When Credit Lines Open Fault Lines: Economic and Political Effects of Bank Lending

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Abstract

This paper examines how welfare policies, in states with fractured societies, result in political and social changes, using a policy intervention in India. The policy directed commercial banks to increase lending to marginalized religious minorities, such as Muslims, in "minority concentration" districts. Using a regression discontinuity design, we identify improvements in access to credit and economic well-being of Muslims households. The policy also shifted political outcomes: state elections were more likely to have Muslim candidates who also received higher vote shares and exhibited increased likelihood of winning. These changes were likely driven by Muslims overcoming collective action problems: policy districts saw a rise in exclusively Muslim Self-Help-Groups, a strengthening of Muslim identity, and greater belief in the power of collective action to protect minority rights. However, the improved economic and political standing of Muslims triggered a blow-back effect, evidenced by a rise in communal riots, primarily in post-election periods.

JEL Classification: D63, D72, D74, G18, G51, H3,P16

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1 Introduction

States, especially those characterized by diverse and fractured societies, face a fundamental policy dilemma. On one hand, they confront strong normative and economic imperatives to design and implement inclusive policies that promote the upliftment of historically marginalized groups and foster holistic economic growth (Marshall, 1950; Benabou, 1996). On the other hand, such redistributive efforts, by perceiving to favor one group at the expense of others, can evoke political resistance – especially in societies marked by deep racial, religious, or ethnic faultlines – and render such policies to be politically costly, or wholly infeasible (Alesina et al., 2001). Even when welfare enhancing, such policies can generate negative externalities in the form of rising social tensions or increased conflicts (Mitra and Ray, 2014). More fundamentally, the impact of welfare policies can extend beyond immediate economic outcomes. By altering political and social incentives, they can serve as catalysts in reshaping the political landscape, leading to unintended consequences. This makes it necessary to study the political implications of welfare policies and understanding how economic changes interact with existing power structures to either sustain or undermine social progress (Acemoglu and Robinson, 2006).

We examine the structural tension between inclusive economic development and fragile social harmony through the lens of a large-scale welfare policy in the world's most populous democracy – India – targeted towards improving the socioeconomic status of religious minorities by expanding their access to bank credit.[1] By analyzing the consequences of this policy for India's minority Muslim citizens, our paper sheds light on the economic benefits and political risks associated with targeted welfare interventions in a context marked by deep social and religious fissures.

India presents an ideal setting to examine the economic and political implications of credit policies designed to promote the welfare of marginalized groups within a fractured society. The country's formal banking system is extensive, but prone to discriminatory practices (Fisman

[1] Access to credit, by enabling households to smooth consumption, invest in human capital, pursue entrepreneurial activities, and improve labor market outcomes is a well-established lever for poverty alleviation and development (Kaboski and Townsend, 2012; Augsburb et al., 2015; Buera et al., 2020; Cramer, 2021; Breza and Kinnan, 2021; Aydin, 2022). Yet, credit access remains deeply unequal, with significant disparities along the lines of religion, caste, and gender (Blanchflower et al., 2003; Fisman et al., 2020; Brock and de Haas, 2023).

et al., 2020). India's 200 million Muslim population forms the largest religious minority in the world, but remains economically disadvantaged (Sachar, 2006; Maizland, 2020), politically under-represented (Bhalotra et al., 2014; Allie, 2024), and vulnerable to religious violence (Iyer, 2018).

To address widening socio-economic disparities along religious lines, the federal government in 2006 unveiled the Prime Minister's 15 Point Programme for Minorities – a set of policy initiatives designed to enhance the socio-economic conditions for religious minorities: namely, Muslim, Christian, Sikh, Buddhist and Parsi groups. One aspect of this program underscored "equitable" access to bank credit for minorities. Subsequently, the Reserve Bank of India (RBI)[2] in 2007 directed commercial banks to expand credit access for religious minorities in "minority concentration" districts.[3]

For causal identification, we leverage the administrative criteria used by the RBI to classify districts as "minority-concentration": namely, districts where the population share of religious minorities exceeded 25%, as per the 2001 Population Census (RBI, 2007). The use of an arbitrary threshold generates plausibly exogenous variation in policy exposure in the spirit of a regression discontinuity (RD) design, allowing us to compare outcomes across districts barely above the threshold (treatment), vis-a-vis those just below (control) (Lee and Lemieux, 2010). As treatment assignment was undertaken using data predating the policy by six years, it is unlikely that districts could have manipulated their minority shares to influence inclusion. Formally, the McCrary density test (McCrary, 2008) fails to reject the null of no sorting around the discontinuity threshold. Based on the first list of minority concentration districts issued in 2007, we confirm that all districts satisfying the treatment assignment condition were assigned to treatment. However, within 3 months, an additional 18 districts with minority shares less than 0.25 were added to this list. We address this non-compliance in treatment assignment using a fuzzy regression discontinuity design as our main specification. We use the fuzzy RD design to confirm balance on key pre-treatment banking, economic, political and crime outcomes,

[2] The Reserve Bank of India is India's central bank, which also serves as both the regulator, and supervisor for India's commercial banks. This allows it to frame nationwide credit policies for commercial banks.

[3] Districts form the third tier of administration in India, after the federal and state.

suggesting that districts around the cut off were observationally equivalent prior to the policy announcement.

We begin by establishing the first-order impacts of the directed credit credit policy and compare minority households' access to bank credit across treatment and control districts within a narrow neighborhood around the discontinuity threshold. We use nationally representative data from the All India Debt and Investment Survey (AIDIS) conducted in 2019, allowing us to estimate the long-run effects of the directed credit policy on household bank borrowings. Our preferred sample is a fixed set of 95 districts within a 0.072 bandwidth around the policy threshold, selected using the MSERD optimal bandwidth procedure recommended by Calonico et al. (2020). A single bandwidth is preferred to ensure a consistent sample of districts are used as we explore the impacts of the policy across a diverse array of economic, political and conflict outcomes.

Comparing Muslim households in minority concentration districts to observationally equivalent Muslim households in non-minority concentration areas within our preferred bandwidth, the fuzzy RD design identifies a statistically significant 8 percentage point (ppt.) increase in bank credit access.[4] This translates to an additional 3,000 Muslim households per district receiving a bank loan following this policy. Along the intensive margin, we identify an unconditional INR 6,000 increase in per capita bank borrowings, equivalent to 20 percent of Muslim households' annual per capita consumption. The aggregate expansion in bank credit to religious minorities equals 1.2 percent of overall personal loans issued by banks in our sample districts. Greater access to bank credit translated into broader improvements in household well-being: Muslim households in treated districts reported 27 (32) percent higher per capita monthly consumption (household assets) compared to their control counterparts.

We consider three potential mechanisms driving the observed increase in minorities' access to bank credit. First, we offer evidence of banks leveraging existing institutional arrangements to expand credit in environments where information frictions are prevalent. Over the past two decades, India has witnessed a steady rise in loans issued through bank-linked self-help groups

[4] We verify robustness to alternate bandwidths between 0.04 to 0.10, and also confirm the results to not be driven by any single state. Overall findings are unchanged when the sample is extended to non-Muslim religious minorities.

(SHGs), whereby banks lend directly to registered SHGs, which subsequently decide on the internal allocation of credit. While loans obtained directly from commercial banks too increased, we identify that 60 (9) percent of the extensive (intensive) margin increase in bank loans can be attributed to lending through bank-linked SHGs. Second, as minorities possessed fewer pledgeable assets, we investigate whether banks responded by easing collateral requirements. We however find no change in minority borrowers' likelihood of having an unsecured bank loan in treated districts. Third, we test whether banks adjusted interest rates to facilitate credit flow to minorities. We find no evidence of a lowering of interest rates for minority borrowers, suggesting that the expansion in credit access was not driven by pricing changes.

Having established that the credit policy increased Muslims' access to bank credit and improved economic well-being, we explore whether economic empowerment translated to political empowerment and greater security from religious persecution. This is a critical question when considering diverse societies where economic gains for a specific group in equilibrium may not translate to political gains (Acemoglu, 2010; Acemoglu and Robinson, 2013), especially when the beneficiaries are as disadvantaged as Indian Muslims (Sachar, 2006; Mitra and Ray, 2014). Resultantly, we study whether the minority credit policy affected the political and social lives of Muslim minorities through the lens of Muslim participation in state electoral politics and the incidence of Hindu-Muslim conflicts.

Using data compiled by the Trivedi Center for Political Data (TCPD) and extending the fuzzy RD framework to Indian state elections conducted between 2008 and 2019, we identify sizable increases in both the participation, and electoral support for Muslim contestants. The likelihood of a Muslim candidate to contest elections increased by 24 ppt. in minority concentration areas, along with a 3.4 ppt. unconditional (2.8 ppt conditional) increase in the share of votes accruing to these candidates. The likelihood of Muslim candidates contesting on behalf of select non-right-wing, non-majoritarian parties was also significantly higher; indeed, the rise in vote shares for Muslim contestants emanated from candidates contesting on behalf of these select non-majoritarian parties.[5] Rising citizen support translated to a 9 ppt. increase in

[5] We consider 5 major parties which have been vocally opposed to recent majoritarian tendencies in India as

Muslim candidates' likelihood of winning elections, deepening minority representation in state legislatures. These results assuage concerns that economic empowerment might have increased political participation from minority citizens who contested elections solely to enhance their personal visibility. Overall, our findings point to minority political empowerment in districts eligible for the directed credit policy: an uptick in the supply of candidates who received greater electoral support, and had higher winnability.

We consider two pathways through which improved access to formal credit could have facilitated minority political empowerment. First, as the extensive margin expansion in minority bank credit access was facilitated through bank-linked SHG loans, greater interactions through SHG groups could have promoted collective action among Muslim voters, an overall strengthening of minority identity and improved coordination while voting for preferred candidates. We provide two pieces of evidence consistent with this explanation. First, using data from the National Rural Livelihood Mission (NRLM) on membership in bank-registered SHGs, we identify an additional 2,000 Muslim members across SHGs in the average policy district in any given year. Critically, the increase in Muslim SHG membership is driven by SHGs with exclusively Muslim members.

If coordination costs were higher for religious minorities, increased social interactions through bank-registered SHGs (Desai and Joshi, 2014) could have strengthened the salience of Muslim identity (Atkin et al., 2021) and citizens' belief in the efficacy of collective action and political organization (Sen and Sharan, 2024). The second piece of evidence documents the presence of such beliefs amongst Muslim citizens. Using data from *Samaj* Surveys (2017 & 2019) conducted by the Centre for Monitoring the Indian Economy (CMIE), we find Muslim respondents in policy districts were significantly more likely to state religion as a critical part of their identity. Moreover, Muslim respondents were also more likely to believe that collective action along religious lines would evoke favorable policy responses from the government towards their community. Finally,

non-majoritarian parties, these include: the Indian National Congress (INC), the Samajwadi Party (SP), the Rashtriya Janata Dal (RJD), the All India Trinamool Congress (AITC), and the Nationalist Congress Party (NCP). For the year 2015, the Janata Dal (United) (JD(U)) is also considered in this grouping as it was part of an electoral alliance with the RJD and the INC.

Muslim citizens also expressed greater willingness to engage in protests to advocate for their rights. We complement the survey responses with suggestive evidence of Muslim citizens acting on their beliefs. While there is no disaggregation of voter turnout by religious denomination, we identify a significant increase in overall turnout in minority concentration districts. Higher turnout was also driven by constituencies with a relatively high share of Muslim voters, consistent with improved citizen coordination during elections.

An alternate channel through which the credit policy could have affected electoral politics would be changes to candidate supply. Access to bank credit can generate a wealth effect that alters the supply of Muslim candidates in minority concentration areas by allowing them to overcome entry costs. Wealthier candidates can also run a more effective campaign, leading to electoral gains. Using data on candidate affidavits, we find some suggestive evidence that the supply of candidates, particularly those running on non-majoritarian party platforms, was altered. However, the results are not strong enough to provide definite evidence for this explanation.

Noting India's long history of Hindu-Muslim (majority-minority) violence (Varshney, 2003), we conclude our empirical analysis by determining whether the changes in economic and political realms affected social harmony. Inter-group conflict can be shaped by both economic and political empowerment: economic prosperity can increase the opportunity cost of violence, while political empowerment can improve communities' influence over law enforcement authorities, deterring violent actors in opposing groups. Alternately, Mitra and Ray (2014) predict the strategic use of violence by majority groups to reassert dominance when faced with growing minority prosperity.

We focus on Hindu-Muslim riots as a measure of communal conflict. We complement administrative data from the National Crime Records Bureau (NCRB) with newspaper reports of violence, compiled by the Armed Conflict Location and Event Data (ACLED) (Raleigh et al., 2010). Both datasets identify a significant increase in religious conflicts in policy-affected districts. Other crime heads such as non-religious riots or murders remained comparable across treated and control areas, negating a secular rise in criminal activity in minority concentration areas as a plausible alternate explanation.

We conclude by examining the prime driver of religious violence in minority concentration areas. We disentangle whether riots stemmed from minority political empowerment or in response to improvements in minorities' economic status. We contend that if animosity towards minorities emerged in response to their economic empowerment, the increase in violence would be unrelated to the timing of elections. However, if the increase in violence is observed only prior to the elections, it is likely to be driven by an effort to suppress minority turnout in anticipation of electoral gains. Alternately, if violence occurs only after the elections, it indicates an electoral backlash to the political empowerment of minorities. Disaggregating the ACLED data into post (12 months after) and pre-electoral periods (12 months before), we identify a significant increase in religious riots only after the elections. This suggests that the violence was strategically timed as a mechanism to punish minorities for their opposition to majoritarian politics.

Our paper contributes to two broad strands of literature. First, we show how a welfare policy designed to improve access to formal credit for marginalized groups can improve economic well-being and enhance political empowerment through stronger within-group coordination. While prior research has separately established credit access to improve household welfare (Breza and Kinnan, 2021) and institutional mechanisms – such as electoral quotas – to bolster political representation (Pande, 2003; Chattopadhyay and Duflo, 2004; Besley et al., 2017; Gulzar et al., 2021), this paper, to the best of our knowledge, is the first to demonstrate that economic empowerment can translate into political agency for minority groups in a fractured society. Consequently, our paper links interventions for economic development to changes in political equilibrium (Acemoglu, 2010).

Furthermore, we document that group-specific economic and political gains may provoke backlash through rising religious violence targeting the beneficiary group. In the process, we contribute to a nascent literature on the political and social costs of redistributive policies aimed at marginalized communities (Chyn et al., 2024; Avenancio-León et al., 2024; Choi et al., 2024). While previous work on inter-group conflicts has linked violence to efforts undertaken by the majority to preserve economic hierarchies (Mitra and Ray, 2014), or to suppress minority political participation (Wilkinson, 2006; Condra et al., 2018; Iyer and Shrivastava, 2018), we offer an

additional mechanism: that political backlash may emerge as a form of punishment following a weakening of majoritarian political dominance. These findings speak to a central dilemma in state capacity: how efforts to correct market failures and promote inclusive development can generate countervailing political risks in polarized and fragmented societies (Acemoglu and Robinson, 2013).

Second, this paper provides evidence of the effectiveness of an at-scale implementation of affirmative action in formal credit markets. While affirmative action has widely been studied in labor (Leonard, 1990; Holzer and Neumark, 2000; Miller, 2017), housing (Chetty et al., 2016), and political markets (Pande, 2003; Chattopadhyay and Duflo, 2004; Jensenius, 2015; Bhavnani, 2017; Gulzar et al., 2021), little is known about its implementation within the banking industry. We examine a nationwide directive issued by the Indian government to expand credit access for religious minorities through the banking system – a context in which discrimination against religious minorities is well-documented (Fisman et al., 2020; Blanchflower et al., 2003; Brock and de Haas, 2023).

We also engage with critiques of affirmative action that focus on unintended consequences, such as misaligned targeting (Barrow et al., 2020) or the crowding out of non-minority beneficiaries (Arcidiacono et al., 2022). Our findings indicate that credit to religious minorities was unaccompanied by a worsening in loan performance; neither do we find strong evidence of a crowd out of non-minority borrowers. Instead, banks partnered with self-help groups (SHGs) to mitigate information asymmetries and improved outreach to creditworthy minority borrowers. Our findings underscore the potential for institutional intermediation to enhance the effectiveness and precision of financial inclusion policies.

The remainder of our paper is organized as follows: Section 2 describes the policy intervention of interest; Section 3 discusses the data used for the empirical analysis and presents some descriptive trends; Section 4 presents the empirical strategy for causal identification; Section 5 presents our key findings on access to credit; and Section 6 presents out results on political outcomes.

2 Background

This section offers details about the Prime Minister's 15 Point Programme for Welfare of Minorities.

2.1 Prime Minister's 15 Point Programme for Welfare of Minorities

The Prime Minister's (PM) 15 Point Programme for the Welfare of Minority Communities was a set of policies outlined by India's federal government, aimed at improving the socio-economic conditions of India's religious minorities – namely Muslims, Christians, Sikhs, Buddhists and Parsis.[6] Collectively, citizens from these religious denominations accounted for 19 percent of India's population in 2001. Muslims comprised the largest group, accounting for 13 percent or 138 million individuals, making it the largest religious minority in the world. The policies covered the realms of education, employment, infant health, housing, sanitation, access to credit, and protection from religious violence. The overarching policies were framed by the federal government and implemented through various public agencies.

Access to credit under the PM's minority welfare programme aimed at ensuring the "smooth flow of bank credit to minority communities" from commercial banks (RBI, 2007). The central bank – the Reserve Bank of India (RBI) – was tasked with framing the regulations for minority credit allocation, and also responsible for overall monitoring. The RBI in 2007 issued a set of guidelines, notifying that the federal government had classified 103 districts (out of 593 districts) as "minority concentration" (RBI, 2007).[7] These districts were deemed such as the share of religious minorities (as per the 2001 population census) exceeded 25% of the district's population.[8] Commercial banks were instructed to monitor credit flow to minority borrowers in these 103 districts. A few months after the initial notification, the federal government added

[6] Later, Jains too were included under this policy as religious minorities.

[7] Districts from 5 states and 1 union territory – namely Jammu and Kashmir, Punjab, Meghalaya, Mizoram, Nagaland and Lakshadweep were excluded from this policy as religious minorities in these areas formed a numerical majority.

[8] To the best of our understanding, no other policy within the 15-point minority welfare programme used the 25% minority population share threshold.

18 districts to this list, bringing the number of minority concentration districts to 121. No subsequent additions or deletions have been made to this set.

2.2 Incentive for the Banks

The primary incentive for banks to comply with the directed credit policy is that lending to minority borrowers allows banks to meet key regulatory targets. Banks are mandated to direct 12 percent of their loan portfolio towards "weaker sections" – namely small and marginal farmers, village and cottage industries, and loans issued to borrowers hailing from historically marginalized *Dalit* (Scheduled Castes or SCs) and *Adivasi* (Scheduled Tribes or STs) communities. With the advent of targeted lending to religious minorities, the RBI expanded the definition of "weaker section" to include borrowers from religious minorities. While no explicit targets were assigned, the RBI directed banks to ensure that minority borrowers were "adequately represented" within weaker sections RBI (2007, 2021).

3 Data

We describe here the data on household balance-sheets, electoral outcomes, and religious conflicts. Additional datasets are described as discussed in the empirical analysis.

3.1 All India Debt-Investment Survey

The nationally representative All-India Debt and Investment Survey (AIDIS) is conducted decennially by the National Sample Survey Organisation (NSS) as repeated cross-sections.[9] Sampling over 100,000 households, it offers extensive information on household borrowings, savings and ownership of select assets. Location identifiers in the form of districts are also provided, in addition to the household's religion. For each outstanding loan at the time of survey, the AIDIS details the initial amount borrowed, the loan origination year, amount outstanding,

[9] The AIDIS was conducted in the years 1992, 2003, 2013 and 2019.

loan securitization, the source of credit, and interest rate. Repayment information over the past 6 months is also provided. We use the 2019 AIDIS survey to estimate long-term treatment effects of the directed credit policy on household borrowing. The 2003 AIDIS survey is used to verify balance for key pre-treatment outcomes of interest. Appendix Tables C1.A-C1.D present select summary statistics from the 2019 AIDIS. In all, 42 (23) percent of households had some outstanding (bank) loan at the time of the survey. While 17 percent of households borrowed directly from commercial banks, 7 percent received loans from a bank-linked SHG.

3.1.1 Barriers to Credit Access for Muslims: Pre-Treatment Descriptives

Using the 2003 AIDIS, Appendix A presents descriptive evidence that among religious minorities, Muslim households faced substantial barriers to access bank credit. While overall credit take-up was comparable to other households, Muslim households had a much lower likelihood of having a bank loan; conditional on having a bank loan, loan amounts for Muslim households were also markedly lower. Bank interest rates, however, were comparable across communities, indicating no higher credit risk for Muslims. This makes the lack of credit demand an unlikley explanation for low bank credit to Muslims.

An Oaxaca-Blinder decomposition in Appendix section A.2 offers suggestive evidence of discrimination, particularly along the extensive margin. The top panel of Appendix Figure A6 indicates that 40 percent of the extensive margin difference in bank credit for Muslim households – relative to Hindu households – could be explained by household observables.

Taking cognizance of the descriptive evidence documenting the limited participation of Muslims in formal credit markets and the fact that they account for 80 percent of India's religious minorities, we restrict ourselves to studying how the credit policy affected Muslim households. For the remainder of the paper, unless explicitly stated, the term "minorities" is used interchangeably with Muslims.

3.2 Election Data

To assess the electoral consequences of the directed credit policy, we use data from the Trivedi Center for Political Data (TCPD), which compiles data on elections to India's state legislative assemblies. This includes candidate names, party affiliation, and votes received, along with the number of registered voters, and voter turnout. Our primary sample comprises of elections conducted between 2008 and 2019. We append to this information from candidate affidavits pertaining to their incomes and assets. We also use the share of Muslim voters in each electoral constituency, estimated by Gulzar et al. (2024).

To identify whether the directed credit policy affected the political empowerment of religious minorities, we compare the participation of Muslim politicians in state legislative assembly elections, and the vote share received by them. Since the administrative data doesn't identify candidate religion, we use a machine learning algorithm developed in Chaturvedi and Chaturvedi (2024), and validated by Allie (2024), to ascertain politicians' religious identity.[10]

Between 2008 and 2019, Appendix Table D1 shows that over half of the elections in our sample had no Muslim contestant. Out of 12 candidates in the average election to the state legislative assembly, only 1 was from the minority religion. In 13 percent of electoral contests, there was a Muslim candidate representing a non-majoritarian party – one of 5 parties which in the past decade has regularly highlighted injustices perpetrated against India's religious minorities.[11] Minority candidates received 7 percent of the votes cast and emerged victorious in 6 percent of elections. Akin to evidence shown by Farooqui (2020), these descriptive figures also confirms the under-representation of Muslims across India's legislative bodies. Over this period, the majoritarian right-wing Hindu nationalist Bharatiya Janata Party (BJP) was the most successful political formation, winning a third of the electoral contests and capturing a fourth of the popular vote.

[10] This algorithm has been specifically developed and validated for India.

[11] These are the Indian National Congress, Samajwadi Party, Rashtriya Janata Dal, Nationalist Congress Party and the All India Trinamool Congress. As the Janata Dal (United) contested the 2015 Bihar legislative assembly elections in an alliance with the Rashtriya Janata Dal and the Indian National Congress, we also classify this party as non-majoritarian for this specific state-electoral cycle.

3.3 Conflict Data

To evaluate how the directed credit policy affected social harmony and religious conflict, we rely on both administrative data from the National Crime Records Bureau (NCRB), as well as the Armed Conflict Location and Event Dataset (ACLED) (Raleigh et al., 2010). The NCRB offers information on district-level crimes in each year, based on first information reports (FIR) filed at police stations.[12] Alongside traditional offenses such as murder, property crimes, rapes and abductions, the NCRB also captures riots, and since 2014, separately reports religious riots.

A variety of factors can make the administrative NCRB data prone to reporting and recording biases. Relevant to our study, majoritarian tendencies have sharply increased in India since 2014 (Jaffrelot, 2021), which might lead to an under-reporting of religious violence, especially if the victims are religious minorities and the police are susceptible to political pressures. Resultantly, we supplant the administrative NCRB data with the ACLED database, which covers major conflict events in India from 2016 on the basis of newspaper reports. We map events to districts using the conflict location information in the ACLED, and also classify events as riots involving majority (Hindu) and minority (Muslim) groups, based on the event description and actors involved.

Appendix Table D2 shows that between 2014 and 2019, the administrative NCRB recorded 2 incidences of religious violence in the average district. Religious riots formed a small fraction of overall rioting: during this period, the average district witnessed 128 incidences of rioting. The incidence of Hindu-Muslim (majority-minority) riots reported in the ACLED (Appendix Table D2) is much lower than the NCRB data. This is possibly due to two factors: first, the NCRB data is based on first information reports filed at police stations while the ACLED relies on newspaper coverage by major English media outlets. As a result, the ACLED might omit incidents of religious conflicts in smaller urban or rural locations, or low-intensity conflicts which are unreported in the English media. Second, we focus exclusively on Hindu-Muslim riots in the ACLED, while the NCRB considers all religious conflicts, which can also include

[12] Filing a FIR forms the basis for beginning any preliminary criminal investigation.

inter-minority conflicts. Similar to the administrative data, religious riots in the ACLED forms a small proportion of overall rioting.

4 Empirical Strategy

The use of an arbitrary threshold – fraction of religious minorities exceeding 25% of the district's population – for treatment assignment lends itself to causal estimation using a regression discontinuity design (Lee and Lemieux, 2010). The running variable is defined as:

$$Runvar_{ds} = ShMinority_{ds} - 0.25 \tag{1}$$

$ShMinority_{ds}$ is the population share of religious minorities in district d, located in state s. Figure C2 shows the distribution of $ShMinority_{ds}$, with the broken vertical line representing the 0.25 threshold. Using $Runvar_{ds}$ from equation (1), the district-level treatment indicator – $Treat_{ds}$ – equals 1 if $Runvar_{ds} > 0$ and 0 otherwise.

Districts were assigned to treatment in 2007, using data from the 2001 population Census. This makes it implausible for districts or states to anticipate the policy and strategically manipulate their minority population shares to lie on either side of the treatment threshold. Formally, Figure C1 presents the McCrary test (McCrary, 2008), and we are unable to reject the null of a discontinuity in the running variable at the threshold of 0.25. This allays concerns of any strategic sorting of districts around the discontinuity threshold. Based on the first list of minority concentration districts issued in 2007, we confirm that all districts which satisfied the treatment assignment condition were assigned to treatment. However, as discussed in Section 2, an additional 18 districts with minority shares less than 0.25 were later included as minority concentration districts. This non-compliance in treatment assignment is addressed using a fuzzy regression discontinuity design, where the running variable serves as an instrument for treatment assignment. The fuzzy RD is estimated using the following two equations:

$$Treat_{hds} = \alpha_s + \tau \mathbb{1}(Runvar_{ds} > 0) + \gamma f(Runvar_{ds}) + \phi \mathbf{X}_{hds} + \mu_{hds} \quad (2)$$

$$Y_{hds} = \alpha_s + \beta \widehat{Treat}_{ds} + \theta f(Runvar_{ds}) + \delta \mathbf{X}_{hds} + \epsilon_{hds} \quad (3)$$

The unit of observation in the above equations is the household h, located in district d of state s. *Treat* is a dummy equaling 1 if the district is classified as a minority concentration district. Actual treatment assignment is instrumented with treatment assignment defined according to the policy rule ($Runvar_{ds} > 0$). As recommended by Lee and Lemieux (2010) and Calonico et al. (2020), we include a linear polynomial – $f(.)$ – in the running variable and its interaction with the treatment indicator. The coefficient of interest is β, comparing household outcomes across treated (minority concentration) and control districts (non-minority concentration).

All specifications include state-region fixed effects (α), and binary indicators for rural location and marginalized caste groups.[13] This restricts our comparison of household outcomes to households located in geographically homegeneous regions within a state, with the cross-sectional variation arising from a household's location in a minority concentration district. We use a triangular kernel, assigning greater weight to observations located near the discontinuity threshold, as well as survey weights provided in the AIDIS data.[14] Standard errors are clustered by district – the level at which the treatment varies.

Conditional on districts being unable to strategically manipulate assignment to treatment, non-minority concentration districts serve as a valid counterfactual to minority concentration districts within a narrow window of the discontinuity threshold. Our main results uses a fixed sample of 95 districts located within a bandwidth of .072 around the discontinuity threshold.[15] The choice of bandwidth is obtained from data-driven optimal bandwidth selection procedures recommended by Calonico et al. (2020) and applied to the two main outcomes for bank credit

[13] A state-region is a NSS-defined classification of geographically contiguous districts located within the same state (Gadenne et al., 2024).

[14] These weights make the AIDIS data nationally representative.

[15] Out of these, 45 are minority concentration (treated), and the rest non-minority concentration (control).

access.

4.1 Pre-Treatment Balance

A causal interpretation of the RD coefficients is subject to the assumption that pre-treatment covariates are continuous in the running variable at the discontinuity threshold. Appendix B verifies this using pre-treatment data. Out of a total of 92 pre-treatment observables, we detect a statistically significant difference for 10 variables at the 10 percent level or lower.[16] Importantly, Appendix Table B2 shows statistically indistinguishable levels of pre-treatment bank credit access along both the intensive and extensive margins for Muslim households across treated and control districts. Appendix Tables B6 and B9 show balance for pre-treatment electoral outcomes and various crimes. The absence of selective sorting of districts into treatment, combined with the comparabality of key pre-treatment outcomes of interest allow us to assign a causal interpretation to the fuzzy RD coefficients estimated using equation (3).

4.2 Other Policy Interventions for Minority Welfare

Section 2 noted that the Prime Minister's 15 Point Programme listed a number of policy measures for minority welfare, one of which was preferential lending for religious minorities. While none of the other policies relied on the district-level 25% minority population share threshold, two other minority welfare schemes were implemented using an identical threshold at the level of census towns and blocks. To this effect, Appendix Table B12 shows that the number of blocks and census towns listed as minority concentration, and the share of minority population residing across minority concentration blocks and census towns were comparable across treated and control districts. This assuages concerns that minority concentration districts were disproportionately exposed to other concurrent policies included in the Prime Minister's 15 Programme. This assures that the fuzzy RD coefficients estimated using specification (3) are

[16] The 2003 AIDIS is used to compare pre-treatment household assets and liabilities. District-level data from the administrative BSR between 2001 and 2007 is used for banking outcomes, while household survey data from the 2007-08 employment-unemployment survey is used to verify balance across labor market outcomes. We also verify balance on electoral and crime characteristics using pre-treatment data.

identifying minorities' access to bank credit, as opposed to the impact of a bundled package of minority welfare policies.

5 Results: Minority Credit Access and Economic Empowerment

This section presents our key findings. We start by examining the treatment's impact on bank credit access for Muslim households, along with other measures of economic well-being. Next, we explore whether the directed credit policy contributed to the political empowerment of minority citizens, and identify plausible mechanisms driving the economic and political effects.

5.1 Economic Impacts of the Directed Credit Policy

We begin by assessing how the directed credit policy affected bank credit access for religious minorities and two key measures of household well-being: consumption and assets. As the AIDIS data is measured at the household level, monetary values are normalized by household size. We use a fixed set of districts within a bandwidth of 0.072 and compare households on either side of the discontinuity threshold. Section 5.1.1 details the rationale behind the choice of bandwidth. All specifications are estimated using a fuzzy RD design, where actual treatment assignment is instrumented using the discontinuous jump in treatment assignment at the arbitrary threshold of 0.25 (minority population share). Specifications are weighted using sampling weights and a triangular kernel, assigning higher weight to households located in districts closer to the discontinuity threshold.

Compared to observationally equivalent Muslim households in non-minority concentration areas, column (1) of Table 1 identifies a statistically significant 8 ppt. increase in Muslim households' likelihood of receiving a bank loan in treated districts. The point estimate is large in magnitude: as 10 percent of Muslim households in control districts had an outstanding bank loan, the directed credit policy led to a near doubling in bank credit access for religious minorities.

Compared to the average control district in the pre-treatment period having 39,000 Muslim households, the policy intervention increased bank credit access for an additional 3,000 Muslim households. Together with extensive margin changes in bank credit access, column (2) shows a statistically significant INR 6,200 unconditional increase in per capita bank loan amounts in treated districts.[17] The treatment effect reflects a near doubling in unconditional bank loan sizes relative to non-minority districts. The additional volume of bank lending equates to 20 percent of households' annual per capita consumption.

Turning to our two measures of household well-being, we find the policy to have yielded tangible improvements in Muslims' economic well-being. Column (3) identifies a 27 percent increase in Muslim households' per capita monthly consumption. In monetary values, the INR 700 increase in per capita monthly consumption in treated districts is slightly larger than the INR 660 per capita Hindu-Muslim consumption gap in control districts. Our second measure of economic well-being is aggregate household assets.[18] Column (4) identifies a 32 percent increase in per capita household assets, which is monetarily equivalent to 50 percent of the Hindu-Muslim household asset gap in control districts. Comparing the point estimates, higher bank borrowings can account for up to 80 percent of the increase in household consumption and 5 percent of the increase in household assets. Based on these point estimates, the positive treatment effect on household assets in particular cannot solely be explained by higher leverage taken on by Muslim households.

Finally, following existing studies which document credit access to influence labor market choices (Bruhn and Love, 2014; Breza and Kinnan, 2021), we study whether the policy altered labor market participation in minority concentration districts using data from the nationally representative Periodic Labor Force Survey (PLFS) 2017–18. Columns (1) and (2) of Appendix Table C10 show that the directed credit policy did not affect overall labor force participation or unemployment rates. However, we identify a reallocation of minority workers from wage work (column 4) to self-employment (column 3). The switch to self-employment is financially

[17] Loan amounts are based on initial loan sizes and thereby unaffected by repayments and capitalized interest.

[18] Household assets include cash, savings in short and long-term financial instruments, livestock, land and real estate.

beneficial for Muslim workers: hourly self-employment earnings increased by 24 percent in treated areas, while wage earnings remained largely unchanged.

5.1.1 Robustness of Economic Impacts

Appendix Table C2 shows our results to be robust to the omission of covariates and to data-driven MSERD optimal bandwidths prescribed by Calonico et al. (2020). Based on the outcome of interest and the inclusion of additional household covariates, MSERD optimal bandwidths range between .055 and .072. We select the latter to maximize sample size and use a fixed set of 95 districts that fall within the bandwidth of .072 for all specifications. Columns (3) and (6) show that the omission of covariates has little impact on either the magnitude or the precision of the fuzzy RD estimates. The top panel of Appendix Figure C3 shows our results to be robust to a range of bandwidths between 0.04 and 0.10. The bottom panel shows robustness to the omission of individual states. This allays concerns that our findings might be a manifestation of other state-specific policies which also might have been implemented using the 0.25 minority share threshold.

Appendix Tables C3 and C4, show the baseline results to be stable to alternate specification and sample choices. Our results are unchanged if we opt to use a uniform kernel as opposed to a triangular kernel; a quadratic polynomial in the running variable as opposed to a linear polynomial; clustering at the more conservative level of state-region as opposed to district; or the omission of household multiplier weights. Columns (2) and (4) of Appendix Table C4 show the results to be very comparable when using a sharp RD specification and interpreting the RD coefficients as intent-to-treat estimates. Columns (3) and (6) of Appendix Table C4 show comparable estimates when we omit the 18 districts for which the policy rule was violated, leading to non-compliance in treatment assignment. Finally, columns (1) and (4) of Appendix Table C4 show robustness to expanding the sample to all religious minorities, as opposed to only Muslims. While the results are directionally similar, the point estimates decline in magnitude and are now significant only at the 10% level. This suggests that the directed credit policy

primarily affected credit access for Muslim households.[19]

5.1.2 Credit Quality and Crowding out of Non-Minorities

Appendix Table C6 shows that the directed credit policy did not lead to any deterioration in bank loan performance, either in terms of worsening in loan repayments, or heightened loan write-offs. Instead, the evidence, if anything, points to a decline in the value of bank loan write-offs in minority concentration areas. We also explore whether preferential bank lending to religious minorities came at the expense of non-minority borrowers. The results are mixed: while Appendix Table C8 reports a decline in the size of bank credit to non-minorities along the intensive margin, there is also an accompanying expansion in bank credit access along the extensive margin. Pooling minorities and non-minorities in columns (4) and (8), we find an overall decline in average bank loan size in minority concentration areas, suggesting that the intensive margin decline in non-minority bank credit access stemmed from an overall contraction of bank credit in treated areas, as opposed to the reallocation of credit from non-minorities to minorities.[20] Importantly, columns (2) and (6) offer no evidence of lower credit access for historically marginalized SC and ST households. This rules out bankers reallocating credit within the weaker section quota from SC/ST borrowers to religious minorities.

5.1.3 Mechanisms of Bank Credit Expansion

We consider three mechanisms through which the directed credit policy could have expanded bank credit access for Muslim households

5.1.3.1 Self-Help Groups

The first mechanism considered is whether banks expanded lending by lowering the costs of screening and monitoring for minority borrowers. Through annual guidelines, the central bank

[19] Descriptively, there is little evidence of non-Muslim religious minorities facing barriers to accessing formal credit markets. It is thereby not entirely unsurprising that the primary beneficiaries of the directed credit policy was Muslim households.

[20] If this was the case, we would have expected the treatment to have yielded a null effect after pooling minority and non-minority households.

continually recommended that lenders collaborate with local self-help groups (SHGs) to reach eligible minority borrowers. The specific scheme is the bank-SHG linkage, whereby banks lend directly to SHGs registered with the bank. The SHG subsequently decides on the within-group allocation of credit. If minority borrowers had limited credit histories and lenders were unwilling to extend credit to such borrowers owing to high screening and monitoring costs, it is possible that such costs could have been ameliorated by lending through SHGs (Banerjee and Duflo, 2010). Independently, banks too had an incentive to lend to SHGs, as such loans allowed them to meet priority sector targets.[21]

Separating bank loans into those obtained directly from commercial banks and those obtained through bank-linked SHGs, columns (1)–(4) of Table 2 report an increase in minority households' access to bank credit from both sources. Comparing the point estimates in column (1) of Table 2 and column (1) of Table 1, lending through bank-linked SHGs accounted for 60 percent of the extensive margin expansion in minority bank credit access. As expected, though, the size of loans issued directly by commercial banks is an order of magnitude larger than those obtained through bank-linked SHGs. Importantly, columns (1) and (3) of Appendix Table C9 show no corresponding increase in credit access through bank-linked SHGs for non-minority borrowers. The point estimates are substantially smaller than those for Muslim borrowers and statistically non-significant. This rules out the possibility that higher lending to minorities through bank-linked SHGs emanated from an overall expansion in bank-SHG lending in treated districts.

5.1.3.2 Collateral Requirement

Inadequate collateral is considered a major hindrance to accessing credit markets marked by information frictions (Banerjee and Duflo, 2010). If banks abstained from lending to Muslim households due to limited collateral, it is possible that the directed credit policy nudged banks to extend unsecured loans to reach eligible Muslim borrowers. Using data on loan securitization,

[21] Commercial banks in India are mandated to allocate 40% of their aggregate loan portfolio to the "priority" sector. This includes farm loans, loans to small and micro manufacturing and service enterprises, and select personal loans. Lending to self-help groups also count as priority sector credit.

column (5) rules out any change in unsecured bank lending for minority households. The point estimate, although positive and relatively large in magnitude, is not statistically significant.

5.1.3.3 Interest Rate

The final mechanism considered is the cost of credit. While the central bank's policy documents do not make any recommendations regarding interest rates, it is possible that banks reduced interest rates for minority borrowers to facilitate the flow of credit. Column (6) of Table 2 estimates a precise null effect on bank lending rates. Importantly, column (6) of Appendix Table C9 identifies no change in interest rates for non-minority borrowers, assuaging concerns that lenders were cross-subsidizing minority borrowers by charging higher rates to non-minorities.

6 Economic Well-being to Political Empowerment: Electoral Implications of Minority Credit Access

Section 5.1 established that the policy successfully expanded access to bank loans for Muslim households, and enhanced the economic well-being of minorities. We now study whether these improvements translated into political empowerment for Muslim citizens. Prior to a discussion of our results though, we offer a brief outline of the factors which might lead preferential credit access for religious minorities to generate spillovers in the realm of politics. Section 6.2 uses this discussion to test for mechanisms linking the directed credit policy to political empowerment for Muslim citizens.

At the outset, we hypothesize two broad channels through which the credit policy could have altered the participation of Muslim citizens in electoral politics. First, politics is built around the collective action of voters, but collective action is costly in the absence of co-ordination mechanisms such as organizations (Battaglini and Palfrey, 2023). We contend that the credit policy lowered the cost of collective action for Muslims by incentivizing the formation of bank-registered self-help groups in order to access bank credit. This is evident from Section 5.1.3.1 which showed the extensive margin expansion in bank credit for Muslim households to be largely

facilitated through bank-SHG lending. Heightened participation of religious minorities in SHGs could have led to spillovers in terms of citizen interactions, generating greater awareness among citizens and easing the costs of collective action, which can ultimately lead to co-ordinated group actions in the sphere of politics.

Second, access to bank credit and greater economic well-being could have enhanced political engagement through a "wealth effect". Higher household assets could have allowed Muslims, who were otherwise interested in politics but deterred by entry costs, to enter the political arena. An increase in the personal wealth of candidates could also have enabled them to run a more effective campaign, generating electoral gains.

6.1 Political Empowerment of Muslims

We begin by evaluating whether public policy designed for the economic betterment of vulnerable populations can also aid their political empowerment. Section 3.2 noted the relevance of this question in light of the low political representation of minority Muslim citizens in India's legislative bodies (Farooqui, 2020). We consider political representation by studying whether Muslim citizens are more likely to contest and garner support in elections to state legislative bodies. Akin to Section 5.1, we use the fuzzy RD framework to identify the electoral impacts of the directed credit policy. The unit of observation now is the electoral constituency. As we have multiple years of electoral data, we augment specification (3) with electoral year fixed effects in addition to state-region fixed effects.[22] To account for variation in the number of registered voters across constituencies, all regressions are weighted by the number of registered voters. Standard errors remain clustered by district.

Column (1) of Table 3 identifies a 24 ppt. increase in the likelihood of a Muslim politician contesting an election to the state legislative assembly. Conditional on participation, column (2) shows the average Muslim candidate to have received an additional 2.8 ppt. of popular support in minority concentration areas (p-value .011). Aggregating the popular support received by

[22] No other controls are included in these specifications.

Muslim candidates in each constituency, column (3) reports an imprecisely estimated 3.3 ppt. increase in popular support (p-value .111), amounting to an additional 4,800 votes.[23]

A plausible explanation for higher participation by Muslims in electoral politics would be that a positive wealth effect spawned a desire for greater social visibility. If so, we would expect Muslim citizens to not be considered viable candidates by major parties but contest instead as independents. We examine this hypothesis by identifying whether Muslim candidates contested on behalf of major non-right-wing parties, such as the Indian National Congress or the Samajwadi Party. Column (4) shows that non-majoritarian parties in minority concentration areas had a 11.6 ppt. higher likelihood of fielding a Muslim candidate—a large increase relative to the control group mean of 18.7 percent. Column (5) shows these candidates to be electorally successful, registering 6.4 ppt. higher vote shares (p-val $< .01$), or 8,000 additional votes.

Column (6) identifies a significant decrease in the overall number of Muslim contestants. Relative to a mean of 1.87 Muslim candidates in control districts, elections in treated districts saw 0.41 fewer Muslim contestants. This suggests increased participation of higher-quality Muslim candidates in electoral politics, accompanied by a reduction in the number of frivolous candidates, pointing to within-group coordination. Finally, column (7) of Table 3 shows that the increase in popular support translated to a higher win likelihood for Muslim contestants. Column (8) shows this effect emanated from Muslim candidates contesting on behalf of non-majoritarian parties. With the average district witnessing 8 electoral contests, a 10 ppt. increase in the likelihood of a Muslim winner from non-majoritarian parties is equivalent to an additional Muslim legislator being elected to the state legislative assembly from a minority concentration district.[24]

[23] The average constituency in these districts had 217,885 voters. Turnout in minority concentration areas equaled 67 percent during this period. The increase in the number of votes is computed as $0.033 \times 0.67 \times 217885.2$. Constituencies without any Muslim candidate are assigned a value of 0 in these regressions.

[24] The political economy literature has discussed how politicians can improve economic well-being for constituents. In a context like India where state-owned lenders are prevalent, legislators can also influence the allocation of bank credit. The electoral success of minority Muslim candidates nominated by major non-right-wing parties thereby raises the question as to whether the directed credit policy increased bank credit access and household well-being, or whether the 15-point programme led to political empowerment, which in turn generated economic empowerment. We assess this in Appendix Table C7 by controlling for the share of Muslim legislators from non-right wing parties in a district in the fuzzy RD specifications. Our results remain comparable in both magnitude and precision to the inclusion of this additional covariate. This allays concerns that the directed

Appendix Table D3 compares overall support for majoritarian and non-majoritarian parties across minority and non-minority concentration areas. Column (1) shows no change in electoral support for non-majoritarian parties across minority and non-minority concentration areas, while column (2) identifies a higher win likelihood for non-majoritarian parties. The absence of an overall increase in electoral support for these parties underlines the role of legislator identity as opposed to party identity: vote shares for non-majoritarian parties in treated districts rise only when these parties nominated Muslim candidates, and not due to an overall swelling of support for these parties. Columns (3) and (4) document a sharp reduction in electoral support for the right-wing Bharatiya Janata Party, which has been consistently inimical to minority interests. Relative to the control group mean, the coefficient in column (4) points to a nearly 50 percent decline in the majoritarian party's win likelihood, or a reduction of 2 BJP legislators. In summary, areas eligible for preferential access to bank credit for religious minorities witnessed increased participation of Muslim contestants in electoral politics. Non-majoritarian parties exhibited a greater likelihood of nominating Muslim candidates, who in turn were successful in garnering electoral support. Higher electoral support translated into higher win likelihoods for Muslim candidates. There was also a corresponding decline in electoral support for the majoritarian BJP in minority concentration areas, resulting in a lower win likelihood for the right-wing party.

Did the rise in electoral support for minority Muslim politicians emanate from both non-minority and Muslim voters voting for Muslim candidates, or from Muslims exclusively voting for Muslim candidates? As the administrative data does not offer a religious breakdown of the electorate, we use data from Gulzar et al. (2024) to classify constituencies as high and low Muslim share based on the sample median. Columns (1)–(6) of Appendix Table A1 re-estimate the RD specifications after applying this sample split: while both types of constituencies saw a rise in electoral participation and popular support for Muslim candidates, the point estimates are substantially larger in magnitude in constituencies with a relatively high share (above median)

credit policy had little impact on minority bank credit access and all our findings on economic outcomes are a manifestation of enhanced minority political representation.

of registered Muslim voters.[25] This would again be consistent with Muslim citizens coordinating to vote in favor of Muslim candidates.

6.2 Mechanisms: Credit and Politics

We use the discussion at the beginning of Section 6 to examine mechanisms driving the link between preferential credit access for religious minorities and the electoral success of minority Muslim politicians.

6.2.1 Voter Cohesion and Mobilization Through Social Interactions

We begin by exploring whether the directed credit policy could have heightened within-group social interactions between religious minorities through the formation of SHGs. Since the directed credit policy altered Labor market choices of minority workers, we also consider whether it might have affected opportunities for interactions between minorities and non-minorities in the Labor market.

6.2.1.1 Formation of Self-Help Groups

Table 2 showed that up to 50 percent of the extensive margin increase in bank credit access for Muslim households could have been explained by lending through bank-linked SHGs. To be eligible for bank credit, SHGs must first be registered with the bank. The National Rural Livelihood Mission disseminates a list of all registered SHGs in banks, along with their year of registration, as well as demographic details about individual members. We aggregate this information to the district and compare whether minority concentration areas witnessed increased participation of Muslims in bank-registered SHGs. The unit of observation is the district. As we have multiple years of data, the fuzzy RD specification conditions on both state-region and year fixed effects.

Columns (1) and (2) of Appendix Table A2 show that overall SHG formation and total members

[25] In percentage terms, though, the treatment effects are larger in constituencies with a low share of registered Muslim voters, as the outcome variable means in non-minority concentration districts are very small.

in bank-registered SHGs remained comparable across treated and control areas. Consistent with higher credit access for Muslims through bank-linked SHGs, column (3) of Appendix Table A2 identifies 2,000 additional Muslim members in bank-registered SHGs in minority concentration areas (p-value .055). Using the religious denomination of each member, columns (4) and (5) show higher Muslim participation to be driven by SHGs comprised exclusively of Muslim members. Appendix Table A2 offers two insights: first, there was a significant increase in the number of Muslims in bank-registered SHGs. Moreover, this emanated from the formation of exclusively Muslim SHGs. If SHGs foster social interactions, it can translate into stronger collective action and cohesion among citizens, leading to higher electoral participation and co-ordinated voting for Muslim candidates. Greater interactions with co-religionists can also lead to a strengthening of religious identity (Atkin et al., 2021), which too can yield greater support for Muslim candidates. In terms of magnitudes, the increase in Muslim SHG members can explain up to a fourth of the increase in electoral support accruing to Muslim contestants nominated by non-majoritarian parties in treated districts.

6.2.1.2 Labor Market Polarization

Section 5.1 noted that bank credit access for religious minorities led to a reallocation of minority workers from wage work to self-employment. In a society marked by residential segregation (Adukia et al., 2025), the labor market offers an alternate platform for communities to engage with one another. A shift away from wage work to self-employment can dampen such opportunities. Furthermore, column (5) of Appendix Table C11 shows that Muslim workers' exit from wage employment also occurred in sectors which had a relatively high (above median) concentration of Hindu workers. This is likely to diminish opportunities for minority workers to interact with non-minorities, which too can contribute to a strengthening of minority religious identity and engender support for minority politicians.

6.2.1.3 Strengthening of Minority Identity and Perceptions about Collective Action

Section 6.2.1.1 offered evidence of increased participation of Muslim citizens in self-help groups. Section 6.2.1.2 showed a shift away from wage work in industries with a relatively high share of Hindu workers. The former can increase collective interactions with fellow Muslim citizens, fostering a sense of religious identity; the latter can reduce interactions with non-minorities and contribute to the solidifying of religious identity. Did this amplify minority citizens' identification with their community and alter their beliefs about collective action? We probe this using data from a nationally representative survey on citizen perceptions about religious identity and the efficacy of collective action.

We capture citizen attitudes using data from the Centre for Monitoring the Indian Economy (CMIE). Alongside nationwide surveys capturing households' income and consumption, investment, savings and employment in every trimester, the CMIE also conducted ten *Samaj* Surveys to capture household attitudes along a varying range of topics. The surveys were conducted between 2014 and 2019 using the same sampling frame as the CMIE surveys. Two such surveys in 2017 and 2019 inquire households about the primacy of religion in their identity, whether they felt organized collective action along religious lines could bring forth changes in government policies towards their community, and whether they would support protests undertaken to redress injustices perpetrated against their religious group. Responses were recorded on a 5-point Likert scale. We opt to be conservative and define a binary variable equaling 1 if households responded as "strongly agreed" to the question of interest.[26]

Applying our fuzzy RD design, column (1) of Appendix Table A3 shows a significantly greater likelihood among Muslim respondents in minority concentration areas to repose faith in the potency of collective action. We identify more than a doubling in Muslim respondents' likelihood

[26] Collective action was measured by asking, "*If people from my religious group are united we can pressurize the government to improve our lives.*" Willingness to protest religious injustices was measured by asking, "*I would be willing to participate in protests and demonstrations to support the rights of my religious group.*" Importance of religious identity was measured by asking, "*My religion plays an important role in how I see myself.*" Responsiveness of institutions to concerns about violence was measured by asking, " *In your opinion, if your safety and security were under threat, who would be the most responsive to your needs?*" The latter included "Police" and "MPA/MLA" as separate response categories.

of stating that collective action along religious lines could elicit a favorable response from the government. Increased faith in collective action would be consistent with co-coordinating with other Muslim citizens to vote for Muslim candidates, or forming pressure groups and nudging non-majoritarian parties to nominate higher quality Muslim candidates in state elections.

Along similar lines, column (2) shows that Muslims in policy districts reported significantly higher willingness to support protests against injustices perpetrated against their community. This again would be consistent with increased faith in the process of collective action among Muslim citizens, which in turn can influence voting behavior. In line with higher inter-group interactions fostering greater group identification, column (3) shows Muslim respondents in minority concentration areas were significantly more likely to report religion playing a pivotal role in shaping their identity. Finally, greater coordination and cooperation could foster a belief in the power of the vote. This is reflected in last two columns that show Muslims were more likely to believe elected representatives in treated districts would be more responsive if they were under a threat of violence (column 4), and were less likely to believe the police would be responsive to their security needs (column 5).

Our data does not permit a direct assessment of whether exclusively Muslim SHGs led to enhanced dialogue among Muslim citizens on topics of politics and religion. Neither can we assess whether SHG members engaged in political activism. However, studying individual attitudes through the *Samaj* Survey point to a higher belief among Muslim citizens in treated areas that collective action can evoke a favorable response from the government for their community. These citizens also expressed greater willingness to agitate against injustices perpetrated against their communities, and there is an overall strengthening in religious identification. These would be consistent with increased interactions among Muslim citizens through SHGs leading to greater faith in collective action and the salience of religious identity. Higher affiliation with one's religious identity can influence voter choices and support for political formations espousing that identity (Atkin et al., 2021). Similarly, increased faith in organizing along communal lines to bring forth responses from the government can boost turnout, and pressurize parties to nominate Muslim candidates for elections.

6.2.1.4 Voter Turnout

While we don't have information on citizen voter preferences or whether citizens persuade political parties to nominate specific candidates, we can directly test whether turnout increased in minority concentration areas using electoral data from the TCPD. In line with citizens stating a greater willingness to engage in collective action to demand responsive governance, column (5) of Appendix Table D3 detects a 6 ppt. increase in voter turnout in minority concentration areas, amounting to an additional 8,000 votes being cast in such districts. The number of additional voters casting their ballot is very similar in magnitude to the rise in electoral support for Muslim candidates contesting on behalf of non-majoritarian parties. The size of the electorate remains statistically indistinguishable across treated and control areas. While the administrative ECI data does not separate turnout across communities, we use the estimated share of Muslim voters in each constituency from (Gulzar et al., 2024) and show in column (5) of Appendix Table A1 that the rise in turnout was restricted to constituencies with a relatively high share of Muslim voters. This would be in line with higher turnout emanating from Muslim voters, as opposed to non-minority voters.

6.2.2 Altering the Supply of Muslim Politicians

The positive wealth effect arising from improved access to bank credit could have eased entry costs for aspiring politicians, allowing them to use their personal wealth to cover campaign costs or ease financing constraints. A lowering of entry barriers could also have improved candidate quality and led to higher support for such contestants. We use data from candidates declarations to compare the wealth, education, criminal background of Muslim politicians across minority and non-minority concentration areas.

Columns (1)-(3) of Appendix Table D4 identifies positive point estimates for candidate assets, liabilities and net assets, but except the coefficient corresponding to liabilities, none of the other point estimates are statistically significant. Upon restricting the sample to Muslim candidates contesting on behalf of key non-majoritarian parties, column (4) identifies a significant increase in

candidate assets, while column (6) shows a significant increase in net assets. There is no impact on the liabilities of Muslim candidates contesting on non-majoritarian party platforms. Columns (1)-(4) Appendix Table D5 show Muslim candidates in treated districts had greater experience (proxied by age) and were also more likely to have a college degree. These treatment effects are larger for minority politicians contesting on behalf of non-majoritarian parties. However, these politicians were also more likely to have a criminal record. Overall, we find suggestive evidence for improvement in the quality of Muslim politicians, as measured by wealth and education, suggesting that supply of better politicians is likely played some role in driving the political results.

7 Social Costs of Minority Welfare

We conclude our empirical analysis by studying whether the economic and political empowerment of religious minorities affected religious conflicts. A priori, economic prosperity can reduce conflicts by increasing the opportunity cost of violence. Economic advancement of religious minorities can also mitigate conflicts through improved access to law enforcement authorities or legal resources. Alternatively, Mitra and Ray (2014) and Bonomi et al. (2021) have shown that majority groups can resort to violence to assert their dominance when faced with minority economic empowerment.

We proceed by comparing the incidence of religious violence across minority and non-minority concentration areas using administrative data from the National Crime Records Bureau. Using first information reports filed under specific sections of the erstwhile IPC for sectarian violence, Table 4 identifies a substantial increase in the incidence of per capita religious conflicts in minority concentration areas. Column (4) reports one additional religious conflict in minority concentration districts in any given year between 2014 and 2019 (per million population) – a near doubling in the incidence of religious violence.[27] Columns (2) and (3) show non-religious

[27] When considering the high likelihood of under-reporting and under-recording of such incidents, the increase in religious conflicts is almost certainly an under-estimate of the increase in overall religious animosity in minority concentration areas.

conflicts and homicides, if anything, were lower in minority concentration areas. This assuages concerns of a secular increase in criminal activity in treated districts.[28]

Results from the administrative NCRB data are confirmed in Appendix Table A4, which uses the ACLED database and also detects significantly higher religious strife in treated districts. As the ACLED relies on newspaper reports to geolocate incidents of conflict, captures major conflict incidents, and also separately classifies Hindu-Muslim conflicts, the positive coefficient in column (1) of Appendix Table A4 (p-value .059) confirms a rise in major incidents of Hindu-Muslim violence in minority concentration districts. This alleviates concerns that the rise in sectarian conflicts identified in the administrative data was a manifestation of reporting biases, changes in the manner in which police recorded cases of Hindu-Muslim conflicts, conflicts between two religious minorities, or conflicts between Hindus and a non-Muslim religious minority. Similar to the administrative data, column (2) does not find any aggregate increase in non-religious riots in treated districts.

Based on the description of events in ACLED, we hand code whether the violence was instigated by Hindus or Muslims. Using this classification of events, column (3) identifies a statistically significant rise in Hindu-Muslim conflicts instigated by Hindu groups. Comparing the point estimate in column (3) to that in column (1), a fourth of the increase in religious conflicts can be attributed to those instigated by Hindu groups. Though substantial, we consider this analysis suggestive, as we were able to hand-code only a portion of the reported incidents due to a lack of information on the action that prompted the conflict.[29]

[28] The NCRB only began to disaggregate riots into various sub-categories following 2014. Resultantly, we cannot undertake a pre-treatment analysis to verify that religious conflicts were "balanced" across treated and control districts prior to 2008. However, as seen from column (1) Appendix Table B9, out of the 9 crime heads considered, only pre-treatment per capita riots (all riots) was imbalanced across minority and non-minority concentration districts. To account for this potential imbalance in a key pre-treatment outcome of interest, Appendix Table D6 replicates the results in Table 4 after controlling for average pre-treatment per capita riots in all specifications. Reassuringly, our results are unchanged in both magnitude and direction to the inclusion of this additional covariate.

[29] In our data, only a minuscule fraction of religious conflicts are initiated by Muslims.

7.1 Mechanisms of Violence

Three potential channels might explain the rise in religious conflicts in minority concentration areas. First, is a backlash against minority prosperity. Mitra and Ray (2014) theorized Hindu-Muslim conflicts to escalate following a narrowing of the Hindu-Muslim consumption gap, as the majority group resorts to violence to preserve economic dominance. Second, majoritarian forces could have increased the production of violence prior to elections to suppress minority turnout. This is a view suggested by existing scholarship on Hindu-Muslim violence (Iyer and Shrivastava, 2018). Third, minority political assertion could have engendered a violent backlash against religious minorities.

Empirically, we distinguish between these channels using the ACLED data. Unlike the NCRB which only records total registered offenses during a year, the ACLED provides information on the event date, allowing us to map conflict events to elections. Our hypothesis is that if majoritarian violence is predominantly a backlash against the economic prosperity of minority households, violence should not be tied to the timing of elections. Alternately, if religious conflicts are initiated to suppress minority turnout, the increase in conflicts should occur primarily before the elections. Finally, if religious violence is a blow-back against the political empowerment of minorities, the violence should be concentrated in the aftermath of the elections.

Columns (4)-(6) of Appendix Table A4 shows that the rise in Hindu-Muslim conflicts in minority concentration areas was entirely driven by events occurring within 1 year of the elections. There is no corresponding increase in violence in the year prior to the elections; neither do we detect any significant increase in the interim period between elections. This supports the explanation that majoritarian violence emanated in response to the electoral success of Muslim politicians in state elections. As the electoral gains to Muslim candidates was largest in absolute terms in constituencies with a relatively high share of Muslim voters, we would also expect the rise in religious violence to be concentrated in such areas if it was linked to electoral politics. As the ACLED offers precise geo-locations, we can map religious conflicts to electoral constituencies. In line with our expectations, columns (7) and (8) of Appendix Table A4 show that the rise in

Hindu-Muslim violence primarily originated from constituencies with a higher share of Muslim voters. This bolsters the contention that the rise in religious conflicts emanated as a blow-back effect to increased political empowerment of religious minorities.

8 Conclusion

Many polarized countries have large minorities that are marginalized economically and politically. If governments want to improve the economic well-being of these minorities, increasing access to credit is one important policy lever available to them. This paper shows that such a policy can be successful at expanding credit and improving well-being without a big cost to the majority group in the credit markets. However, there are important considerations that are the key mechanisms for the success of the program. The policy has to be designed to work in the context of the institutional setup of the country. As the paper shows, Indian banks were asked to work with Self Help Groups of citizens which reduced the likely discrimination the minority citizens faced in the status quo when dealing with banks individually and also reduced the costs of screening that banks had to otherwise incur to extend credit to the minority borrowers. Further, banks have to be flexible in their requirement of collateral if they are to get more clients from the marginalized sections of society.

While such policies targeting the economic well-being of minorities are not explicitly designed to politically empower minorities, it is natural that with better economic conditions, minorities are more likely to claim a stake in politics. But this does come at the cost of violence. While it is possible that violence may be used as a tool to keep minorities out of politics, the paper sheds light on a subtle use of violence: as a punishment after the election for minorities becoming more active politically.

Credit policies are commonly used as a means to help people out of poverty. This paper opens a new avenue for research by highlighting that political empowerment may be a consequence of economic empowerment. But since this comes at the cost of violence, it is not clear if, in the long run, the well-being effect may persist. Even though this paper uncovers the positive

effects of directed credit policy on economic well-being and political empowerment ten years after the policy was enacted, it will still be important to uncover if the policy managed to reduce inequality in the long run.

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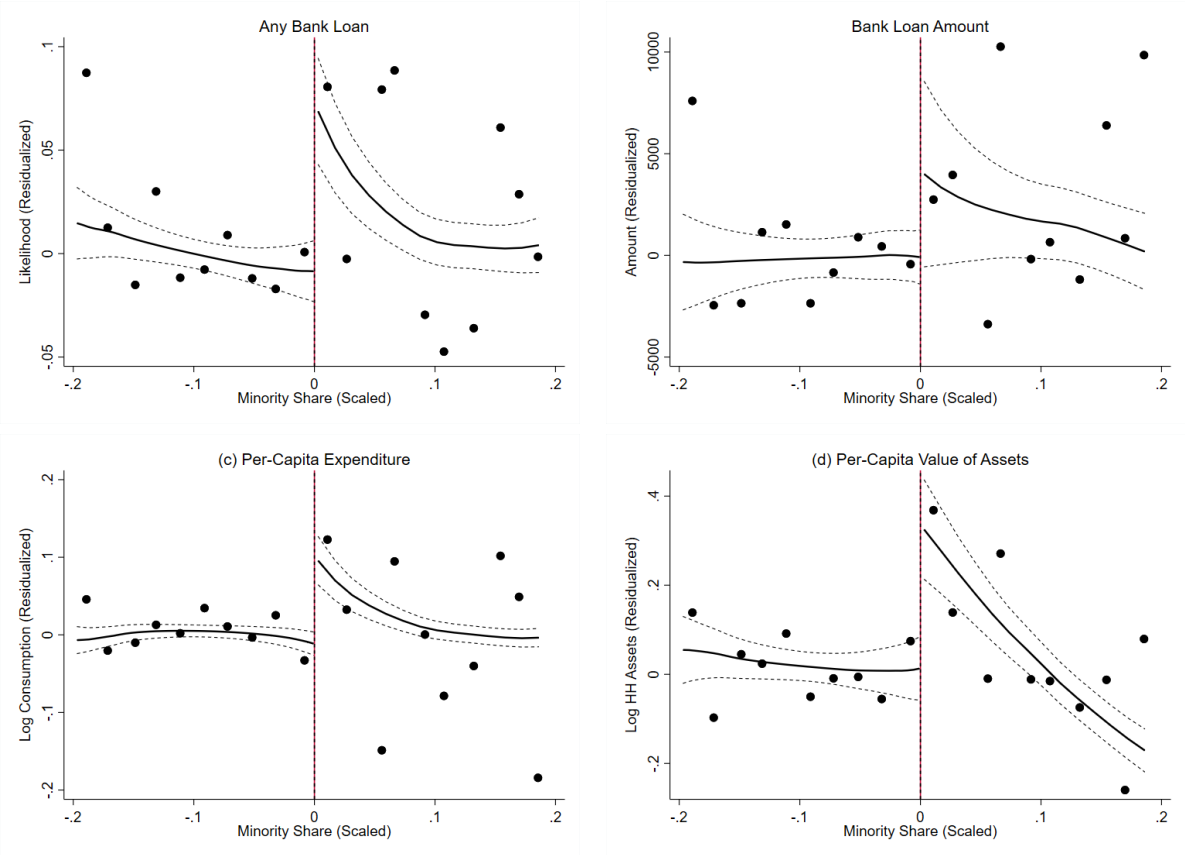
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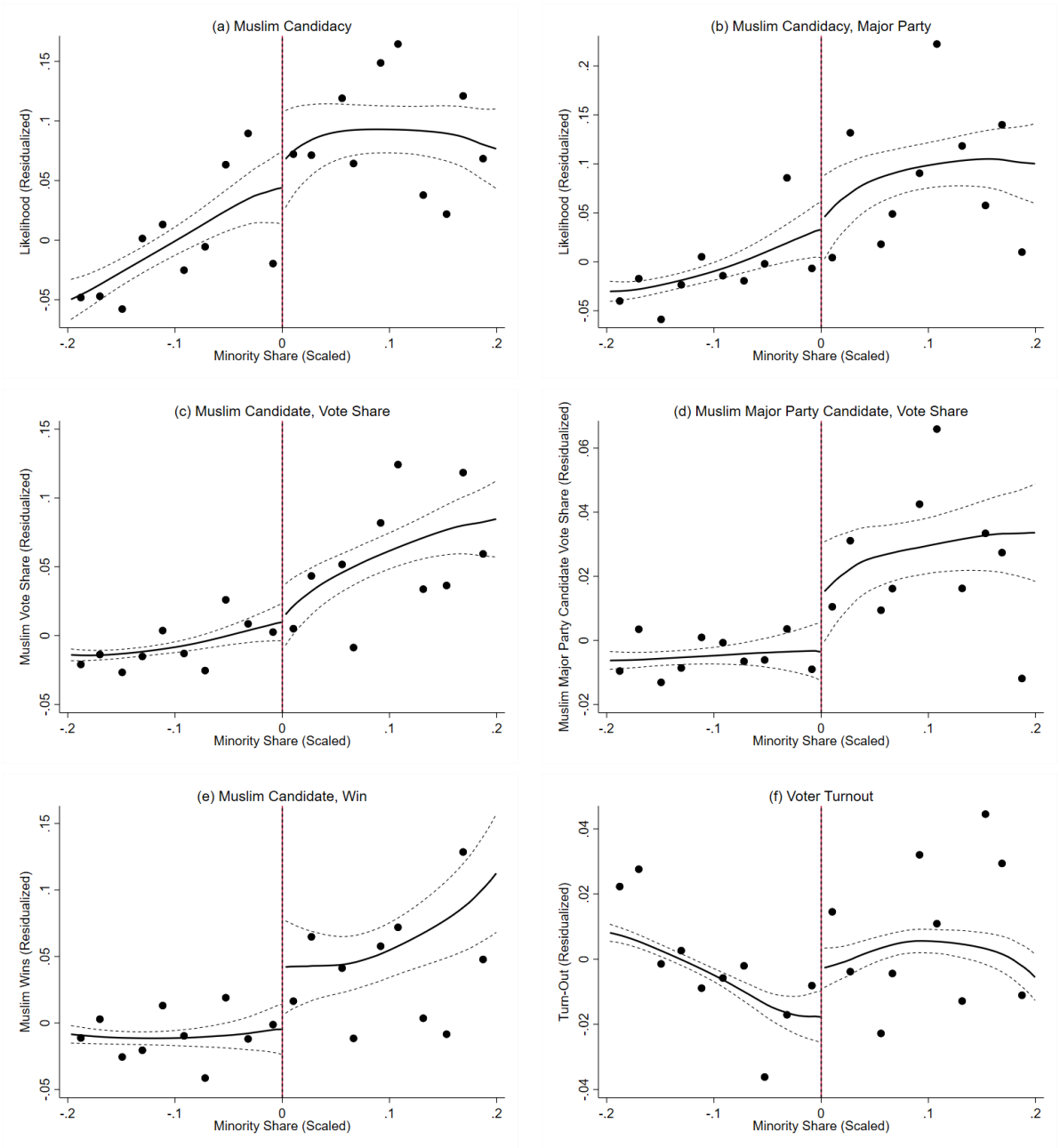
9 Figures & Tables

Figure 1: RD Plots of Economic Effects of the Credit Policy



*Notes:*The above figures show the treatment effects on economic outcomes of Muslim households. The unit of observation is the household. (a) Any Bank Loan reports the effect on the likelihood that the household has a loan from a bank. (b) Bank Loan Amount reports the unconditional effect on the size of the loan. (c) Per-Capita Expenditure reports the effect on monthly per-capital consumption. (d) Per-Capita Value of Assets reports the effect on the value of assets held by households. The outcome variable is residualized, conditional on state-region fixed effects and household covariates. The solid dots show residualized means of the outcome variables, corresponding to each bin of the running variable. The horizontal lines show the linear fit from a local linear regression, and the dashed lines show 90% confidence intervals. Estimates are weighted using a triangular kernel and household specific weights provided by the AIDIS.
Source: AIDIS 2019

Figure 2: RD Plots of Political Effects of the Credit Policy



Notes: The above figures show the treatment effects on political outcomes of Muslim candidates. The unit of observation is constituency-election. (a) Muslim Candidacy reports the effect on the likelihood of there being a Muslim candidate in the election. (b) Muslim Candidacy, Major Party reports the effect on there being Muslim Candidate running from a major party platform. (c) Muslim Candidate, Vote Share reports the effect on the vote share received by Muslim candidates. (d) Muslim Major Party Candidate, Vote Share reports the effect on the vote share received by Muslim candidates running on major party platforms. (e) Muslim Candidate, Win reports the effect on the likelihood of Muslim candidate winning. (f) Voter Turnout reports the effect of policy on turnout of voters. All outcome variables are residualized, conditional on year of election, state-region, and constituency-type fixed effects. The solid dots show residualized means of the outcome variables, corresponding to each bin of the running variable. The horizontal lines show the linear fit from a local linear regression, and the dashed lines show 90% confidence intervals. Estimates are weighted using a triangular kernel and number of electors in a constituency-election.

Table 1: Directed Credit Policy, Bank Credit Access and Economic Well-Being: Muslim Households

	(1)	(2)	(3)	(4)
	Bank Credit		Consumption and Assets	
	Any Bank Loan	Loan Amt. Bank	Consumption	Assets
Treat	.085***	6282.393***	.274***	.324**
	(.027)	(1462.718)	(.042)	(.147)
Observations	3660	3660	3660	3644
Control Mean	.099	5155.280	2593.000	269136.285

Notes: This table reports the impact of the directed credit policy on bank borrowings, consumption and household assets for Muslim households. The unit of observation is the household. The outcome of interest in column (1) is a binary equaling 1 if the household has a bank loan; in column (2), the value of bank loans; in column (3), log monthly household consumption; in column (4), log value of household assets. All monetary values are scaled by household size,. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification and are weighted by AIDIS assigned household multiplier weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: AIDIS, 2019.

Table 2: Directed Credit Policy and Bank Credit Access for Muslim Households: Exploring Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)
	Source of Bank Credit				Bank Loan Characteristics	
	Any Loan Comm. Bank	Any Loan Bank-SHG	Loan Amt Comm. Bank	Loan Amt. Bank-SHG	Any Unsec. Bank Loan	Bank Interest Rates
Treat	.036*	.051***	5706.083***	576.310**	.021	-.004
	(.019)	(.014)	(1331.590)	(272.800)	(.038)	(.013)
Observations	3660	3660	3660	3660	3660	1008
Control Mean	.082	.022	5016.209	139.071	.039	.108

Notes: This table reports potential mechanisms explaining the increased credit access for Muslim households. The unit of observation is the household. The outcome of interest in column (1) is a binary equaling 1 if the household has a bank loan through a bank-linked SHG; in column (2), the value of bank loans from a bank-linked SHG; in column (3), a binary equaling 1 if the household has a loan from a commercial bank; in column (4), the value of bank loans from commercial banks; in column (5), a binary equaling 1 if the household has an unsecured bank loan; in column (6), bank interest rates. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 , and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification and are weighted by AIDIS assigned household multiplier weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: AIDIS, 2019.

Table 3: Electoral Effects of the Directed Credit Policy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Muslim Candidate			Muslim Candidate Major Parties		Total Candidates	Muslim Candidate Win	
	Any Candidate	Vote Share	Vote Share	Any Candidate	Vote Share	All Parties	Any Party	Major Party
Treat	.242***	.028***	.034	.116***	.064***	-.414**	.090***	.103***
	(.039)	(.008)	(.021)	(.042)	(.015)	(.197)	(.034)	(.027)
Observations	1400	2663	1400	1400	1400	1400	1400	1400
Dep Var Mean	.663	.050	.093	.187	.037	1.870	.061	.027

Notes: This table shows the treatment's impact on electoral outcomes. The unit of observation is the electoral constituency. The outcome of interest in column (1) is a binary equaling 1 if a minority Muslim candidate contested elections. Column (2) considers vote shares received by each Muslim candidate while column (3) aggregates votes received by all Muslim candidates contesting in a constituency. The outcome of interest in column (4) is a binary equaling 1 if a Muslim contestant from a non-majoritarian party contests the election; in column (5), the vote share received by Muslim contestants from a non-majoritarian party; in column (6), total Muslim contestants; in column (7), a binary equaling 1 if a Muslim candidate wins the election; in column (8), a binary equaling 1 if a Muslim candidate from a non-majoritarian party wins the election. All specifications include state-region and year fixed effects, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification and are weighted by the number of registered voters in the constituency. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Trivedi Centre for Political Data, 2008-2019.

Table 4: Directed Credit Policy and Religious Conflicts

	(1)	(2)	(3)
		Crimes Per Capita	
	Religious Riots	Non Religious Riots	Murders
Treat	1.188**	1.081	
	(.503)	(5.123)	(3.142)
Observations	568	568	568
Dep Var Mean	.614	48.145	30.355

Notes: This table compares religious and non-religious conflicts across minority and non-minority concentration districts. The unit of observation is the district. The outcome of interest in columns is per capita crimes for the category mentioned. Per capita crimes scales annual incidences of crime by the district's population in 2001. All specifications include a linear polynomial in the running variable, state-region and year fixed effects. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted by the number of registered voters in the constituency. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: National Crime Records Bureau, 2015-2019.

A Appendix: Additional Tables

Table A1: Electoral Effects of the Directed Credit Policy: Heterogeneity by Share of Muslim Voters

				Panel A: High Muslim			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pr(Muslim Candidate = 1)		Muslim Candidate Vote Shares		Muslim Candidate Win		
	Any Party	Major Party	Any Party	Major Party	Any Party	Major Party	Turnout
Treat	.083*	.540***	.140***	.210***	.338***	.337***	.106***
	(.046)	(.097)	(.040)	(.030)	(.070)	(.049)	(.010)
Observations	578	578	578	578	578	578	578
Dep Var Mean	.924	.425	.198	.079	.113	.053	.641
				Panel B: Low Muslim			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pr(Muslim Candidate = 1)		Muslim Candidate Vote Shares		Muslim Candidate Win		
	Any Party	Major Party	Any Party	Major Party	Any Party	Major Party	Turnout
Treat	.371***	.052	.054***	.034**	.057**	.062***	.002
	(.062)	(.048)	(.020)	(.013)	(.026)	(.024)	(.015)
Observations	649	649	649	649	649	649	649
Dep Var Mean	.472	.047	.032	.008	.015	.000	.704

Notes: This table compares electoral outcomes across minority and non-minority concentration areas, after splitting the sample by the share of Muslim voters in the constituency. Panel restricts the sample to *High Muslim* constituencies, which have a relatively high (above median) share of registered Muslim voters; Panel B restricts the sample to *Low Muslim* constituencies with a below median share of registered Muslim voters. The outcome of interest in columns (1) and (2) is a binary equaling 1 if any Muslim candidate contested the elections; in columns (3) and (4), the vote share received by Muslim candidates; in columns (5) and (6), a binary equal to 1 if a Muslim candidate won the election. In column (7), the outcome of interest is the voter turnout. The outcome of interest in columns (2), (4) and (6) consider Muslim candidates contesting on behalf of non-majoritarian parties. Columns (1), (3) and (5) consider all Muslim candidates. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using the number of registered voters in the constituency. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Election Commission of India, 2008-2019.

Table A2: Treatment Effect on Bank-Registered Self-Help Group Formation and Composition

	(1)	(2)	(3)	(4)	(5)	(6)
	Total SHG	SHG Members	Muslim Members	Any Muslim in SHG	Only Muslims in SHG	No Muslims in SHG
Treat	-36.819	899.688	2088.518*	127.170	152.258*	772.518
	(917.559)	(928.103)	(1088.767)	(130.263)	(82.933)	(976.679)
Observations	792	792	792	792	792	792
Dep Var Mean	3985.803	45451.225	3921.225	598.167	186.808	44853.058

Notes: The above table shows the treatment effect on the formation of bank-registered self-help groups (SHGs). The unit of observation is the district. The outcome of interest in column (1) is the number of bank registered SHGs in the district; in column (2), the total number of members across all bank-registered SHGs; in column (3), the total number of Muslim members across all bank-SHGs; in column (4), the number of SHGs with at least 1 Muslim member; in column (5), the number of SHGs with only Muslim members; in column (6), the number of SHGs with no Muslim member. All specifications include state-region fixed and year fixed effects, and a linear polynomial in the running variable. Columns (2)-(6) control for total number of SHGs registered. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: National Rural Livelihood Mission, 2008-19.

Table A3: Directed Credit Policy and Household Attitudes

	(1)	(2)	(3)	(4)	(5)
	Citizen Attitudes			Responsiveness to Violence	
	Collective Action Govt. Response	Protest Against Religious Injustice	Religious Identity	Politician	Police
Treat	.564***	.314**	.208***	.039***	-.162**
	(.070)	(.129)	(.079)	(.013)	(.066)
Observations	4582	4600	4580	3011	3011
Dep Var Mean	.31	.33	.41	.01	.48

Notes: This table compares citizen attitudes across minority and non-minority concentration areas following the directed credit policy. The unit of observation is the household. The outcome of interest in all columns are binary. The outcome of interest in column (1) equals 1 if the household strongly agrees that organizing along religious lines can pressurize the government to create favourable policies; in column (2), the outcome of interest equals 1 if the household strongly agrees to engage in protests to secure the rights of their religious group; in column (3), the outcome of interest equals 1 if the household strongly agrees that religion plays a key role in their identity. In column (4), the outcome of interest equals 1 if the household believes members of federal or state legislatures would be most responsive to incidents of violence; in column (5), the outcome of interest equals 1 if the household believes the police would be most responsive to incidents of violence. All specifications include state-region and year fixed effects, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using household weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Centre for Monitoring the Indian Economy, Samaj Surveys: 2017-2019.

Table A4: Treatment Effect on Religious Violence: ACLED Database

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Riots Per Capita							
	Hindu Muslim	Other	Hindu Instigated	Post Election Year	Pre Election Year	Non-Election Years	High Muslim Concentration	Low Muslim Concentration
Treat	.059**	1.252	.014***	.061**	.005	.010	.941***	.282***
	(.031)	(1.466)	(.003)	(.024)	(.004)	(.017)	(.191)	(.065)
Observations	360	360	360	337	337	337	221	256
Dep Var Mean	.038	4.327	.021	.006	.005	.029	.078	.029

Notes: The above table shows the treatment effect on religious violence from the ACLED database. The unit of observation in the first six columns is the district and it is a constituency in the last two columns. Column (1) considers Hindu-Muslim religious riots; column (2) considers non-religious riots; column (3) considers Hindu-Muslim riots instigated by Hindus; column (4) considers Hindu-Muslim riots occuring within 12 months of an election to the state legislative assembly; column (5) considers Hindu-Muslim riots occuring 12 months before elections to the state legislative assembly; column (6) consider Hindu-Muslim riots in non-election years; column (7) splits the sample by constituencies with a relatively high (above median) share of registered Muslim voters; column (8) splits the sample by constituencies with a low share of registered Muslim voters. Riots are measured as the number of incidents, scaled by the district population in 2001 (in millions). All specifications include state-region and year fixed effects, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using household weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.

Source: ACLED, 2016-19.

Online Appendix

A Pre-Treatment Minority Credit Access: Descriptive Evidence

A.1 Credit Access Across Communities

We present the descriptive analysis by disaggregating households into 5 mutually exclusive groups: Hindu forward castes, Hindu marginalized castes, Muslims, and other religious minorities.[1] We also disaggregate the source of credit into bank loans, loans from non-bank financial institutions (NBFIs) such as co-operative banks and non-banking financial corporations (NBFCs), and loans from informal sources. The latter is further disaggregated into loans from professional money lenders and input suppliers, and loans from community networks such as friends, relatives, employers and landlords.

Appendix Figure A1 shows the share of outstanding credit accounted for by each of the communities, disaggregated by the source of credit. The top-left panel shows that overall, Muslims accounted for 7 percent of outstanding credit across all sources, which shrunk to 6 percent when considering only bank credit (middle-panel, top row). For reference, Muslims accounted for 13 percent of India's population according to the 2001 Census. This under-representation is seen across all credit sources, with the exception of loans from community sources. In contrast, other non-Muslim religious minorities accounted for 7 percent of bank credit, while comprising 5 percent of the population. Unsurprisingly, Hindu forward castes accounted for 40 percent of bank credit, despite accounting for less than a quarter of India's population.

The top-left panel of Appendix Figure A2 shows that between 30 and 40 percent of households across all five groups had some outstanding loan in 2003, with informal loans being the primary source of credit. While less than 10 percent of households had any outstanding bank loan, this was particularly low for Muslim households (3 percent), relative to Hindu forward castes (5.6 percent) or other religious minorities (5.6 percent). Similarly, the top-right panel of Appendix Figure A2 shows Muslim households to also have lower credit access along the intensive margin. Relative to Hindu forward castes or other religious minorities, and conditional on having an outstanding bank loan, Muslim households on average had 30 percent lower bank loan amounts. The bottom row of Appendix Figure A2 compares the cost of credit across communities and shows no variation in bank interest rates across the five groups. This indicates that Muslim households were not inherently riskier borrowers. However, these households were charged the

[1] We refer to *Dalits*, *Adivasis*, and Other Backward Classes (OBCs) when referring to Hindu marginalized castes. The descriptive analysis also distinguishes between SC/STs and OBCs.

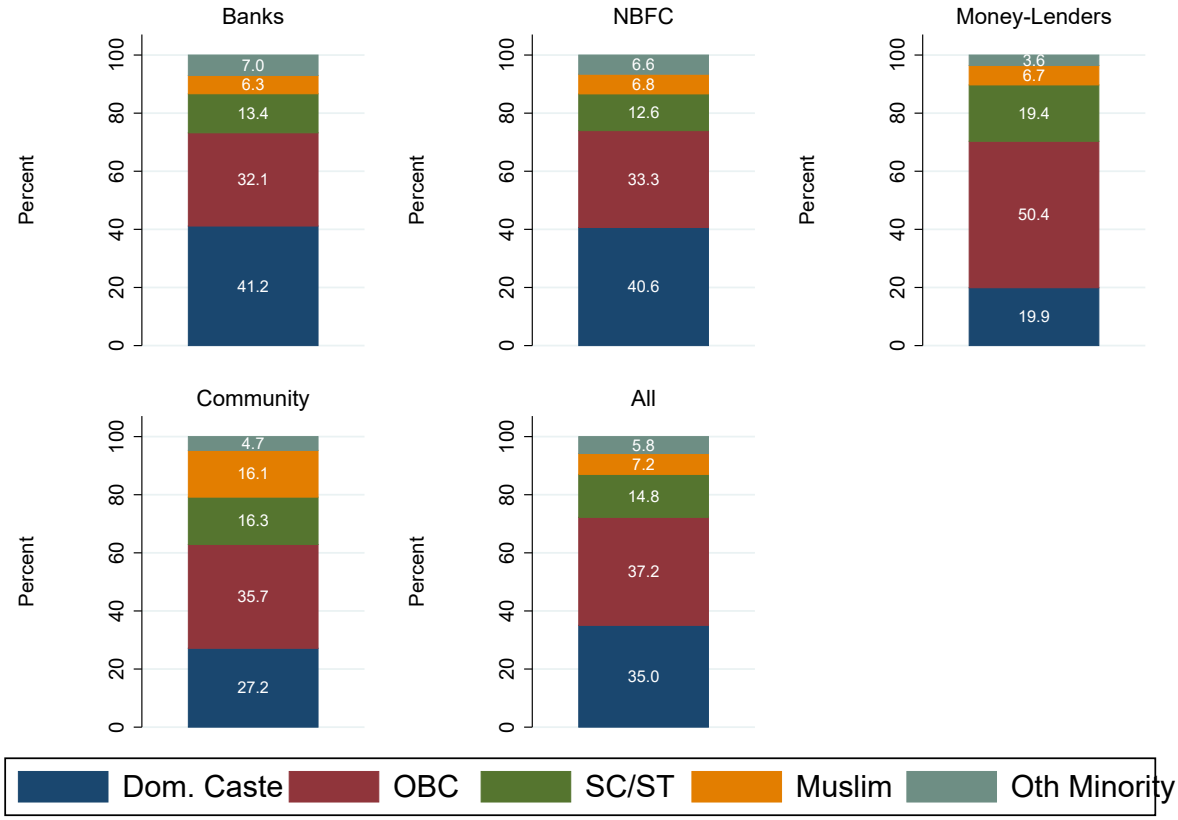
highest rates of interest by professional money lenders. Collectively, Appendix Figure A2 points to a rationing of formal credit for Muslim households along both the extensive and intensive margins. This is unlikely to be explained by a lower demand for credit as the fraction of Muslim households with outstanding loans is very similar to Hindu forward castes. In the absence of formal credit, Muslim households were pushed towards informal credit markets, where they faced the highest cost of credit.

A.2 Oaxaca Blinder Decomposition

We formally use an Oaxaca-Blinder decomposition to gauge whether the descriptive difference in credit access for minority Muslim households, discussed in Section A.1, can be explained by observable characteristics. We use the 2003 AIDIS and restrict the sample to Hindu and Muslim households. District fixed effects are used to partial out aggregate district characteristics which uniformly affect credit access for both communities. Consequently, our decomposition tests whether observable characteristics can explain the difference in bank credit access for Muslim households, relative to Hindu households residing in the same district. We condition for demographic features such as household size and rural location, as well as educational characteristics such as whether the household has a secondary educated member. Additional covariates includes whether the household's earnings are primarily from self-employment or wage work, and the broad industry of occupation. To factor in variations in households' demand for credit, we also control for monthly per capita household consumption. Finally, as the ability to extend collateral often determines credit access Banerjee and Duflo (2010), we control for the value of land and buildings owned by the household. The sample is restricted to households in non-minority concentration districts and standard errors are clustered by district.

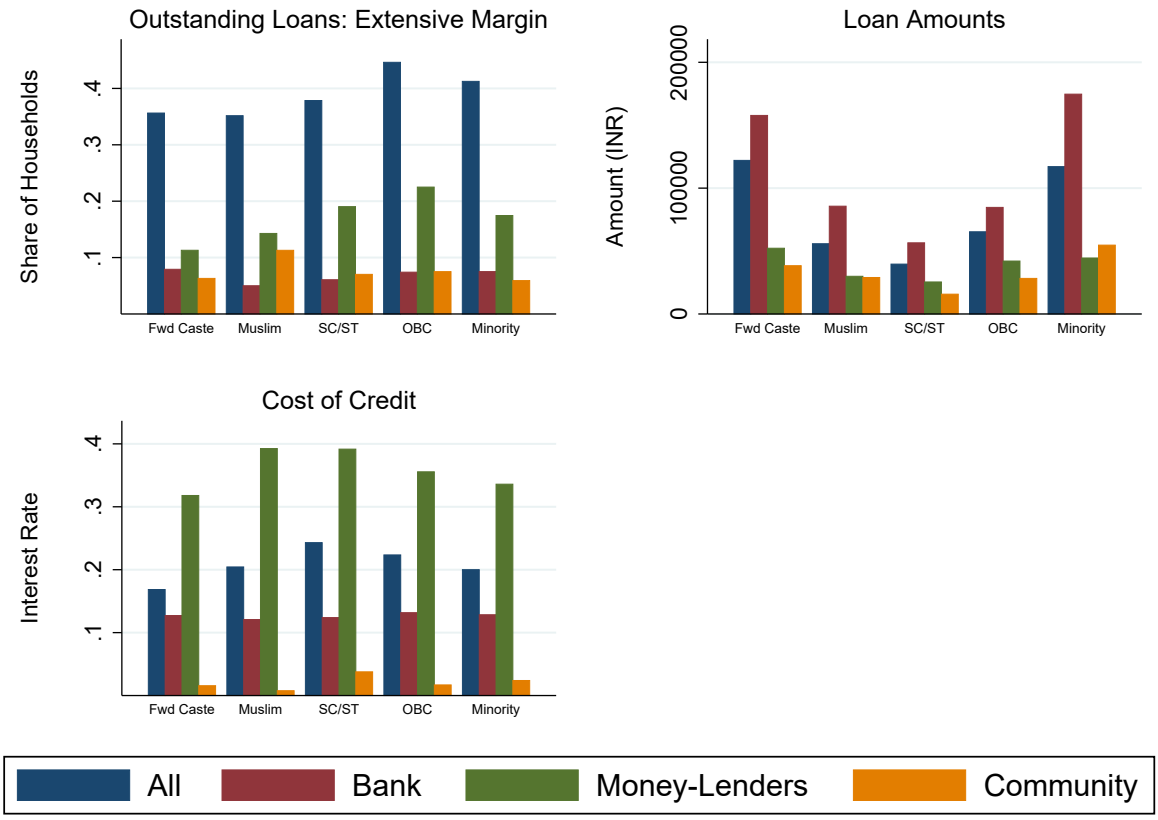
Appendix Figure A6 shows that observable characteristics cannot fully explain the variation in bank credit access across Hindu and Muslim households, pointing to the presence of discrimination. After conditioning on the fixed effects and covariates, Muslim households have a statistically significant 2.3 percentage point lower likelihood of having a bank loan, relative to Hindu households. Observable characteristics can only explain 38 percent of this difference. Along the intensive margin, unobservable characteristics explain 30 percent of the difference in the amount of bank credit, although the point estimate is not statistically significant (p-value .229).

Figure A1: Distribution of Credit Across Communities and Credit Source: Pre-Treatment Shares



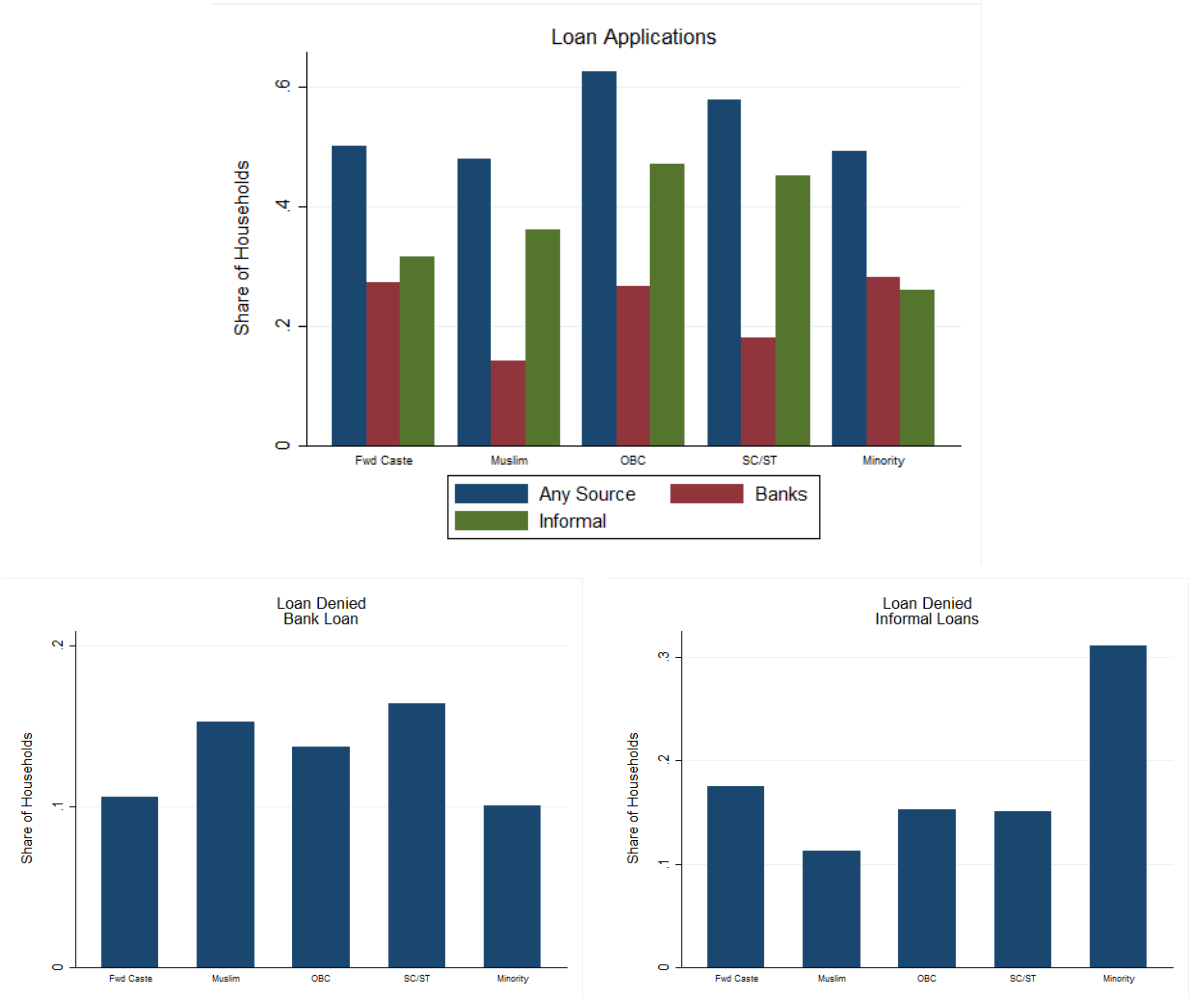
Notes: The above figure shows the pre-treatment distribution of credit across communities and credit source. The bar graph shows the share of outstanding credit for each community, based on credit source. *Oth Minority* refers to non-Muslim religious minorities; *Dom Caste* refer to non-SC/ST/OBC Hindu households. Data is from the AIDIS, 2003.

Figure A2: Pre-Treatment Household Credit Across Communities



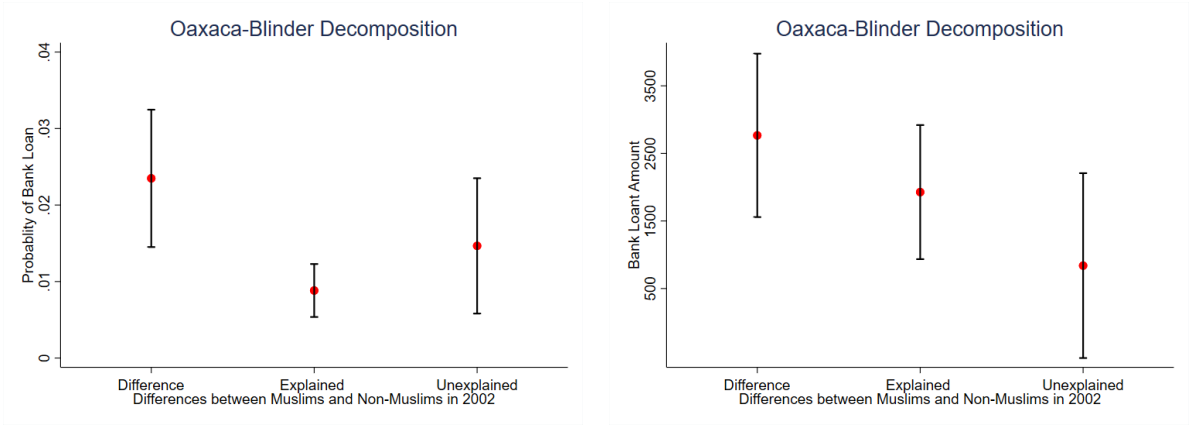
Notes: The above figure presents pre-treatment descriptive statistics for loan outcomes by community. The top-left panel shows the likelihood of households having loans from any of the above-mentioned sources; the top right-panel shows the average volume of credit received by households from each source; the bottom-right panel shows the average rate of interest from each source. *Minority* refers to non-Muslim religious minorities; *Fwd Caste* refer to non-SC/ST/OBC Hindu households. Data is from the AIDIS, 2003.

Figure A3: Loan Application and Denial Across Communities and Credit Source



The above figures compares loan application and denial rates across communities using data from the Indian Human Development Survey (IHDS), conducted in 2011-12. Loan applications cover any loan application made by the household in the past five years, from the source mentioned. Loan denial is conditional on the household applying for the loan from the source mentioned.

Figure A6: Pre-Treatment Credit Access Across Minority and Non-Minority Households: Oaxaca-Blinder Decomposition



Notes: The above figures uses a Oaxaca-Blinder decomposition to test the role of unobservables in explaining the difference in bank credit access across Hindu and Muslim households in the pre-treatment period. The sample is restricted to Hindu and Muslim households. All specifications condition on district fixed effects and a rich set of household covariaes. The outcome of interest in the left-figure is a dummy equaling 1 if the household has any bank loan; in the right-figure, the value of bank loans. Standard errors are clustered by district.

B Appendix: Balance Checks

Table B1: Directed Credit Policy and Household Characteristics: Pre-Treatment Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Rural	Marginalized	Farm	Non Farm	Farm Work	Non Farm Self Employed	Wage Work
Treat	-.020	.033	.080	-.001	.021	.067	-.096***
	(.077)	(.030)	(.073)	(.070)	(.027)	(.055)	(.018)
Observations	4060	4060	4060	4060	4060	4060	4060
Control Mean	.536	.396	.257	.636	.116	.316	.200

Notes: This table shows pre-treatment balance on household characteristics for Muslim households. The outcome of interest are all binary. The outcome of interest in column (1) is a binary equaling 1 if the household resides in a rural area; in column (2), a binary equaling 1 if the household belongs to the *Dalit*, *Adivasi* or OBC communities; in column (3), a binary equaling 1 if the household's primary earnings are from farm activities; in column (4), a binary equaling 1 if the household's primary earnings are from non-farm work; in column (5), a binary equaling 1 if the household's primary earnings are from farm labour; in column (6), a binary equaling 1 if the household's primary earnings are from non-farm self-employment; in column (7), a binary equaling 1 if the household's primary earnings are from salaried work. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B2: Directed Credit Policy and Loan Characteristics: Pre-Treatment Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Pr(Loan = 1)						Loan Amount (INR)					
	Any Source	Bank	NBFI	Inf.	Money Lender	Comm.	Any Source	Bank	NBFI	Inf.	Money Lender	Comm.
Treat	.02	.012	-.03*	.02	-.02	.03	-872.34**	201.01	-766.36***	-273.13	-361.87	88.74
	(.03)	(.01)	(.02)	(.03)	(.03)	(.02)	(415.61)	(237.60)	(215.80)	(300.93)	(255.68)	(132.50)
Observations	4060	4060	4060	4060	4060	4060	4060	4060	4060	4060	4060	4060
Control Mean	.29	.03	.04	.22	.12	.11	2506.09	312.84	730.51	1236.13	762.98	473.14

Notes: This table shows pre-treatment balance on household borrowing for Muslim households. The unit of observation is the household. The outcome of interest in columns (1)-(6) is whether the household has a loan from the source mentioned; in columns (7)-(12), the value of loans from the source mentioned. *NBFI* refers to non-bank financial institutions. *Inf.* refers to informal loan sources. *Money Lenders* include both traditional money-lenders, and also input suppliers. *Comm.* refers to loans from community networks such as friends, families and landlords. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using household weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: AIDIS, 2003.

Table B3: Directed Credit Policy and Bank Loan Characteristics: Pre-Treatment Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Purpose						Other Characteristics		
	Any Farm Loan	Any Non Farm Loan	Any Expenditure Loan	Farm Loan Amt.	Non Farm Loan Amt.	Expenditure Loan Amt.	Unsecured Loan	Long-Term Loan	Interest Rate
Treat	-.010	.012**	.012***	-98.754	124.987*	51.234	.012	.002	-.021
	(.009)	(.005)	(.004)	(196.982)	(65.610)	(56.608)	(.008)	(.013)	(.018)
Observations	4060	4060	4060	4060	4060	4060	4060	4060	273
Control Mean	.015	.006	.006	85.699	87.128	129.135	.013	.024	.131

Notes: This table shows pre-treatment balance on bank loans for Muslim households. The unit of observation is the household. The outcome of interest in columns (1)-(6) shows balance by the purpose of bank borrowings; in columns (7)-(9), balance by loan characteristics. The outcome of interest in columns (1)-(3) is a binary equaling 1 if the household has a bank loan for the purpose mentioned; in columns (4)-(6), the value of bank loans for the purpose mentioned. The outcome of interest in columns (7)-(8) are also binary. Interest rates in column (9) is the loan-volume weighted value of bank interest rates for the household. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using household weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: AIDIS, 2003.

Table B4: Directed Credit Policy and Household Assets: Pre-Treatment Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Log Value (INR)					Pr(Asset = 1)			
	Consumption	Assets	Assets Excl. Real Estate	Real Estate	Prod. Assets	Any Land	Any Farm Land	Any Bld.	Res. Bld.
Treat	.056	.358	.302	.264	.632	.041	-.030	.030	.028
	(.047)	(.469)	(.431)	(.441)	(.539)	(.045)	(.056)	(.046)	(.047)
Observations	4060	4060	4060	4060	4060	4060	4060	4060	4060
Control Mean	618.661	30452.222	1609.579	14822.483	8524.967	.776	.262	.785	.782

Notes: This table shows pre-treatment balance on household consumption and assets for Muslim households. The outcome of interest in columns (1)-(5) are logged; in columns (6)-(9), a binary equal to 1 for households owing the asset specified. The outcome of interest in column (1) is households' per capita consumption; in column (2), household per capita assets; in column (3), households' per capita assets excluding the value of land and buildings; in column (4), households' per capita land and buildings; in column (5), households' per capita productive assets, defined as the value of land, machinery, transport assets and livestock; in column (6), a binary equal to 1 if the household owns any land; in column (7), a binary equal to 1 if the household owns any farm land; in column (8), a binary equal to 1 if the household owns any buildings; in column (9), a binary equal to 1 if the household owns any residential building. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Standard errors are in parentheses, clustered by district.Significant levels: *10%, **5%, and ***1%
Source: AIDIS, 2003.

Table B5: Balance on Worker Characteristics and Labour Market Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Rural	Female	Educ.	LFP	Unemp.	Self Emp.	Wage Work	Farm	Non Farm	Mfg.	Trade Services	Daily Earn
Treat	.012	-.036*	-.027	.018	-.026	.029	.012	-.004	.034	.026*	-.031	-.005
	(.053)	(.019)	(.023)	(.019)	(.029)	(.029)	(.031)	(.018)	(.021)	(.015)	(.022)	(.098)
Observations	9112	9112	9112	9112	9112	9112	9112	9112	9112	9112	9112	1958
Dep Var Mean	.592	.487	.174	.540	.064	.297	.228	.207	.320	.098	.178	23.312

Notes: This table shows pre-treatment balance on labour market characteristics for Muslim working age individuals. The unit of observation is the individual, aged between 18 and 60 years. The outcome of interest in columns (1)-(11) are binaries; in column (12), logged value of daily earnings. Daily earnings are computed as total weekly earnings, divided by total number of days worked during the week preceding the survey. Daily earnings are computed only for workers reporting a positive wage during the week and excluded self-employed workers. *Educ.* is a binary equaling 1 if the worker had completed secondary education; *Unemp.* refers to the worker being unemployed during the week; *Self-Emp.* refers to the worker being self-employed during the week; *Mfg.* refers to workers engaged in manufacturing activities during the week. All specifications include state-region fixed effects, individual covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using household weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: NSS Employment-Unemployment Survey, 2007.

Table B6: Directed Credit Policy and Electoral Outcomes: Pre-Treatment Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Minority Candidates						
	Any Muslim	No. Muslims	Any Muslim Major Party	Vote Share	Vote Share Major Party	Muslim Win	Muslim Major Party Win
Treat	.111	.257	.060	.008	-.026	-.013	-.066
	(.084)	(.250)	(.074)	(.051)	(.035)	(.060)	(.048)
Observations	1423	1423	1423	1423	1423	1423	1423
Dep Var Mean	.497	1.112	.166	.090	.043	.089	.050

Notes: This table shows pre-treatment balance on electoral characteristics. The unit of observation is the electoral constituency. The outcome of interest in column (1) is a binary equaling 1 if any Muslim candidate contested the election; in column (2), the total number of Muslims contesting; in column (3), a binary equaling 1 if a Muslim candidate contested on behalf of a non-majoritarian party; in column (4), vote share received by Muslim candidates; in column (5), vote share received by the Muslim candidate contesting on behalf of a non-majoritarian party; in column (6), a binary equaling 1 if the Muslim candidate won the election; in column (7), a binary equaling 1 if a Muslim candidate from a non-majoritarian party won the election. All specifications include state-region and year fixed effects, along with a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using the number of registered voters in the constituency. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Election Commission of India, 1996-2007.

Table B7: Directed Credit Policy and Electoral Outcomes: Pre-Treatment Support for INC and BJP

	(1)	(2)	(3)	(4)
	BJP		INC	
	Vote Share	Win	Vote Share	Win
Treat	-.002	.025	.030	.080
	(.019)	(.038)	(.032)	(.061)
Observations	1423	1423	1423	1423
Dep Var Mean	.197	.233	.214	.211

Notes: This table shows pre-treatment balance on party performance across minority and non-minority concentration areas. The unit of observation is the electoral constituency. The outcome of interest in odd-numbered columns is the vote shares for the party mentioned; in the even numbered columns, a binary equaling 1 if the political party won the election. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. All specifications include state-region and year fixed effects, along with a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using the number of registered voters in the constituency. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Election Commission of India, 1996-2007.

Table B8: Pre-Treatment Bank-Registered Self-Help Group Formation and Composition

	(1)	(2)	(3)	(4)	(5)	(6)
	Total SHG	SHG Members	Muslim Members	Any Muslim in SHG	Only Muslims in SHG	No Muslims in SHG
Treat	201.682	204.101	125.250	-8.174	16.770	212.276
	(155.644)	(246.485)	(126.816)	(14.733)	(10.952)	(251.667)
Observations	462	462	462	462	462	462
Dep Var Mean	372.367	4456.629	236.762	42.719	10.562	4413.910

Notes: This table shows pre-treatment balance for bank-registered self-help group (SHG) formation and composition. The unit of observation is the district. The outcome of interest in column (1) is the number of bank registered SHGs in the district; in column (2), the total number of members across all bank-registered SHGs; in column (3), the total number of Muslim members across all bank-SHGs; in column (4), the number of SHGs with at least 1 Muslim member; in column (5), the number of SHGs with only Muslim members; in column (6), the number of SHGs with no Muslim member. Columns (2)-(6) control for the total number of SHGs registered. All specifications include state-region fixed and year fixed effects, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: National Rural Livelihood Mission, 2001-2007.

Table B9: Pre-Treatment Balance on District Crimes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Per Capita Crimes								
	Riots	Murder	Rape	Abduction	Arson	Hurt	Property	Cheating	Total
Treat	21.237***	-.198	-2.221	-1.900	-1.846	53.978	30.871	3.476	30.364
	(5.597)	(3.487)	(2.686)	(2.965)	(2.114)	(40.263)	(42.336)	(8.866)	(23.308)
Observations	665	665	665	665	665	665	665	665	665
Dep Var Mean	49.529	36.030	23.708	30.153	11.098	239.159	438.439	66.702	246.389

Notes: This table shows pre-treatment balance on district crimes. The unit of observation is the district. The outcome of interest is per capita crimes for the categories mentioned. Per capita refers to the district population as per the 2001 population Census. Crimes are based on first information reports filed during the year. All specifications include state-region fixed effects, year fixed effects, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: National Crime Records Bureau (2001-07).

Table B10: Balance on Bank Branches and Bank Deposits

	(1)	(2)	(3)	(4)	(5)
	Branches			Deposits	
	Total	State Own	Private	Amt.	Act.
Treat	-27.092	-22.465	-4.627	-.138	15.555
	(42.815)	(37.455)	(6.160)	(.440)	(19.902)
Observations	637	637	637	637	637
Dep Var Mean	213.276	188.844	24.432	1.613	48.843

Notes: This table shows pre-treatment balance on bank branches and deposits. The unit of observation is the district. The outcome of interest in columns (1)-(3) are the number of bank branches; in columns (4)-(5), bank deposits. The outcome of interest in column (1) is total bank branches; in column (2), state-owned bank branches; in column (3), private bank branches; in column (4), bank deposit amounts; in column (5), bank deposit accounts. Bank deposit amounts are in billions of INR. All specifications include state-region fixed effects, year fixed effects, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Basic Statistical Returns (2001-07).

Table B11: Balance on Bank Credit Disbursement

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Credit		Farm Loans		Manufacturing Loans		Personal Loans	
	Act.	Amt.	Act.	Amt.	Act.	Amt.	Act.	Amt.
Treat	-9873.541	41.598	4795.143	2.022	-4774.879	24.975	1029.820	.771
	(34696.119)	(43.675)	(8129.731)	(1.654)	(5464.193)	(24.707)	(20646.159)	(4.510)
Observations	640	640	640	640	640	640	640	640
Dep Var Mean	359684.134	173.553	78319.785	10.735	12138.260	67.553	211795.986	43.049

Notes: This table shows pre-treatment balance on bank credit. The unit of observation is the district. The outcome of interest in odd-numbered columns is loan amounts; in even-numbered columns, loan accounts. Columns (1)-(2) pertain to all bank credit; in columns (3)-(4), farm loans; in columns (5)-(6), manufacturing loans; in columns (7)-(8), personal loans. Bank loan amounts are in billions of INR. All specifications include state-region fixed effects, year fixed effects, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Basic Statistical Returns (2001-07).

Table B12: Balance on Minority Concentration Towns and Blocks Across Treated and Control Districts

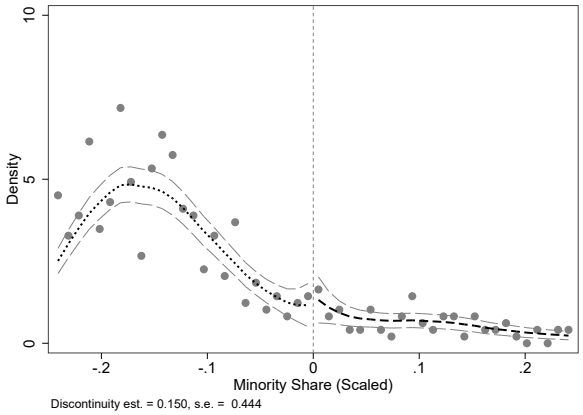
	(1)	(2)	(3)	(4)	(5)
	No. of Blocks and Towns		Population Share Minority Concentration Areas		
	Blocks	Towns	Block	Town	Block + Town
Treat	-.063	-3.417	.171	-.159	.011
	(.874)	(3.064)	(.174)	(.169)	(.140)
Observations	96	96	74	74	74
Control Mean	.480	2.000	.039	.239	.278

Notes: This table shows balance across the number of minority concentration towns and blocks in districts. The unit of observation is the district. The outcome of interest in columns (1)-(2) shows balance across the number of minority concentration towns/blocks in a district; in columns (3)-(5), the share of population in minority concentration towns and blocks. Minority concentration towns (blocks) are towns (blocks) where at least 25% of the population hail from religious minority groups, as defined by the 2001 population Census. All specifications include a linear polynomial in the running variable, restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Standard errors are in parentheses. Significant levels: *10%, **5%, and ***1%.
Source: Population Census, 2001.

C Appendix: Additional Results on Household Credit Access

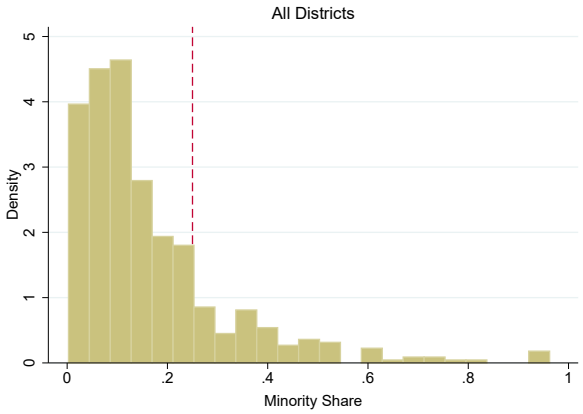
C.1 Figures

Figure C1: Smoothness in Treatment Assignment at Discontinuity Threshold



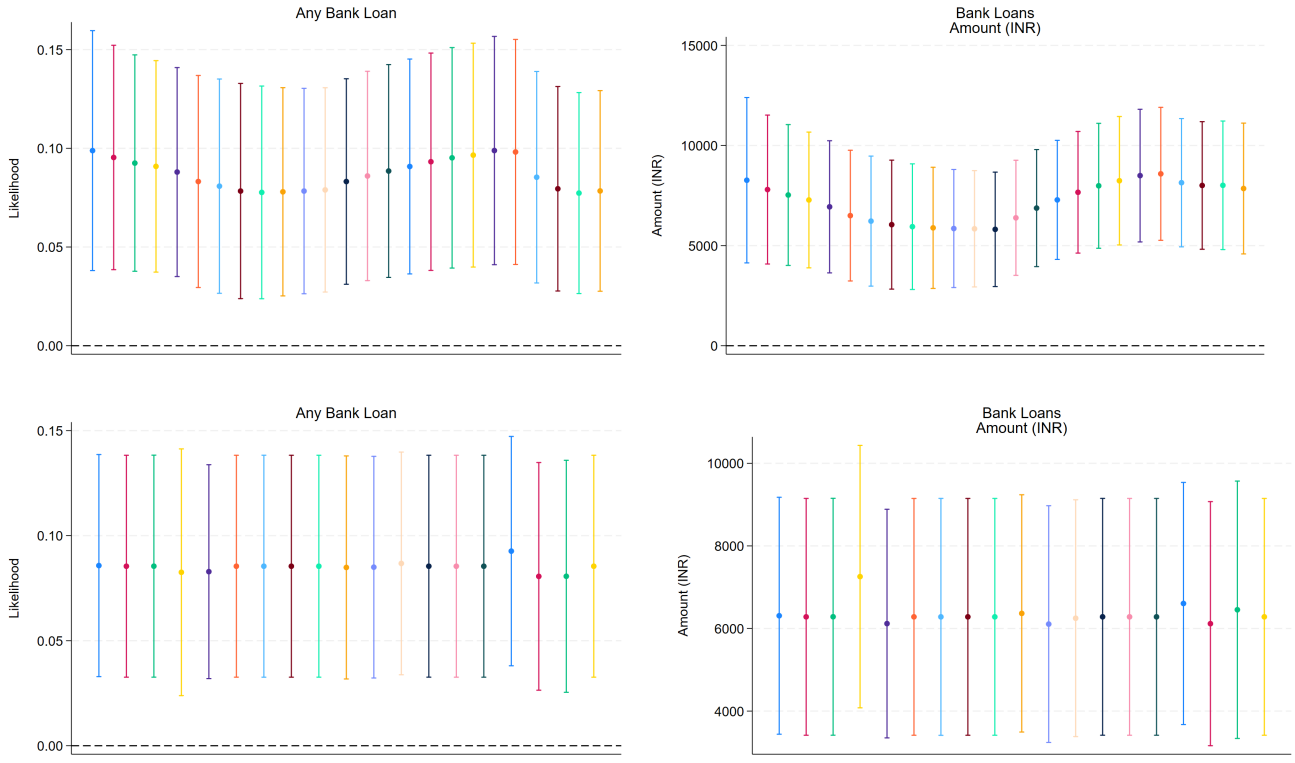
Notes: This figure presents the McCrary test (McCrary, 2008), testing for selective sorting of districts as minority concentration around the 0.25 threshold. The horizontal axis depicts the running variable – defined in equation (1). The horizontal axis shows the range of the running variable between -.25 and .25. The discontinuity threshold of 0 is indicated by the broken vertical line. The discontinuity estimate and standard error is depicted below the figure.

Figure C2: Distribution of Running Variable



Notes: This figure shows the distribution of the share of religious minorities across districts in India. Religious minorities include the religious denominations Muslims, Christians, Sikhs, Buddhists and Zoarastrians, classified as religious minorities in the Census 2001. The population shares are computed using the 2001 Census. The red vertical line shows the population threshold of 25 percent, using to classify districts as "minority concentration".

Figure C3: Robustness to Alternate Bandwidths and Dropping Individual States



This figure shows the robustness of the baseline results to alternate bandwidths and omission of individual states. The outcome of interest in the left-panel is a dummy equaling 1 if the household has any bank loan; in the right-panel, the volume of bank loans (initial loan value). All specifications are restricted to Muslim households. The vertical lines denote 95% confidence intervals of the coefficient estimates. The top panel shows robustness to alternate bandwidths; the bottom panel shows robustness to dropping individual states. The first coefficient in the top-panel is computed using a bandwidth of .04. Subsequent estimations sequentially increase the bandwidth by 0.005. The last point estimate is based on a bandwidth of .10. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. All specifications in the bottom panel restrict the sample to a bandwidth of .072 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household weights. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Standard errors are clustered by district.
Source: AIDIS 2019.

Table C1.A: Summary Statistics: Household Characteristics

	N	Mean	SD
Rural	97154	0.673	0.469
Minority	97154	0.150	0.357
Muslim	97154	0.113	0.316
SC/ST	97154	0.287	0.452
OBC	97154	0.456	0.498
Any secondary educated individual	97154	0.614	0.487
Any higher educated individual	97154	0.234	0.423
Farm	97154	0.372	0.483
Non-farm self-Employed	97154	0.155	0.362
Non-farm casual Work	97154	0.183	0.387
Non-farm salaried Work	97154	0.197	0.398
Monthly Per Capital Household Expenditures	97154	2643.365	2333.916

Notes: This table shows the summary statistics for household demographic characteristics from the AIDIS, 2019 .

Table C1.B: Summary Statistics: Household Credit, Extensive Margin

	N	Mean	SD
Any loan	97154	0.417	0.493
Any bank loan	97154	0.231	0.422
Any commercial bank loan	97154	0.171	0.377
Any bank-SHG loan	97154	0.073	0.261
Any non-bank financial institution loan	97154	0.106	0.308
Any money-lender loan	97154	0.131	0.338
Any community network loan	97154	0.085	0.279
Any farm loan	97154	0.132	0.338
Any non-farm business loan	97154	0.037	0.189
Any expenditure loan	97154	0.168	0.374

Notes: This table shows the summary statistics for household credit along the extensive margin. Bank-SHG loans are loans issues by bank-linked SHGs. Non-bank financial institution loans include loans issued by co-operative banks and other non-banking financial corporations. Money-lender loans include loans obtained from input-suppliers. Community network loans include loans obtained from friends, relatives, employers and landlords. Expenditure loans include loans obtained for consumption, housing, health and education.

Table C1.C: Summary Statistics: Household Credit, Intensive Margin

	N	Mean	SD	P25	P50	P75
Total loan	60683	216998	442825.5	30501	73337	203339
Total bank loan	30003	235294	497198.3	40000	75787	201669
Commercial bank loan	24553	298823	562999.3	50835	101669	285750
Bank-SHG loan	6342	44964	33412.7	20334	40000	54570
Non-bank financial institution loan	12886	138326	286264.6	28000	50835	105501
Money-lender loan	16864	111975	159061.9	20334	50835	130968
Com loan	14125	70170	121709.9	10167	26501	70000
Farm loan	15534	147068	242104.8	32742	71169	159008
Non-Farm business loan	5555	222833	456930.3	39222	75000	203339
Health/education loan	8334	106350	230131.5	20334	50000	101669
Expenditure loan	22540	98220	172915.8	20334	45751	100835

Notes: This table shows the summary statistics for household credit along the intensive margin. The sample is limited to households with some outstanding loan in each category. Bank-SHG loans are loans issues by bank-linked SHGs. Non-bank financial institution loans include loans issued by co-operative banks and other non-banking financial corporations. Money-lender loans include loans obtained from input-suppliers. Community network loans include loans obtained from friends, relatives, employers and landlords. Expenditure loans include loans obtained for consumption, housing, health and education.

Table C1.D: Summary Statistics: Loan Characteristics

	N	Mean	SD
Any long-term loan	60683	0.835	0.371
Any long-term bank loan	30003	0.824	0.381
Any unsecured loan	60683	0.622	0.485
Any unsecured bank loan	30003	0.370	0.483
Any unsecured NBFI loan	60683	0.074	0.261
Any unsecured money-lender loan	16864	0.798	0.402
Any unsecured friend/relatives loan	14125	0.914	0.280
Interest rate	60675	0.140	0.126
Bank interest rate	30003	0.106	0.048
NBFI interest rate	12886	0.122	0.076
Money-lender interest rate	16864	0.303	0.175
Friends/relatives interest rate	14125	0.021	0.090
Loan delinquent	57286	0.347	0.476
Bank loan delinquent	28766	0.264	0.441
Informal loan delinquent	27154	0.413	0.492

Notes: This table shows the summary statistics for other loan characteristics. Bank-SHG loans are loans issues by bank-linked SHGs. Non-bank financial institution (NBFI) loans include loans issued by co-operative banks and other non-banking financial corporations. Money-lender loans include loans obtained from input-suppliers. Community network loans include loans obtained from friends, relatives, employers and landlords. Expenditure loans include loans obtained for consumption, housing, health and education. Interest rates are weighted using loan size. Summaries are computed only for households with outstanding loans in the referred categories.

Table C2: Directed Credit Policy and Access to Bank Credit: Bandwidth Selection and Exclusion of Covariates

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Any Bank Loan = 1)			Bank Loan Amount (INR)		
Treat	.071***	.085***	.072***	4398.896***	6263.371***	4849.674***
	(.026)	(.027)	(.026)	(1676.827)	(1635.814)	(1658.979)
Observations	3637	3639	3660	3324	2899	3660
Bandwidth	.071	.072	.072	.066	.055	.072
Covariates	N	Y	N	N	Y	N

Notes: This table shows the robustness of the baseline results to the use of MSERD-data driven optimal bandwidths recommended by Calonico et al. (2020), and the omission of controls. The unit of observation is the household. The outcome of interest in columns (1)-(3) is a binary equal to 1 if the household has a bank loan; in columns (4)-(6), the volume of bank loans. Columns (1)-(2) and (4)-(5) consider data-driven MSERD optimal bandwidths. Columns (3) and (6) consider a fixed bandwidth of .072. Columns (1), (3), (4) and (6) exclude all covariates except state-region fixed effects. All specifications include state-region fixed effects, in addition to a linear polynomial in the running variable. All specifications are weighted using a triangular kernel and household specific AIDIS multiplier weights. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Significant levels: *10%, **5%, and ***1%.
Source: AIDIS 2019.

Table C3: Robustness of Baseline Results to Alternate Specification Choices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Any Bank Loan = 1)				Bank Loan Amount (INR)			
Treat	.145***	.067*	.085***	.067***	14907.284***	9133.768***	6282.393***	4956.548**
	(.040)	(.037)	(.014)	(.026)	(3271.669)	(2410.845)	(1129.087)	(1967.431)
Observations	3660	3660	3660	3660	3660	3660	3660	3660
Control Mean	.099	.099	.099	.267	5155.280	5155.280	5155.280	13882.671
Kernel	Uniform	Triangular	Triangular	Triangular	Uniform	Triangular	Triangular	Triangular
Polynomial	Linear	Quadratic	Linear	Linear	Linear	Quadratic	Linear	Linear
Cluster	District	District	Region	District	District	District	Region	District
Weights	Yes	Yes	Yes	No	Yes	Yes	Yes	No

Notes: This table shows the robustness of the baseline estimates to alternate specification choices. The unit of observation is the household. The outcome of interest in columns (1)-(4) is a binary equal to 1 if the household has a bank loan; in columns (5)-(8), the volume of bank loans. Columns (1) and (5) consider an uniform kernel instead of a triangular kernel; columns (2) and (6) consider a quadratic polynomial instead of a linear polynomial; columns (3) and (7) consider state-region clusters as opposed to district clustering; columns (4) and (8) exclude household weights. All specifications include state and electoral year fixed effects, in addition to a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Significant levels: *10%, **5%, and ***1%.
Source: AIDIS 2019.

Table C4: Robustness of Baseline Results to Alternate Sample and Specification Choices

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Any Bank Loan = 1)			Bank Loan Amount (INR)		
Treat	.036*	.080***	.094***	2328.901*	5911.086***	6626.275***
	(.021)	(.025)	(.024)	(1288.614)	(1382.417)	(1506.492)
Observations	5082	3660	2266	5082	3660	2266
Control Mean	.133	.145	.099	7948.315	5034.709	5155.280
Sample	Minorities	Muslim	Muslim	Minorities	Muslim	Muslim
Specification	Fuzzy	Sharp	Fuzzy	Fuzzy	Sharp	Fuzzy
Non-Compliers	Y	Y	N	Y	Y	N

Notes: This table shows the robustness of the baseline estimates to alternate specification and sample choices. The unit of observation is the household. The outcome of interest in columns (1)-(3) is a binary equal to 1 if the household has a bank loan; in columns (4)-(6), the volume of bank loans. Columns (1) and (4) consider all religious minorities; columns (2) and (5) uses a sharp RD instead of a fuzzy RD specification, presenting ITT estimates based on the policy rule; columns (3) and (6) uses a sharp RD specification after excluding all the districts which did not comply with the treatment assignment rule. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Significant levels: *10%, **5%, and ***1%.
Source: AIDIS 2019.

Table C5: Directed Credit Policy and Purpose of Bank Borrowings

	(1)	(2)	(3)	(4)	(5)
	Purpose of Bank Borrowing				
	Farm	Non-Farm Business	Health Education	Housing	Consumption
Treat	-.009	-.028	.013***	.082***	.081***
	(.010)	(.019)	(.005)	(.017)	(.018)
Observations	3660	3660	3660	3660	3660
Control Mean	.026	.013	.006	.019	.047

Notes: This table shows the impact of the directed credit policy on the purpose of bank borrowings. The unit of observation is the household. The outcome of interest in columns (1)-(5) a binary equal to 1 if the household to a bank loan for the mentioned purpose. Column (1) considers farm loans; column (2) considers borrowings for non-farm businesses; column (3) considers borrowings for health and education investments; column (4) considers borrowings for real estate; column (5) considers borrowings for household consumption. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using household weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: AIDIS 2019.

Table C6: Directed Credit Policy and Credit Quality: Bank Defaults and Writeoffs

	(1)	(2)	(3)	(4)
	Default		Writeoff	
	Any Default	Default Amt.	Any Writeoff	Writeoff Amt.
Treat	-.043	486.520	.000	-24.719***
	(.030)	(449.962)	(.000)	(4.708)
Observations	983	2252	3660	3660
Dep Var Mean	.254	1513.590	.000	42.747

Notes: This table shows the impact of the directed credit policy on bank credit quality. The unit of observation is the household. The outcome of interest in columns (1) and (3) is a binary equaling 1 if the household has defaulted on any bank loan, or had a writeoff on a bank loan. The outcome of interest in columns (2) and (4) is the value of bank loan defaults/writeoffs. Rupee values are scaled by household size. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification and are weighted using household weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%. *Source:* AIDIS 2019.

Table C7: Effects of Credit Policy: Controlling for Other Potential Confounders

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Bank Credit				Consumption and Assets			
	Any Bank Loan		Loan Amt. Bank		Consumption		Assets	
Treat	.070**	.095***	5659.237***	7061.426***	.251***	.194***	.254*	.517***
	(.027)	(.029)	(1429.416)	(1695.074)	(.044)	(.042)	(.146)	(.145)
Observations	2921	3660	2921	3660	2921	3660	2918	3644
Control Mean	.174	.174	8251.054	8251.054	2593.000	2593.000	269136.285	269136.285
Minority Population	Y	N	Y	N	Y	N	Y	N
Muslim Legislators	N	Y	N	Y	N	Y	N	Y

Notes: This table shows the stability of the baseline results of the credit policy on economic well-being after controlling for the share of Muslim legislators in a district, and the share of minority population eligible for other non-credit policies at the block level in a district. The unit of observation is the household. The outcome of interest in columns (1)-(2) is a binary equaling 1 if the household has a bank loan; in column (3)-(4), the value of bank loans; in column (5)-(6), monthly household consumption; in column (7)-(8), aggregate household assets. All monetary values are scaled by household size. All specifications include state-region fixed effects, household covariates, and a linear polynomial in the running variable. The odd-numbered columns control for the share of minority population in blocks eligible for other non-credit policies under the PM's 15 point programme; the even-numbered columns control for the share of Muslim legislators representing major non-right wing parties. All specifications restrict the sample to a bandwidth of .072 , and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification and are weighted by AIDIS assigned household multiplier weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%. *Source:* AIDIS, 2019.

Table C8: Credit Access for Non-Minority Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Loan = 1)				Loan Amount (INR)			
Treat	.062***	.024	.089***	.060***	-15548.418***	2717.298	-19816.296***	-9664.610***
	(.023)	(.054)	(.020)	(.020)	(2660.877)	(2397.721)	(3274.663)	(2234.976)
Observations	18285	4926	13359	23367	18285	4926	13359	23367
Sample	Hindu	SC/ST	Non-SC/ST	All	Hindu	SC/ST	Non-SC/ST	All
Dep. Var. Mean	.199	.158	.215	.187	17325.578	5178.139	22009.641	15652.516

Notes: The above table shows the treatment effect credit access for non-minority households. The unit of observation is the household. Columns (1) and (5) show the results for all non-minorities (Hindu households); columns (2) and (6) restrict the sample to Hindu *Dalit* and *Adivasi* (SC/ST) households; columns (3) and (7) restrict the sample to Hindu non-*Dalit* and non-*Adivasi* households; columns (4) and (8) expand the sample to all households in the district. The outcome of interest in columns (1)-(4) is a dummy equaling 1 if the household has an outstanding bank loan; in columns (5)-(8), the value of the bank loan. All specifications include state-region fixed effects and household covariates, in addition to a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using household weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: AIDIS, 2019.

Table C9: Mechanisms: Collateral Requirements and Cost of Credit, Non-Minority Households

	(1)	(2)	(3)	(4)	(5)	(6)
	Any Bank-SHG Loan	Bank-SHG Loan Amt.	Any Comm. Bank Loan	Comm. Bank Loan Amt.	Any Unsec. Bank Loan	Bank Interest Rate
Treat	-.016	-35.376	.082***	-15513.042***	-.040	-.031***
	(.016)	(119.781)	(.019)	(2654.119)	(.028)	(.008)
Observations	18285	18285	18285	18285	18285	5871
Dep Var Mean	.040	368.104	.164	16957.474	.054	.107

Notes: The above table shows the treatment effect on the sources of bank credit, bank loan securitization and interest rates for non-minority households. The unit of observation is the household. The outcome of interest in column (1) is a dummy equaling 1 if the household has a loan through the bank-SHG programme; in column (2), the amount of bank loans through the bank-SHG programme; in column (3), a binary equaling 1 if the household has a commercial bank loan; in column (4), the value of loans obtained directly from commercial banks; in column (5), a binary equaling 1 if the household has an unsecured bank loan; in column (6), the interest rate faced by the household for bank loans. Average household interest rates are weighted by loan volume. All specifications include state-region fixed, household covariates, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 , and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%. *Source:* AIDIS 2019.

Table C10: Labour Market Impacts of Minority Bank Credit Access

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Pr(Outcome = 1)				Work Hours			Hourly Earnings (Log)	
	LFP	Unemployed	Self Employed	Wage Work	Total	Self Employed	Wage Work	Self Employed	Wage Work
Treat	-.001	-.005	.112***	-.108***	-3.047***	3.562***	-6.650***	.245**	-.090
	(.015)	(.005)	(.011)	(.010)	(.748)	(.869)	(.434)	(.112)	(.066)
Observations	8631	8631	8631	8631	8631	8631	8631	1513	1632
Dep Var Mean	.440	.046	.218	.176	20.687	11.095	9.529	38.380	59.925

Notes: The above table shows the treatment effect on weekly labour market outcomes. The unit of observation is the working-age individual. Working-age individuals are individuals aged between 18 and 60 years. The outcomes of interest in columns (1)-(4) are binary; in columns (5)-(7), hours worked in the week preceding the survey; in columns (8) and (9), logged hourly earnings. The outcome of interest in column (1) is whether the individual participates in the labour force; in column (2), whether the individual is unemployed; in column (3), whether the individual is self-employed; in column (4), whether the individual is engaged in wage employment; in column (5), total hours of work in the week; in column (6), total hours of self-employment work during the week; in column (7), total hours of wage work during the week. Column (8) considers self-employment earnings; column (9) considers earnings from wage work. Hourly earnings is based on hours of work during the week preceding the survey. All specifications include state-region fixed effects, individual controls, and a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Primary Labour Force Survey, 2017.

Table C11: Sectoral Labour Market Impacts of Minority Bank Credit Access

	(1)	(2)	(3)	(4)	(5)
	Pr(Self-Employment = 1)		Pr(Wage Work = 1)		
	Non Tradable	Tradable	Non Tradable	Tradable	High Hindu
Treat	.035**	-.012	-.060***	-.022***	-.042***
	(.015)	(.008)	(.010)	(.005)	(.014)
Observations	8631	8631	8631	8631	8631
Dep Var Mean	.104	.019	.109	.044	.044

Notes: This table shows the treatment effect on self-employment and wage work across different sectors. The unit of observation is the working-age individual. Working-age individuals are individuals aged between 18 and 60 years. The outcome of interest in columns (1)-(2) is a dummy equal to 1 if the individual reported being self-employed in the week prior to the survey; in columns (3)-(5), a dummy equal to 1 if the individual reported participating in wage work in the week prior to the survey. Columns (1) and (3) consider employment in non-tradable sectors; columns (2) and (4) consider employment in tradable sectors; column (5) considers employment in sectors with a relatively high (above median) share of Hindu workers. All specifications include state-region fixed effects, in addition to a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using household weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Primary Labour Force Survey, 2017.

D Appendix: Additional Tables for Political Effects and Religious Violence

Table D1: Summary Statistics: Electoral Outcomes

	N	Mean	SD
Any Muslim Candidate	8381	0.467	0.499
Muslim Candidates	8381	1.133	1.900
Any Muslim Candidate, Non-Majoritarian Party	8381	0.131	0.337
Muslim Vote Share	8381	0.067	0.162
Muslim Vote Share, Non-Majoritarian	8381	0.030	0.103
Muslim Win	8381	0.058	0.233
Muslim Win, Non-Majoritarian	8381	0.035	0.185
Turnout	8381	0.685	0.115
Candidates	8381	12.158	5.410

Notes: This table shows the summary statistics for electoral outcomes. The unit of observation is the electoral constituency, for elections conducted between 2008 and 2019. The data is sourced from the Trivedi Centre for Political Data (TCPD).

Table D2: Summary Statistics: Religious Conflicts

	N	Mean	SD
Administrative Data from NCRB			
Religious Riots	2573	1.640	9.613
Religious Riots Per Capita	2573	0.662	4.562
Non-Religious Riots	2573	128.342	168.690
Non-Religious Riots Per Capita	2573	47.780	52.014
Murder	2573	58.754	41.675
Murder Per Capita	2573	23.787	14.679
ACLED Data			
Hindu Muslim Riots	1824	0.085	0.351
Hindu Muslim Riots Per Capita	1824	0.031	0.143
Other Riots	1824	4.901	8.264
Other Riots Per Capita	1824	1.939	5.049
Hindu-Instigated Riots	1824	0.020	0.175
Muslim-Instigated Riots	1824	0.002	0.045

Notes: This table shows the summary statistics for crimes obtained from the administrative National Crime Records Bureau (NCRB) and Armed Conflict Location and Event Data Project (ACLED).

Table D3: Directed Credit Policy and Electoral Outcomes: Electoral Wins and Voter Participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Electoral Performance				Candidates and Citizen Participation		
	Major Party Vote	Major Party Win	BJP Vote Share	BJP Win	Turnout	Candidates	Registered Voters
Treat	-.025	.116**	-.117***	-.235***	.042***	-1.536**	-8915.535
	(.020)	(.059)	(.018)	(.042)	(.013)	(.673)	(8888.035)
Observations	1348	1400	1400	1400	1400	1400	1400
Dep Var Mean	.316	.312	.288	.424	.620	13.672	217885.223

Notes: This table identifies the impact of the directed credit policy on electoral outcomes for political parties and other electoral characteristics. The unit of observation is the electoral constituency. The outcome of interest in columns (1) and (3) are vote shares; in columns (2) and (4), a binary equaling 1 if a candidate won the election; in column (5) voter turnout; in column (6), number of candidates contesting, and in column (7), total registered voters. Columns (1)-(2) pertain to non-majoritarian parties; columns (3)-(4) to the right-wing Bharatiya Janata Party (BJP). All specifications include state and electoral year fixed effects, in addition to a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted using the number of registered voters in the constituency. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Trivedi Centre for Political Data (TCPD), 2008-2019.

Table D4: Treatment Effects on Candidate Assets

	(1)	(2)	(3)	(4)	(5)	(6)
	All Muslim Candidates			Muslim Candidates Major Parties		
	Assets	Liabilities	Net Assets	Assets	Liabilities	Net Assets
Treat	6.443	1.490**	4.953	57.791***	.884	56.906***
	(5.266)	(.617)	(4.846)	(15.531)	(2.075)	(13.927)
Observations	1946	1946	1946	346	346	346
Dep Var Mean	13.868	1.088	12.780	50.100	2.940	47.160

Notes: This table compares the assets and liabilities of Muslim politicians across minority and non-minority concentration districts. The unit of observation is candidates contesting elections to the state legislative assembly. Assets and liabilities are measured in millions of rupees (INR). Columns (1)-(3) consider all Muslim contestants; columns (4)-(6) consider contestants from non-majoritarian parties. All specifications include state and electoral year fixed effects, in addition to a linear polynomial in the running variable. All specifications restrict the sample to a bandwidth of .072, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD and are weighted using the number of registered voters in the constituency. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Trivedi Centre for Political Data (TCPD), 2008-2017.

Table D5: Treatment Effects on Candidate Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Muslim Candidates				Muslim Candidates Major Parties			
	Age	Any College	Criminal Cases	Any Conviction	Age	Any College	Criminal Cases	Any Conviction
Treat	.941	.112***	-.012	-.004	12.455***	.411***	1.012***	.163***
	(.847)	(.040)	(.158)	(.021)	(2.265)	(.130)	(.310)	(.046)
Observations	1929	1461	1946	1932	346	331	346	342
Dep Var Mean	43.963	.179	.601	.087	47.164	.339	.710	.170

Notes: This table compares characteristics of Muslim politicians across minority and non-minority concentration districts. The unit of observation is candidates contesting elections to the state legislative assembly. Columns (1)-(4) consider all Muslim candidates; columns (5)-(8) consider Muslim candidates contesting on behalf of non-majoritarian parties. The outcome of interest in columns (1) and (5) is the politician's age; in columns (2)-(6), a binary equal to 1 if the politician has a college degree; in columns (3) and (7), the number of criminal cases against the candidate; in columns (4) and (8), a binary equal to 1 if the politician has been convicted in a criminal case. All specifications restrict the sample to a bandwidth of .072, and are weighted using a triangular kernel. All specifications include state-region and year fixed effects, and a linear polynomial in the running variable. Specifications are estimated using a fuzzy RD specification and are weighted using the number of registered voters in the constituency. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: Trivedi Centre for Political Data (TCPD), 2008-2017.

Table D6: Directed Credit Policy and Religious Conflicts: Control for Pre-Treatment Riots

	(1)	(2)	(3)
	Crimes Per Capita		
	Religious Riots	Non-Religious Riots	Murders
Treat	.960**	-8.234*	-3.171
	(.489)	(4.353)	(3.251)
Observations	562	562	562
Dep Var Mean	.614	48.145	30.355

Notes: This table compares religious and non-religious conflicts across minority and non-minority concentration districts. The unit of observation is the district. The outcome of interest is per capita crimes for the category mentioned. Per capita crimes scales annual incidences of crime by the district's population in 2001. All specifications include a linear polynomial in the running variable, pre-treatment district-level per capita riots, state-region and year fixed effects. All specifications restrict the sample to a bandwidth of .072 around the discontinuity threshold, and are weighted using a triangular kernel. Specifications are estimated using a fuzzy RD specification, instrumenting actual treatment assignment with treatment assignment based on the policy rule. All specifications are weighted by the number of registered voters in the constituency. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%.
Source: National Crime Records Bureau, 2015-2019.