Economic and Political Consequences of Credit Policy for Minorities: Evidence from India

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Abstract

Can targeted bank credit policies improve the economic and political well-being of marginalized groups and promote social harmony in a fractured society? We examine this question by studying a unique policy intervention in India that encouraged commercial banks to increase lending to minority borrowers in "minority concentration" districts based on a random threshold of population. Using a regression discontinuity design, we identify substantial increases in minorities' access to bank credit, a higher monthly household consumption of minority households, and a reduction in the consumption inequality between minority and majority households. The increase in bank credit does not come at the cost of lower lending to non-minorities or higher rate of delinquencies among the minority borrowers. These changes in economic well-being carry on to political outcomes. Elections are more likely to have minority candidates who receive a higher share of votes cast compared to minority candidates in the non-policy districts. The policy also reduced the vote share of candidates from the main right-wing party with a prominent anti-minority platform. These electoral changes lead to an increase in violence, primarily in post-election periods suggesting a blow-back effect.

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1 Introduction

Countries often introduce policies for the economic upliftment of marginalized groups.[1]. Such policies can have a dual impact: the economic and political empowerment of the marginalized group, and the promotion of social harmony. Alternately, the economic empowerment of minorities can come at the cost of inter-group violence if the majority group feels threatened by the narrowing inequality (Mitra and Ray, 2014). This paper studies a large-scale place-based affirmative action policy in India, designed for the economic upliftment of religious minorities by increasing their access to bank credit.[2] Our paper assesses the impact of this directed credit policy on minorities' economic well-being, and whether economic empowerment leads to political empowerment and a reduction in religious conflict.

India offers an ideal context to study whether access to bank credit affect economic well-being and political empowerment for marginalized groups in a fractured society. The formal banking system is extensive, but prone to discriminatory practices (Fisman et al., 2020). India's 200 million Muslim population form the world's largest religious minority group.[3] Indian Muslims however are economically backward (Sachar, 2006; Maizland, 2020), politically under-represented (Bhalotra et al., 2014; Allie, 2024), and often victims of violently persecution (Mitra and Ray, 2014; Iyer, 2018).

We study in this backdrop the Prime Minister's New 15 Point Programme for Welfare of Minority Communities – a set of policy initiatives unveiled by India's federal government in 2009 to improve the socio-economic conditions for religious minorities – namely, Muslim, Christian, Sikh, Buddhist and Parsi communities. We focus on the policy's directive to banks to expand lending to religious minorities. While no explicit lending targets were provided,[4] the directed credit policy classified select districts as "minority concentration", and encouraged banks to expand lending to religious minorities in these districts.[5] To facilitate the selection of credit-worthy minority borrowers, the central bank repeatedly urged lenders to collaborate with local self-help groups (SHGs). Indian commercial banks

[1] For example, the Freedmen's Bureau Act of 1865 was designed to uplift newly emancipated Black people (Chyn et al., 2024). Similarly, many place-based policies are aimed at developing economically distressed areas, and all the people living or operating there (Neumark and Simpson, 2015).

[2] A large body of literature has documented welfare-enhancing aspects of credit access through consumption smoothing, investments in human capital, entrepreneurship, and labor market outcomes (Kaboski and Townsend, 2012; Augsburb et al., 2015; Buera et al., 2020; Cramer, 2021; Breza and Kinnan, 2021; Aydin, 2022). However, access to credit is not uniform across individuals and households (Blanchflower et al., 2003; Fisman et al., 2020; Brock and de Haas, 2023).

[3] Population Census 2001.

[4] The only hard requirement was that banks were mandated to file half-yearly reports on the quantum of credit allocated across minority groups in each minority concentration district.

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

are required to allocate at least 10 percent of their lending portfolio to "weaker sections" – namely women and historically marginalized citizen groups[6]. The central bank expanded the definition of "weaker sections" to include religious minorities, offering banks a larger pool of borrowers with which to meet their regulatory target.

For causal identification, we exploit the administrative criteria used to classify districts as "minority concentration". Specifically, districts where the share of religious minorities exceeded 25% of the district population were deemed as "minority concentration" (RBI, 2007). The use of an arbitrary threshold to classify districts into treatment (minority concentration) and control (non-minority concentration) lends itself to causal identification using a regression discontinuity (RD) design (Lee and Lemieux, 2010). Importantly, data from the 2001 population Census was used to classify districts as "minority concentration" in 2007, making it unlikely for districts to strategically sort themselves around the discontinuity threshold. We verify the absence of selective sorting around the discontinuity threshold using the McCrary test (McCrary, 2008), and also confirm balance across pre-treatment household and district characteristics. This makes minority households in non-minority concentration districts a valid counterfactual for minority households in minority concentration districts.

We first identify the impact of the directed credit policy on minority credit access and economic well-being. Our empirical analysis uses data from the All India Debt and Investment Survey (AIDIS) – a nationally representative household survey undertaken decennially by the National Sample Survey Organisation (NSS). We use the AIDIS conducted in 2019 to identify the long-term impacts of the directed credit policy; the 2003 AIDIS survey is used to check for pre-treatment balance. Descriptively, the 2003 AIDIS indicates credit rationing for Muslim households. These households have substantially lower access to bank credit, receive smaller bank loans, face higher rates of interest in informal credit markets, and have significantly lower values of household savings and pledgeable assets.[7] An Oaxaca-Blinder decomposition indicates that unobservables explain 40 percent of the difference in bank credit access across minority Muslim and non-minority households. The descriptive evidence, combined with the fact that Muslims comprise over 80 percent of religious minorities in India, leads us to focus on Muslim households as the primary unit of analysis.

[6] Historically marginalized citizen groups refer to the Scheduled Castes (SCs) and Scheduled Tribes (STs) who have faced centuries of social discrimination and have been denied access to public goods and services.

[7] Financial outcomes for non-Muslim religious minorities were comparable to relatively privileged Hindu "forward caste" groups.

We compare minority Muslim households across minority concentration and non-minority concentration districts within a narrow window of the discontinuity threshold. The sharp RD design identifies a statistically significant 12-18 percentage point (ppt.) increase in bank credit access for minority households in minority concentration districts. As the average control district had 1.6 million minority households, 11 percent of whom had some outstanding bank loan, the coefficient implies increased access to bank credit for an additional 0.3 million minority households. We also identify corresponding positive treatment effects along the intensive margin: the average minority household in minority concentration districts witnessed a INR 20,000 increase in the amount of bank loans received – equivalent to 13 percent of minority household consumption.

Our preferred sample is a fixed set of 61 districts located within a bandwidth of 0.058 around the discontinuity threshold. We verify robustness to using data-driven outcome-specific MSERD bandwidths, and also show the baseline results to be invariant to a number of alternate bandwidths between .04 and .10. The treatment effects are very similar when extending the sample to non-Muslim religious minorities. Our results are unaffected when estimated using a fuzzy RD specification to address the issue of non-compliance in treatment assignment for 18 districts (out of 121).[8]

An often raised concern about affirmative action policies is that the cost of expanded opportunities for minorities are borne by non-minority groups (Holzer and Neumark, 2000; Marion, 2009). Comparing non-minority households across treatment and control areas, we find limited evidence of a negative treatment effect on bank credit. Instead, we show that the treatment resulted in a significant reduction in informal borrowings for non-minority households, accompanied by increased borrowings from non-bank financial institutions (NBFIs). This is matched by a reallocation of minority borrowings from NBFIs to banks, which possibly freed up NBFI credit for non-minorities. Resultantly, as opposed to the directed credit policy crowding-out non-minorities from bank credit, our results points to the treatment's potentially positive impact on non-minorities' access to NBFI credit.

Directed lending policies have also been critiqued for generating credit market inefficiencies by allocating credit to non-creditworthy borrowers. For instance, if banks held private information regarding limited repayment abilities of minority borrowers, we would expect higher defaults following the directed credit policy. Using self-reported loan repayment data, we find bank loan delinquency to

[8] As no rationale is provided on the inclusion of these districts, we omit them from the main analysis and use a sharp RD specification.

be comparable across minority and non-minority concentration areas. This assuages concerns that the directed credit policy led to a worsening in loan performance by forcing banks to lend to sub-optimal borrowers.

To gauge the impact of the directed credit policy on overall minority well-being, we analyze its impact on household consumption. Relative to minority households in control districts, minority households in treated districts had 13 percent higher per capita monthly consumption. There is no statistically distinguishable difference in non-minority consumption across treated and control areas, although the point estimate is negative. Pooling our sample of minority and non-minority households and comparing households in the same district, we find minority households in treated districts to have 19 percent higher monthly per capita expenditures. As the conditional consumption gap between minorities and non-minorities in control areas equaled 27 percent, our findings suggest that affirmative action in credit markets reduced the minority-non-minority consumption gap by 70 percent.

We consider three mechanisms to explain the increase in minorities' access to bank credit. First, in light of the central bank's recommendations to banks to collaborate with local SHGs, we identify whether the treatment affected lending from "bank-linked" SHGs. Credit to bank-linked SHGs are loans issued directly by commercial banks to SHGs, with the SHG internally selecting the final recipient. Disaggregating bank loans across loans received directly from commercial banks and through bank-linked SHGs, we find up to 60 (20) percent of the extensive (intensive) margin increase in minorities' bank credit access to be accounted for by bank-linked SHGs. Second, as minorities have fewer pledgeable assets, we examine whether banks relaxed collateral requirements while lending to minority borrowers. Indeed, we find a sizable positive treatment effect on the likelihood of minority households to have an unsecured bank loan. There is however limited evidence of banks lowering interest rates in response to the directed credit policy – while the point estimate for commercial bank interest rates is negative, it is imprecisely estimated. The rise in unsecured bank lending in the absence of a corresponding increase in the cost of credit or delinquency point to improvements in banks' information gathering regarding minority borrowers (Fisman et al., 2017).

Having established that the directed credit policy improved religious minorities' access to bank credit and household consumption, we next study whether economic empowerment translated to political empowerment. Using data on elections to state legislative assemblies from the Trivedi Center for Political Data (TCPD), we compare the participation of minority candidates in the electoral process

across treated and control districts. We also consider electoral support for the Hindu nationalist Bharatiya Janata Party (BJP) which has stridently opposed any preferential treatment for religious minorities.

Our sharp RD framework identifies an 17.8 percent increase in the likelihood of Muslim politicians contesting state elections in minority concentration districts. These contestants also witnessed a 4.3 ppt. rise in their vote shares. Muslim candidates are also more likely to secure nominations from major parties, although the point estimate is not statistically significant (p-val .144). This is accompanied by a 5.9 ppt. decline in BJP vote shares in treated districts, equivalent to a 15 percent reduction in popular support for the BJP. The decline in BJP support cannot be attributed to a mechanical increase in the number of Muslim candidates, as the vote share of Muslim candidates representing major parties also increase by a statistically significant 4.1 ppt. There was also a statistically significant 7 ppt. increase in Muslim candidates' likelihood of winning elections when contesting on major party platforms, pointing to an overall increase in minority representation in legislative bodies. Overall, our findings point to higher political empowerment in districts eligible for the directed credit policy: Muslim politicians were more likely to participate in the electoral process, received greater electoral support, and had a higher win likelihood. There was also a corresponding reduction in support for the party inimical to minority interests.

Improvements in the economic status of religious minorities can lead to political empowerment through a number of pathways. First, the directed credit policy could have generated a positive wealth effect for minorities, allowing them to overcome entry costs to run for office. The positive wealth effect could also have allowed minority citizens to financially aid parties which were better representing their interests. Second, societal harmony could have improved due to greater social interactions across communities through exchanges via social institutions or economic markets. Third, within-group co-ordination amongst minorities could have increased through the formation of homogeneous SHGs to access bank loans, or greater interactions in the economic sphere.

Empirically we find evidence consistent with the directed credit policy generating a positive wealth effect for Muslim politicians. Using data on politicians' public affidavits, we find Muslim contestants in treated districts to have significantly higher assets. Candidate quality (years of education or experience) for Muslim politicians however remained comparable across treated and control districts. There is limited evidence of increased inter-group interactions. We scrape information from the National Rural

Livelihood Mission (NRLM) which lists SHG members at the time of group formation, along with their caste and religion. For SHGs registered with banks after 2008, the likelihood of a SHG having both Hindu and Muslim (or other minority) members is unaffected by the treatment. Instead, the likelihood of SHGs being formed with exclusively Muslim (religious minority) members is significantly higher in minority concentration areas. There is also no evidence of greater Hindu-Muslim engagement in the labor market. While the directed credit policy caused Muslim workers to shift from trade and service jobs to manufacturing work, there is little evidence of these workers being employed in sectors with a relatively higher share of Hindu workers. Consequently, instead of increased social interactions with non-minorities, the concentration of Muslim workers in manufacturing activities dominated by Muslims, and the positive treatment effect on the formation of exclusively minority SHGs point to improved co-ordination across minority voters. This, alongside a positive wealth effect form the two key channels linking minorities' economic empowerment to political empowerment.

As our RD design restricts the sample to districts within a narrow window around the discontinuity threshold, there are no districts where minorities constitute in excess of a third of the population. However, residential segregation along religious lines could have led to the clustering of minorities in select areas of districts. Using data on the share of Muslim voters in each constituency from Gulzar et al. (2024), we explore whether our electoral findings are driven by constituencies with a low or high share of Muslims. This allows us to examine whether minority voters were acting in unision to punish right-wing candidates and support parties better representing their interests. Disaggregating our sample and applying the RD specification, we find the decline in BJP vote shares to emerge from constituencies with a relatively low (below median) share of Muslims. High participation of Muslim politicians in the electoral process and the rise in popular support is observed in both high and low Muslim areas. The findings are consistent with the explanation that co-ordination problems were more acute in areas with a low concentration of religious minorities. The directed credit policy by encouraging the formation of SHG groups was likely to have facilitated social interactions between minorities, which in turn improved the ability of minority voters to better co-ordinate and strategically vote against the party inimical to their interests.

Finally, we examine whether the directed credit policy affected religious conflict. This is particularly relevant in the context of India, which has a long history of sectarian Hindu-Muslim violence (Varshney, 2003). Both economic and political empowerment can affect inter-group conflict. Economic prosperity

can increase the opportunity cost of violence, while political empowerment can improve communities' influence over law enforcement authorities. Alternately, Mitra and Ray (2014) have shown the lowering of inequality across religious groups to increase religious conflict as the majority group's attempts to reassert their dominance through targeted violence.

We use data from two sources to compare religious conflicts across minority and non-minority concentration areas. First is the Armed Conflict Location and Event Data (ACLED) between 2016 and 2019, which uses newspaper reports to provide details on violent religious conflicts (Raleigh et al., 2010). We also use administrative data between 2014 and 2019 from the National Crimes Records Bureau (NCRB) to validate the results on conflict. Using both data sources, we identify a significant increase in the number of religious riots in treated districts. Using the NCRB data, we show that other crime heads such as total riots (religious or otherwise), and murders remained comparable across treated and control districts. This assuages concerns that the positive impact on religious conflicts emanated from a secular increase in criminal activity in minority concentration areas.

Did the preferential treatment of religious minorities in credit markets worsen social harmony? Or does the spike in religious violence emerge as a backlash to minority political empowerment. We examine this by locating the precise timing of religious violence. If resentment against minorities stemmed from their economic empowerment, we would expect increased violence both before, and after the elections. The same would be true if non-minorities were using violence as a tool for reasserting their dominance in response to lower inter-group inequality. 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 the violence is observed only after the elections, it is likely an electoral backlash to the rising political empowerment of minorities. Disaggregating the ACLED data into the post (12 months after) and pre-electoral periods (12 months before), we identify no increase in Hindu-Muslim violence prior to the elections, but a significant rise in religious conflicts following the elections. This suggests that the violence was strategically timed as a mechanism to punish minority voters voting against majoritarian politics.

To the best of our knowledge, ours is the first paper to document how the preferential treatment of minorities in credit markets can improve minority well-being, reduce inter-group inequality, and facilitate minorities' political empowerment by improving within-group co-ordination. Existing literature has shown credit access to improve welfare (Breza and Kinnan, 2021), and political empowerment to

emerge from affirmative action quotas in legislative bodies (Pande, 2003; Chattopadhyay and Duflo, 2004; Besley et al., 2017; Gulzar et al., 2021). Ours is the first to argue that economic empowerment through improved credit access can also enhance the political standing of marginalized groups in a fractured society. Further, by showing that these improvements come at the cost of rising violence – a blow-back against minority empowerment – we add to a nascent literature exploring how policies meant to uplift the marginalized can elicit a violent pushback from the majority (Chyn et al., 2024) and political polarization (Avenancio-León et al., 2024; Choi et al., 2024). This increase in violence can be motivated by an attempt by the majority to preserve economic hierarchy (Mitra and Ray, 2014); or it can be part of a strategic calculus to prevent minority participation in politics (Wilkinson, 2006; Condra et al., 2018; Iyer and Shrivastava, 2018). Our paper offers an additional explanation to rising violence following economic empowerment: the punishment of minorities following a weakening of majoritarian politics.

Our paper also provides evidence of the effectiveness of an at-scale implementation of affirmative action in credit markets. While affirmative action has been extensively studied in labor (Leonard, 1990; Holzer and Neumark, 2000; Miller, 2017), residential (Chetty et al., 2016), and political (Pande, 2003; Chattopadhyay and Duflo, 2004; Jensenius, 2015; Bhavnani, 2017; Gulzar et al., 2021) markets, we explore a unique setting where an affirmative action policy was implemented in credit markets through formal banking channels, and targeted towards an otherwise excluded marginalized community.[9] Our paper forwards a nascent literature on the accessibility of formal bank credit to minorities. On similar lines, existing studies have highlighted the importance of minority representation in banks (see, for example, Fisman et al. (2017) and Frame et al. (2017)) as a potential avenue for mitigating the adverse effects of discrimination. To the best of our knowledge, ours is the first to study affirmative action through a government mandated expansion in access to formal credit for under-represented groups.

A critique of affirmative action arises from concerns regarding adverse negative effects (Sowell, 2004; Agan and Starr, 2018). These can manifest either through a mismatch between the expected beneficiaries of the policy, and the ex-post recipient group: for instance, matching students with schools (Barrow et al., 2020). Alternatively, affirmative action could come at the cost of a crowd out of non-minority beneficiaries (Arcidiacono et al., 2022). Our paper shows that financial affirmative

[9] A small but strong strand of literature has documented the existence of race-based discrimination in credit markets in the United States (Blanchflower et al., 2003), gender-based discrimination in Turkey (Brock and de Haas, 2023), and religion-based discrimination in India (Fisman et al., 2020).

action does not necessarily crowd-out non-minorities. Instead, banks' collaboration with SHGs to reach minority borrowers alleviated information frictions and improved the targeting of credit-worthy borrowers in under-represented communities.

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

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.[10] Collectively, citizens from these religious denominations accounted for 19 percent of India's population in 2001, with Muslims comprising the largest group of 13 percent or 138 million individuals. This makes India's Muslim population the largest religious minority group in the world.

The policies covered the realms of education, employment, infant health, housing, sanitation, access to credit, and protection from targeted discrimination and violence. The overarching policies were framed by the federal government and implemented through various public agencies, with financing coming from the federal exchequer. The initial set of policies were revised and expanded in 2009, and renamed as the Prime Minister's New 15 Point Programme for the Welfare of Minority Communities.

Access to credit under the PM's minority welfare programme aimed at ensuring the "smooth flow of bank credit to minority communities" from state-owned and private commercial banks (RBI, 2007). The central bank – the Reserve Bank of India (RBI) – was tasked with framing the regulations for credit allocation to religious minorities, and also responsible for overall monitoring. The RBI subsequently issued a set of guidelines to commercial banks in July 2007, notifying that the federal government had classified a set of 103 districts (out of 593 districts) as "minority concentration" districts (RBI,

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

2007).[11] These districts were so deemed as the population share of religious minorities (as per the 2001 population census) exceeded 25% of the district's population.[12] Commercial banks were instructed to specifically monitor credit flow to minority borrowers within these 103 districts. Within a few months of the initial notification, the federal government expanded this list by adding 18 more districts, bringing the total number of minority concentration districts to 121. No subsequent additions or deletions were made to this set. As no rationale was provided for the inclusion of these 18 additional districts, we omit them from our analysis and focus exclusively on the preliminary set of 103 districts which perfectly complied with the policy rule.[13]

2.2 Monitoring by Central Bank

The RBI in 2007 issued a set of instructions to achieve banks' compliance with the directed credit policy for religious minorites (RBI, 2007). Almost all of these instructions remain in effect at the time of writing and are reiterated through annual notifications issued by the RBI (RBI, 2021).[14] First, each bank was instructed to set up a special division, headed by a senior officer to provide oversight in relation to the policy. Second, the "lead bank" in each minority concentration district was tasked with assigning a senior officer whose sole responsibility was to look into challenges faced by religious minorities in accessing credit.[15] This officer was also tasked with generating local awareness amongst minority communities regarding the policy and other related government schemes. The officer was also expected to design credit schemes to fulfill the objectives of the directed credit policy, in collaboration with other branch officers in the district (RBI, 2007).

Lead banks were also directed to co-ordinate with other non-banking financial corporations in an effort to reach creditworthy borrowers in minority communities. This included issuing advertisements

[11] 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. The policy applied to Hindus in these areas, with minority concentration districts being those where Hindus formed 25 percent of the district's population. We omit districts from these areas in our analysis.

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

[13] In addition to these 18 districts, the government also classified 4 districts whose share of minority population was between 24.6% and 24.9% as minority concentration. It is possible that the government was rounding off the minority share during treatment assignment. However, as no rationale is provided for the same, we omit these 4 districts too from the analysis.

[14] These are issued by the Financial Inclusion and Development Department (FIDD), responsible for promoting financial inclusion.

[15] The central bank in each district assigns a "lead bank" to facilitate rural banking. The lead bank co-ordinates with other commercial banks in the district on matters of financial inclusion and credit disbursement to farm activities, as well as farm and small enterprises. Lead banks are state-owned banks.

across print and visual media, and engaging in information campaigns at the site of religious congregations (RBI, 2007). The RBI also recommended banks to engage with self-help groups to improve their selection of underprivileged minority borrowers, and lead banks in minority concentration districts were expected to be "proactive" in this regard (RBI, 2007). Both the lead bank, and individual banks operating in minority concentration districts were advised to impart adequate training to sensitize employees to the credit needs of minority borrowers. Finally, banks were mandated to file half-yearly reports with both the RBI and the federal Ministry of Welfare, detailing the disbursement of credit to minority borrowers. This bi-annual reporting forms the sole tangible monitoring of the policy undertaken by the central bank (RBI, 2007).

2.3 Incentives for Banks

The primary incentive for banks to comply with the directed credit policy is that lending to minority borrowers would allow them to meet key regulatory targets. First, every bank operating in India is required to allocate at least 40 percent of its aggregate annual loan portfolio towards the "priority sector" – farm credit, rural borrowers, and credit to small and micro-enterprises (RBI, 2020).[16] Banks are also mandated to direct 10 (presently 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. This was inclusive of both personal loans made to minority borrowers, as well as loans made to non-registered enterprises owned by religious minorities.[17] While no explicit targets were assigned, the RBI's annual guidelines directed banks to ensure that minority borrowers were "adequately represented" within weaker sections RBI (2007, 2021).

Second, since 1991, loans for housing, education, renewable energy and self-help groups also qualified under the priority sector. As the RBI's directives explicitly encouraged banks to lend to SHGs in an effort to reach out to minority borrowers, banks could utilize lending to minority-concentrated

[16] Banks unable to meet this are expected to contribute the residual amount to a rural infrastructure development fund.

[17] In the event of the firm being a partnership, it qualified towards minority credit as long as the majority of partners hailed from minority groups. However, registered companies, by virtue of being a separate legal entity were ineligible to receiving credit under this scheme, irrespective of the type of ownership.

SHGs to achieve their priority sector targets. This is particularly salient in light of the bank-SHG linkage programme unveiled in 2008, which encouraged banks to directly lend to SHGs. Consequently, the directed credit policy for religious minorities also offered banks a larger pool of potential borrowers to meet their priority sector targets.

3 Data

This section describes the key datasets used for the empirical analysis in the paper. Additional datasets used are described during the discussion of the results.

3.1 All India Debt-Investment Survey

The primary dataset used is the All India Debt-Investment Survey. The AIDIS is a nationally representative survey, conducted decennially by the National Sample Survey Organisation (NSS) in the form of repeated cross-sections. The survey samples in excess of 100,000 households and collects extensive information on household balance sheets, including borrowings, savings and ownership of select productive assets. Locational identifiers in the form of districts are also provided, in addition to the household's caste, religion, and demographic details such as household size and educational qualifications. We focus on the borrowing component of the AIDIS, which is a loan-level data set on all outstanding loans for the household at the time of survey. For each outstanding loan, the AIDIS provides information on the initial amount borrowed, the year in which the loan was taken, amount outstanding on the date of survey, whether the loan was secured by any collateral, the source of credit, and interest rate charged. Repayment information over the past 6 months is also provided.

The AIDIS was conducted in the years 1992, 2003, 2013 and 2019. As qualitative impact evaluations undertaken by the government reported poor implementation of the policy up to 2015, we use the 2019 AIDIS survey to estimate the impact of the directed credit policy on lending outcomes. This implies that we are estimating long-term treatment effects, in equilibrium. The 2003 AIDIS survey is used to verify balance along household characteristics and outcomes of interest in the pre-treatment period.

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 of households received loans from a bank-linked SHG. Almost 20 percent of households had credit from informal sources, which includes

professional money lenders, input suppliers, friends, relatives, employers and landlords. Households primarily borrowed to finance various expenditures (17 percent), while only 4 percent of households reported taking loans for non-farm businesses. Farm loans were reported by 13 percent of households.

We use initial loan size to measure intensive margin responses to the directed credit policy.[18] Appendix Table C1.C shows that conditional on having some outstanding loan, average aggregate household debt almost equaled INR 200,000 – approximately 1.4 times aggregate annual household consumption. The sum of loans obtained directly from commercial banks was larger – almost INR 250,000. Expectedly, loans from bank-linked SHG were smaller, amounting to less than INR 50,000. Informal loans from money lenders equaled almost INR 120,000. Loan volumes were largest for non-farm business loans, followed by expenditure loans. Appendix Table C1.D shows that the majority of households had at least one unsecured loan, primarily from informal sources. 40 percent of households reported having at least one unsecured bank loan. The majority of loans were of a long-term nature. The average annual interest rate faced by households was 14%, with banks charging a lower rate of interest (12%) than money lenders (30%).[19]

Absence of administrative data on repayments makes it challenging to assess loan delinquency and borrower quality. Additionally, as the AIDIS collects information solely on outstanding household loans, it is biased towards capturing delinquencies as such loans continue to remain on a household's balance sheet. Estimates of borrower delinquency from the AIDIS should thereby be interpreted with caution, and are likely to provide an upper bound of loan delinquency. We use information on repayments made between June 30, 2018 and the time of survey to measure loan delinquency. As households were surveyed in 2019 and 2020, non-repayment since June 30, 2018 implies the loan being delinquent for at least 6 months.[20] Nonetheless, Appendix Table C1.D shows high self-reported loan delinquency, with almost 35% of households reporting at least 1 outstanding loan on which no repayment was made over the past 6 months. In line with the risk-averseness of banks, the delinquency for bank loans (26%) was lower than loans sourced from informal sources (41%).[21]

[18] Initial loan volumes offer an accurate measurement of the credit extension, unaffected by capitalized interest and debt accumulation due to non-repayment.

[19] Household-specific interest rates are computed as the loan volume weighted average interest rate across all outstanding loans.

[20] As the 2019 AIDIS only informs us of the year in which the loan was obtained and not the month, we are unable to obtain delinquency measures for a number of loans obtained in the years 2018 and 2019.

[21] The vast majority of informal loans from friends and relatives were zero-interest loans, which probably did not require any regular repayments to be undertaken. Regardless, we classify these loans too as delinquent if no repayment was made in the past 6 months.

3.2 Election Data

The Trivedi Center for Political Data (TCPD) compiles data on every election to state legislative assemblies held in India. The data includes names of each candidate, their party affiliation, and the votes received, along with the number of registered voters, and voter turnout. We use elections conducted between 2008 and 2019, and complement it with information from mandated disclosures of candidates pertaining to their incomes and assets. We append to this information from Gulzar et al. (2024) who estimate the share of minority Muslim voters in each electoral constituency based on the names of registered voters in the voter list.

To identify whether the directed credit policy affected the political empowerment of religious minorities, we compare the participation of Muslim politicians in state legislative assemby elections, and the vote share received by Muslim candidates. To ascertain the religious identity of politicians, we rely on the machine learning algorithm developed in Chaturvedi and Chaturvedi (2024) and validated by Allie (2024).[22] As a third measure of minority political empowerment, we also identify the impact of the directed credit policy on popular support for the right-wing Hindu nationalist BJP. The BJP has been steadfastly critical of any positive discrimination for religious minorities, and overall inimical to minority interests (Jaffrelot, 2021; Das, 2024).

3.3 Conflict Data

To ascertain whether the directed credit policy affected religious conflicts, we access data from the Armed Conflict Location and Event Dataset (ACLED) (Raleigh et al., 2010). ACLED covers conflict events in India from 2016, and we map these events to districts in our sample. Using the description of events and actors involved, we classify riots involving the majority (Hindus) and minority (Muslims) populations. We supplant this analysis with district-level administative data from the National Crimes Records Bureau (NCRB), which separately reports communal (religious) riots from 2014.

3.4 Barriers to Credit Access for Minorities: Pre-Treatment Descriptives

Descriptive evidence in Appendix A using the 2003 AIDIS survey offers suggestive evidence that amongst religious minorities, Muslim households faced substantial barriers to credit access. Relative to

[22] This algorithm has been specifically developed for India.

other minority communities, these households also had significantly lower financial assets. Not only did Muslim households have a lower likelihood of having a bank loan, conditional on having a bank loan, Muslim households had substantially lower loan amounts. Bank interest rates however were comparable across all communities, which suggests that lending to Muslim households did not entail higher credit risk. These households however were charged the highest interest rates by professional money lenders, who were the primary source of credit in the pre-treatment period.

Diminished access to bank credit could be a function of lower credit demand from Muslim households, or lower credit supply by lenders. In Appendix A.1.2, we use an Oaxaca-Blinder decomposition using a rich set of pre-treatment household covariates to provide suggestive evidence of discrimination, particularly along the extensive margin. The top panel of Appendix Figure A2 indicates that only 40 percent of the extensive margin difference in bank credit for Muslim households – relative to Hindu households – can be explained by household observables. We return in Section 5.1.4 to assess whether there is evidence of statistical or taste-based discrimination against Muslim households in India's formal credit markets.

Taking cognizance of the descriptive evidence documenting the exclusion of Muslim households from formal credit markets and the fact that they account for 80 percent of India's religious minority population, our paper primarily focuses on the impact of the directed credit policy on financial outcomes for Muslim households. For the remainder of the paper, unless explicitly stated, we use the term "religious minorities" or "minorities" to exclusively refer to Muslim households.

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 C1 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. Using the first list of minority concentration districts issued by the RBI in 2007, we confirm that all districts which satisfied the treatment assignment condition were indeed assigned to treatment (RBI, 2007). Formally, Figure 1 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 alleviates concerns of any strategic sorting of districts around the discontinuity threshold.

We exploit the sharp discontinuity in treatment assignment to estimate local linear regressions:

$$Y_{hds} = \alpha_s + \beta Treat_{ds} + \gamma f(Runvar_{ds}) + \delta \mathbf{X}_{hds} + \epsilon_{hds} \tag{2}$$

The unit of observation in equation (2) 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, based on the assignment rule described above. 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 treatment (minority concentration) and control districts (non-minority concentration). All our specifications include state fixed effects (α), household size and binary indicators for rural location and marginalized caste groups. 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. 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 use a fixed sample of 61 districts located within a bandwidth of .058 around the discontinuity threshold.[23] This bandwidth is chosen using data-driven optimal bandwidth selection procedures recommended by Calonico et al. (2020) (see Section 5.1.1 for details). Appendix E shows robustness to outcome-specific MSERD

[23] Out of these, 23 are minority concentration, and the rest non-minority concentration.

bandwidths recommended by Calonico et al. (2020).

A causal interpretation of the RD coefficients is subject to the assumption that pre-determined covariates were continuous in the running variable at the discontinuity threshold. We verify this using pre-treatment data on household, district, and individual worker characteristics. Appendix Tables B1-B9 report a statistically significant difference for only 4 of the 82 pre-treatment observables.[24] Importantly, Appendix Table B5-B8 shows statistically indistinguishable levels of credit access across bank and non-bank sources for minority households in treated and control districts.[25] Pre-treatment cost of credit and delinquency rates were also comparable. Appendix Table B10 and B11 shows comparable levels of pre-treatment district financial infrastructure and bank loans across minority and non-minority concentration districts.[26] The absence of selective sorting of districts into treatment and control status (Figure 1), combined with the overall balance of pre-treatment household and district characteristics across treated and control districts (Appendix Tables B1-B9) allow us to assign a causal interpretation to the RD coefficients estimated using equation (2).

5 Results: Minority Credit Access and Economic Empowerment

This section presents the key findings of our paper on economic well-being. We first examine how the directed credit policy affected credit access for minority households. We next identify how credit access affected household assets and consumption. We conclude by assessing three potential mechanisms through which the directed credit policy improved credit access for religious minorities.

5.1 Access to Credit in Minority Concentration Districts

5.1.1 Baseline Results

Figure 2 graphically compares bank credit access across minority households in treated and control regions. All specifications include state fixed effects and household covariates. The sample is restricted

[24] The 2003 AIDIS is used to compare pre-treatment household assets and liabilities. District-level data from the administrative BSR in 2007 is used for banking outcomes, while household survey data from the 2007-08 employment-unemployment survey is used to check balance on labor market outcomes.

[25] All balance checks are conducted for households in 61 districts located within a bandwidth of .058 around the discontinuity threshold. Akin to the main specification, balance checks are conducted after controling for state fixed effects, household size, and a dummy for the household's rural location.

[26] Owing to the small sample size (61 districts), these are unconditional balance checks.

to households in 61 districts located within our preferred bandwidth of 0.058.[27] The horizontal axis shows the running variable. Each point in the figures show the residualized outcome variable mean corresponding to each bin. The solid lines show a smoothened linear polynomial fit, while the broken lines depict 95% confidence intervals. Observations are weighted using a triangular kernel. The left panel shows a sharp jump in minority households' likelihood of receiving a bank loan as one moves to the right of the discontinuity threshold. This jump is also visible in the right panel, albeit not as pronounced. Visually, there is a higher density of points to the right of the discontinuity threshold in both figures, pointing to a positive treatment effect.

Table 1 shows local linear regression estimates corresponding to Figure 2. The outcome of interest in columns (1)-(3) is a dummy equaling 1 if the household had a bank loan; in columns (4) and (5), the volume of bank loans. Columns (1) and (4) include no covariate except for the state fixed effects; columns (2), (3) and (5) control for household size, a rural dummy, and dummies for whether the household belongs to a marginalized caste group. Except for column (3), all specifications use Calonico et al. (2020) reported MSERD outcome-specific optimal bandwidths. Column (1) identifies a positive and statistically significant treatment effect, which increases in magnitude upon the inclusion of covariates in column (2). Compared to observationally equivalent minority househols in control districts, minority households in treated areas saw a 12-20 ppt. increase in the likelihood of having an outstanding bank loan. Column (4) identifies a INR 20,000 increase in the amount of bank credit issued to minority households in minority concentration districts. The coefficient increases slightly upon the inclusion of covariates, amounting to a INR 21,000 increase in bank credit. The penultimate row of Table 1 shows that the optimal bandwidth along the extensive margin without (with) controls is .055 (.047); along the intensive margin, the bandwidth without controls is .063, and falls to .058 upon the inclusion of controls. To maximize sample size, we opt to use .058 as our preferred bandwidth in all specifications. Using the bandwidth of .058, column (3) shows that minority households saw an 18 ppt. increase in bank credit access in treated districts.

The point estimates are large when compared to the outcome mean in control districts. In non-minority concentration districts around the discontinuity threshold, 12 percent of minority Muslim households had some outstanding bank loan, while the average bank loan amount equaled INR 20,801. Our preferred specification in columns (3) and (5) show that the policy doubled credit access for

[27] We exclude the district of North Cachar Hills where all Muslim households report having a bank loan.

minority households along both the extensive, and the intensive margins. As the average control district had 1.6 million Muslim households, the extensive margin coefficient equates to bank credit access for an additional 0.28 million Muslim households in treated districts. Since the AIDIS only collects information on outstanding loans, the treatment effects are likely to capture a lower bound of the directed credit policy's impact on bank credit access for minorities.

As the RD design compares households within a narrow window of the discontinuity threshold, a possible concern with the RD estimates in Table 1 is whether they emanate from an increase in bank credit for religious minorities in treated districts, as opposed to a reduction in bank credit for religious minorities from non-treated districts. The latter in particular would be true if banks complied with the directed credit policy by reallocating credit issued to religious minorities from control districts. While the nature of the RD design makes it challenging to conclusively negate this explanation, we provide one descriptive statistic to indicate that the treatment effects reflect an actual expansion of bank credit to religious minorities, as opposed to a within-minority reallocation across districts. Based on the 2003 AIDIS survey, Muslim households accounted for 7 percent of total bank loans issued in non-treated districts.[28] In the 2019 AIDIS, Muslim households accounted for 8.8 percent of overall bank credit. Resultantly, in a purely descriptive sense, the share of bank credit to minority Muslim households in non-minority concentration areas remained unaffected by the directed credit policy.[29]

In addition to bank credit, the AIDIS provides an extensive break-up of credit from other institutional and non-institutional sources. We use this to unpack whether the substantial increase in bank credit access for minority households in minority concentration areas reflects an overall expansion in credit access. Specifically, as banks are the cheapest source of credit, we would expect households to substitute non-bank credit with bank credit in the absence of credit constraints.[30] Alternately, if minority households faced binding credit constraints, access to cheaper sources of credit should result in an expansion in aggregate household debt (Banerjee and Duflo, 2014).

Appendix Table C5 empirically examines this by identifying the treatment effect across two other

[28] Non-treated districts exclude districts in states exempted from the directed credit policy, where religious minorities comprised of a population majority. We also exclude the 18 districts which were later classified as minority concentration, despite not satisfying the 25 percent minority share cutoff.

[29] The results are unchanged if we redo the analysis after excluding control districts within a bandwidth of .058 of the discontinuity threshold. Muslim households accounted for 4.4 percent of total bank credit in the pre-treatment period, which rose slightly to 4.7 percent in the 2019 AIDIS.

[30] Commercial bank lending rates were 1 percentage point lower than co-operative bank lending rates, and 5 percentage points lower than rates charged by non-banking financial corporations.

sources of household borrowing: namely, informal loans and credit from non-bank financial institutions (NBFIs).[31] While not always precisely estimated, the point estimates in Appendix Table C5 point to a reallocation in household borrowing across credit sources. Columns (2) and (5) reproduce our primary result, identifying significant increases in minority households' access to bank credit along both the extensive, and intensive margins. This is accompanied by a reduction in credit from NBFIs: minority households in treated districts are 14 ppt. less likely to have an outstanding loan from these sources. The intensive margin coefficient indicates a INR 10,000 decline in NBFI loans (p-value .183). While column (1) shows overall credit access to be unaffected by the treatment, column (5) suggests a INR 22,000 increase in aggregate household debt. While not statistically significant (p-value .221), the point estimate is large and reflects a 49 percent increase in household credit for minority households. In addition to higher bank loans, higher household debt emanates from a statistically non-significant increase in informal loans (column (7)).

Appendix Table C6 disaggregates informal loans into those from professional money lenders, and community networks, such as friends and relatives.[32] Pre-treatment descriptives in Appendix Figure A1 show that the former comprise of relatively expensive sources of credit, while the latter are mostly interest free loans.[33] Columns (1) and (2) of Appendix Table C6 suggests that the treatment reduced minority households' likelihood of borrowing from professional money lenders and input suppliers, but increased their likelihood of having an informal loan through community networks. Column (7) suggests that the increase in informal loans came solely through borrowings from community networks, although the large standard errors hinder us from drawing any definite conclusion. Columns (3)-(5) present an alternate test for credit constraints by comparing whether minority households had outstanding loans from both bank and informal sources. Column (3) indicates a 3.6 ppt. (p-value .059) increase in minority households' likelihood of having loans from both banks and informal sources. Column (5) shows that this is driven by a statistically significant (p-value .017) increase in minority households having both loans from banks, and community networks.

Collectively, Appendix Tables C5 and C6 offers suggestive evdidence of minority households being credit-constrained. The directed credit policy improved access to bank credit, and allowed these

[31] Institutional credit from non-bank sources include co-operative bank loans and loans from non-bank financial corporations (NBFCs).

[32] Loans from professional money lenders also includes those obtained from input suppliers. Extreme residential segregation along religious lines in India lead us to classify loans from landlords as loans from community networks.

[33] There however might be other social or non-pecuniary costs of borrowing from community networks.

households to substitute credit from relatively more expensive non-banking financial institutions. The remaining borrowing needs were accounted for by community networks, with minority households in treated areas being more prone to borrowing from both banks and community networks.

5.1.2 Robustness

Appendix Table C3-C4 shows robustness of our baseline treatment effects to alternate specifications and sampling choices. Columns (1) and (4) of Appendix Table C3 show robustness to including a quadratic polynomial in the running variable, while columns (2) and (5) show robustness to the inclusion of district covariates.[34] Columns (3) and (6) show that the precision of our results are not affected if we opt to use robust standard errors, instead of clustering by district.[35] Columns (1) and (4) of Appendix Table C4 show robustness to excluding new districts formed from existing minority concentration districts.[36] Columns (2) and (5) show the results to be comparable if the sample is extended to include other religious minorities – namely Christian and Sikh communities. Finally, columns (3) and (6) expands the sample to include the 13 districts which were later classified as minority concentration areas, despite having a minority population share below 25%. Using a fuzzy RD specification, we show our results to be unaltered to the inclusion of these districts.[37]

Our main specifications restrict the sample to a bandwidth of .058, which is the MSERD optimal bandwidth for our intensive margin outcome. Figure 3 depicts the stability of the baseline results to a host of alternate bandwidths between .04 and .10. Appendix E replicates our key results using outcome-specific MSERD bandwidths.

5.1.3 Credit Access for Non-Minorities

We examine aggregate consequences of the directed credit policy by comparing bank credit access for non-minority households across treated and control districts. If banks kept overall lending volumes fixed

[34] We control for pre-treatment per capita monthly household consumption, branch per capita, gender ratio, fraction of rural population, fraction of marginalized caste groups, fraction secondary educated, rates of labor force participation and unemployment, fraction of salaried and casual workers, fraction of workers in farm, manufacturing, trading and service activities.

[35] We use unweighted heteroskedasticity robust errors.

[36] The list of 121 minority concentration has remained unchanged since 2008. Over this period, three of the existing minority concentration districts were bifurcated to form new districts. As the policy documents do not explicitly exclude these new districts, we continue to include them in our sample, and show our results are robust to excluding these districts.

[37] The fuzzy RD includes all three major religious minority communities, and district covariates. The first stage coefficients are positive and statistically significant at the 1% level.

in minority concentration areas, increased credit allocation for minorities would imply a reallocation of credit from non-minority borrowers.

Visually, Appendix Figure F1 shows bank credit access to be comparable for non-minority households across the discontinuity threshold. Columns (1) and (5) in Panel A of Appendix Table F1 identify a statistically non-significant treatment effect on bank credit for non-minorities. The point estimate is positive along the extensive margin (p-value .301), but negative along the intensive margin (p-value .829). While the coefficients are non-trivial when compared to the control group means, the lack of precision disallows us from drawing any strong conclusions. There is however evidence of a reallocation of credit from informal sources to NBFIs for non-minority households. Columns (2) and (6) identify a significant decline in informal loans for non-minority households along both the extensive and intensive margins.[38] This was accompanied by increased credit access from NBFIs. The intensive margin increase in NBFI loans (column (7)) is two-thirds in magnitude to the decline in informal credit (column (6)). Combining the results with those in Appendix Table C5, the coefficients suggests that the directed credit policy led minority households to substitute NBFI loans with credit from commercial banks. On the other hand, non-minority households substituted informal credit with credit from NBFIs.

Section 2.3 noted that a key incentive to comply with the directed credit policy was that it offered banks a larger pool of borrowers to meet their regulatory targets in terms of lending to "weaker sections" – namely loans to female borrowers and borrowers hailing from historically marginalized *Dalit* and *Adivasi* groups. While we cannot distinguish between male and female borrowers owing to the household-level nature of the AIDIS data, Panel B of Appendix Table F1 alleviates concerns that bank credit expansion for minorities emanated from a crowding out of *Dalit* and *Adivasi* borrowers. Although the intensive margin point estimate in column (5) is negative, it is statistically non-significant (p-value .486). Along the extensive margin [column (1)], the point estimate is positive, large, but imprecisely estimated (p-value .284). Similar to other non-minority borrowers, columns (2) and (6) also point to non-minority *Dalit* and *Adivasi* borrowers exiting informal credit markets in minority concentration areas. Overall, there is limited evidence that banks complied with the directed credit policy by reallocating credit across communities previously qualifying as "weaker sections." This is not

[38] The extensive margin coefficient is statistically non-significant at the conventionals levels of significance (p-value .155).

entirely unsurprising: Appendix A.1.1 uses the 2003 AIDIS to provide a back of the envelope estimate that *Dalit* and *Adivasi* households received less than 3 percent of bank credit in the pre-treatment period. This suggests that banks enjoyed some slack within the weaker section quota and could accommodate religious minorities, without having to cutback credit to other eligible groups.

5.1.4 Credit Quality

Pre-treatment descriptives in Appendix A pointed to the rationing of bank credit for Muslim households. An economic explanation for this would be that Muslim households were riskier borrowers, and banks rationally eschewed from lending this to group. If lending to minority borrowers was indeed sub-optimal, we would expect a rise in bank delinquency in response to the directed credit policy. Using self-reported data on borrowers' repayment behavior, Appendix Figure C2 shows comparable rates of non-repayment for bank loans, and overall loans, at the discontinuity threshold. If anything, the left-hand RD plot point to lower bank loan delinquency in treated areas. The corresponding RD point estimate in column (1) of Appendix Table C7 also shows no increase in bank loan delinquency in minority concentration areas. This assuages concerns that the directed credit policy lead to a worsening in loan quality by pushing banks to lend to sub-optimal borrowers. It also indicates that banks' prior reluctance to lend to minority borrowers cannot be explained by these borrowers' inherent riskiness.

The Oaxaca Blinder decomposition in Appendix A.1.2 hinted at the presence of discrimination against minority Muslim borrowers in formal credit markets. Appendix A.1.3 explores this in greater detail using information on lead banks across districts and offers suggestive evidence indicative of the presence of taste-based discrimination. Specifically, if statistical discrimination drove lenders' reluctance to lend to minority borrowers, learning about the quality of minority borrowers through the directed credit policy should lead to an updating of lenders' beliefs about the creditworthiness of these borrowers. Since lead banks in minority concentration districts were tasked with the implementation of the directed credit policy, we would expect them to have updated their beliefs regarding the creditworthiness of minority borrowers. As these lead banks were also lead banks in other non-minority concentration areas, assuming information sharing across branches of the same banking group, we would expect an increase in bank credit access for minorities in control districts with a common lead bank. Appendix Table A1 however offers no such evidence, pointing to the presence of taste-based discrimination. Additional details are provided in Appendix A.1.3.

5.1.5 Loan Characteristics

The AIDIS provides information on the duration and purpose for which loans were undertaken. Defining long-term loans as loans exceeding 1 year in duration, column (1) of Appendix Table C8 identifies a doubling in minority households' likelihood of having a long-term bank loan. Column (3) identifies a corresponding reduction in households' likelihood of having a long-term NBFI loan, pointing to the substitution of non-bank credit with cheaper bank loans by minorities in treated areas. Access to long-term bank credit at a relatively low cost, combined with a flexible repayment schedule, offer households the opportunity to utilize these loans for long-term investments yielding higher returns.

Using the self-reported purpose of household borrowing, Appendix Table C9 shows that minority households in treated districts borrowed from banks for farm and expenditure purposes. Column (4) shows that farm loans accounted for 30 percent of the increase in bank borrowings. Borrowing for operating non-farm businesses however was unaffected by the directed credit policy. Within expenditure loans obtained from banks, Appendix Table C10 shows a significant increase along the extensive margin for all three categories of expenditure loans: namely, loans taken for health and education, housing, and consumption. Along the intensive margin, housing loans formed the chief component of expenditure loans, making up 63 percent of the intensive margin treatment effect. Column (4) shows a statistically significant intensive margin increase in the amount of health and education loans obtained by minority households from banks. While the coefficient implies a doubling relative to the control group mean, it is substantially smaller in magnitude when compared to the treatment effect for housing loans.

5.2 Effects on Minority Well-Being

This section identifies the aggregate impacts of directed credit for religious minorities on household assets, labor market outcomes, and household consumption. We use the latter as a proxy for overall household well-being.

5.2.1 Household Assets

The AIDIS provides information on households' ownership of select assets. Consistent with the increase in farm and housing loans from banks, Appendix Tables C11, C12 and C13 identifies positive treatment effects on minority households' ownership of both farm assets, and residential real estate. Columns (1) and (2) of Appendix Table C12 show large increase in the ownership of farm machinery, while

columns (3) and (4) indicate higher ownership of farm transport in the form of tractors. Appendix Table C11 shows that while the directed credit policy did not affect overall farm area, there is a noisy increase in the ownership of irrigated farm land.[39] This translates into a large and positive treatment effect in columns (3) and (5) for the value of irrigated farm lands, and overall farm value for minority households. Column (2) of Appendix Table C13 identifies a weakly significant positive impact on residential housing. Column (1) however shows the area of residential buildings to be unimpacted by the treatment. The appreciation in housing assets thereby emanated from quality upgradation, or an overall appreciation of property prices. There is also a significant increase in the overall value of real estate owned by minority households in treated districts (column (3), Appendix Table C13).

5.2.2 Labor Market Outcomes

A large literature has explored the labor market impacts of credit access for households (see for instance Breza and Kinan (2021); Bruhn and Love (2014)). Our poaper uses weekly employment data from a nationally representative employment survey conducted in 2017 to assess whether the directed credit policy affected labor market outcomes for minority workers. The unit of observation is the individual, and the sample is restricted to individuals aged between 18 and 60.

We present a summary of the key findings in this section. A detailed description of the data, empirical strategy, and the results are provided in Appendix D. Appendix Tables D2 and D3 shows that the directed credit policy led to a reallocation of minority workers' labor market activities from services to the manufacturing sector. Thus, while overall labor force participation, rate of unemployment and total hours worked during the week remained unaffected, column (7) of Appendix Table D2 shows that minority workers were 6 ppt. more likely to be employed in the manufacturing sector. As seen from column (8) of Appendix Table D2, this was accompanied by a comparable decline in the likelihood of being employed in trade and service activities. Along the intensive margin, column (6) of Appendix Table D3 shows that this was equivalent to 3 additional hours of manufacturing work in the week by minority workers in minority concentration areas. As seen in Appendix Tables D4 and D5, the increase in manufacturing employment is primarily in the form of self-employment, as opposed to wage work. Disaggregating the manufacturing, trade and service sectors into its largest components, Appendix

[39] The corresponding p-values along the extensive and intensive margins in columns (1) and (2) are .103 and .110 respectively.

Table D6 shows that the positive treatment effect for minority workers in manufacturing employment was driven by higher self-employment in textiles, with labor being drawn away from wage work in the transport and hospitality sectors.

5.2.3 Household Consumption

We finish by examining how the directed credit policy affected households' expenditures as a comprehensive measure of minority well being. Section 5.1.1 identified a statistically significant increase in expenditure loans, which could have directly increased household consumption through increased expenditure on durable goods, health or education services. Alternately, farm credit from banks for minority households resulted in a mechanization of farm work and increased holdings of irrigated farm land, which in turn could have raised crop yields and boosted household consumption. In contrast, Augsburb et al. (2015) carefully details why household consumption could remain unchanged, or even reduce, despite higher access to household credit.[40] Ex-ante therefore, the treatment effect of the directed credit policy on household consumption is ambiguous.

Figure 5 shows a sharp jump in monthly per capita consumption for minority households located to the right of the discontinuity threshold. Column (1) of Table 4 confirms this by identifying a positive treatment effect, significant at the 1% level. As the outcome of interest is logged, the coefficient equates to a 13 percent increase in monthly per capita consumption for minority households in minority concentration districts. Relative to the average consumption level in control districts, the point estimate reflects a INR 328 increase in per capita monthly household consumption. Multiplying by average household size and annualizing, the coefficient amounts to a INR 17,760 increase in aggregate household consumption. Column (2) of Table 4 extends the sample to Christian and Sikh households, and finds slightly smaller treatment effects, significant at the 10% level (p-value .066). Column (3) shows that household consumption for non-minorities remained comparable across treated and control districts.

A related question of interest is whether access to bank credit facilitated a closing of the consumption gap between minority and non-minority households. We examine this by pooling minority and non-minority households and estimating:

[40] For instance, if households were planning to use bank credit to purchase a lumpy asset, and the quantum of bank credit was insufficient to fully cover the cost of purchase, households could cutback on consumption to finance the asset purchase.

$$\ln(MPCE)_{hd} = \alpha_s + \beta_1 Treat_d + \beta_2 Muslim_{hd} \times Treat_d + \beta_3 Muslim_{hd} + \phi f(Runvar)_d + \delta \mathbf{X}_{hd} + \epsilon_{hd} \quad (3)$$

The outcome of interest in specification (3) is logged monthly per capita consumption of household h, residing in district d, and located in state s. β_1 compares household consumption for non-minority households across treated and control districts. β_2 identifies the differential treatment effect on household consumption across minority and non-minority households within treated districts. β_3 captures the unconditional consumption gap between minority and non-minority households in control districts. We include a linear polynomial in the running variable ($f(.)$) and its interactions with the minority and treatment indicators. The treatment effects are estimated, conditional on household covariates $\mathbf{X}$ and state fixed effects (α). The sample is restricted to districts located within a bandwidth of .058 around the discontinuity threshold, with the coefficient estimates being weighted using NSS assigned household weights and a triangular kernel. Standard errors are clustered by district.

Column (4) of Table 4 identifies a positive and statistically significant coefficient associated with β_2. Column (5) replaces state fixed effects with district fixed effects, restricting the comparison of minority and non-minority households to those located within the same district. As treatment is assigned at the level of district, we are no longer able to identify the unconditional treatment effect (β_1) for non-minority households. The inclusion of district fixed effects in column (6) causes a slight decline in the magnitude of β_2, estimating the differential treatment effect, but the point estimate remains statistically significant at the 5% level. The inclusion of Christian and Sikh households in columns (7) and (8) do not alter the findings.

β_2 in column (5) reports a 19 percent increase in monthly per capita household consumption for Muslim households in minority concentration districts, relative to non-minority households in the same district. The point estimate corresponding to β_3 states that monthly per capita consumption of Muslim households was 27 percent lower than non-minority households in non-minority concentration districts. Consequently, the directed credit policy resulted in a 70 percent reduction in the consumption gap between minority and non-minority households. This highlights the ability of financial affirmative action to substantially reduce long-standing inequities for disadvantaged religious minorities, and

promoting overall household well-being.

5.3 Mechanisms

We examine in this section three channels through which banks could have facilitated credit access for disadvantaged religious minorities.

5.3.1 Bank-SHG Linkages

The central bank through its annual policy guidelines on the directed credit policy urged lenders to collaborate with local self-help groups 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 lend to such borrowers owing to high screening and monitoring costs, it is possible that such costs could have been ameliorated by lending through SHGs, which use social networks to overcome such costs (Banerjee and Duflo, 2010). Banks too had an incentive to lend to SHGs as such loans counted towards meeting their priority sector targets.

Figure 4 and Table 2 distinguishes between direct lending by commercial banks to minority households, and bank lending through SHGs. The first two columns of Figure 4 show minority households in minority concentration districts were significantly more likely to have loans from both these sources. Loan sizes from both commercial banks, and bank-linked SHGs were also significantly larger in treated districts. Comparing the RD coefficients in Table 2, the treatment effects in relative terms are significantly larger for bank-linked SHGs: while 2.4 percent of minority households in non-minority concentration districts had outstanding credit from a bank-linked SHGs, the corresponding impact in minority concentration districts was 10 ppt. higher. In comparison to Table 1, Table 2 suggests that almost 60 percent of the extensive margin increase in bank credit access for minority households could have been accounted for by loans issued through bank-linked SHGs.

Resultantly, Table 2 shows that commercial banks responded to the directed credit policy by directly lending to religious minorities, and also extending credit in collaboration with SHGs. In contrast, columns (2) and (4) of Appendix Table F2 show no evidence of higher lending to non-minorities through bank-linked SHGs. The treatment effects along both the extensive and intensive margins are an order of magnitude smaller and statistically non-significant. This rules out that the increase in

minority bank credit through bank-linked SHGs emanated through an overall expansion in bank-SHG lending in treated districts.

5.3.2 Collateral Requirements

The second channel considered is collateral requirement by lenders. Pre-treatment descriptives in Appendix Figure A5 showed minority Muslim households to have significantly lower levels of pledgeable assets in terms of land and real estate. If lenders use collateral to compensate for limited information pertaining to borrowers, the lack of collateral can exacerbate existing information frictions and distort households' access to credit (Fisman et al., 2017). While the central bank in its annual policy statements did not explicitly mention a relaxation of collateral requirements, banks could have independently opted to relax their collateral requirements to reach out to minority borrowers. The top-right panel in Figure 4 shows minority households to the right of the discontinuity threshold had a higher likelihood of having an unsecured bank loan. Correspondingly, column (1) of Table 3 identifies a 12 ppt. increase in minority households receiving an unsecured bank loan in treated districts, relative to a control district mean of 4 percent. Columns (2) and (3) shows that this positive treatment effect is observed for loans obtained directly from commercial banks, as well as loans from bank-linked SHGs. Columns (1)-(3) of Appendix Table F3 show no such impact for non-minority households: the treatment effects, while positive, are statistically non-significant, and an order of magnitude smaller than those for minority households.

Fisman et al. (2017) posit that lower collateral requirements points to improvements in information acquisition as lenders are less reliant on costly collateral to secure loans. While it is not unlikely for SHGs to extend collateral-free loans, the increase in collateral-free lending by commercial banks to Muslim households points to an improvement in banks' capabilities to screen borrowers belonging to disadvantaged minority groups. The results cannot be explained by an overall improvement in banks' screening capabilities as non-minority borrowers did not witness any increase in the likelihood of having an unsecured loan. Improvements in banks' information gather about minority borrowers would also be consistent with the findings in Section 5.1.4, which confirmed no deterioration in credit quality for loans issued to such borrowers.

Broadly, these results showcase how a relaxation of terms in loan covenants can improve credit allocation for disadvantaged borrowers. Brock and de Haas (2023) present experimental evidence

of gender discrimination in Turkish credit markets, where loan officers are significantly more likely to require guarantors for female loan applicants, leading to lower take-up rates. Similarly, Table 3 suggests that relaxing collateral requirements can improve credit access for disadvantaged borrowers. This is particularly relevant when considering the large gap in pleadgable assets across minority and non-minority households.

5.3.3 Cost of Credit

The final mechanism considered is the cost of credit. Similar to collateral requirements, the central bank's policy documents do not contain any recommendations pertaining to interest rates charged to minority borrowers. Nonetheless, it is possible that lenders on their part reduced lending rates for religious minority borrowers to facilitate the flow of credit. This would be true if banks held private information that prevailing lending rates constrained minority borrowers from accessing bank credit. Column (4) of Table 3 shows no overall reduction in the cost of bank credit in treated districts. The bottom-left panel of Figure 4 also offer no evidence of a reduction in interest rates at the discontinuity threshold. Column (5) shows comparable rates of interest charged for minority borrowers across underbanked and non-underbanked districts. On the contrary, column (6) points to a 3 ppt. increase in the cost of bank-SHG loans. This could either be a demand effect, or indicate that the recipients of bank-SHG loans in treated districts possibly comprised of riskier borrowers. Commercial bank interest rates for non-minority borrowers remained comparable across treatment and control districts [Appendix Table F3, column (5)]. This alleviates concerns that lenders were cross-subsidizing minority borrowers by charging higher rates to non-minority borrowers.

Collectively, Tables 2 and 3 offer two key mechanisms through which commercial banks increased lending to disadvantaged minority borrowers in minority concentration areas: namely collaborating with SHGs who are likely to have superior information and monitoring capabilities, and a relaxation of collateral requirements when lending directly to minority borrowers. This is similar to the findings of Fisman et al. (2017) who showed loan officers to exploit soft-information on co-ethnic/religious borrowers to expand access to bank credit for underprivileged communities, with lower collateral requirements. There is however no reduction in the cost of credit in minority concentration areas. On the contrary, increased borrowing from bank-linked SHGs by minority households in treated districts, despite the increase in lending rates, suggests that the cost of credit was not the primary barrier for

minority borrowers in accessing bank credit.

6 Economic Empowerment, Political Empowerment and Religious Conflict

Does economic empowerment of religious minorities facilitate their political empowerment and improve social harmony? Sections 5.2.1 and 5.2.3 showed higher asset values and consumption for Muslim households in treated districts. If there are entry costs to political participation, asset creation through increased credit access can ease entry constraints. Alternately, Muslim households can offer financing to political parties which are more responsive to representing their interests. Economic empowerment of minorities can also result in higher civic engagement and economic ties between communities, particularly if economic empowerment leads to a diversification of minority occupations, resulting in greater economic collaboration between religious groups (Jha, 2013). Alternately, 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. The impact of economic empowerment of minority groups on religious conflict therefore is ex-ante ambiguous.

6.1 Electoral Consequences of the Directed Credit Policy

We begin by examining the impact of the directed credit policy on the political empowerment of religious minorities and the performance of right-wing parties.[41] We focus on elections to the state legislative assembly and rely on two measures of political empowerment: participation in electoral politics and popular support received by minority candidates. The unit of observation is the electoral constituency and we use data from elections conducted between 2008 and 2019. The empirical specification is very similar to the cross-sectional RD specification in equation (2). The sample is restricted to the 61 districts located within a bandwidth of .058 of the discontinuity threshold.[42]

Compared to elections conducted in non-minority concentration areas, column (1) of Table 5

[41] Right-wing parties are conspicuous for their anti-minority platforms. Minority empowerment can result in greater identification with their group identity. This in turn can mobilize minorities to vote against a right-wing party (Bonomi et al., 2021).

[42] Due to the presence of multiple electoral cycles, we include election year fixed effects, in addition to state fixed effects. Fixed effects for constituency type (reservation status for candidates from marginalized caste backgrounds) are also included.

reports a 17.8 ppt. increase in the likelihood of a minority Muslim politician contesting an election to the state legislative assembly: a striking increase relative to the control group mean of 58.1 percent. Muslim candidates also witness a rise in popular support: column (2) shows the vote share of minority politicians to rise by 4.3 ppt., relative to the control group mean of 8.4 percent. Politicians typically seek party nominations to contest elections and column (3) offers noisy evidence that Muslim politicians in minority concentration areas are more likely to secure nomination from a major political party (p-val .144).[43] Column (4) shows that the rise in electoral support for minority politicians was driven by the rise in popular support for Muslim politicians contesting on major party platforms – a statistically significant 4.3 ppt. rise in vote share, relative to a control group mean of 2.8 percent. Column (3) shows that the rise in minority political participation is stemming from serious politicians contesting for office and securing the backing of major political platforms for the same. Similarly, column (4) assuages concerns that the rise in popular support for Muslim politicians is not a mechanical increase owing to more Muslim politicians contesting elections. Finally, columns (5) and (6) show that the rise in popular support for minority politicians is matched by a reduction in popular support for the right-wing Hindu nationalist BJP. BJP vote shares declined by a statistically significant 5.9 ppt. – a 25 percent drop relative to the control group mean – in elections conducted in minority concentration areas.[44] The rise in electoral support for minority politicians contesting on major party platforms is very similar in magnitude to the loss in electoral support for the majoritarian BJP. Appendix Table G1 shows that the rise in popular support for minority politicians translated into a higher win likelihood for these candidates. Muslim candidates in treated districts were 9 ppt. more likely to emerge victorious in an electoral contest.

In summary, Table 5 and Appendix Table G1 offers strong evidence that economic empowerment does indeed lead to political empowerment for religious minorities: minority politicians are significantly more likely to contest elections, and garner a higher share of votes. The increase in popular support also increases their chances of winning these elections, leading to greater representation of religious minorities in state legislatures. Moreover, the rise in popular support for minority candidates is matched

[43] We consider 5 major parties which have been vocal against recent majoritarian tendencies in India. Namely, these are 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 as it was part of an electoral alliance with the RJD and the INC.

[44] The point estimate in column (5) is conditional on the BJP contesting elections, and is statistically significant at the 10 percent level (p-value .094). The coefficient remains largely unchanged in magnitude, but the standard errors are now larger when we assign a vote share of 0 to constituencies in which the BJP did not contest.

by a decline in electoral support for the right-wing BJP, which has historically been inimical to minority interests.

6.1.1 Mechanisms of Electoral Effects

We examine four non-exclusive channels to explain the electoral impacts of the directed credit policy. First, economic empowerment through the directed credit policy might lead to an overall sense of empowerment for minority voters, which in turn can boost their participation in the electoral process. If a higher share of minority voters exercise their franchise, and minority voters prefer minority candidates and vote against right-wing parties, it can explain both the increase in support for Muslim politicians, and the loss in support for the BJP. Second, the directed credit policy facilitated the formation of self-help groups, which could have facilitated social interactions between minority citizens. Third, the directed credit policy expanded economic opportunities for minority workers, which could have promoted greater civic engagement between minorities and non-minorities. Frequent economic interactions could have altered perceptions held by non-minorities regarding minorities, resulting in higher support for minority candidates, and lower support for political groups bearing hostility towards minorities. If non-minorities voted in line with minority interests, it would also point to increased social harmony. Finally, the positive wealth effect from increased access to bank credit could have aided minorities in producing qualified contestants with higher electoral management skills. We explore each of these channels in the subsequent sections.

6.1.1.1 Voter Registration and Turnout

We begin by identifying whether economic empowerment increased minority citizens' engagement with electoral process. If economic empowerment created a sense of overall empowerment and higher voter participation, we would expect higher turnout of minority citizens in treated areas. If the turnout of non-minority citizens remained unaffected, this would result in an overall increase in voter turnout in minority concentration areas. Relatedly, economic empowerment could have led to increased voter registration amongst minorities, leading to an overall increase in the number of voters. Column (4) of Appendix Table G1 shows no impact of the directed credit policy on voter turnout: the coefficient corresponding to the RD estimate is relatively small in magnitude and statistically non-significant. Column (5) shows that overall voter registration too remained comparable across treatment and control

districts. As the effects on Muslims and non-Muslim voters could have counteracted each other and negated the overall impact of the policy, the results in columns (4) and (5) should be interpreted as a suggestive lack of evidence for the impact of the directed credit policy on voter registration and turnout.

6.1.1.2 Wealth of Muslim Candidates

Improved access to bank credit could have generated a positive wealth effect, enabling aspiring politicians to overcome entry costs for participating in electoral politics. A lowering of entry barriers could also improve candidate quality. Furthermore, higher personal wealth of candidates can positively affect their campaigning ability, allowing them to draw greater votes. To assess this effect, we use publicly available data from candidate affidavits and compare observable characteristics of Muslim politicians across minority and non-minority concentration areas. Appendix Table G3 shows that Muslim candidates in treated districts have significantly higher net assets (column 1) and annual incomes (column 2). However, they continue to be comparable in terms of age (a proxy for experience) and years of education. This indicates that the directed credit policy resulted in wealthier candidates running for public office, but this did not necessarily mean candidates of higher quality. Higher personal wealth of candidates could have led to greater visibility of candidates through a more aggressive campaign, which would be consistent with the higher votes received by these contestants.

6.1.1.3 Voting Along Communal Lines

We next analyze whether citizens voted along communal lines: namely, the increase (decrease) in vote share of Muslim (BJP) candidates were driven by Muslim voters exercising their franchise in favor of Muslim candidates, or were non-minority voters also supporting minority candidates. Since, individual voting decisions are not available, we identify treatment effects on vote shares separately in areas where Muslims constituted a higher share of voters, vis-a-vis areas where they constituted a lower share. Using estimates from Gulzar et al. (2024) on fraction of Muslim voters in electoral constituencies, we classify constituencies as high Muslim concentration if the share of registered Muslim exceeded the sample median.[45]

[45] Gulzar et al. (2024) estimated the shares of Muslims from the list of registered voters for every polling station. This data was shared with us after aggregating the data to the level of electoral constituencies. We express our gratitude to the authors for sharing this data. The sample median is based on electoral constituencies located in districts falling within our preferred bandwidth.

Columns (1) to (6) of Appendix Table G2 show that irrespective of whether constituencies had a relatively low or high share of Muslim voters, the directed credit policy significantly increased Muslim candidates' likelihood of contesting elections. However, in terms of vote share, only candidates in constituencies with low share of Muslim voters received higher electoral support. Further, contrary to expectations, columns (7) and (8) show that the decline in support to the right-wing BJP was driven by constituencies where Muslims comprised a relatively small share of the electorate (p-value = 0.107).[46] Stronger, results in constituencies with lower share of Muslim voters can be interpreted in two ways. First, it is possible that compared to the areas with higher share of Muslims, those in constituencies with smaller number of voters find it easier to solve the collective action problem, and thus, vote in line with their interests. Alternatively, the results also suggest that non-Muslim voters too may have voted in line with the preferences of Muslims, by voting for their candidates and against the right-wing platform, which may signal increased social harmony between the minority and majority group as a result of the credit-policy.

6.1.2 Economic Empowerment, Civic Engagement and Electoral Outcomes

The directed credit policy could have affected both within and across group social interactions. Section 5.3.1 showed that a key pathway explaining minorities' access to bank credit was credit received through bank-linked self-help groups. As SHGs have multiple members who coalesce to form an account, SHG formation could have increased social interactions. Based on group composition, this could have heightened social interactions within, or across communities. Greater within-group conversations could have enabled information sharing and co-ordination across minority voters. Alternately, social interactions across communities could have led to a lowering of prejudices held against minorities, and lower support for majoritarian parties. Similarly, Section 5.3.2 argued that an increase in unsecured bank lending to minorities in treated districts pointed to an improvement in information collection about minority borrowers. As bank officers and staff are primarily drawn from non-minorities (Fisman et al., 2017), superior information collection about minorities, or increased social interaction between minorities and non-minorities in the course of bank visits could also have lowered prejudices against

[46] As the share of Muslims in each constituency is estimated based on classifying names in the voter list as Muslim, the share is possibly affected by measurement error. We verify that the results are unaffected if we omit districts where the share of Muslims based on the estimation of Gulzar et al. (2024) differs by 5 ppt. or more from the share of Muslims reported in the population Census.

minorities. This in turn could have aided popular support for minority politicians, and lower support for the BJP. Consequently, we explore whether the directed credit policy affected the composition of SHGs, and also economic interactions across groups in the labor market.

The National Rural Livelihood Mission (NRLM) provides a list of all SHGs registered with banks. Along with information on the date of registration and the bank branch with which it is registered, demographic information on every member is also provided, which includes the member's religion, caste and gender. We use this data to ascertain whether a SHG registered with a commercial bank was a "mixed-SHG", or an exclusively "minority-SHG". The former refers to a SHG with both minority and non-minority members.

Following the directed credit policy, columns (1)-(2) of Appendix Table C14 reports positive treatment effects for whether a SHG has some Muslim member, and whether a SHG is comprised exclusively of Muslim members. The latter is statistically significant at the 10 percent level (p-value .087). There is however no impact on the likelihood of forming a SHG group with both minority and non-minority members. The point estimate is negative, albeit statistically non-significant. As group formation might take time and occur only after agents have learned about the directed credit policy, columns (4)-(6) restrict the sample to SHGs registered between 2014 and 2019. The treatment effects remain similar in magnitude but the precision of the coefficients improve: column (5) shows that the likelihood of forming a SHG group comprised of only Muslim members in minority concentration areas significantly increased by 4.6 ppt., relative to a control group mean of 5 percent. Columns (7)-(9) show that SHG group formation was comparable across treatment and control areas in the pre-treatment period.

In Section 5.2.2, we documented that the directed credit policy led to a reallocation of minority workers from wage work in the service sector, to self-employment in manufacturing activities. If minorities opted to work in sectors dominated by non-minority workers, this can facilitate across-community interactions during the process of economic transactions. We gauge this by identifying whether the directed credit policy led to minority workers being employed in sectors with a relatively high share of non-minority workers. Using data from the 2011-12 employment survey, we obtain the share of Hindu and Muslim workers in each non-farm (2-digit) industry and use our RD design to identify whether the directed credit policy affected minority individuals' likelihood of working in

non-farm sectors with a relatively high share of non-minority workers.[47]

The results in Appendix Table D7 offer mixed evidence. First, column (2) shows that minority workers have a significantly higher likelihood of being employed in manufacturing activities in sectors with a relatively high share of minority workers. Column (3) offers suggestive evidence of minority workers being more likely to be engaged in non-farm work in sectors with a relatively high share of Hindu workers (p-val .220). Column (4) however identifies a weakly significant negative impact on the likelihood of minority workers being employed in manufacturing sectors with a relatively large share of non-minority workers (p-value .077). As the increase in manufacturing employment for religious minorities in minority concentration areas was primarily through self-employment, column (5) examines whether minority workers were more likely to be employed in manufacturing work, in sectors with a relatively high share of self-employed non-minority workers. The RD coefficient is positive and statistically significant, pointing to a 5 ppt. rise in the likelihood of minority workers being employed in such sectors. The point estimate is also very similar to the average treatment effect of the directed credit policy on minority workers gaining employment in manufacturing work.

6.2 Communal Violence

The preceding section argued that the credit policy affected electoral outcomes in the policy targeted districts, by increasing the supply of and vote-shares of Muslims candidates, and decreasing the vote-share of candidates belonging to the right-wing party with anti-Muslim platform. The main driver of political effects appear to be the institution of self-help groups promoting coordination among Muslims. This may suggest increase in-group cohesion among Muslims that can lead to an increase in violence as theorized by Mitra and Ray (2014). However, scholars have also indicated the importance of civic (Varshney, 2003) and economic (Jha, 2013) cooperation in containing and at times eliminating the need for violence between Hindus and Muslims. In the analysis of political data, we found suggestive evidence that the decrease in vote-share of the right-wing party candidates is largely present only in constituencies where Muslims do not constitute a large share of the population. One reasons for this effect could be improved civic relationships between the minority and majority communities which

[47] This is a nationally representative household sample survey, conducted by the National Sample Survey Organisation (NSS). A continuation of these surveys is the Primary Labour Force Survey, used in Section 5.2.2 to identify how the directed credit policy affected labor market outcomes. As the NSS undertook a set of major changes to the classification of industries in 2008, we use the employment survey conducted in 2011-12 as opposed to surveys conducted in the pre-treatment period.

can lead to improved social harmony. Given these competing possibilities, we now explore if political empowerment of Muslims translates into reduced or increased violence between the minority (Muslims) and the majority (Hindus).

To test this hypothesis, we rely on two sources of data. First, we use official administrative data on religious/communal riots reported annually at the district level by the National Crimes Records Bureau (NCRB) of India for the period of 2014 to 2019. While the data does not identify the identity of rioting parties, the reports are of inter-religious violence which are historically events involving the majority and the minority, and are likely to pick up on any change in rioting specifically between Hindus and Muslims. This data only reports religious riots as a separate category from 2014 and for inexplicable reasons exclude the year 2016 from these reports. Second, to corroborate the findings from administrative data, we access data on political violence from 2016 to 2019 in India from the Armed Conflict Location and Event Dataset (ACLED) (Raleigh et al., 2010). The data is constructed based on news reports of violent incidents. We map each riot reported in this data with the districts in our sample and classify them into Hindu-Muslim riots using description of the actors. This data is aggregated to calendar-year-district level for the main analysis and 12-month-district level for the exploration of mechanisms. Any district that is not found to have any incidence of violence is assumed to have zero Hindu-Muslim riots.

Table 6 reports the effects of credit policy on violence. Column 1 reports an increase of 4.686 riots per year likely between Hindus and Muslims as a result of the policy using information from the administrative data. This effect is supported by analysis of the ACLED data, as column 4 reports an increase of 0.066 Hindi-Muslim riots per year in the minority concentration districts as a result of the credit policy. These increases appear specific to Hindu-Muslim or religious violence as there is no corresponding increase in rioting overall in both datasets (columns 2 and 5). As a placebo, we also check the effects on murders in the NCRB data (column 3) and do not find any corresponding increase in this type of violent incident, addressing the concern that there maybe an overall increase in violence.

6.2.1 Mechanisms of Conflict

The increase in Hindu-Muslim violence goes against the interpretation that credit policy led to social harmony between the majority and minority groups. It is likely the improved economic well-being of Muslims elicited violent responses from the majority group as theorized by Mitra and Ray (2014).

Additionally, it is likely the increased violence is not directly due to the improved economic well-being but, instead, is related to elections. The right-wing party can strategically use violence for electoral payoffs (Iyer and Shrivastava, 2018), or they can use violence to punish the minority for the negative effects in elections. We find evidence for both of these explanations.

First, using ACLED data we hand-code from the text of the reported incidents if the event was likely instigated by the majority group (Hindu). Column 1 of Appendix Table G5 shows that the policy districts witness a rise in violent riots instigated by Hindus. This finding is inline with the theoretical prediction and descriptive finding of Mitra and Ray (2014).

Second, we can test for strategic use of violence in relation to elections using information in the ACLED data on the timing of the violent events. For each assembly constituency falling in the sample of districts within our preferred bandwidth, we identify Hindu-Muslim riots at monthly levels. We split the data into 12-months before and 12-months after election time periods, and run separate analysis using our regression discontinuity design specification. The analysis eliminates the month of election from the data as we cannot identify if the violent event happened before or after the election with certainty in the month of the election. Columns 2 and 3 of Appendix Table G5, reports the effects of credit policy on the number of Hindu-Muslim riots separately for twelve months before and twelve months after the elections.We find no evidence for increase in violence before the elections, but we do find that violence spiked after the elections in minority concentration districts. This pattern of violence suggests that it was strategically used as a response to the electoral effects.

In order to further confirm that the violence was indeed due to electoral effects, we probe if violent events are concentrated in places where the right-wing party lost electorally. For this analysis we divide the sample of districts based on whether the main right-wing party won most of the races or not, and separately report the effect of policy on violent events using the same RD specification. Column 4 of Appendix Table G5 reports there is no effect on violent incidents in districts where the right-wing party won most of the races (more than 50%), however, Column 5 shows that Hindu-Muslim riots increase in post election months in districts where the right-wing party did not win majority of the races, further confirming that the negative effect on social harmony in the society is a blowback for the electoral empowerment of the minority group.

7 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 contrary to the expectation, such a policy can be successful at expanding credit and improving well-being without a cost to the majority group. 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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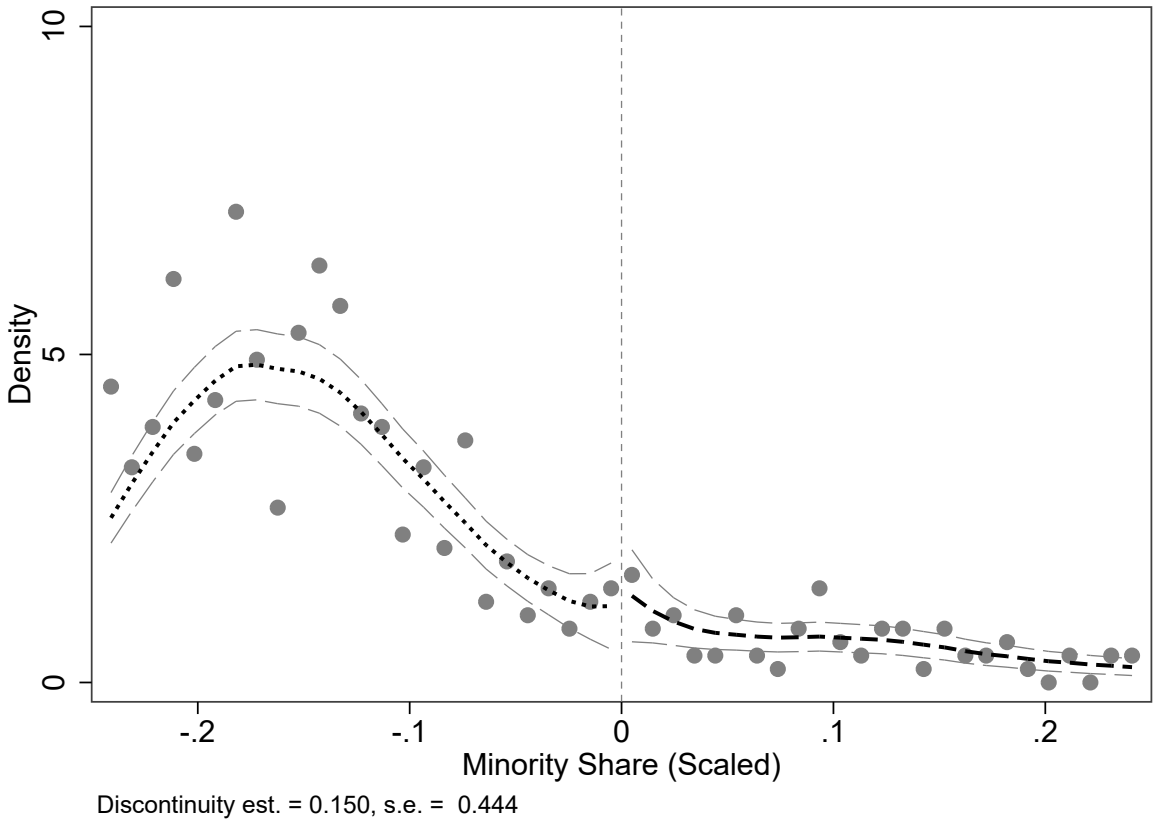
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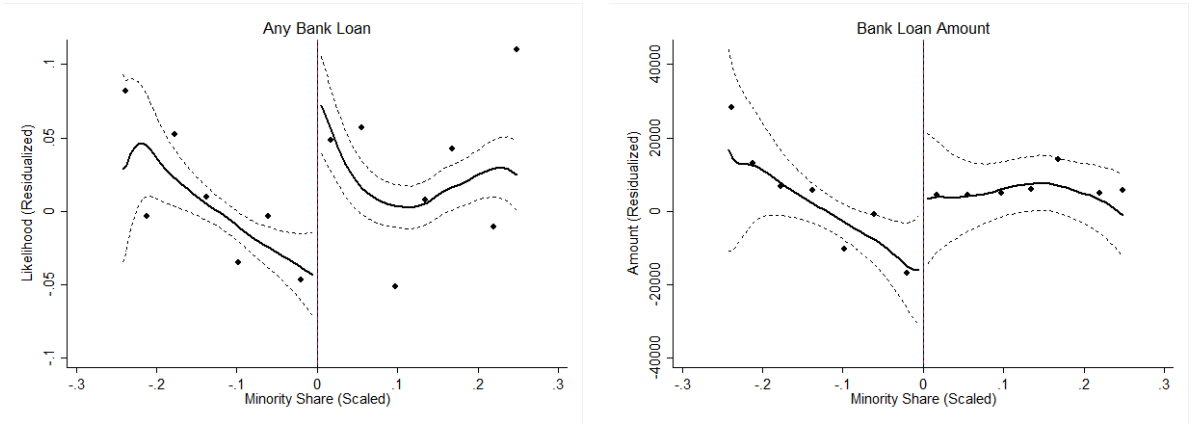
8 Figures

Figure 1: Smoothness in Treatment Assignment at Discontinuity Threshold



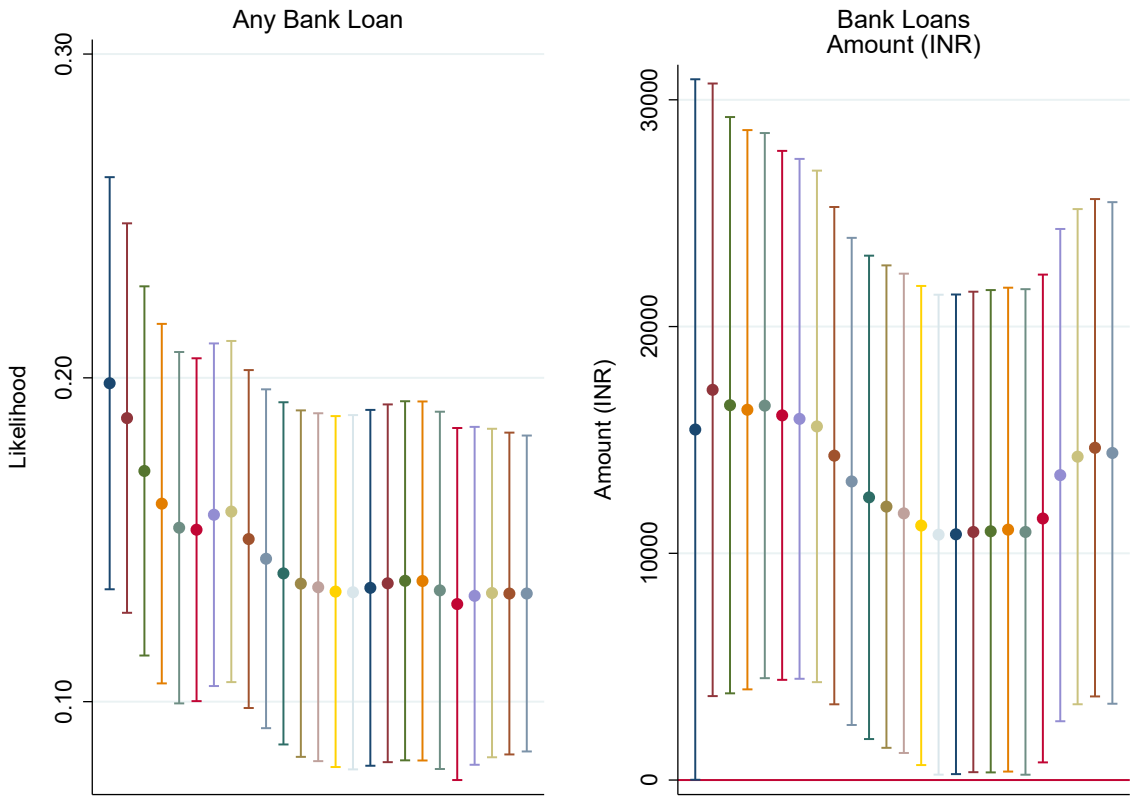
Notes: This figure presents the McCrary test (McCrary, 2008), testing for selective sorting of districts as minority concentration around the 0.25 threshold. The horizontal axis depicts the running variable – defined in equation (1). The horizontal axis shows the range of the running variable between -.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 2: Bank Credit for Minority Households



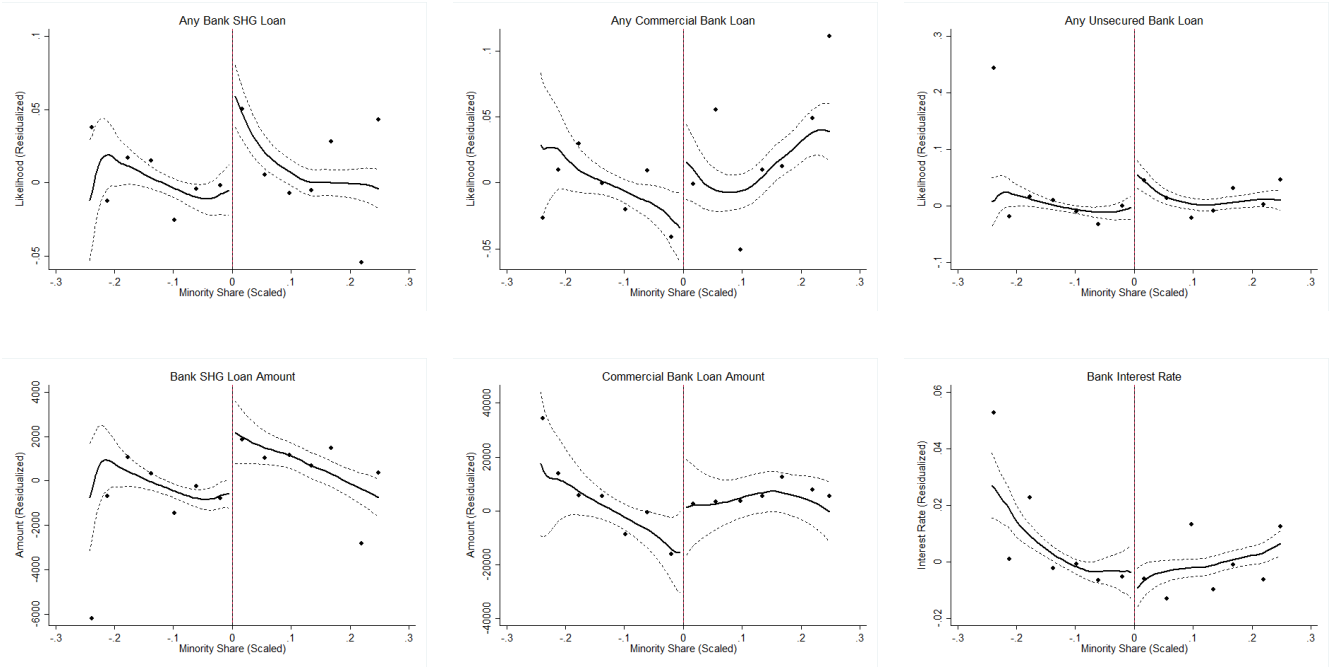
The above figures shows the treatment effect for access to bank credit for minority households. The unit of observation is the household. The outcome of interest in the left-panel is a dummy equaling 1 if the household has any bank loan; in the right-panel, the volume of bank loans (initial loan value). The outcome variable is residualized, conditional on state 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 95% confidence intervals. Estimates are weighted using a triangular kernel and household specific weights provided by the AIDIS.

Figure 3: Bank Credit for Minority Households: Robustness to Alternate Bandwidths



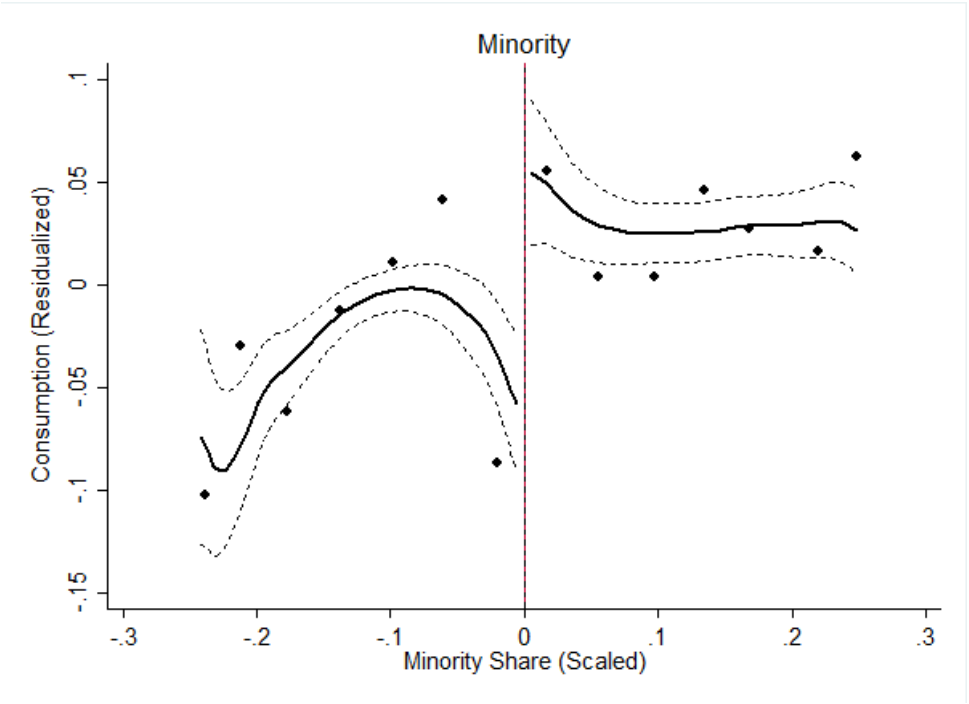
Notes: This figure shows the robustness of the baseline results to alternate bandwidths. The unit of observation is the household. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). The outcome of interest in the left-panel is a dummy equaling 1 if the household has any bank loan; in the right-panel, the volume of bank loans (initial loan value). The vertical lines denote 95% confidence intervals of the coefficient estimates. The first coefficient is computed using a bandwidth of .04. Subsequent estimations sequentially increase the bandwidth by 0.003. The last point estimate is based on a bandwidth of .10. All specifcations include state fixed effects, household covariates and a linear polynomial in the running variable. Standard errors are clustered by district. Estimates are weighted using a triangular kernel and household specific weights provided by the AIDIS.

Figure 4: Mechanisms: Source of Bank Credit, Loan Securitization and Cost of Credit for Minority Households



Notes: The above figure shows RD plots corresponding to the three mechanisms examined in the paper. The unit of observation is the household. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). The outcome of interest in the top-left panel is a dummy equaling 1 if the household has a loan from a bank-linked SHG; in the top-middle panel, a dummy equaling 1 if the household has a loan from a commercial bank; in the top-right panel, a dummy equal to 1 if the household has an unsecured bank loan; in the bottom-left panel, the amount of loans from bank-linked SHGs; in the bottom-middle panel, the amount of loans from commercial banks; in the bottom-right panel, bank interest rates. All outcome variables are residualized, conditional on state 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 95% confidence intervals. Estimates are weighted using a triangular kernel and household specific weights provided by the AIDIS.

Figure 5: Treatment Effects on Household Consumption: Minorities



The above figure identifies the treatment effect on monthly per capita household consumption. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). The outcome variable is residualized, conditional on state 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 95% confidence intervals. Estimates are weighted using a triangular kernel and household specific weights provided by the AIDIS.

9 Tables

Table 1: Treatment Effect on Bank Credit: Minority Households

	(1)	(2)	(3)	(4)	(5)
	Pr(Loan = 1)			Loan Amount (INR)	
Treat	.120***	.197***	.178***	20089.469***	21064.418***
	(.036)	(.033)	(.031)	(7630.758)	(6328.599)
Observations	1505	1378	1765	1930	1765
Dep Var Mean	.131	.128	.115	20344.221	20801.635
Bandwidth	.0547	.0469	.0580	.0634	.0580
Controls	N	Y	Y	N	Y

Notes: The above table shows the treatment effect on bank credit access for minority households. The unit of observation is the household. The outcome of interest in columns (1)-(3) is a dummy equaling 1 if the household had any outstanding bank loan; in columns (4)-(5), the amount of bank loans received by the household. All specifications include state fixed effects and a linear polynomial in the running variable and its interaction with the treatment indicator; columns (2), (3) and (5) also control for household size and a rural indicator. The bandwidth in columns (1), (2), (4) and (5) is the MSERD optimal bandwidth, computed as recommended by Calonico et al. (2020). In column (3), the sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 2: Mechanism: Nature of Bank Lending in Minority Concentration Districts

	(1)	(2)	(3)	(4)
	Pr(Loan = 1)		Loan Amount (INR)	
	Commercial Bank	Bank SHG	Commercial Bank	Bank SHG
Treat	.096***	.102***	17107.647***	3941.329***
	(.023)	(.018)	(6012.407)	(1310.464)
Observations	1765	1765	1765	1765
Dep Var Mean	.096	.024	20046.831	754.804

Notes: The above table shows the treatment effect by type of bank loan for minority households. The unit of observation is the household. The outcome of interest in columns (1)-(2) is a dummy equaling 1 if the household has any outstanding loan from the source mentioned; in columns (3)-(4), the amount of outstanding loan from the source mentioned. *Commercial Bank* refers to loans obtained directly from commercial banks; *Bank SHG* refers to loans received through bank-linked SHGs. All specifications include state fixed effects, a linear polynomial in the running variable, household size, and a rural indicator. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 3: Mechanisms: Treatment Effects on Collateral Requirements and Cost of Credit

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Unsecured Loan = 1)			Interest Rates		
	All Bank	Commercial Banks	Bank SHG	All Bank	Commercial Banks	Bank SHG
Treat	.118***	.051**	.087***	.017	-.001	.027**
	(.044)	(.021)	(.033)	(.017)	(.011)	(.012)
Observations	1765	1765	1765	473	383	98
Dep Var Mean	.044	.035	.044	.108	.107	.108

Notes: The above table shows the treatment effect on bank loan securitization and interest rates for minority households. The unit of observation is the household. The outcome of interest columns (1)-(3) is a dummy equaling 1 if the household has any unsecured bank loan; in columns (4)-(6), the interest rate faced by the household for bank loans. The outcome of interest in columns (1) and (4) include all bank loans; in columns (2) and (5), loans obtained directly from commercial banks; in columns (3) and (6), loans obtained from bank-linked SHGs. Average household interest rates are weighted by loan volume. All specifications include state fixed effects, a linear polynomial in the running variable, household size, and a rural indicator. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 4: Minority Credit Access and Household Consumption

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Monthly Per Capita Household Consumption (Log)						
	Sub-Samples			Pooled Sample			
	Muslim	Minorities	Non Minorities	Muslim and Non-Minority	Muslim and Non-Minority	Minority and Non-Minority	Minority and Non-Minority
Treat	.130***	.105*	-.112	-.072		-.078	
	(.047)	(.053)	(.071)	(.109)		(.107)	
Muslim × Treat				.213**	.193**		
				(.097)	(.091)		
Minority × Treat						.247**	.222**
						(.100)	(.096)
Muslim				-.285***	-.272***	-.348**	-.305**
				(.094)	(.078)	(.131)	(.124)
Minority						-.287*	-.196
						(.162)	(.145)
Observations	1765	2480	9763	11528	11528	12243	12243
R^2				.50	.58	.51	.59
Control Mean	2524.28	2524.28	3295.13	3295.13	3295.13	3295.13	3295.13
State FE	Y	Y	Y	Y	N	Y	N
District FE	N	N	N	N	Y	N	Y

Notes: The above table shows the treatment effect on household consumption. The unit of observation is the household. The outcome of interest is per capita monthly household consumption (logged). The sample in column (1) is restricted to minority Muslim households; in column (2), all religious minorities; in column (3), non-minorities. Columns (4)-(7) pool the sample across minority and non-minority households. Columns (4) and (5) restrict religious minorities to Muslim households only. All specifications include a linear polynomial in the running variable, and household covariates. Columns (1)-(4) and (6) also include state fixed effects; columns (5) and (7) include district fixed effects. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using household-specific weights; specifications in columns (1)-(3) are also weighted using a triangular kernel. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 5: Directed Credit Policy and Electoral Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Minority Candidates				Right Wing Party Candidates	
	Any Candidate	Vote Share	Any Candidate Major Party	Vote Share Major Party	Vote Share	Vote Share
Treat	.178***	.043**	.081	.043**	-.059*	-.051
	(.058)	(.020)	(.056)	(.018)	(.032)	(.050)
Observations	1064	1064	1064	1064	876	1064
Dep Var Mean	.581	.084	.146	.028	.329	.291

Notes: This table identifies the impact of the directed credit policy on electoral outcomes for politicians from minority communities and those representing the right-wing party. The unit of observation is the electoral constituency. The outcome of interest in column (1) is a dummy equaling 1 if there was a Muslim candidate contesting the election; in column (2), the total vote share received by Muslim politicians contesting elections to the state legislative assembly; in column (3), a dummy equaling 1 if any Muslim candidate contested on behalf of a major political party; in column (4), the vote share received by a Muslim candidate contesting on behalf of a major political party; in column (5), the vote share received by the BJP candidate, conditional on participation; in column (6), the vote share received by the BJP candidate. 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 .058 around the discontinuity threshold, and are weighted using a triangular kernel. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 6: Effects on Hindu-Muslim Violence

	(1)	(2)	(3)	(4)	(5)
	Religious Riots	Total Riots	Murders	Hindu-Muslim Riots	Total Riots
Treat	4.686***	-.162	-1.163	.066**	-2.428
	(1.591)	(17.030)	(8.161)	(.029)	(1.523)
Observations	323	323	323	248	248
Dep Var Mean	1.565	77.662	40.896	.057	4.200
Data Source	Admin	Admin	Admin	ACLED	ACLED
Years	2014-2019	2014-2019	2014-2019	2016-2019	2016-2019

Notes: This table identifies the impact of the directed credit policy on violence between the minority and majority communities. The unit of observation is the year-district level. Columns 1 to 3 use administrative data from National Crimes Report Bureau to test the effect on Religious Riots, Total Riots irrespective of religious link, and Murders. Columns 4 to 5 use data from Armed Conflict Event Database to corroborate the findings of admin data, by exploring effects directly on Hindu-Muslim riots (column 4), and total riots (column 5) reported in the media. Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A Pre-Treatment Minority Credit Access: Descriptive Evidence

This section offers descriptive evidence from the 2003 AIDIS to highlight that among religious minorities, Muslim households in particular faced substantial barriers to credit access and reported significantly lower financial assets.

A.1 Access to Credit and Cost of Credit

We present the descriptive analysis by disaggregating households into 5 mutually exclusive groups: Hindu forward caste, Hindu marginalized castes, Muslims, and other religious minorities.[1] We also disaggregate the source of credit into bank loans, 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.

The top-left panel of Appendix Figure A1 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 A1 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 A1 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 highest rates of interest by professional money lenders. Collectively, Appendix Figure A1 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 face the highest cost of credit.

A.1.1 Credit Allocation for Historically Marginalized Communities

In order to facilitate credit flow to under-represented minorities, the central bank directs commercial banks to allocate 10 percent of their aggregate credit to "weaker sections" – namely borrowers from *Dalit* (Scheduled Caste) and *Adivasi* (Scheduled Tribe) communities, along with female borrowers. However, it is unclear how well this is monitored or what penalties are faced by banks in case they fail to meet their target.[2] To facilitate lending to religious minorities, the RBI expanded the definition of weaker sections to include religious minorities. showed that the priority sector guidelines were binding for commercial banks, with banks being unwilling to extend more than 40 percent of their loan portfolio towards the priority. If banks viewed the 10 percent allocation for weaker sections in a similar manner, it is possible that the inclusion of religious minorities in this category would lead to banks reallocating credit from women and socially marginalized groups, to religious minorities.

In the absence of administrative data, we assess this by combining administrative data from the Basic Statistical Returns (BSR), with household lending data from the AIDIS. The RBI hosted publicly available BSR offers a sectoral break up of bank credit, which includes a separate classification

[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.

[2] This is contrary to the priorty sector regulations, whereby the RBI sets explicit guidelines for banks if they fail to allocate 40 percent of their credit to the priority sector.

of agricultural loans directly issued to farmers (direct finance), as well as personal loans issued to individual borrowers. We assume these two categories to correspond to the household-level borrowing data in the AIDIS.[3] In the pre-treatment period in 2003, the value of outstanding bank loans to households in the AIDIS data equaled INR 1,359 billion. In the administrative BSR data, outstanding personal loans and direct agricultural loans equaled INR 1,730 billion. While we cannot account for female borrowers owing to the household-level nature of the AIDIS data, *Dalit* and *Adivasi* borrowers accounted for 13 percent of total household loans in the AIDIS. As farm and personal loans accounted for 22 percent of aggregate commercial bank credit in 2003, loans to socially marginalized groups comprised of 3 percent of banks' lending portfolio. As long as banks did not issue over 7 percent of their loans to female borrowers, this suggests that banks could have extended loans to religious minorities without breaching the 10 percent cap.

A.1.2 Oaxaca Blinder Decomposition

More formally, we use an Oaxaca-Blinder decomposition to gauge whether the descriptive difference in credit access for minority Muslim households can be explained by observable characteristics. We restrict the sample to Hindu and Muslim households and partial out district fixed effects. We thereby examine 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 A2 shows that observable characteristics cannot fully explain the variation in bank credit access across Hindu and Muslim households, pointing to the presence of discrimination. Along the extensive margin, 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).

A.1.3 Testing for Statistical Discrimination

Section 5.1.1 showed that the directed credit policy resulted in higher bank lending to religious minorities. This did not come at the expense of non-minority borrowers. Neither was it accompanied by a deterioriation in credit quality, which rules out that banks' aversion to lend to these groups stemmed from private information on the poor creditworthiness of these borrowers. Appendix A.1.2 contended that banks were rationing credit to minority households in the pre-treatment period, with the Oaxaca-Blinder decomposition implying that observable characteristics could explain only 30 percent of the extensive margin difference in bank credit across Hindu and Muslim households. Taken together with the finding of Fisman et al. (2020) that childhood exposure of Hindu loan officers to communal conflicts resulted in lower credit access for Muslim borrowers, the descriptive evidence points

[3] The remaining loan categories are manufacturing loans, loans to trading and service enterprises, loans to financial institutions, and loans to transport operators. These are likely to be loans issued to firms, as opposed to households.

to discrimination against minority borrowers in formal credit markets.

Discrimination can be statistical or taste-based. If lenders were previously rationing credit to religious minorities owing to statistical discrimination, information gained on borrower quality from the directed credit policy should have led to increased lending to religious minorities, ex-post. The ideal experiment thereby would be to assess whether religious minorities received higher bank credit in minority concentration areas upon the discontinuation of the directed credit policy. As the policy remains in effect, we propose an alternate suggestive test for statistical discrimination.

We exploit the fact that the primary responsibility of implementing the directed credit policy was assigned to lead banks. Lead banks in minority concentration districts were also lead banks in other non-minority concentration areas. If lead banks learnt about the quality of minority borrowers through the directed credit policy and shared this information with other branches, we would expect increased credit access from these banks to minority borrowers in control areas, relative to other banks which were less likely to update their priors regarding the quality of minority borrowers.[4] The empirical specification is the following:

$$Y_{hds} = \alpha_d + \beta_1 Muslim_{hds} + \beta_2 MatchLeadBank_{ds} \times Muslim_{hds} + \delta \mathbf{X}_{hds} + \epsilon_{hds} \tag{A4}$$

In specification (A4), $MatchLeadBank$ is a dummy equaling 1 if the lead bank in district d is also a lead bank in one of our treated districts. The sample is restricted to Hindu and Muslim households in non-minority concentration areas. All specifications condition on district fixed effects (α) and household level covariates ($\mathbf{X}$). Assuming information sharing across lead banks, $\beta_2 > 0$ would offer suggestive evidence in favour of statistical discrimination. Appendix Table A1 disallows us from arriving towards any such conclusion. Across both the extensive and intensive margins, β_1 in columns (1) and (3) is negative and statistically significant. β_2 too is negative and large, albeit not statistically significant. Columns (3) and (4) restrict the outcome of interest to loans obtained through bank-linked SHGs and find no evidence indicative of statistical discrimination.

While the empirical strategy of specification (A4) precludes us from making any causal claims, the fact that the negative correlation estimated in β_1 for access to bank credit is not over-turned in β_2 offers suggestive evidence that either there was no information sharing across lead bank branches, or the reluctance of banks to lend to Muslim borrowers did not emanate from statistical discrimination.

A.2 Loan Applications

As the AIDIS has no information on loan applications, we use data from the nationally representative Indian Human Development Survey (IHDS) to compare trends in loan application and denial across bank and non-bank sources, and communities. The IHDS in their 2011-12 survey inquired whether households had ever applied for a loan, and whether their application was accepted or rejected. The top panel of Appendix Figure A3 show that while 50-60 percent of households had applied for a loan over the past five years, Muslim households were least likely to apply for a bank loan. Thus, only 11 percent of Muslim households applied for a bank loan, as opposed to 24 percent of Hindu forward caste households, or other religious minorities. The low rate of bank loan applications cannot be explained by low credit demand, as 34 percent of Muslim households over the same period applied for loans from informal sources – an application rate comparable to other communities. The bottom left panel of Appendix Figure A3 shows that conditional on applying, bank loan applications of Muslim households were also more likely to be rejected. Thus, while the bank loan rejection rate was about 10 percent of

[4] As the vast majority of lead banks are state-owned banks, information sharing can occur through the transfer of loan officers. As detailed in Fisman et al. (2017), loan officers are regularly rotated across bank branches, and such rotations can occur across districts.

Hindu forward castes and other religious minorities, the corresponding denial rate for Muslim borrowers equaled 15 percent. While the evidence in Appendix Figures A1 and A3 is purely descriptive, it is consistent with discrimination faced by Muslim borrowers in formal credit markets.

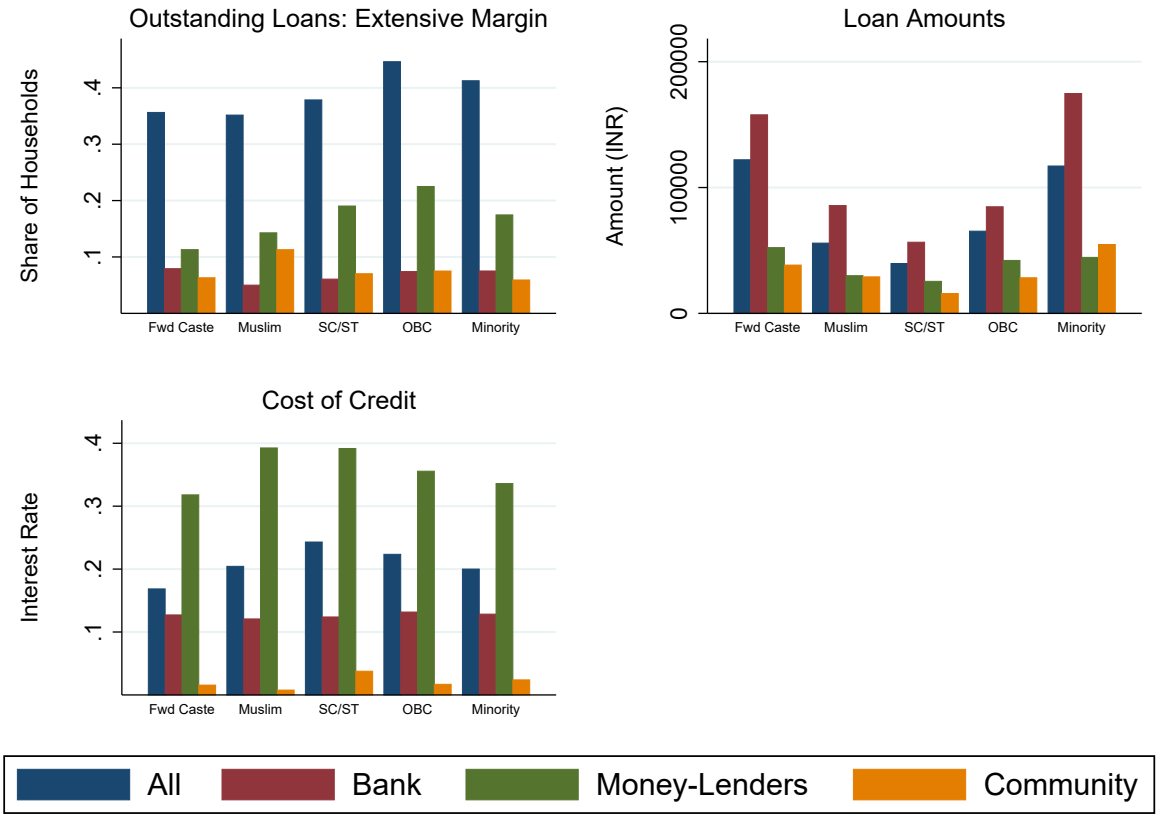
A.3 Household Assets

Appendix Figures A4 and A5 shows that relative to Hindu forward castes and other religious minorities, Muslim households had significantly lower levels of households savings – both bank deposits and retirement savings – and immovable assets in the form of land and real estate. As land and real estate often serve as collateral in loan covenants, lower values of pledgeable assets could also have contributed to the exclusion of these borrowers from credit markets, and a shrinking of loan size (Banerjee and Duflo, 2010).

In summary, Figures A1 - A5 show substantially lower access to bank credit for Muslim households along both the extensive and intensive margins. They were least likely to apply for a bank loan, and their bank loan applications were also more likely to be rejected. Muslim households also faced the highest rates of interest from informal money lenders, and had significantly lower levels of financial and physical assets. This suggests that the directed credit policy was unlikely to to be inframarginal for Muslim households. In contrast, the financial status of other religious minorities in terms of credit access and asset ownership were comparable to Hindu forward castes.

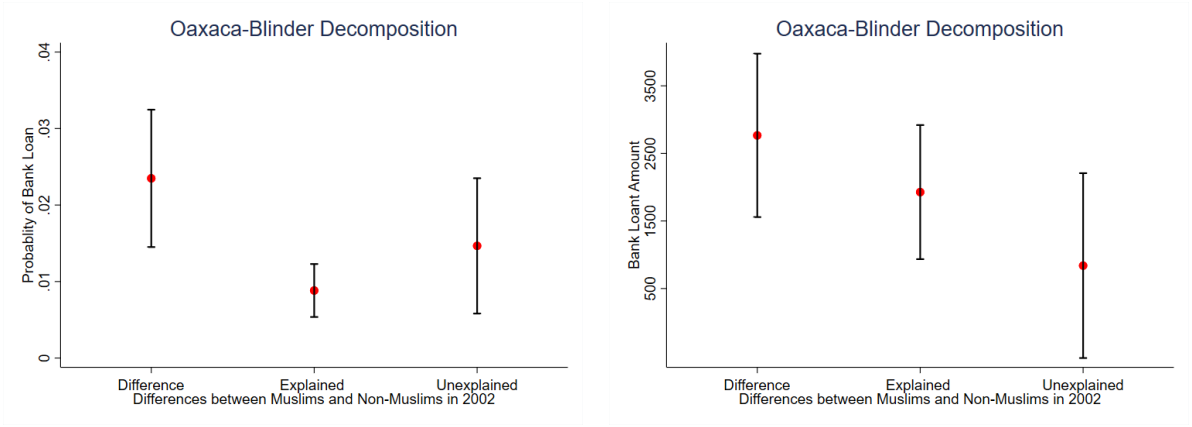
A.4 Figures

Figure A1: Pre-Treatment Household Credit Across Communities



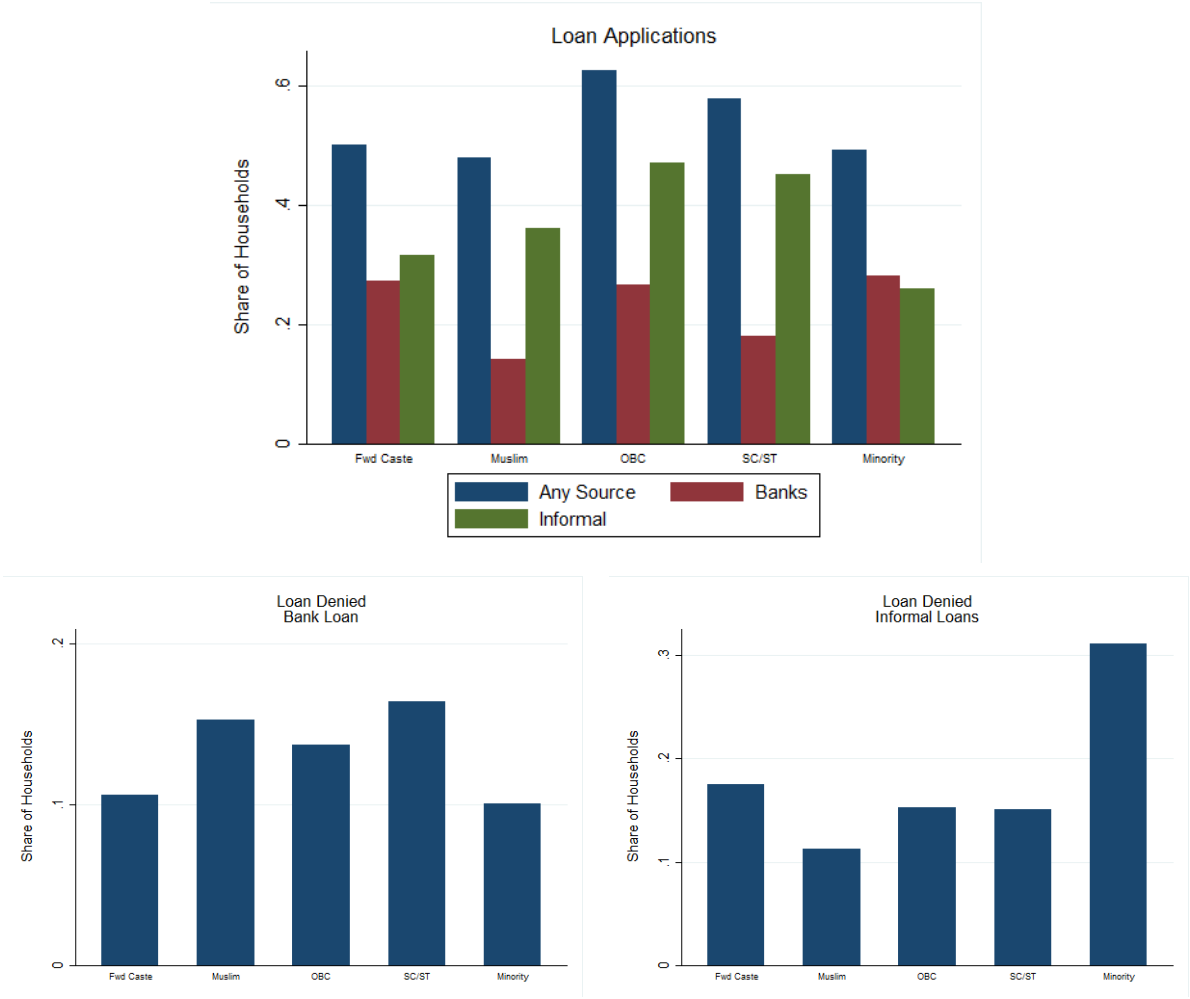
Notes: The above figure presents pre-treatment descriptive statistics for loan outcomes by community. The top-left panel shows the likelihood of households having loans from any of the above-mentioned sources; the top right-panel shows the average volume of credit received by households from each source; 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 A2: Pre-Treatment Credit Access Across Minority and Non-Minority Households: Oaxaca-Blinder Decomposition



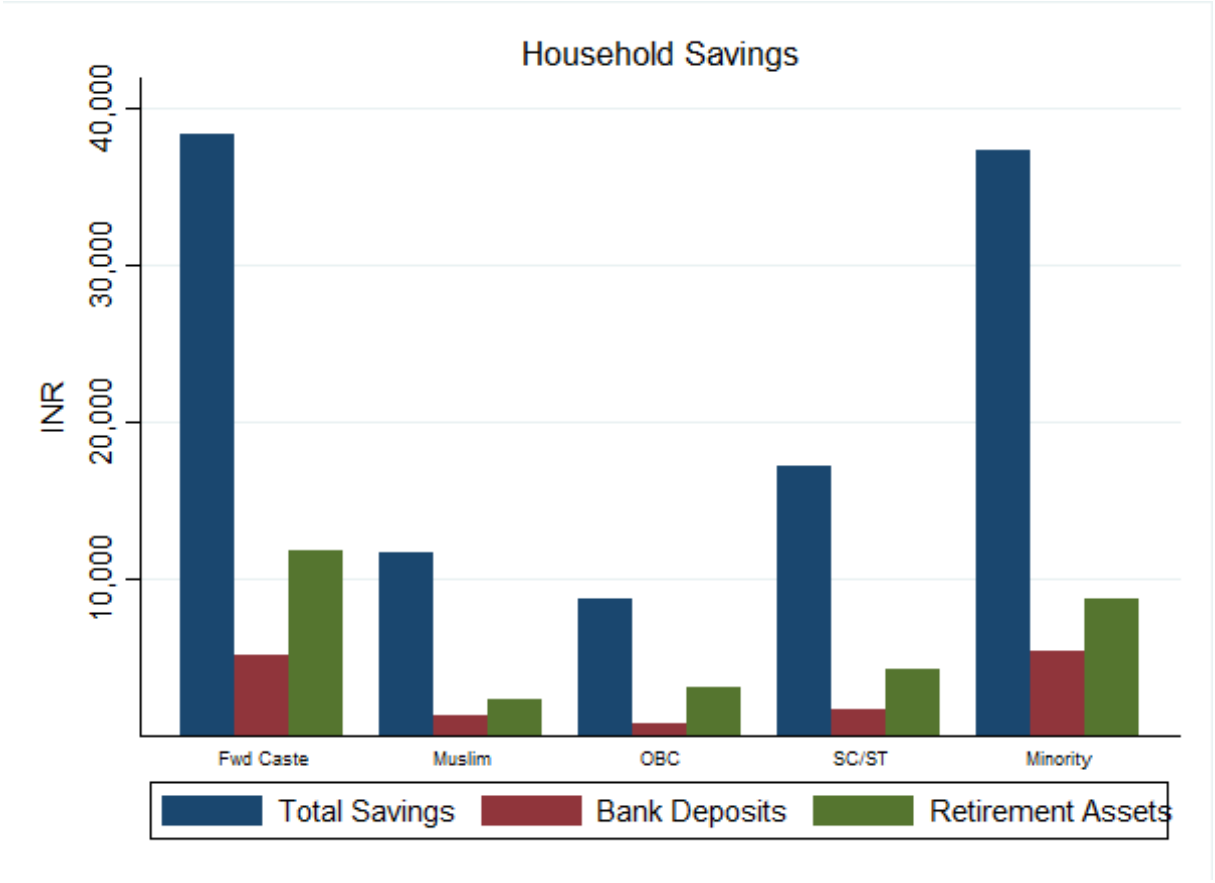
The above figures uses a Oaxaca-Blinder decomposition to test the role of unobservables in explaining the difference in bank credit access across Hindu and Muslim households in the pre-treatment period. The sample is restricted to Hindu and Muslim households. All specifications condition on district 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.

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



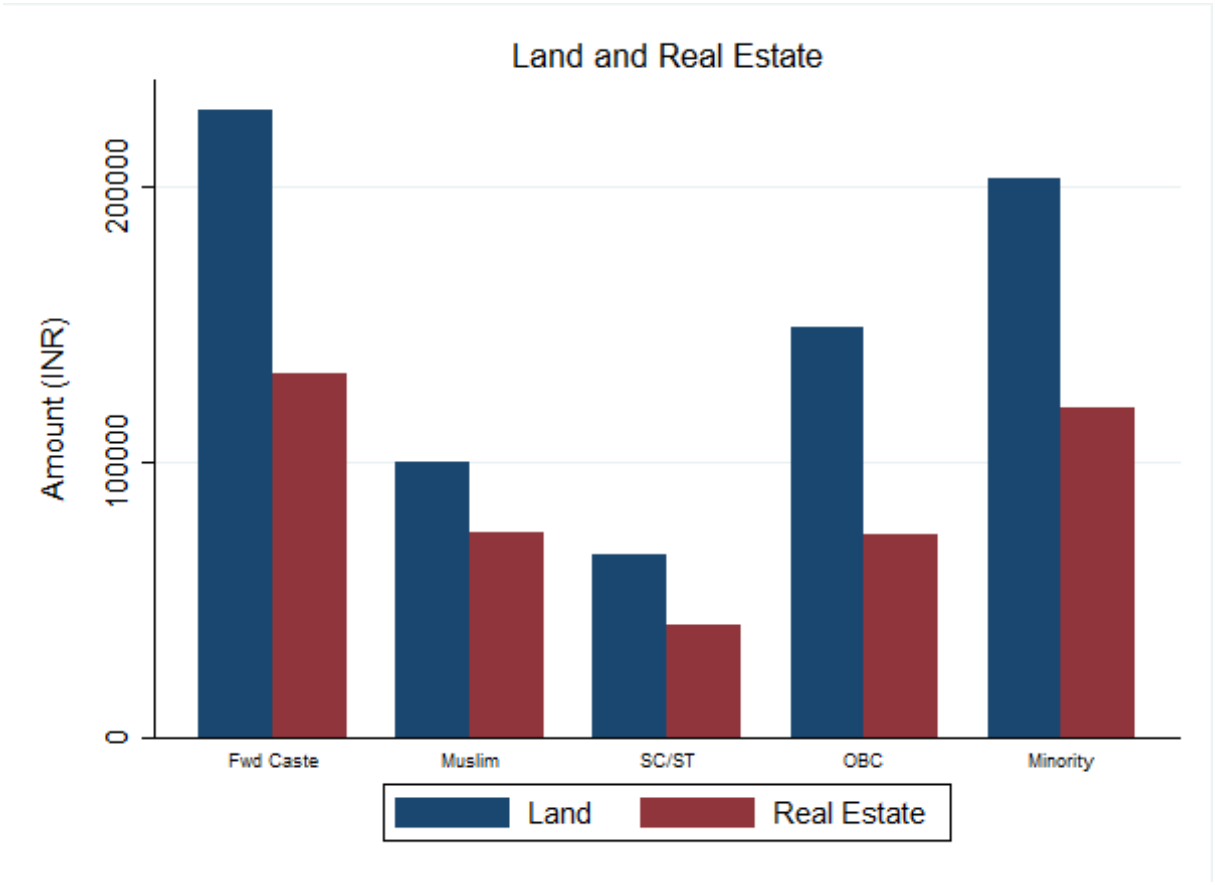
The above figures compares loan application and denial rates across communities using data from the Indian Human Development Survey (IHDS), conducted in 2011-12. Loan applications cover any loan application made by the household in the past five years, from the source mentioned. Loan denial is conditional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

Figure A4: Pre-Treatment Household Financial Assets Across Communities



Notes: The above figure presents pre-treatment descriptive statistics for risk-free financial assets held by households, across communities. *Bank* refers to bank deposits. *Minority* refers to non-Muslim religious minorities; *Fwd Caste* refer to non-SC/ST/OBC Hindu households. Data is from the AIDIS, 2003.

Figure A5: Pre-Treatment Household Land and Real Estate Across Communities



Notes: The above figure presents pre-treatment descriptive statistics for land and real estate held by households, across communities. *Real Estate* includes both residential and non-residential real estate. *Minority* refers to non-Muslim religious minorities; *Fwd Caste* refer to non-SC/ST/OBC Hindu households. Data is from the AIDIS, 2003.

A.5 Tables

Table A1: Access to Bank Credit for Religious Minorities in Control Districts: Heterogeneity by Common Lead Banks

	(1)	(2)	(3)	(4)
	Pr(Loan = 1)		Loan Amount (INR)	
	All Bank Loans	Bank SHG Loans	All Bank Loans	Bank SHG Loans
Muslim	-.034	-.002	-15060.845*	-420.745
	(.024)	(.014)	(8689.922)	(851.956)
Muslim*Lead Bank Match	-.012	-.002	-174.951	-56.735
	(.026)	(.017)	(11163.227)	(952.254)
Observations				
Dep Var Mean	.233	.077	50716.052	3432.639

Notes: The above table shows the treatment effect on bank credit access for minority households in non-minority concentration districts. The unit of observation is the household. The outcome of interest in columns (1)-(2) is a dummy equaling 1 if the household had any outstanding bank loan; in columns (3)-(4), the amount of bank loans received by the household. Columns (2) and (4) consider bank loans received through bank-linked self-help groups (SHGs). *Lead Bank Match* is a dummy equaling 1 if the lead bank in the district has also been a lead bank in a minority concentration district. All specifications include district fixed effects and household covariates. The sample is restricted to non-minority concentration districts, not located within a bandwidth of .058 around the discontinuity threshold. Specifications are weighted using household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

B Appendix: Balance Checks

Table B1: Pre-Treatment Balance on Household Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Demographics					Education		Employment			Consumption
	Rural	HH Size	Females	Children	Female Head	Secondary Educated	Higher Educated	Self Employed	Non-Farm Self Employed	Farm Work	Per Capita
Treat	.089	.885**	.089	.102	.039	.064	.018	.063	.055	.077	27.319
	(.055)	(.403)	(.191)	(.167)	(.027)	(.047)	(.015)	(.057)	(.095)	(.103)	(33.164)
Observations	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993
Dep Var Mean	.627	5.584	2.695	2.496	.110	.251	.049	.525	.351	.316	554.779

Notes: The above table shows the pre-treatment balance on household land holdings, real estate, savings and consumption for minority households. The unit of observation is the household. The outcome of interest in column (1) the area of irrigated farm land owned by the household; in column (2), total farm area; in column (3), tota land owned; in column (4), value of residential real estate; in column (5), value of total real estate; in column (6), value of bank deposits; in column (7), total risk-free financial assets; in column (8), per capita monthly household consumption. All specifications include state fixed effects, a linear polynomial in the running variable. Columns (2)-(10) also include household covariates. All specifications are weighted using a triangular kernel and AIDIS assigned household weights. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B2: Pre-Treatment Balance on Household Farm Land and Livestock

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Land					Livestock	
	Any Irrigated Area	Irrigated Farm Area	Irrigated Farm Value	Farm Area	Farm Value	Any Livestock	Livestock Value
Treat	-.004	.061	-4143.607	.137*	15595.739	-.011	119.653
	(.042)	(.046)	(13141.852)	(.078)	(11901.311)	(.079)	(390.440)
Observations	1993	1993	1993	1993	1993	1993	1993
Dep Var Mean	.169	.082	23500.044	.202	37660.714	.339	1934.010

Notes: The above table shows the pre-treatment balance on household land holdings, real estate, savings and consumption for minority households. The unit of observation is the household. The outcome of interest in column (1) the area of irrigated farm land owned by the household; in column (2), total farm area; in column (3), tota land owned; in column (4), value of residential real estate; in column (5), value of total real estate; in column (6), value of bank deposits; in column (7), total risk-free financial assets; in column (8), per capita monthly household consumption. All specifications include state fixed effects, a linear polynomial in the running variable. Columns (2)-(10) also include household covariates. All specifications are weighted using a triangular kernel and AIDIS assigned household weights. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B3: Pre-Treatment Balance on Household Farm Machinery

	(1)	(2)	(3)	(4)	(5)	(6)
	Farm Machinery		Transport			
	Any Machine	Machinery Value	Any Transport	Transport Value	Any Tractor	Tractor Value
Treat	-.116	-1021.445	-.014	-96.632	-.005	-15.926
	(.110)	(978.701)	(.014)	(187.739)	(.004)	(205.007)
Observations	1993	1993	1993	1993	1993	1993
Dep Var Mean	.488	1004.220	.033	174.769	.002	98.378

Notes: The above table shows the pre-treatment balance on household land holdings, real estate, savings and consumption for minority households. The unit of observation is the household. The outcome of interest in column (1) the area of irrigated farm land owned by the household; in column (2), total farm area; in column (3), tota land owned; in column (4), value of residential real estate; in column (5), value of total real estate; in column (6), value of bank deposits; in column (7), total risk-free financial assets; in column (8), per capita monthly household consumption. All specifications include state fixed effects, a linear polynomial in the running variable. Columns (2)-(10) also include household covariates. All specifications are weighted using a triangular kernel and AIDIS assigned household weights. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B4: Pre-Treatment Balance on Household Assets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Real Estate		Financial Assets		Business Assets		Transport	
	Buildings	Land and Buildings	Bank Savings	Financial Assets	Any Machine	Machine Value	Any Transport	Transport Value
Treat	-3741.423	40518.854	-358.433	8926.687**	-.004	-262.098	.025	2367.358
	(15782.407)	(28890.198)	(823.554)	(4200.977)	(.018)	(337.308)	(.103)	(3268.986)
Observations	1993	1993	1993	1993	1993	1993	1993	1993
Dep Var Mean	67847.308	1.45e+05	1158.907	7429.465	.092	766.218	.560	4150.895

Notes: The above table shows the pre-treatment balance on household land holdings, real estate, savings and consumption for minority households. The unit of observation is the household. The outcome of interest in column (1) the area of irrigated farm land owned by the household; in column (2), total farm area; in column (3), tota land owned; in column (4), value of residential real estate; in column (5), value of total real estate; in column (6), value of bank deposits; in column (7), total risk-free financial assets; in column (8), per capita monthly household consumption. All specifications include state fixed effects, a linear polynomial in the running variable. Columns (2)-(10) also include household covariates. All specifications are weighted using a triangular kernel and AIDIS assigned household weights. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B5: Pre-Treatment Balance on Household Credit: Extensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Any Loan = 1)					
	Any Source	Bank	Non Bank	Informal	Money Lender	Community
Treat	-.000	.004	-.014	.047	.028	.014
	(.033)	(.016)	(.015)	(.042)	(.028)	(.030)
Observations	1993	1993	1993	1993	1993	1993
Dep Var Mean	.285	.027	.032	.211	.124	.092

Notes: The above table shows the pre-treatment balance on household credit for minority households. The unit of observation is the household. The outcome of interest in column (1)-(3) is a dummy equaling 1 if the household has a loan from the source mentioned; in columns (4)-(6), the value of loans obtained from that source. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. All specifications are weighted using a triangular kernel and AIDIS assigned household weights. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B6: Pre-Treatment Balance on Household Credit: Intensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)
	Loan Amount (INR)					
	Any Source	Bank	Non Bank	Informal	Money Lender	Community
Treat	-2895.303	106.123	-856.669	-1935.327	-871.601	-1063.726
	(3474.605)	(2505.643)	(1517.745)	(1667.373)	(1080.519)	(998.547)
Observations	1993	1993	1993	1993	1993	1993
Dep Var Mean	11312.726	1457.078	2736.800	5946.208	3236.216	2709.991

Notes: The above table shows the pre-treatment balance on household credit for minority households. The unit of observation is the household. The outcome of interest in column (1)-(3) is a dummy equaling 1 if the household has a loan from the source mentioned; in columns (4)-(6), the value of loans obtained from that source. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. All specifications are weighted using a triangular kernel and AIDIS assigned household weights. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B7: Pre-Treatment Balance on Bank Loan Purpose

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Any Bank Loan = 1)			Bank Loan Amount (INR)		
	Farm	Non Farm	Consumption	Farm	Non Farm	Consumption
Treat	-.002	-.000	.003	150.173	-449.626	573.479
	(.011)	(.007)	(.006)	(2246.359)	(680.935)	(428.136)
Observations	1993	1993	1993	1993	1993	1993
Dep Var Mean	.014	.006	.007	540.775	451.256	350.700

Notes: The above table shows the pre-treatment balance on purpose of bank credit for minority households. The unit of observation is the household. The outcome of interest in column (1)-(3) is a dummy equaling 1 if the household has any bank loan for the purpose mentioned; in columns (4)-(6), the value of bank loans obtained for that purpose. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. All specifications are weighted using a triangular kernel and AIDIS assigned household weights. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B8: Pre-Treatment Balance on Loan Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Cost of Credit			Pr(Default = 1)			Pr(Long Term Loan = 1)			Pr(Unsecured Loan = 1)		
	Any Source	Bank	Informal	Any Source	Bank	Informal	Any Source	Bank	Informal	Any Source	Bank	Informal
Treat	-.007	.005	-.105	.104	-.007	-.051	.034	-.006	.011	.046	.006	.027
	(.074)	(.012)	(.072)	(.082)	(.282)	(.101)	(.040)	(.016)	(.038)	(.035)	(.010)	(.041)
Observations	877	116	633	748	107	488	1993	1993	1993	1993	1993	1993
Dep Var Mean	.192	.131	.211	.646	.749	.656	.167	.022	.114	.229	.016	.188

Notes: The above table shows the pre-treatment balance on loan characteristics for minority households. The unit of observation is the household. The outcome of interest in column (1)-(3) is the interest rate from the loan sources mentioned; in columns (4)-(6), a dummy equaling 1 if the household has an unsecured loan from the source mention. For households with multiple outstanding loans, the average interest rate is weighted by initial loan size. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. All specifications are weighted using a triangular kernel and AIDIS assigned household weights. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B9: Balance on Weekly Labour Market Activities: Extensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pr(Outcome = 1)						
	Labour Force Participation	Unemployed	Self Employed	Wage Work	Farm	Non Farm	Manufacturing
Treat	.019	-.034	.078***	-.033	.028	-.001	.002
	(.023)	(.029)	(.028)	(.031)	(.026)	(.022)	(.021)
Observations	5050	5050	5050	5050	5050	5050	5050
Dep Var Mean	.566	.067	.302	.250	.205	.347	.095

Notes: The above table shows the treatment effect on weekly labour market outcomes along the extensive margin for minority working-age individuals. 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 column (1) is a dummy equal to 1 if the individual participated in the labour force in any of the 7 days preceding the survey; in column (2), a dummy equal to 1 if the individual was unemployed in any of the 7 days preceding the survey; in columns (3)-(6), a dummy equal to 1 if the individiual was engaged in the sector referred. All specifications include state and survey month fixed effects; a linear polynomial in the running variable; controls for demographic and education covariates at the individiual level; household level controls. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B10: Pre-Treatment Balance on District Banking Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Bank Branches			Deposits		Credit	
	All Banks	State-Owned Banks	Private Banks	Accounts	Amount	Accounts	Amount
Treat	-6.065	-1.706	-4.359	-.195	-22395.830	-.040	-18953.575
	(30.870)	(27.755)	(4.573)	(.235)	(34629.274)	(.066)	(33846.067)
Observations	59	59	59	59	59	59	59
Dep Var Mean	67.404	62.730	4.674	.427	23212.736	.099	19430.789

Notes: The above table shows the pre-treatment balance on district-level banking outcomes. The unit of observation is the district. The outcome of interest in column (1)-(3) is the number of bank branches in the district; in columns (4)-(5), bank deposits; columns (6)-(7), bank loans; and columns (8)-(10), bank loans by sector of lending. Credit and deposit amounts are in millions of rupees. All specifications include a linear polynomial in the running variable and are weighted using a triangular kernel. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B11: Pre-Treatment Balance on District Banking Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
			Per Capita Bank Credit			
	Farm Account	Farm Amount	Manufacturing Account	Manufacturing Amount	Personal Loans Account	Personal Loans Amount
Treat	.002	924.474	.001	-12897.358	-.042	-608.173
	(.009)	(1497.737)	(.001)	(22206.290)	(.059)	(3218.739)
Observations	59	59	59	59	59	59
Dep Var Mean	.039	3772.214	.003	10909.024	.043	3962.045

Notes: The above table shows the pre-treatment balance on district-level banking outcomes. The unit of observation is the district. The outcome of interest in column (1)-(3) is the number of bank branches in the district; in columns (4)-(5), bank deposits; columns (6)-(7), bank loans; and columns (8)-(10), bank loans by sector of lending. Credit and deposit amounts are in millions of rupees. All specifications include a linear polynomial in the running variable and are weighted using a triangular kernel. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B12: Directed Credit Policy and Electoral Outcomes: Pre-treatment Balance

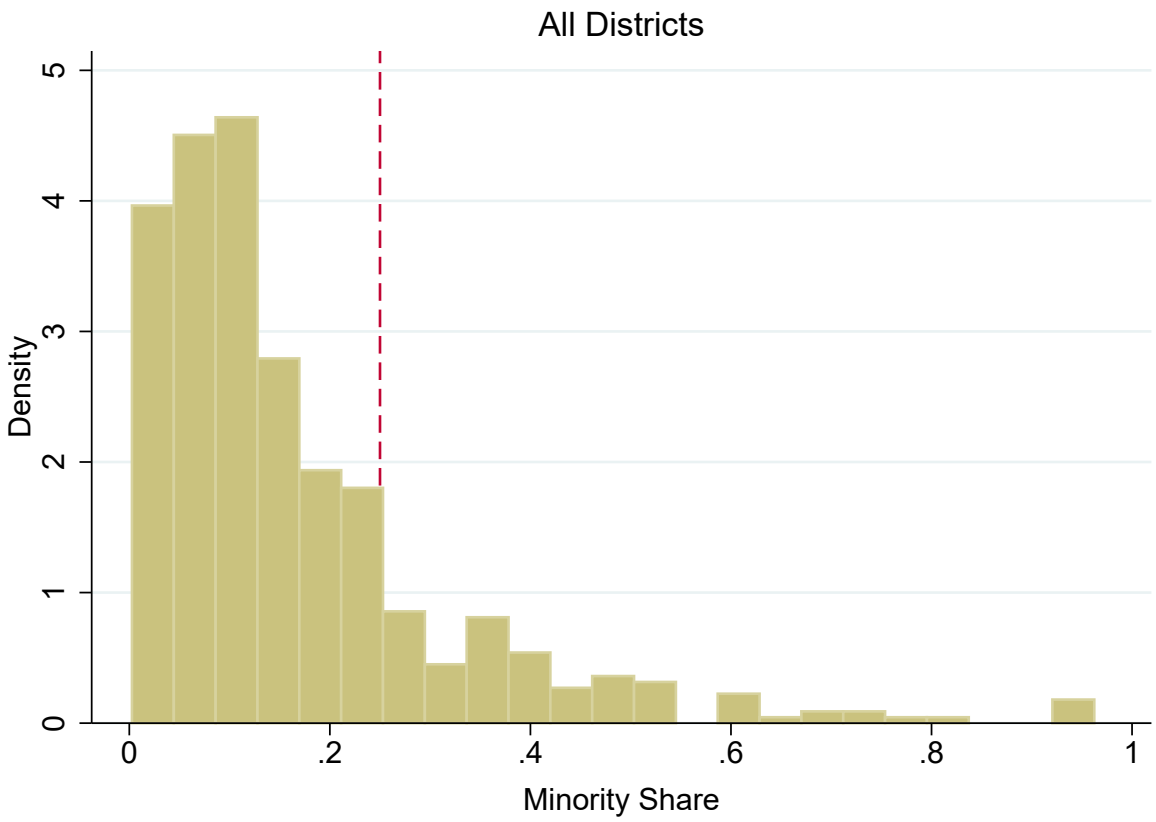
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Muslim Candidates						Right Wing Party			Constituency Characteristics	
	Vote Share	Any Candidate	Major Party Candidate	Major Party Vote Share	Win	Major Party Win	Vote Share	Vote Share	Win	Turnout	Voters
Treat	-.017	-.027	.076	.013	.086	-.006	-.035	-.011	.021	.058	-4.81e+04**
	(.033)	(.091)	(.053)	(.028)	(.058)	(.038)	(.032)	(.023)	(.039)	(.045)	(20619.682)
Observations	393	422	422	422	422	422	284	422	422	422	422
Dep Var Mean	.079	.466	.133	.029	.056	.022	.278	.205	.298	.585	1.60e+05

Notes: This table identifies the impact of the directed credit policy on electoral outcomes for politicians from minority communities and those representing the right-wing party. The unit of observation is the electoral constituency. The outcome of interest in column (1) is the total vote share received by Muslim politicians contesting elections to the state legislative assembly; in column (2), a dummy equaling 1 if there was a Muslim candidate contesting the election; in column (3), a dummy equaling 1 if any Muslim candidate contested on behalf of a major political party; in column (4), the vote share received by a Muslim candidate contesting on behalf of a major political party; in column (5), the vote share received by the BJP candidate, conditional on participation; in column (6), the vote share received by the BJP candidate. 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 .058 around the discontinuity threshold, and are weighted using a triangular kernel. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

C Appendix: Additional Figures and Tables

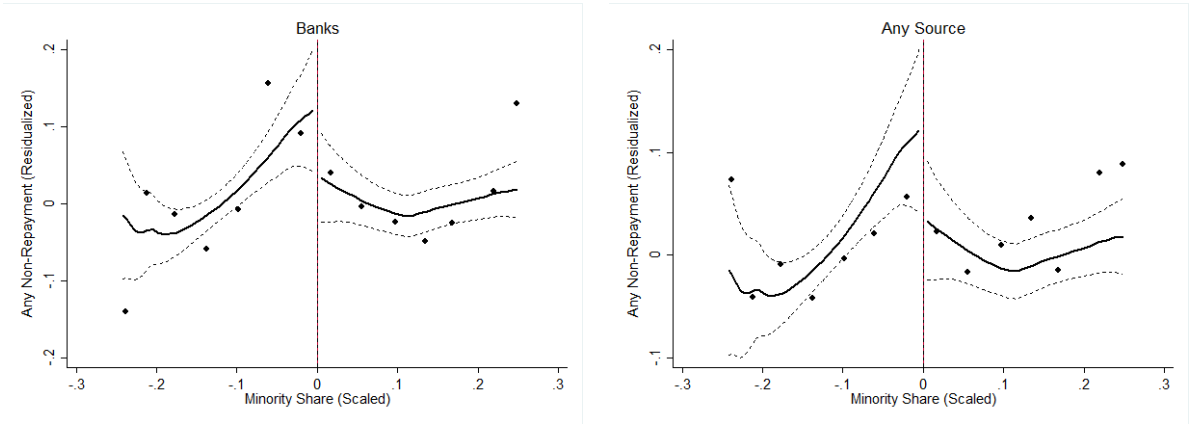
C.1 Figures

Figure C1: 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 C2: Loan Delinquency for Minority Households



The above figures shows the treatment effect for access to bank credit for minority households. The unit of observation is the household. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). 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). The horizontal lines show the linear fit from a local linear regression. The local linear regressions include state fixed effects, household covariates and a linear polynomial in the running variable. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. Estimates are weighted using a triangular kernel and household specific weights provided by the AIDIS.

C.2 Tables

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 C3: Robustness of Baseline Treatment Effects to Alternate Specification Choices

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Loan = 1)			Loan Amount (INR)		
Treat	.295***	.177***	.178***	35987.118**	59420.081***	21048.977**
	(.048)	(.023)	(.063)	(14191.902)	(5312.745)	(8476.572)
Observations	1765	1586	1765	1765	1586	1765
Dep Var Mean	.115	.115	.115	20801.635	20801.635	20801.635
Polynomial	Quadratic	Linear	Linear	Quadratic	Linear	Linear
Dist. Controls	N	Y	N	N	Y	N
Dist. Cluster	Y	Y	N	Y	Y	N

Notes: The above table shows robustness of the baseline treatment effects to alternate specification and sample choices. The unit of observation is the household. The outcome of interest in columns (1)-(3) is a dummy equaling 1 if the household had any outstanding bank loan; in columns (4)-(6), the amount of bank loans received by the household. Columns (1) and (3) include a quadratic polynomial in the running variable. Columns (2) and (5) extend the covariate vector to include select district covariates. Columns (3) and (6) use robust standard errors as opposed to clustering by district. Except for columns (2) and (5), all specifications include a linear polynomial in the running variable, state fixed effects, and household covariates. All specifications are restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using household-specific weights, and a triangular kernel. Standard errors in parentheses, clustered by district, except for columns (3) and (6). Significant levels: *10%, **5%, and ***1%

Table C4: Robustness of Baseline Treatment Effects to Alternate Samples

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Loan = 1)			Loan Amount (INR)		
Treat	.180***	.170***	.298***	21362.226***	21339.415***	39614.209**
	(.032)	(.028)	(.107)	(6176.088)	(7120.914)	(15574.208)
Observations	1721	2480	3776	1721	2480	3776
Dep Var Mean	.115	.148	.166	20801.763	28452.875	24523.878
Excl. New Dist.	Y	N	N	Y	N	N
Incl. Oth. Minorities	N	Y	Y	N	Y	Y
RD Type	Sharp	Sharp	Fuzzy	Sharp	Sharp	Fuzzy

Notes: The above table shows robustness of the baseline treatment effects to alternate specification and sample choices. The unit of observation is the household. The outcome of interest in columns (1)-(3) is a dummy equaling 1 if the household had any outstanding bank loan; in columns (4)-(6), the amount of bank loans received by the household. Columns (1) and (4) exclude new districts formed from existing Census districts, whose treatment status are unspecified. Columns (2) and (5) expand the sample to include other religious minorities. Columns (3) and (6) use a fuzzy RD specification to include all districts classified as minority concentration. All specifications include a linear polynomial in the running variable, state fixed effects, and household covariates. All specifications restrict the sample to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using household-specific weights, and a triangular kernel. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table C5: Treatment Effect on Overall and Non-Bank Sources of Credit for Minority Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Loan = 1)				Loan Amount (INR)			
	Any Source	Bank	Informal	Non-Bank Financial Institutions	Total Loans	Banks	Informal	Non-Bank Financial Institutions
Treat	.015	.178***	.040	-.144***	21564.692	21048.977***	10418.295	-9891.569
	(.080)	(.031)	(.066)	(.044)	(17613.971)	(6386.733)	(8538.272)	(7429.366)
Observations	1765	1765	1765	1765	1765	1765	1765	1765
Dep Var Mean	.323	.115	.165	.091	44059.071	20801.635	14671.783	8566.542

Notes: The above table shows the treatment effect on household credit across alternate sources of credit. The unit of observation is the household. The outcome of interest in columns (1)-(4) is a dummy equaling 1 if the household had any outstanding loan from the sources mentioned; in columns (5)-(8), the amount of bank loans received by the household. *Informal* loans include loans from professional money lenders, input suppliers, and friends, relatives and landlord. Non-bank financial institutions include co-operative banks and non-bank financial corporations, including micro-finance institutions. All specifications include state fixed effects, a linear polynomial in the running variable, and controls for household size and rural location. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table C6: Treatment Effect on Sources of Informal Credit for Minority Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pr(Any Loan = 1)		Pr(Bank and Informal Loan = 1)			Loan Amount (INR)	
	Professional Money Lenders	Community Networks	Any Informal Source	Professional Money Lenders	Community Networks	Professional Money Lenders	Community Networks
Treat	-.051	.092	.036*	-.001	.037**	-238.169	10656.464
	(.039)	(.081)	(.019)	(.013)	(.015)	(1217.230)	(9051.379)
Observations	1765	1765	1765	1765	1765	1765	1765
Dep Var Mean	.064	.103	.030	.012	.018	4668.111	10003.671

Notes: The above table shows the treatment effect across sources of informal credit. The unit of observation is the household. The outcome of interest in columns (1)-(5) is a dummy equaling 1 if the household had any outstanding loan from the sources mentioned; in columns (6)-(7), the amount of bank loans received by the household. *Professional Money Lenders* include loans from professional money lenders and input suppliers. *Community Networks* include loans from friends, relatives, employers and landlord. The outcome of interest in column (3) is a dummy equaling 1 if the household has an outstanding loan from a bank and any informal source; in column (4), a dummy equaling 1 if the household has an outstanding loan from a bank and a professional money lender; in column (5), a dummy equaling 1 if the household has an outstanding loan from a bank and community networks. All specifications include state fixed effects, a linear polynomial in the running variable, household size and a rural indicator. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table C7: Treatment Effect on Loan Delinquency

	(1)	(2)	(3)	(4)
	Pr(Any Delinquent Loan = 1)			
	Banks	Bank SHG	Commercial Banks	Any Source
Treat	-.045	-.250***	.022	.064
	(.066)	(.076)	(.070)	(.056)
Observations	465	95	376	991
Dep Var Mean	.292	.348	.281	.315

Notes: The above table shows the treatment effect on the likelihood of having a delinquent loan for minority households. The unit of observation is the household. The outcome of interest is a dummy equaling 1 if the household has a delinquent loan from the sources mentioned. A loan is classified as delinquent if there has been no repayment towards that loan for at least 6 months preceding that survey. *Bank-SHG* in column (2) refers to loans received through bank-linked self-help groups. *Commercial Banks* in column (3) refers to loans received directly from commercial banks. All specifications include state fixed effects, a linear polynomial in the running variable, and controls for household size and rural location. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights.Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table C8: Treatment Effect on Loan Duration: Minority Households

	(1)	(2)	(3)	(4)
	Pr(Loan Duration > 1 Yr = 1)			
	Banks	Informal	Non Bank Financial Institution	Any Source
Treat	.125***	-.022	-.118***	-.070
	(.026)	(.057)	(.028)	(.067)
Observations	1765	1765	1765	1765
Dep Var Mean	.101	.141	.071	.275

Notes: The above table shows the treatment effect on the likelihood of having a long-term loan for minority households. Long-term loans are defined as a loan whose duration exceeds 1 year. The unit of observation is the household. The outcome of interest is a dummy equaling 1 if the household has a long-term loan from the sources mentioned. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table C9: Purpose of Bank Credit: Minority Households

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Loan = 1)			Loan Amount (INR)		
	Farm	Non Farm	Expenditure	Farm	Non Farm	Expenditure
Treat	.051***	.018	.092***	6359.716**	-862.725	15231.539**
	(.019)	(.024)	(.027)	(2701.171)	(2177.439)	(6554.996)
Observations	1765	1765	1765	1765	1765	1765
Dep Var Mean	.033	.014	.051	3909.646	2064.700	11634.652

Notes: The above table shows the treatment effect for the purpose of bank borrowing for minority households. The unit of observation is the household. The outcome of interest in columns (1)-(3) is a dummy equaling 1 if the household had any outstanding bank loan for the purpose mentioned; in columns (4)-(6), the amount of bank loans received by the household for the purpose mentioned. *Expenditure* loans include household loans obtained for expenditure on consumption items, health and education, and housing. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table C10: Purpose of Bank Expenditure Loans: Minority Households

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Loan = 1)			Loan Amount (INR)		
	Health and Education	Housing	Consumption	Health and Education	Housing	Consumption
Treat	.018***	.050**	.045***	609.541**	13271.860**	1350.138
	(.005)	(.023)	(.016)	(254.883)	(6604.250)	(1685.966)
Observations	1765	1765	1765	1765	1765	1765
Dep Var Mean	.004	.020	.027	773.008	8902.867	1958.777

Notes: The above table shows the treatment effect on three types of expenditure loans for minority households. The unit of observation is the household. The outcome of interest in columns (1)-(3) is a dummy equaling 1 if the household had any outstanding bank loan for the purpose mentioned; in columns (4)-(6), the amount of bank loans received by the household for the purpose mentioned. *Consumption* loans include household loans obtained for household expenditures which does not include spending on health or education, or spending on housing materials, land, or the direct purchase of housing. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table C11: Treatment Effect on Minority Household Farm Assets

	(1)	(2)	(3)	(4)	(5)	(6)
	Farm Machinery		Farm Transport			
	Any Machine	Machine Value	Any Farm Transport	Farm Transport Value	Any Tractor	Tractor Value
Treat	.261***	3007.985***	.036***	1570.863	.001	1686.520
	(.100)	(1072.893)	(.014)	(1143.729)	(.003)	(1067.701)
Observations	1765	1765	1265	1765	1765	1765
Dep Var Mean	.390	2772.477	.049	3322.419	.012	2961.867

Notes: The above table shows the treatment effect on household farm machinery and farm transport assets. The unit of observation is the household. The outcome of interest in column (1) is a binary equaling 1 if the household owned any farm machinery; in column (2), value of farm machinery (INR); in column (3), a binary equaling 1 if the household owns any farm transport; in column (4), the value of farm transport (INR); in column (5), a binary equaling 1 if the household owned a tractor; in column (6), the value of tractors (INR). All specifications include state fixed effects, a linear polynomial in the running variable and household covariates. All specifications are weighted using a triangular kernel and AIDIS assigned household weights. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table C12: Treatment Effect on Minority Household Farm Land and Livestock

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Farm Land					Livestock	
	Any Irrigated Land	Irrigated Farm Area	Irrigated Farm Value	Farm Area	Farm Value	Any Livestock	Livestock Value
Treat	.094	.120	144539.375******	-.027	308104.549******	.115	-751.228
	(.058)	(.075)	(43900.653)	(.061)	(93520.890)	(.126)	(1696.726)
Observations	1765	1765	1765	1765	1765	1765	1765
Dep Var Mean	.157	.207	198955.22	.615	372481.147	.311	9218.514

Notes: The above table shows the treatment effect for farm and livestock outcomes for minority households. The unit of observation is the household. The outcome of interest in column (1) is a dummy equaling 1 if the household owns any irrigated land; in column (2), the area of irrigated farm land owned; in column (3), value of irrigated farm area; in column (4), total farm land owned; in column (5), value of farm land owned; in column (6), a binary equaling 1 if the household owns any livestock; in column (7), the value of livestock owned. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. All specifications are weighted using a triangular kernel and AIDIS assigned household weights. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table C13: Treatment Effect on Real Estate and Household Assets: Minority Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Real Estate			Machinery		Transport			
	Res.Bld. Area	Res. Bld. Value	Land and Buildings	Any Machine	Machine Value	Any Trans.	Trans. Value	Any Household Trans.	Household Trans. Value
Treat	-.088	238534.989*	945713.840***	-.020	-1478.255	.018	11914.879**	.064	5782.258
	(8.772)	(141360.523)	(176309.735)	(.055)	(1069.128)	(.083)	(5651.738)	(.058)	(3787.665)
Observations	1765	1765	1765	1765	1765	1765	1765	1265	1765
Dep Var Mean	63.692	549001.824	1527689.182	.078	2197.101	.720	61334.582	.946	50310.033

Notes: The above table shows the treatment effect on minority households' ownership of real estate and non-farm productive assets. The unit of observation is the household. The outcome of interest in column (1) is the area of residential real estate; in column (2), the value of residential real estate; in column (3), the combined value of land and buildings; in column (4), a binary equal to 1 if the household owns any non-farm machinery; in column (5), the value of non-farm machinery; column (6), a binary equal to 1 if the household owns any transport equipment; in column (7), the value of transport equipment; in column (8), a binary equaling 1 if the household owns any transport equipment for household use; in column (9), the value of transport owned for household use. All specifications include state fixed effects, a linear polynomial in the running variable and household covariates. All specifications are weighted using a triangular kernel and AIDIS assigned household weights. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table C14: Minority Credit Access and Minority Participation in Self-Help Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	SHG Formation 2008-19			SHG Formation 2014-19			SHG Formation 2001-07		
	Any Muslim	Only Muslim	Mixed Muslim	Any Muslim	Only Muslim	Mixed Muslim	Any Muslim	Only Muslim	Mixed Muslim
Treat	.030	.044*	-.017	.027	.046**	-.021	.008	.023	-.021
	(.035)	(.026)	(.017)	(.032)	(.022)	(.016)	(.026)	(.020)	(.014)
Observations	278533	278533	278533	224868	224868	224868	160056	160056	160056
Dep Var Mean	.170	.057	.112	.173	.052	.119	.132	.041	.090

Notes: The above table shows the treatment effect on the participation of religious minorities in self-help groups (SHGs) registered with commercial banks. The unit of observation is the self-help group. The outcome of interest columns (1), (4) and (7) is a binary equaling 1 if the SHG has any Muslim member; in columns (2), (5) and (8), a binary equaling 1 if the self-help group has only Muslim members; in columns (3), (6) and (9), a binary equaling 1 if the SHG has both Muslim and other non-minority members. Columns (1)-(3) consider all SHGs registered between 2008 and 2019; columns (4)-(6) consider SHGs registered between 2014 and 2019; columns (7)-(9) show balance over the pre-treatment period. All specifications include state, year and bank fixed effects, along with a linear polynomial in the running variable. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using a triangular kernel. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table C15: Minority Credit Access and District Population

	(1)	(2)	(3)	(4)	(5)
	Share of Households		Population		
	Muslim	Hindu	Muslim	Hindu	Total
Treat	-.043	.132	.010	.353	.310
	(.078)	(.127)	(.175)	(.382)	(.551)
Observations	61	61	61	61	61
Dep Var Mean	.147	.700	.280	1.338	1.743

Notes: The above table shows the treatment effect on district population. The unit of observation is the district. The outcome of interest in the first two columns is the share of households in the community of interest; in columns (3)-(4), the district population (in millions) for the community of interest; in column (5), the total district population. The data is from the AIDIS, conducted in 2019. The sample is restricted to a bandwidth of .058 around the discontinuity threshold. All specifications are weighted using a triangular kernel. Standard errors in parentheses. Significant levels: *10%, **5%, and ***1%

D Appendix: Labor Market Outcomes

Recent work by Breza and Kinan (2021) showed adverse labor market impacts in response to a credit contraction by micro-finance institutions in India, both through its direct impact on business operations, and indirect effects through lower aggregate demand. Bruhn and Love (2014) showed financial deepening in Mexico to enable self-employed business owners to continue operating their business, and not have to seek wage employment. This section details the impact of increased credit access on labor market outcomes for minority households.

At the outset, the directed credit policy could have altered occupational choices of minority workers through two possible channels. First, higher farm credit could have directly affected farm labor. Section 5.2.1 points to increased mechanization of farm work through investments in farm machinery and irrigation, but no accompanying increase in land holdings. If households used farm credit from banks to invest in labor saving technology, it could have freed labor for non-farm activities – the labor push channel, seen in Bustos et al. (2016). Alternately, the mechanization of agriculture could have improved farm yields, leading to higher local demand. This in turn could have generated higher employment in local non-farm employment to meet the additional demand for goods and services – the labor pull channel, evidenced by Emerick (2018). Relatedly, expenditure loans from banks could also have increased local demand for goods and services, generating higher demand for non-farm labor.

We examine the labor market impacts of the directed credit policy using data from the Primary Labour Force Survey (PLFS). The PLFS is a nationally representative survey conducted by the NSS and provides data on labor force characteristics. It is the successor to the quinquennial employment-unemployment surveys conducted by the NSS between 1987 and 2011. We use weekly data from the PLFS which inquires individuals for their labor force participation in each of the 7 days preceding the survey. Conditional on participation, the PLFS also provides detailed information on the type of activity undertaken, as well as the hours of work accorded to that activity in each of the seven days. Locational identifiers at the level of district, along with information on individual age, educational attainment, caste and religion are also provided in the PLFS.

Summary statistics of weekly labor market activities are shown in Appendix Tables D1.A and D1.B. Religious minorities comprised about 15% of working-aged individuals, and 70% of working-aged individuals were in rural areas. Less than 40% of working-aged individuals had completed secondary or higher education, and under 15% of working-aged individuals had a college degree. Overall weekly labor force participation was 55%. Conditional on participation in the labor force, individuals worked 46 hours a week, with almost 40% of workers engaged in farm activities. 30% of the workers were engaged in trade or service activities, with manufacturing and construction work accounting for approximately 10% of workers. 12% of weekly non-farm workers reported being self-employed. Amongst manufacturing workers, a third were self-employed, and three-fourths of self-employed manufacturing workers operated establishments located within their residence.

We extend our RD strategy for causal identification and estimate the following specification:

$$Y_{ids} = \alpha_s + \beta Treat_{ds} + f(Runvar_{ds}) + \gamma \mathbf{Z}_{ids} + \epsilon_{ids} \tag{D5}$$

The unit of observation is now the working-aged individual, defined as individuals aged between 18 and 65. $\mathbf{Z}$ includes individual controls for gender, and a quadratic in age. We continue to include a linear polynomial in the running variable. A triangular kernel is used to weight the regressions, along with individual-specific weights provided by the PLFS. Standard errors are clustered by district for inference and the sample is restricted to districts located within a bandwidth of .058 around the discontinuity threshold. We identify the impact of the directed credit policy on labor market outcomes along both the extensive and intensive margins.

Columns (1) and (2) of Appendix Table D2 shows that the directed credit policy left unaffected

overall labor force participation, or unemployment rate for minority workers. Column (5) rules out the labor push channel by identifying a positive, but statistically non-significant coefficient corresponding to farm work. Instead, columns (3) and (4) points to a reallocation of labor from salaried work to self-employment: minority workers in minority concentration areas have a 11 ppt. higher likelihood of being self-employed during the week, and a 9 ppt. lower likelihood of being employed in salaried work. Columns (7) and (8) also offer evidence of sectoral reallocation: minority workers in treated districts were 6 ppt. more likely to be employed in the manufacturing sector, with a comparable decline in their likelihood of working in the trade and service sectors. Appendix Table D3 identifies very similar effects along the intensive margin. While total labor hours during the week was unaffected by the treatment (column (1)); column (2) shows 5 additional hours of self-employment for minority workers, accompanied by a decline in wage work of 6 hours (column (3)). Likewise, column (4) shows time spent in farm activities to be unimpacted by the directed credit policy, and column (6) reports 3 additional hours of manufacturing work. As seen in Appendix Tables D4 and D5, the increase in manufacturing employment is primarily in the form of self-employment, as opposed to wage work. Disaggregating the manufacturing, trade and service sectors into its largest components, Appendix Table D6 shows that the positive treatment effect for minority workers in manufacturing employment was driven by higher self-employment in textiles, with labor being drawn away from wage work in the transport and hospitality sectors.

Appendix Tables D4-D5 examine this reallocation across occupation type and sectors by combining workers' occupation with their sector of employment. Across both tables, columns (3) and (6) show that the treatment led to minority workers reallocating their labor from wage work in trade and services, to manufacturing self-employment. The findings are similar to Bruhn and Love (2014), who showed that the sudden expansion of bank branches in Mexico increase the survival of small business owners, and reduced their propensity to engage in wage work. This is however contrary to Gupta and Dehejia (2021), who found that financial deepening by state-owned banks in India facilitated a shift away from self-employment towards wage work. To further identify the specific sectors driving this reallocation, we disaggregate manufacturing into two of its two largest components (in terms of employment): food and beverages, and textiles. Similarly, trade and services are disaggregated into trade, transport, hospitality and other services. Column (2) of Appendix Table D6 shows that the positive treatment effect in manufacturing employment was driven by increased employment in textiles. Alternately, while directed bank credit did not affect minority employment in trading activities, it moved labor away from [columns (6)-(7)].

Overall, there is little evidence to suggest that the mechanization of farm work led to labor being pushed out from farm to non-farm activities, negating the labor push channel. Instead, exposure to the directed bank credit resulted in the reallocation of minority workers away from wage work in the transport and hospitality sectors, to self-employment in textile manufacturing.

D.1 Tables

Table D1.A: Summary Statistics: Working-Aged Individuals

	N	Mean	SD
Hindu	229488	0.843	0.363
Muslim	229488	0.116	0.320
Christian	229488	0.018	0.134
Sikh	229488	0.004	0.065
SC/ST	229488	0.278	0.448
OBC	229488	0.438	0.496
Rural	229488	0.692	0.462
Female	229488	0.501	0.501
Age	229488	35.797	12.060
Secondary or Higher Educated	229488	0.380	0.485
Higher Educated	229488	0.133	0.340
LFP	229488	0.556	0.497

Notes: This table shows the summary statistics for working-aged individuals from the Primary Labour Force Survey, 2017-18. Working-aged individuals are individuals aged between 18 and 60. LFP is labour force participation rate.

Table D1.B: Summary Statistics: Workforce Characteristics

	N	Mean	SD
Unemployed	126361	0.087	0.282
Farm	126361	0.378	0.485
Manufacturing	126361	0.114	0.318
Construction	126361	0.108	0.311
Trade	126361	0.095	0.293
Services	126361	0.207	0.405
Hours Worked	126361	46.267	19.816
Farm Hours	126361	17.366	23.608
Manufacturing Hours	126361	6.187	17.871
Trade Hours	126361	5.577	17.550
Construction Hours	126361	5.110	15.246
Service Hours	126361	11.416	23.029
Non-Farm Self-Employed	126361	0.122	0.328
Non-Farm Self-Employed, Home Establishment	126361	0.066	0.247
Manufacturing Self-Employed	126361	0.043	0.202
Manufacturing Self-Employed, Home Establishment	126361	0.030	0.172

Notes: This table shows the summary statistics from the Primary Labour Force Survey, 2017-18, for individuals participating in the labour force. The sample is restricted to individuals participating in the labour force during the week and aged between 18 and 60. Hours worked refer to hours worked in the 7 days preceding the survey.

Table D2: Treatment Effects on Weekly Labour Market Activities: Extensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Outcome = 1)							
	Labour Force Participation	Unemployed	Self Employed	Wage Work	Farm	Non Farm	Manufacturing	Trade and Services
Treat	.020	.002	.107***	-.089***	.026	-.016	.060***	-.058**
	(.027)	(.012)	(.027)	(.026)	(.042)	(.032)	(.013)	(.027)
Observations	3379	3379	3379	3379	3379	3379	3379	3379
Dep Var Mean	.497	.048	.232	.217	.093	.352	.103	.187

Notes: The above table shows the treatment effect on weekly labour market outcomes along the extensive margin for minority working-age individuals. 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 column (1) is a dummy equal to 1 if the individual participated in the labour force in any of the 7 days preceding the survey; in column (2), a dummy equal to 1 if the individual was unemployed in any of the 7 days preceding the survey; in columns (3)-(6), a dummy equal to 1 if the individiual was engaged in the sector referred. All specifications include state and survey month fixed effects; a linear polynomial in the running variable; controls for demographic and education covariates at the individiual level; household level controls. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table D3: Treatment Effects on Weekly Labour Market Activities: Intensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Hours Worked in						
	Week	Self Employed	Wage Work	Farm	Non Farm	Manufacturing	Trade and Services
Treat	-.232	5.411***	-5.692***	.215	-.447	3.120***	-2.492
	(1.260)	(1.463)	(1.304)	(1.889)	(1.979)	(.923)	(1.884)
Observations	3379	3379	3379	3379	3379	3379	3379
Dep Var Mean	23.854	12.317	11.501	4.343	19.511	5.565	10.747

Notes: The above table shows the treatment effect on weekly labour market outcomes along the intensive margin for minority working-age individuals. The unit of observation is the working-age individual. Working-age individuals are individuals aged between 18 and 60 years. The outcome of interest is hours worked per week in the sector mentioned. All specifications include state and survey month fixed effects; a linear polynomial in the running variable; controls for demographic and education covariates at the individiual level; household level controls. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table D4: Treatment Effects on Occupation Type and Sector: Extensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Outcome = 1)					
	Farm		Manufacturing		Trade and Services	
	Self Employed	Wage Work	Self Employed	Wage Work	Self Employed	Wage Work
Treat	.040	-.014	.046***	.014	.011	-.069***
	(.032)	(.016)	(.007)	(.009)	(.017)	(.016)
Observations	3379	3379	3379	3379	3379	3379
Dep Var Mean	.067	.027	.053	.051	.099	.088

Notes: The above table shows the treatment effect on weekly labour market outcomes along the extensive margin for minority working-age individuals. 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 column (1) is a dummy equal to 1 if the individual participated in the labour force in any of the 7 days preceding the survey; in column (2), a dummy equal to 1 if the individual was unemployed in any of the 7 days preceding the survey; in columns (3)-(6), a dummy equal to 1 if the individiual was engaged in the sector referred. All specifications include state and survey month fixed effects; a linear polynomial in the running variable; controls for demographic and education covariates at the individiual level; household level controls. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table D5: Treatment Effects on Occupation Type and Sector: Intensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours Worked in					
	Farm		Manufacturing		Trade and Services	
	Self Employed	Wage Work	Self Employed	Wage Work	Self Employed	Wage Work
Treat	1.030	-.815	2.433***	.687	1.478	-3.971***
	(1.416)	(.691)	(.444)	(.606)	(1.231)	(1.159)
Observations	3379	3379	3379	3379	3379	3379
Dep Var Mean	3.074	1.269	2.728	2.837	5.819	4.928

Notes: The above table shows the treatment effect on weekly labour market outcomes along the extensive margin for minority working-age individuals. 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 column (1) is a dummy equal to 1 if the individual participated in the labour force in any of the 7 days preceding the survey; in column (2), a dummy equal to 1 if the individual was unemployed in any of the 7 days preceding the survey; in columns (3)-(6), a dummy equal to 1 if the individiual was engaged in the sector referred. All specifications include state and survey month fixed effects; a linear polynomial in the running variable; controls for demographic and education covariates at the individiual level; household level controls. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table D6: Disaggregating Non-Farm Employment for Minority Workers, Extensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Outcome = 1)							
	Manufacturing			Trade and Services				
	Food	Textiles	Other	Trade	Services	Transport	Hospitality	Others
Treat	.002	.061***	-.003	.017	-.075***	-.038**	-.064***	.027*
	(.002)	(.009)	(.006)	(.025)	(.017)	(.015)	(.015)	(.017)
Observations	3379	3379	3379	3379	3379	3379	3379	3379
Dep Var Mean	.017	.045	.041	.082	.105	.039	.011	.054

Notes: The above table shows the treatment effect on weekly labour market outcomes along the extensive margin for minority working-age individuals. 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 column (1) is a dummy equal to 1 if the individual participated in the labour force in any of the 7 days preceding the survey; in column (2), a dummy equal to 1 if the individual was unemployed in any of the 7 days preceding the survey; in columns (3)-(6), a dummy equal to 1 if the individiual was engaged in the sector referred. All specifications include state and survey month fixed effects; a linear polynomial in the running variable; controls for demographic and education covariates at the individiual level; household level controls. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table D7: Treatment Effect on Non-Farm and Manufacturing Employment: by Concentration of Religious Groups in Industry

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Employment in Sector = 1)							
	High Diversity		High Muslim		High Hindu		High Hindu Self-Employed	
	Non Farm	Mfg.	Non Farm	Mfg.	Non Farm	Mfg.	Non Farm	Mfg.
Treat	.025	.061***	.004	.063***	.020	-.003*	-.040	.049***
	(.038)	(.012)	(.036)	(.013)	(.016)	(.002)	(.032)	(.009)
Observations	3379	3379	3379	3379	3379	3379	3379	3379
Dep Var Mean	.314	.091	.317	.095	.029	.009	.307	.091

Notes: The above table shows the treatment effect on weekly labour market outcomes along the extensive margin for minority working-age individuals. 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 column (1) is a dummy equal to 1 if the individual participated in the labour force in any of the 7 days preceding the survey; in column (2), a dummy equal to 1 if the individual was unemployed in any of the 7 days preceding the survey; in columns (3)-(6), a dummy equal to 1 if the individiual was engaged in the sector referred. All specifications include state and survey month fixed effects; a linear polynomial in the running variable; controls for demographic and education covariates at the individiual level; household level controls. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

E Appendix: Outcome Specific MSERD Bandwidths

We replicate in this section some of our key results using outcome-specific data driven MSERD bandwidths recommended by Calonico et al. (2020).

Table E1: Treatment Effect Across Credit Sources for Minority Households: MSERD Bandwidth

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Loan = 1)			Loan Amount (INR)		
	Any Source	Informal	Non-Bank Financial Institutions	Total Loans	Informal	Non-Bank Financial Institutions
Treat	.246***	-.021	-.139***	59844.760***	21767.700**	-1.00e+04
	(.086)	(.057)	(.043)	(16729.043)	(9712.502)	(7373.080)
Observations	1354	2245	1695	1378	1378	1765
Dep Var Mean	.363	.161	.086	41968.809	10211.738	8566.542
Bandwidth	.042	.072	.057	.045	.046	.058

Notes: The above table shows the treatment effect on non-bank sources of credit for minority households. The unit of observation is the household. The outcome of interest in columns (1)-(3) is a dummy equaling 1 if the household had any outstanding loan from the sources mentioned; in columns (4)-(6), the amount of bank loans received by the household. *Informal* loans include loans from professional money lenders, input suppliers, and friends, relatives and landlord. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. All outcomes are estimated using data-driven MSERD optimal bandwidths, as prescribed by Calonico et al. (2020). All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table E2: Purpose of Bank Credit: Minority Households, MSERD Bandwidth

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Loan = 1)			Loan Amount (INR)		
	Farm	Non Farm	Expenditure	Farm	Non Farm	Expenditure
Treat	.058**	.038	.075***	6326.148**	-1092.835	7459.322
	(.023)	(.025)	(.027)	(2972.968)	(1918.556)	(6868.065)
Observations	1378	1465	2057	1378	2606	2449
Dep Var Mean	.025	.021	.050	3372.215	2764.343	12081.604
Bandwidth	.045	.050	.070	.045	.083	.076

Notes: The above table shows the treatment effect on purpose of bank credit access for minority households. The unit of observation is the household. The outcome of interest in columns (1)-(3) is a dummy equaling 1 if the household had any outstanding bank loan for the purpose mentioned; in columns (4)-(6), the amount of bank loans received by the household for the purpose mentioned. *Expenditure* loans include household loans obtained for expenditure on consumption items, health and education, and housing. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. All outcomes are estimated using data-driven MSERD optimal bandwidths, as prescribed by Calonico et al. (2020). All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table E3: Mechanism: Nature of Bank Lending in Minority Concentration Districts, MSERD Bandwidths

	(1)	(2)	(3)	(4)
	Pr(Loan = 1)		Loan Amount (INR)	
	Commercial Bank	Bank SHG	Commercial Bank	Bank SHG
Treat	.140***	.110***	16242.768***	3944.881***
	(.027)	(.020)	(5934.496)	(1333.656)
Observations	1378	1378	1835	1505
Dep Var Mean	.106	.033	20046.831	1105.784
Bandwidth	.046	.047	.059	.052

Notes: The above table shows the treatment effect by type of bank loan for minority households. The unit of observation is the household. The outcome of interest in columns (1)-(2) is a dummy equaling 1 if the household has any outstanding loan from the source mentioned; in columns (3)-(4), the amount of outstanding loan from the source mentioned. *Commercial Bank* refers to loans obtained directly from commercial banks; *Bank SHG* refers to loans received through bank-linked SHGs. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. All outcomes are estimated using data-driven MSERD optimal bandwidths, as prescribed by Calonico et al. (2020). All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table E4: Mechanisms: Treatment Effects on Collateral Requirements and Cost of Credit, MSERD Bandwidth

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Unsecured Loan = 1)			Interest Rates		
	All Bank	Commercial Banks	Bank SHG	All Bank	Commercial Banks	Bank SHG
Treat	.118***	.048**	.087***	-.005	-.017*	.026**
	(.039)	(.020)	(.032)	(.015)	(.009)	(.012)
Observations	1930	1835	1695	782	454	98
Dep Var Mean	.044	.035	.045	.118	.106	.111
Bandwidth	.064	.060	.058	.089	.068	.058

Notes: The above table shows the treatment effect on bank loan securitization and interest rates for minority households. The unit of observation is the household. The outcome of interest columns (1)-(3) is a dummy equaling 1 if the household has any unsecured bank loan; in columns (4)-(6), the interest rate faced by the household for bank loans. The outcome of interest in columns (1) and (4) include all bank loans; in columns (2) and (5), loans obtained directly from commercial banks; in columns (3) and (6), loans obtained from bank-linked SHGs. Average household interest rates are weighted by loan volume. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. All outcomes are estimated using data-driven MSERD optimal bandwidths, as prescribed by Calonico et al. (2020). All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table E5: Treatment Effect on Loan Delinquency: Minority Households

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Any Delinquent Loan = 1)					
	Banks	Informal	Money Lenders	Community Networks	Non-Bank Financial Institutions	Any Source
Treat	-.019	.282***	.019	.238*	.173	.049
	(.054)	(.099)	(.084)	(.123)	(.201)	(.064)
Observations	531	360	131	228	151	792
Dep Var Mean	.286	.508	.451	.591	.045	.313
Bandwidth	.063	.051	.047	.053	.038	.047

Notes: The above table shows the treatment effect on the likelihood of having a delinquent loan for minority households. The unit of observation is the household. The outcome of interest is a dummy equaling 1 if the household has a delinquent loan from the sources mentioned. A loan is classified as delinquent if there has been no repayment towards that loan for at least 6 months preceding that survey. All specifications include state fixed effects, a linear polynomial in the running variable, and controls for household size and rural location. The sample is restricted to a bandwidth of .061 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights.Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table E6: Treatment Effects on Weekly Labour Market Activities: Extensive Margin, MSERD Optimal Bandwidth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Outcome = 1)							
	Labour Force Participation	Unemployed	Self Employed	Wage Work	Farm	Non Farm	Manufacturing	Trade and Services
Treat	.020	-.019	.131***	-.086***	.012	-.019	.034**	-.034
	(.027)	(.012)	(.026)	(.025)	(.038)	(.027)	(.014)	(.029)
Observations	2747	2506	2747	3596	3889	4452	2652	2652
Dep Var Mean	.510	.065	.249	.207	.137	.319	.040	.194
Bandwidth	.054	.048	.053	.059	.065	.070	.051	.051

Notes: The above table shows the treatment effect on weekly labour market outcomes along the extensive margin for minority working-age individuals. 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 column (1) is a dummy equal to 1 if the individual participated in the labour force in any of the 7 days preceding the survey; in column (2), a dummy equal to 1 if the individual was unemployed in any of the 7 days preceding the survey; in columns (3)-(6), a dummy equal to 1 if the individiual was engaged in the sector referred. All specifications include state and survey month fixed effects; a linear polynomial in the running variable; controls for demographic and education covariates at the individiual level; household level controls. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table E7: Treatment Effects on Weekly Labour Market Activities: Intensive Margin, MSERD Optimal Bandwidth

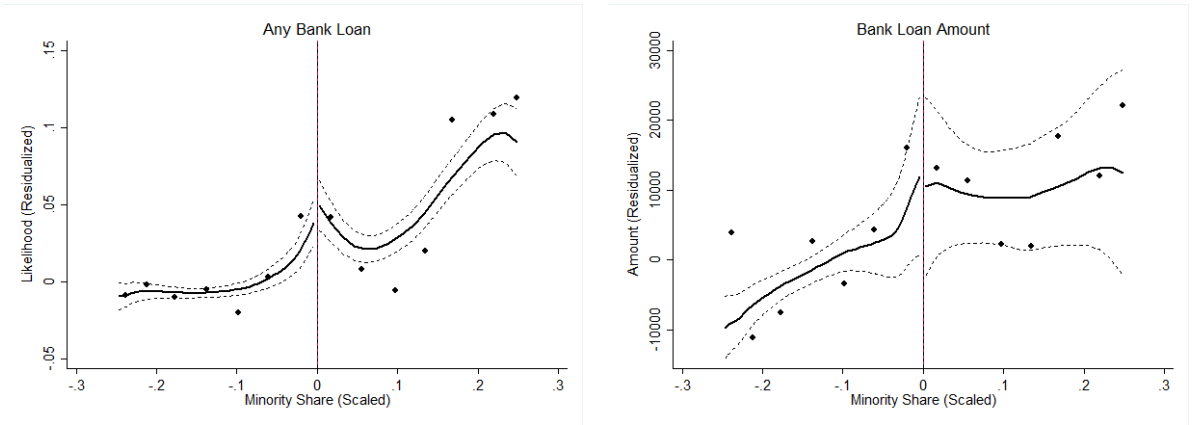
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Hours Worked in			
	Week	Self Employed	Wage Work	Farm	Non Farm	Manufacturing	Trade and Services
Treat	-.267	6.855***	-4.900***	-.541	-.044	2.587***	-.102
	(1.206)	(1.428)	(1.294)	(1.567)	(1.808)	(.954)	(2.049)
Observations	3610	2747	3889	4649	3889	2747	2540
Dep Var Mean	23.776	12.173	10.898	5.760	17.548	2.765	10.862
Bandwidth	.060	.052	.063	.073	.063	.054	.049

Notes: The above table shows the treatment effect on weekly labour market outcomes along the intensive margin for minority working-age individuals. The unit of observation is the working-age individual. Working-age individuals are individuals aged between 18 and 60 years. The outcome of interest is hours worked per week in the sector mentioned. All specifications include state and survey month fixed effects; a linear polynomial in the running variable; controls for demographic and education covariates at the individiual level; household level controls. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

F Treatment Effects for Non-Minorities

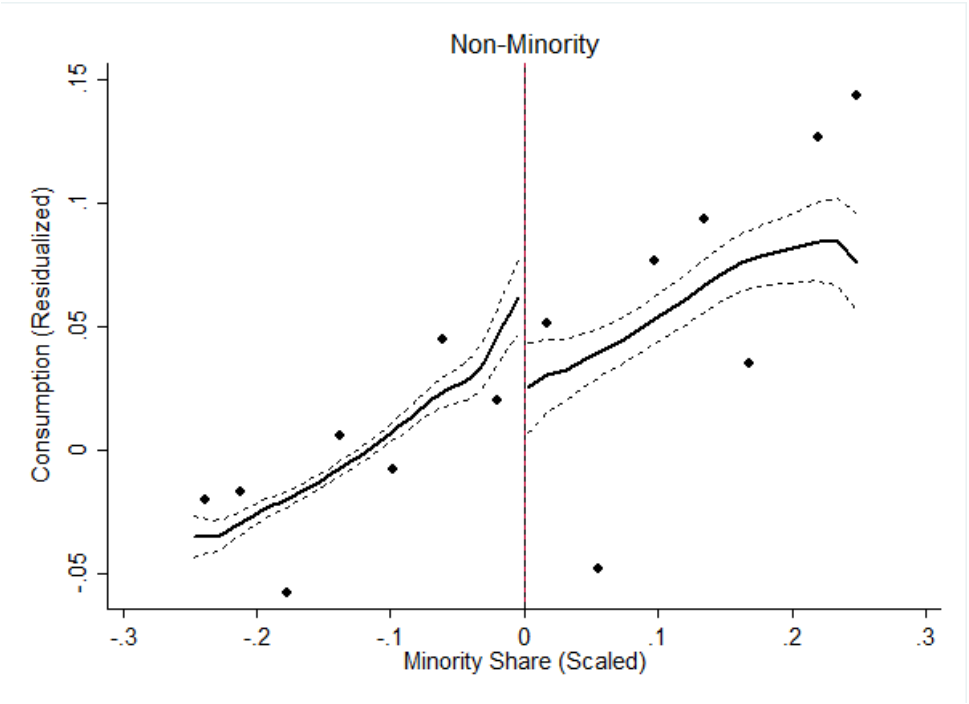
F.1 Figures

Figure F1: Access to Bank Credit for Non-Minority Households



The above figures shows the treatment effect for access to bank credit for non-minority households. The unit of observation is the household. 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). The horizontal lines show the linear fit from a local linear regression. The local linear regressions include state fixed effects, household covariates and a linear polynomial in the running variable. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Estimates are weighted using a triangular kernel and household specific weights provided by the AIDIS.

Figure F2: Treatment Effects on Household Consumption: Non-Minorities



The above figure identifies the treatment effect on household monthly per capita consumption. The sample is restricted to minority households in the left-panel; to non-minority households in the right-panel. The unit of observation is the household. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). The outcome of interest is monthly per capita household expenditures (logged). The horizontal lines show the linear fit from a local linear regression. The local linear regressions include state fixed effects, household covariates, and a linear polynomial in the running variable. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. Estimates are weighted using a triangular kernel and household specific weights provided by the AIDIS.

F.2 Tables

Table F1: Credit Access for Non-Minority Households

				Panel A: All				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Loan = 1)				Loan Amount (INR)			
	Bank	Informal	Non-Bank Financial Institutions	Any Source	Bank	Informal	Non-Bank Financial Institutions	All Sources
Treat	.052	-.055	.082***	.050	-7788.013	–13520.081***	8800.623***	-13520.081
	(.051)	(.039)	(.020)	(.041)	(36089.014)	(5083.625)	(3079.208)	(34490.198)
Observations	9763	9763	9763	9763	9763	9763	9763	9763
Dep Var Mean	.200	.134	.068	.327	58710.405	10699.927	8548.257	78977.693
				Panel B: SC/ST				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Loan = 1)				Loan Amount (INR)			
	Bank	Informal	Non-Bank Financial Institutions	Any Source	Bank	Informal	Non-Bank Financial Institutions	All Sources
Treat	.083	-.085**	-.010	-.013	-7934.688	-8025.348	2663.482	-13237.598
	(.077)	(.043)	(.025)	(.061)	(11383.284)	(5641.843)	(2149.385)	(12038.035)
Observations	2544	2544	2544	2544	2544	2544	2544	2544
Dep Var Mean	.183	.164	.081	.348	25655.867	8714.584	4271.318	38807.468

Notes: The above table shows the treatment effect on transport assets owned by minority households. The unit of observation is the household. Panel A shows the results for the purpose for which the transport is put to use; Panel B shows the results for the specific mode of transport owned by the household. The outcome of interest in columns (1)-(3) is a dummy equaling 1 if the household owns any of the transport modes mentioned (uses transport for purpose mentioned); in columns (4)-(6), the value of transport. All specifications include state fixed effects, a linear polynomial in the running variable, and controls for household size and rural location. The sample is restricted to a bandwidth of .061 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table F2: Mechanism: Nature of Bank Lending in Minority Concentration Districts

	(1)	(2)	(3)	(4)
	Pr(Loan = 1)		Loan Amount (INR)	
	Commercial Bank	Bank SHG	Commercial Bank	Bank SHG
Treat	.043	.017	-8138.938	350.924
	(.052)	(.022)	(35833.548)	(552.129)
Observations	9763	9763	9763	9763
Dep Var Mean	.164	.040	57072.574	1637.831

Notes: The above table shows the treatment effect by type of bank loan for minority households. The unit of observation is the household. The outcome of interest in columns (1)-(2) is a dummy equaling 1 if the household has any outstanding loan from the source mentioned; in columns (3)-(4), the amount of outstanding loans from the source mentioned. *Commercial Bank* refers to loans obtained directly from commercial banks; *Bank SHG* refers to loans received through bank-linked SHGs. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table F3: Mechanisms: Treatment Effects on Collateral Requirements and Cost of Credit, Non-Minority Households

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Unsecured Loan = 1)			Interest Rates		
	All Bank	Commercial Banks	Bank SHG	All Bank	Commercial Banks	Bank SHG
Treat	.021	.010	.018	-.003	-.000	-.012
	(.037)	(.024)	(.023)	(.014)	(.011)	(.036)
Observations	9763	9763	9763	3158	2849	363
Dep Var Mean	.064	.036	.064	.110	.102	.110

Notes: The above table shows the treatment effect on bank loan securitization and interest rates for non-minority households. The unit of observation is the household. The outcoome of interest columns (1)-(3) is a dummy equaling 1 if the household has any unsecured bank loan; in columns (4)-(6), the interest rate faced by the household for bank loans. The outcome of interest in columns (1) and (4) include all bank loans; in columns (2) and (5), loans obtained directly from commercial banks; in columns (3) and (6), loans obtained from bank-linked SHGs. Average household interest rates are weighted by loan volume. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. The sample is restricted to a bandwidth of .06 around the discontinuity threshold. All specifications are weighted using a triangular kernel and household-specific weights. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

G Treatment Effects for Electoral and Violence Outcomes

Table G1: Directed Credit Policy and Electoral Outcomes: Electoral Wins and Voter Participation

	(1)	(2)	(3)	(4)	(5)
	Pr(Electoral Victory = 1)			Citizen Participation	
	Muslim Candidate	Muslim Candidate Major Party	BJP Candidate	Voter Turnout	Registered Voters
Treat	.090***	.076***	-.052	.034	4115.641
	(.034)	(.028)	(.111)	(.035)	(8272.015)
Observations	1064	1064	1064	1064	1064
Dep Var Mean	.057	.018	.420	.639	1.89e+05

Notes: This table identifies the impact of the directed credit policy on electoral outcomes for politicians from minority communities and those representing the right-wing party. The unit of observation is the electoral constituency. The outcome of interest in column (1) is the total vote share received by Muslim politicians contesting elections to the state legislative assembly; in column (2), a dummy equaling 1 if there was a Muslim candidate contesting the election; in column (3), a dummy equaling 1 if any Muslim candidate contested on behalf of a major political party; in column (4), the vote share received by a Muslim candidate contesting on behalf of a major political party; in column (5), the vote share received by the BJP candidate, conditional on participation; in column (6), the vote share received by the BJP candidate. 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 .058 around the discontinuity threshold, and are weighted using a triangular kernel. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table G2: Directed Credit Policy and Electoral Outcomes: Heterogeneity by High and Low Minority Areas

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Muslim = 1)		Pr(Muslim, Major Party = 1)		Muslim Vote Share		BJP Vote Share	
Treat	.330***	.153***	.198***	.122	.090***	-.006	-.093*	.019
	(.088)	(.033)	(.036)	(.098)	(.020)	(.026)	(.049)	(.049)
Observations	520	395	520	395	520	395	520	395
Dep Var Mean	.391	.757	.050	.292	.042	.147	.234	.276
High Muslim	N	Y	N	Y	N	Y	N	Y

Notes: This table identifies the impact of the directed credit policy on electoral outcomes for politicians from minority communities and those representing the right-wing party based on whether they were contesting elections in constituencies with Muslims share of the population above the sample median (High Muslim) or not. 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 .058 around the discontinuity threshold, and are weighted using a triangular kernel. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table G3: Effects on Profile of Muslim Candidates

	(1) Net Assets	(2) Annual Income	(3) Age (Years)	(4) Education (Years)
Treat	10039.591*	1.480*	-.131	.027
	(6018.418)	(.823)	(.255)	(.045)
Observations	1159	1148	1084	1159
Dep Var Mean	9805.841	44.547	9.615	.215

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table G4: Channels of Electoral Effects

	(1) Congress Candidate Voteshare	(2) Congress Candidate Voteshare	(3) Any BJP Candidate	(4) Any Congress Alliance Candidate	(5) Any Congress Candidate	(6) Effective Number of Parties
Treat	-2.170	-5.647***	.048	-.000	-.106***	.224**
	(1.772)	(1.650)	(.048)	(.000)	(.029)	(.106)
Observations	821	1045	1045	985	1045	1040
Dep Var Mean	34.076	27.004	.869	.441	.794	2.941

This table reports the effects of credit policy on electoral outcomes mentioned in the column headers. Column 1 reports effect on vote share of Congress party conditional on there being a candidate and column 2 reports effects on the vote share of Congress party unconditional on there being a candidate. Columns 3 to 5 report effects on the likelihood of there being a candidate with affiliation as mentioned in the column headers at the constituency level in an election. Column 6 reports the effect of policy on electoral competition. Standard errors clustered at the district level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table G5: Mechanism of Hindu-Muslim Violence

	(1)	(2)	(3)	(4)	(5)
		Hindu-Muslim Riots			
	Majority Instigated Riots	Before Elections	After Elections	Right-wing Party Wins	Right-wing Party Loses
Treat	.008***	.005	.023***	.002	.016***
	(.002)	(.006)	(.009)	(.008)	(.001)
Observations	1346	544	496	244	252
Dep Var Mean	.006	.010	.003	.016	.024
Above Median Share of Races Won	-	-	-	Y	N

Table G6: Is Violence Targeted

	(1) Hindu-Muslim Riots	(2) Hindu-Muslim Riots	(3) Total Riots	(4) Total Riots
Treat	-.001	.138***	-.290	.038
	(.003)	(.033)	(.336)	(.242)
Observations	1188	202	1188	202
Dep Var Mean	.018	.042	1.848	1.427
Median and Above Muslims Population	N	Y	N	Y

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$