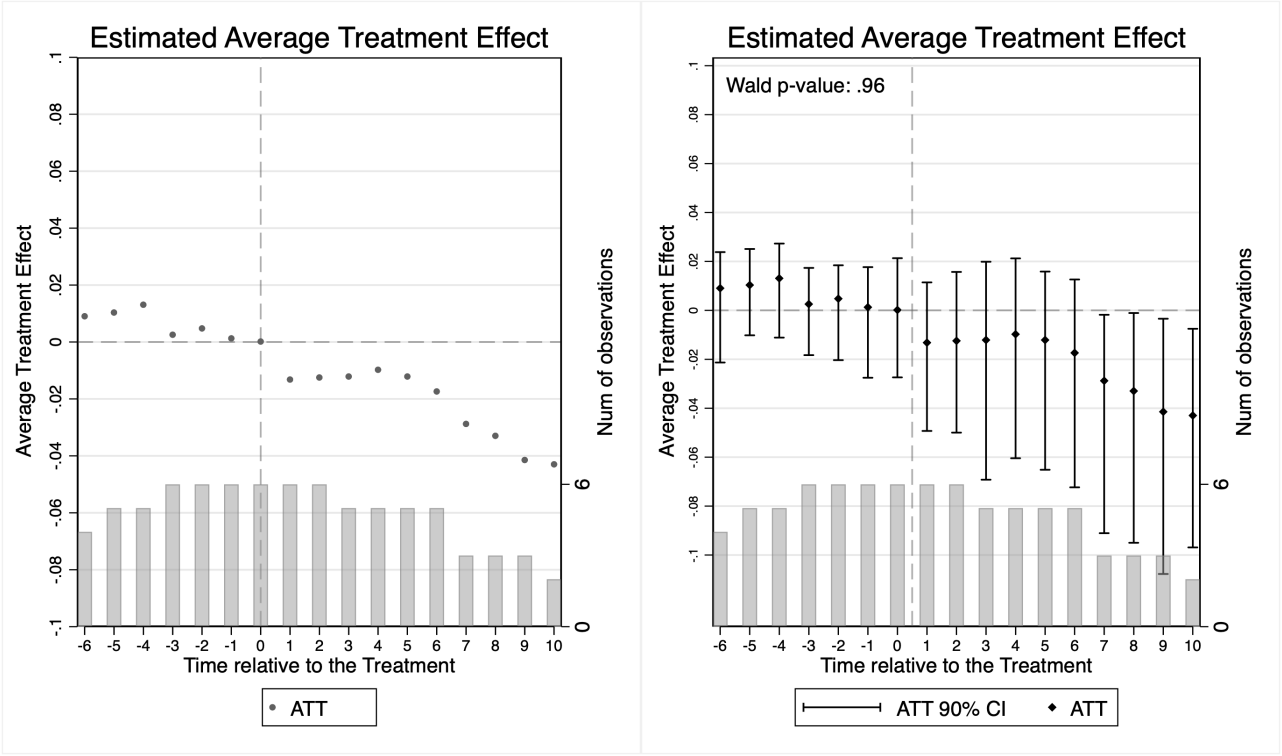
Notes: The figure plots the gap in riots (per 1,000 people) for actual states and their synthetic units using the synthetic controls design. The line colored in orange represents this gap for Madhya Pradesh and Synthetic Madhya Pradesh starting 1968 when the law was first enacted in the state. All the other lines represent gaps in the riots for placebo treatment in the other 9 states.

Figure A.3: Robustness: Synthetic control estimates for effect on Riots (per 1,000 people) without 1967 Riots as predictor



Notes: The figure compares the trend in riots (per 1,000 people) for Madhya Pradesh (with Religious Conversion Law) to Synthetic Madhya Pradesh (without Religious Conversion Law) using the Synthetic Controls methodology (*synth* command in Stata). The dotted red line is for the year 1967, the last pre-treatment year for Madhya Pradesh. For this robustness exercise, riots in the year 1967 have not been included as a predictor to account for potential anticipation.

Figure A.4: Results using Liu, Wang and Xu (2022)



Notes: Estimates from Liu, Wang and Xu (2022) estimator implemented using *fect* command in Stata. The figure shows the estimate and the corresponding 90% confidence intervals. The model includes controls for state level factors such as proportion of urban population, proportion of literate population, female - male ratio, per capita real state net domestic product, proportion of scheduled castes, proportion of scheduled tribes, proportion of christians, proportion of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors are clustered at the state level.

Table A.1: Placebo Effects: Other Crimes

Outcomes ↓	Event Study Effects at time:					
	t-1	*t-2*	*t-3*	*t-4*	*t-5*	*t-6*
Violent Crimes	-0.0009	-0.003	0.0005	-0.002	-0.008	-0.007
	(0.002)	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)
Switchers	5	5	5	4	4	4
N	76	73	73	59	56	56
Economic Crimes	0.002	0.002	0.003	0.006	0.003	0.007
	(0.001)	(0.002)	(0.003)	(0.005)	(0.003)	(0.005)
Switchers	5	5	5	4	4	4
N	74	72	72	58	58	55
Property Crimes	-0.032	-0.016	-0.008	0.004	-0.039	-0.080
	(0.033)	(0.036)	(0.031)	(0.045)	(0.050)	(0.056)
Switchers	5	4	4	4	4	4
N	76	60	61	59	56	56

Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The table reports the dynamic effects for a given period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.2: Impact of RCL on Other Crimes

Outcomes ↓	Event Study Effects at time:									
	t+1	*t+2*	*t+3*	*t+4*	*t+5*	*t+6*	*t+7*	*t+8*	*t+9*	*t+10*
Violent Crimes	-0.001	0.002	0.0008	-0.0002	-0.001	0.0001	-0.0003	0.0009	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Switchers	5	5	5	5	5	5	4	4	4	3
N	70	70	70	70	70	70	53	53	53	36
Economic Crimes	0.003	0.002	-0.002	-0.003	-0.003	-0.0008	-0.001	-0.002	-0.002	-0.002
	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Switchers	5	5	5	5	5	5	4	4	4	3
N	77	75	75	73	73	72	56	56	54	36
Property Crimes	-0.025	0.071	0.038	-0.090	-0.083	-0.039	-0.063	-0.128	-0.283	-0.283
	(0.059)	(0.046)	(0.039)	(0.098)	(0.086)	(0.061)	(0.071)	(0.084)	(0.248)	(0.257)
Switchers	5	5	5	5	5	5	4	4	4	3
N	78	64	64	75	74	74	57	57	57	40

Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The table reports the dynamic effects for a given period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.3: Placebo Effects: 'Never treated' groups as a control group

	Event Study Effects at time:					
	t-1	*t-2*	*t-3*	*t-4*	*t-5*	*t-6*
RCL	-0.005	0.003	0.002	0.004	0.002	0.003
	(0.010)	(0.012)	(0.013)	(0.014)	(0.016)	(0.020)
Switchers	5	5	5	5	5	4
N	68	67	67	66	65	55

Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The table reports the dynamic effects for a given period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.4: Impact of RCL on Riots (per 1,000 people) using 'never treated' groups as a control group

	Event Study Effects at time:									
	t+1	*t+2*	*t+3*	*t+4*	*t+5*	*t+6*	*t+7*	*t+8*	*t+9*	*t+10*
RCL	-0.009	-0.009	-0.020	-0.026*	-0.030*	-0.032**	-0.039*	-0.038*	-0.042*	-0.041
	(0.008)	(0.010)	(0.014)	(0.016)	(0.016)	(0.016)	(0.021)	(0.022)	(0.024)	(0.026)
Switchers	5	5	5	5	5	5	4	4	4	3
N	70	70	70	70	70	70	53	53	53	36

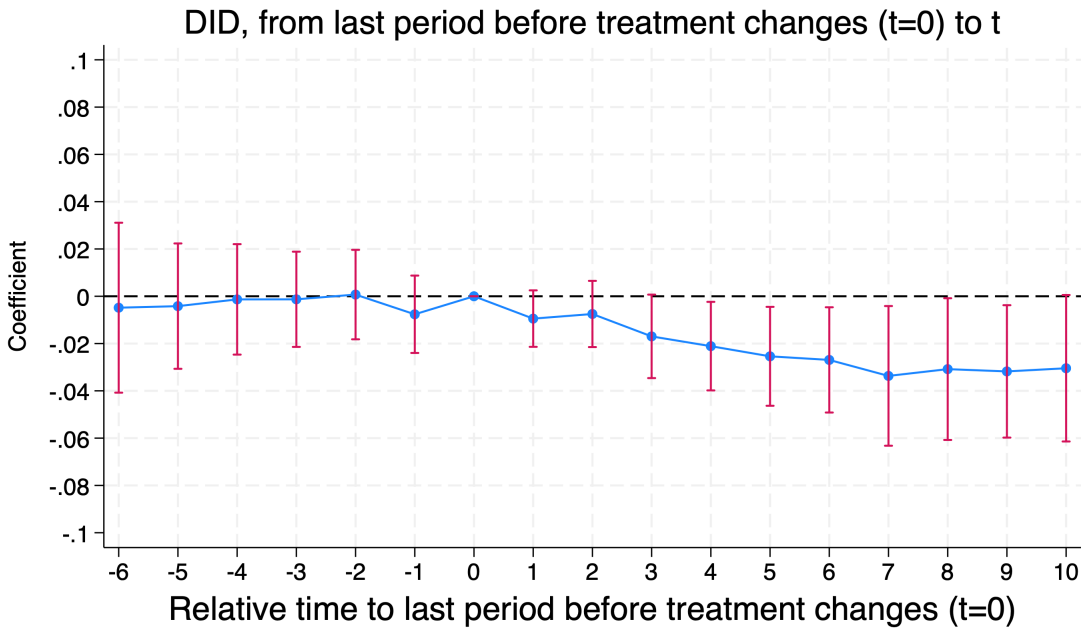
Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The table reports the dynamic effects for a given period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.5: Robustness Check: Impact on Riots (per 1,000 people) without controls

	Riots
Average Total Effect (ATE)	-0.031**
	(0.016)
Average Time over which Treatment Effect Accumulates	5.61
Switch × Periods	45
N	331
P-value (F test for joint sig. of placebos)	0.513
P-value (Sup-t test for placebos)	0.704

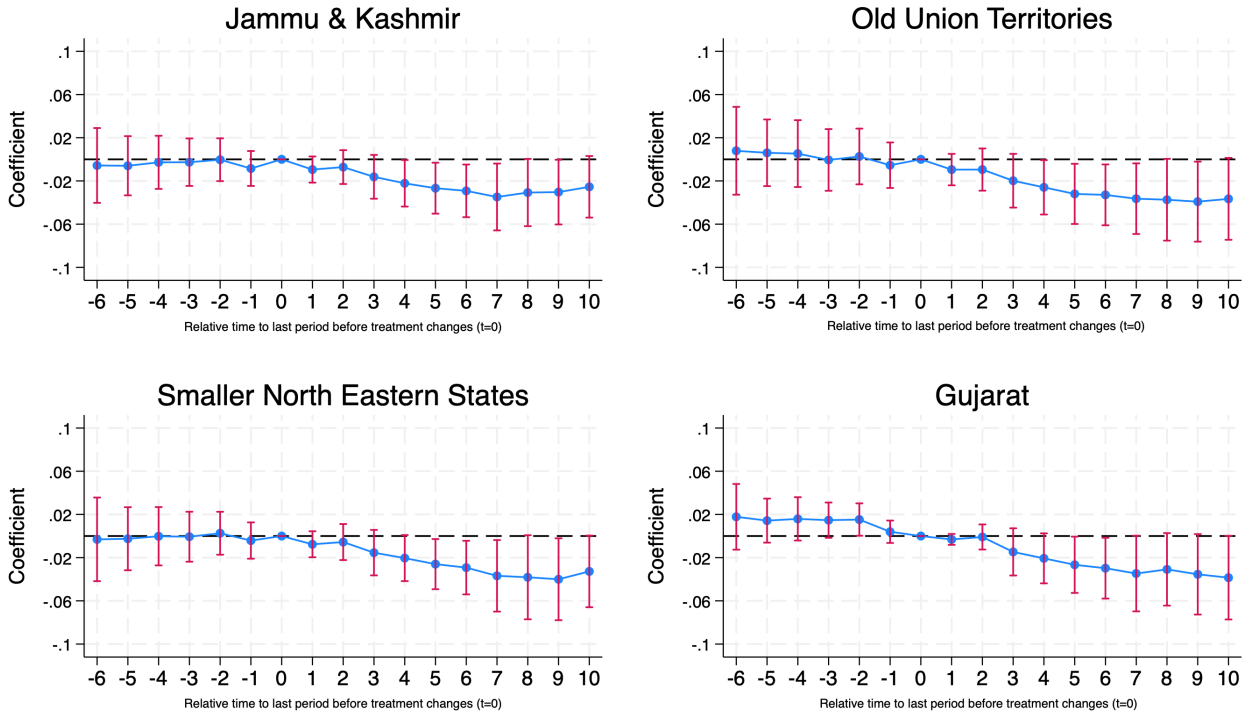
Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The table reports the average total effect. The model includes state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Figure A.5: Robustness Check: Impact on Riots (per 1,000 people) without controls



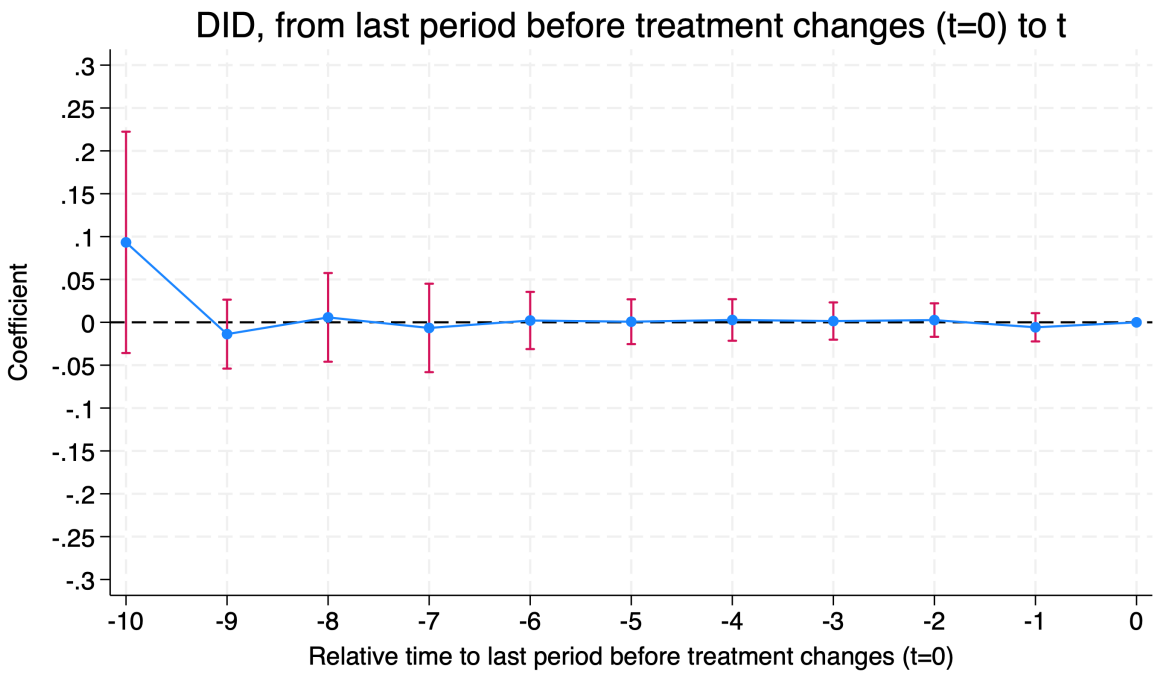
Notes: Unconditional estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. The model includes state and time fixed effects. Standard errors are clustered at the state level.

Figure A.6: Robustness Check: Impact on Riots (per 1,000 people) after excluding certain states from the sample



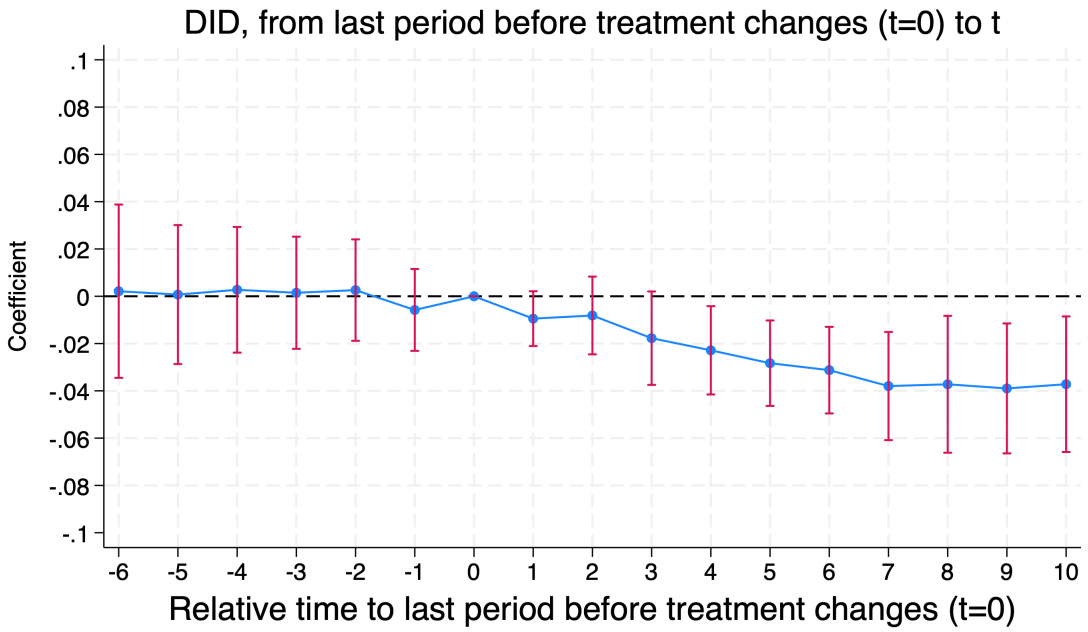
Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors are clustered at the state level.

Figure A.7: Robustness Check: Placebo (pre-trend) estimates for 10 periods prior to the law



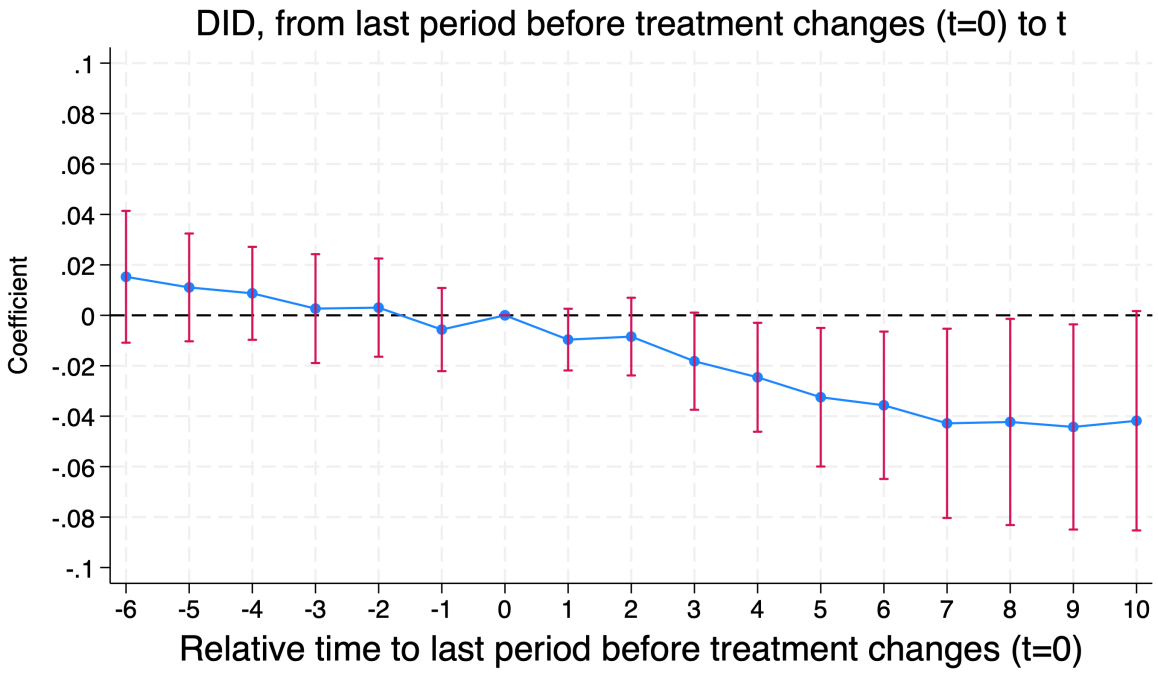
Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors are clustered at the state level.

Figure A.8: Robustness Check: Impact on Riots (per 1,000 people) using bootstrapped standard errors (1000 iterations)



Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Bootstrapped standard errors are clustered at the state level.

Figure A.9: Robustness Check: Impact on Riots (per 1,000 people) after excluding Rajasthan post 2005



Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors are clustered at the state level.

Table A.6: Robustness Check: Impact of RCL after excluding certain states

	Jammu & Kashmir	Old Union Territories	Small North - Eastern States	Gujarat
Average Total Effect (ATE)	-0.030*	-0.039*	-0.033*	-0.033*
	(0.017)	(0.021)	(0.018)	(0.019)
Average Time over which Treatment Effect Accumulates	5.61	6.07	5.61	5.83
Switch × Periods	45	39	45	36
N	310	291	291	313
P-value (F test for joint sig. of placebos)	0.536	0.415	0.714	0.716
P-value (Sup-t test for placebos)	0.650	0.897	0.939	0.248

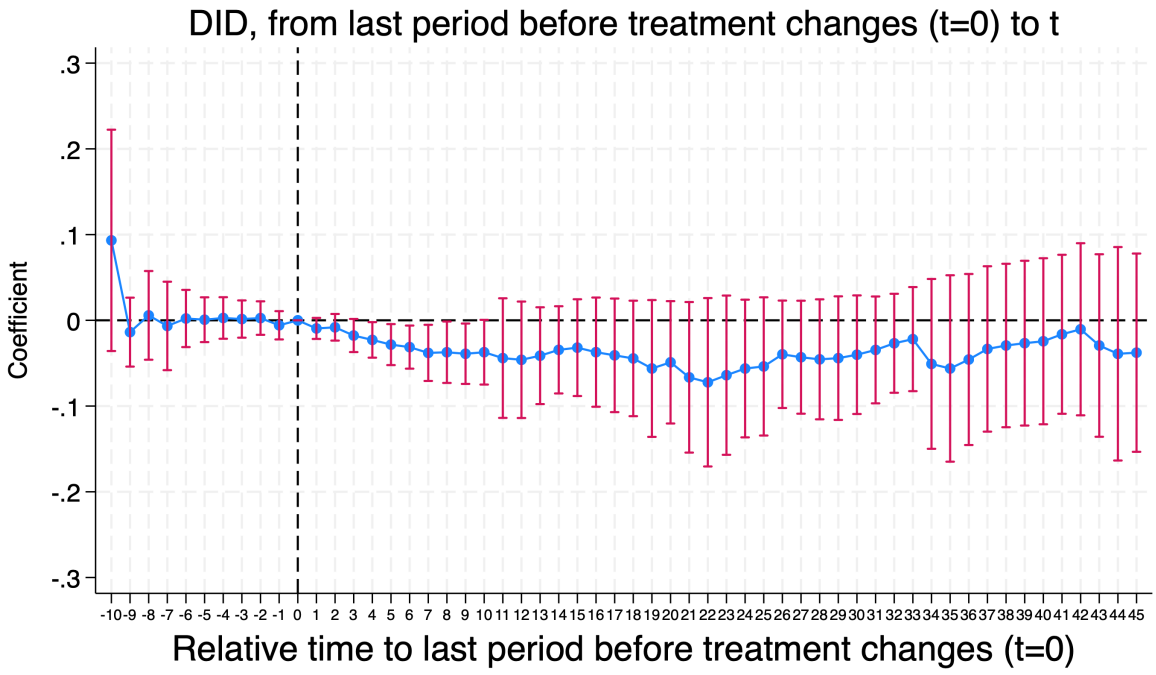
Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The table reports the average total effects. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.7: Robustness Check: Impact on Riots (per 1,000 people) using non-linear models

Method→	Poisson	Tobit
Outcome→	Riots	Riots (per 1,000 people)
	(1)	(2)
RCL	-0.091	-0.0131
	(0.119)	(0.014)
$Pseudo_R^2$	0.81	-1.61
Observations	970	970
Controls	×	×
State FE	×	×
Year FE	×	×
IRR	0.912	-
	(0.109)	
Exposure	Population	-

Notes: Estimates calculated using non-linear methods. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses.

Figure A.10: Robustness Check: Impact on Riots (per 1,000 people) for all periods



Notes: Estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The figure shows the dynamic effects for a given period and the corresponding 90% confidence intervals. Period 0 refers to the last pre-treatment period. The model includes controls for state level factors such as proportion of urban population, literates (per 1,000 people), females (per 1000 males), per capita real state net domestic product, percentage of scheduled castes, percentage of scheduled tribes, percentage of christians, percentage of muslims, year of state elections dummy, presence of Congress (INC) in the government, presence of BJS / JP / BJP in the government, total police strength (per 1,000 people) as well as state and time fixed effects. Standard errors are clustered at the state level.

Table A.8: Impact on share of different religions

	Hindus (1)	Muslims (2)	Christians (3)
t+1	0.413	-0.459	-0.390
	(0.417)	(0.287)	(0.446)
Switchers	5	5	5
N	30	30	30
Average Total Effect	0.079	-0.088	-0.075
	(0.080)	(0.055)	(0.086)
Switch × Periods	5	5	5
N	30	30	30

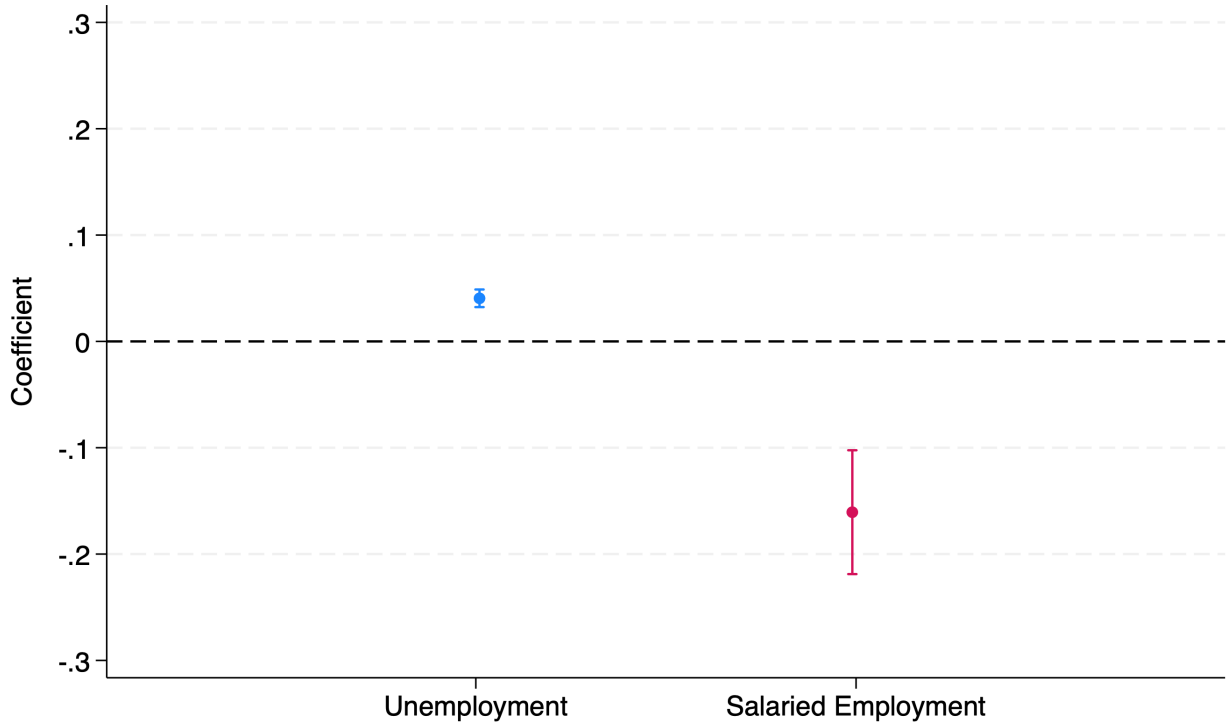
Notes: Unconditional estimates using De Chaisemartin and d'Haultfoeuille (2024) DID estimator implemented using *did_multiplegt_dyn* command in Stata. The table shows the dynamic effects for a given period and the average total effect. The model includes state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.9: Results for Unemployment using NSS

	All (1)	Hindus (2)	Muslims (3)	Christians (4)
RCL	-0.011***	-0.012***	-0.006*	0.040***
	(0.003)	(0.003)	(0.003)	(0.004)
R^2	0.06	0.06	0.06	0.11
Observations	938,191	733,183	116,421	45,079
Individual controls	×	×	×	×
State FE	×	×	×	×
Year FE	×	×	×	×

Notes: The estimates compare outcomes for individuals in Gujarat with those in other untreated states in India before and after the passage of the religious conversion law in the state in 2003, using data from the National Sample Survey. The model includes individual-level controls for age, sex, marital status, literacy, education, rural-urban residence, number of children, and family size, as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Figure A.11: Impact of RCL on labour market outcomes of Christians



Notes: The estimates compare outcomes for Christians in Gujarat with those in other untreated states in India before and after the passage of the religious conversion law in the state in 2003, using data from the National Sample Survey. The model includes individual-level controls for age, sex, marital status, literacy, education, rural-urban residence, number of children, and family size, as well as state and time fixed effects. The figure shows 90% confidence interval, and standard errors are clustered at the state level.

Table A.10: Results for Salaried Employment using NSS

	All (1)	Hindus (2)	Muslims (3)	Christians (4)
RCL	-0.001 (0.013)	0.006 (0.014)	-0.008 (0.018)	-0.160*** (0.033)
R^2	0.12	0.12	0.08	0.27
Observations	884,237	695,244	109,302	41,567
Individual controls	×	×	×	×
State FE	×	×	×	×
Year FE	×	×	×	×

Notes: The estimates compare outcomes for individuals in Gujarat with those in other untreated states in India before and after the passage of the religious conversion law in the state in 2003, using data from the National Sample Survey. The model includes individual-level controls for age, sex, marital status, literacy, education, rural-urban residence, number of children, and family size, as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.11: Freedom of choice and Control

	All (1)	Hindus (2)	Muslims (3)	Christians (4)
RCL	-0.289* (0.150)	-0.150 (0.168)	-1.574*** (0.450)	-2.838*** (0.409)
R^2	0.09	0.07	0.17	0.14
Observations	9,594	7,667	988	281
Individual controls	×	×	×	×
State FE	×	×	×	×
Year FE	×	×	×	×

Notes: The estimates compare outcomes for individuals in Gujarat with those in other untreated states in India before and after the passage of the religious conversion law in the state in 2003, using data from the World Values Survey. The model includes individual-level controls for age, sex, marital status, and education, as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.12: Not like to have neighbours of a different religion

	All (1)	Hindus (2)	Muslims (3)	Christians (4)
RCL	-0.062 (0.077)	-0.107 (0.079)	0.205 (0.136)	0.182** (0.059)
R^2	0.09	0.09	0.12	0.35
Observations	6,498	5,055	745	165
Individual controls	×	×	×	×
State FE	×	×	×	×
Year FE	×	×	×	×

Notes: The estimates compare outcomes for individuals in Gujarat with those in other untreated states in India before and after the passage of the religious conversion law in the state in 2003, using data from the World Values Survey. The model includes individual-level controls for age, sex, marital status, and education, as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.13: Importance of God in life

	All (1)	Hindus (2)	Muslims (3)	Christians (4)
RCL	0.848*** (0.233)	0.984*** (0.219)	-0.410 (0.475)	1.090* (0.567)
R^2	0.14	0.14	0.12	0.19
Observations	10,197	8,146	1,077	299
Individual controls	×	×	×	×
State FE	×	×	×	×
Year FE	×	×	×	×

Notes: The estimates compare outcomes for individuals in Gujarat with those in other untreated states in India before and after the passage of the religious conversion law in the state in 2003, using data from the World Values Survey. The model includes individual-level controls for age, sex, marital status, and education, as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.14: Frequency of Attending Religious Services

	All (1)	Hindus (2)	Muslims (3)	Christians (4)
RCL	0.618*** (0.148)	0.675*** (0.157)	0.906*** (0.192)	1.160** (0.396)
R^2	0.07	0.07	0.11	0.25
Observations	10,158	8,098	1,079	298
Individual controls	×	×	×	×
State FE	×	×	×	×
Year FE	×	×	×	×

Notes: The estimates compare outcomes for individuals in Gujarat with those in other untreated states in India before and after the passage of the religious conversion law in the state in 2003, using data from the World Values Survey. The model includes individual-level controls for age, sex, marital status, and education, as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.15: Important Child Quality: Religious Faith

	All (1)	Hindus (2)	Muslims (3)	Christians (4)
RCL	0.123*** (0.032)	0.107** (0.038)	0.175*** (0.049)	0.665*** (0.125)
R^2	0.25	0.26	0.17	0.18
Observations	10,339	8,253	1,087	301
Individual controls	×	×	×	×
State FE	×	×	×	×
Year FE	×	×	×	×

Notes: The estimates compare outcomes for individuals in Gujarat with those in other untreated states in India before and after the passage of the religious conversion law in the state in 2003, using data from the World Values Survey. The model includes individual-level controls for age, sex, marital status, and education, as well as state and time fixed effects. Standard errors clustered at the state level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1