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

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Abstract

Can targeted bank credit expansion 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 which 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 the minority households, and a reduction in the consumption inequality between minority and majority households. 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 caste compared to minority candidates in the non-policy districts. The policy also reduced 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.

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

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1 Introduction

Countries often introduce policies to economically uplift marginalized groups of the society.[1]. Such policies can have dual impact by empowering the marginalized group economically and politically, promoting harmony in the society. Alternatively, economic empowerment may come at the cost of violence if the majority group feels threatened by the narrowing inequality Mitra and Ray (2014). In this paper, we study one such policy in India where the state provided a path to prosperity to religious minorities through mandating commercial banks to extend formal credit to minority households.[2] We study if the policy was successful in achieving its economic goal and also explore implications on elections and conflict. India provides an ideal setting to study if large-scale policies can improve economic and political well-being of marginalized groups, as it has a large religious minority of nearly 200 million Muslims that is economically backward (Maizland, 2020), politically dis-empowered (Allie, 2024), and violently persecuted (Mitra and Ray, 2014; Iyer, 2018).

We study the Prime Minister's New 15 Point Programme for Welfare of Minority Communities – a set of policy initiatives initiated in 2009 by India's federal government to improve the socio-economic conditions for religious minorities – namely Muslim, Christian, Sikh, Buddhist and Parsi communities – who in 2001 accounted for over 15 percent of the national population. The minority welfare policy directed banks to expand credit to religious minorities, which forms the key focus of this paper. Specifically, the directed credit policy classified select districts as "minority concentration", and encouraged banks to expand lending to religious minorities in these districts.[3] The policy was flexibly designed and no specific targets were provided to lenders in terms of overall lending volumes.[4] Lenders instead were recommended to collaborate with local self-help groups (SHGs) to identify creditworthy borrowers from religious minorities. Additionally, commercial banks in India are mandated to allocate at least 10 percent of their lending portfolio to "weaker sections", comprising of women and historically

[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 area, 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; Aydin, 2022; Breza and Kinnan, 2021; Buera et al., 2020; Cramer, 2021). 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] Districts form the third tier of administration in India, after the federal and state.

[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.

marginalized citizen groups.[5] To facilitate compliance with the directed credit policy, the central bank expanded the definition of "weaker sections" to include religious minorities, offering banks a larger pool of potential borrowers with which to meet their annual 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 in the district population exceeded 25% were deemed "minority concentration" RBI (2007). The use of an arbitrary threshold to classify districts into treatment (minority concentration) and control status (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, with the list of minority concentration districts being circulated 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). We also verify balance across pre-treatment household and district characteristics across minority and non-minority concentration districts. This makes minority households in non-minority concentration districts a valid counterfactual for minority households in minority concentration districts.

We proceed by first establishing if the policy impacted credit access to and economic well-being of minority households, and then explore the effects on outcomes of politics and violence. To empirically assess the treatment's impact on minority credit access, we draw on 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 for religious minorities, while the 2003 AIDIS survey is used to verify pre-treatment balance across key outcomes of interest. Within religious minorities, the 2003 AIDIS shows Muslim households to have substantially lower access to bank credit, face higher rates of interest in informal credit markets, and have significantly lower values of household savings and pledgeable assets.[6] This 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. In robustness checks, we show our results to be very similar upon extending the sample to

[5] 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.

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

other religious minorities.

Exploiting a sharp RD design to compare minority households across minority concentration and non-minority concentration districts within a narrow window of the discontinuity threshold, we identify an 11-16 percentage point increase in bank credit access for minority households in minority concentration districts. The treatment effect is both statistically and economically significant, when considering that 11 percent of minority households in control districts had some outstanding bank loan. As the average control district had 1.6 million minority households, 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 17,000 increase in the amount of bank loans recieved. This is equivalent to 11 percent of annual household consumption for minority households in control districts.

Our baseline results are stable to alternate specification choices and bandwidths. Our primary sample uses a fixed set of 63 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 .09. Our preferred specification estimates local linear regressions using a linear polynomial in the running variable, and we show robustness to considering a quadratic polynomial. Our treatment effects are as expected very similar when extending the sample to other religious minorities. Finally, we show our results to be qualitatively similar when estimated using a fuzzy RD specification to address the issue of non-compliance in treatment assignment for 18 districts (out of 121).[7]

An often raised concern about such targeted policies is that the cost of expanding opportunities to minority groups has to be borne by someone (Holzer and Neumark, 2000; Marion, 2009). To understand if the increase in bank credit came at the expense of fellow Indian citizens, we compare non-minority households and find no negative impact of the policy on bank credit. Instead, we identify statistically and economically significant reductions in informal borrowings for non-minority households in treated districts, accompanied by higher borrowings from non-bank financial institutions. This move is likely made possible due to a reallocation of minority borrowing away from non-bank financial institutions to formal banks in response to increased access to bank credit. These results rule out concerns that increase

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

in credit for the minority came at the cost of reduced credit access for non-minorities, and instead point to potentially positive effect on credit from non-bank financial institutions for the non-minorities.

To understand if the credit policy had an impact on the well-being of households, we analyze consumption expenditures. Relative to minority households in control districts, minority households in treated districts have 15 percent higher monthly per capita consumption. There is no statistically distinguishable difference in household consumption for non-minority households across treated and control districts, although the point estimate is negative. These results support the explanation that the improvement in financial and economic outcomes for religious minorities in response to financial affirmative action did not come at the expense of a worsening of outcomes for non-minority groups. Pooling our sample of minority and non-minority households and comparing household consumption within the set of minority concentration districts, we find minority households in treated districts to have 16 percent higher monthly per capita consumption, relative to non-minority households. As the consumption gap between minority and non-minority households in control districts equaled 25 percent, our findings suggest that financial affirmative action contributed to reducing the consumption gap between minority and non-minority households by 60 percent.

We conclude the analysis of the economic impacts of the policy by considering the role of three mechanisms in explaining our findings. 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. Upon disaggregating bank loans across loans received directly from commercial banks and lending through bank-linked SHGs, we find up to 60 (20) percent of the extensive (intensive) margin increase in minority bank credit access to be accounted for by loans issued through bank-linked SHGs. Second, minorities have fewer assets to use a collateral against bank loans. As a result of the policy, it is possible that banks relaxed the collateral requirements for minority borrowers. Indeed, we find a sizable positive treatment effect on the likelihood of minority households to have an unsecured bank loan. Third, we find limited evidence of banks reducing interest rates in response to the directed credit policy– while the point estimate for commercial bank interest rates is negative, the accompanying standard error is too large to draw any strong conclusions (p-value .148).

Next, we study if the policy affected politics and social harmony in districts where minority households were provided access to bank credit. We use four datasets for analysis in this part. First,

we study electoral effects of the policy using data on state elections from 2008 to 2019 compiled by the Trivedi Center, Ashoka University. Since the official records do not report religion of the candidates, we rely on a machine learning classification model developed by Chaturvedi and Chaturvedi (2024) to identify candidates that are likely minority(Muslim). Second, we use Armed Conflict Location and Event Data (ACLED) (Raleigh et al., 2010) for analysis of conflict between majority and minority during 2016 to 2019 period. Third, we use administrative data on crime during 2014-2019 period reported by the National Crimes Records Bureau (NCRB) to validate the results on conflict.

Since the credit policy improved economic well-being of minorities we expect the policy districts to witness political empowerment of minorities. Further, the policy also likely improved civic engagement in the districts as banks' use of self-help groups to give out loans increased substantially. This increase in civic engagement may reflect in improved social harmony through reduction of support for right-wing parties and a reduction in conflict. With these hypothesis, we examine data from all elections held at state assembly level during 2008 and 2019. The analysis shows evidence for both of these hypothesis.

We observe minority group to have higher political empowerment as measured through electoral outcomes. As the economic situation of minority households improves, it is possible that more members of the minority communities enter politics. For this, we analyze the probability of there being at least one candidate from minority on ballot in assembly constituency elections. We indeed find that minority concentration districts see an increase of 13.7 percentage points in the probability that there will be at least one candidate in the election. Further, the vote share of minority candidates also increase by 3.208 percentage points when analyzed unconditional of there being a minority candidate, and by 3.068 when analyzed conditionally. Lastly, we do not have data to assess if minorities register to vote at a higher rate or turnout on election day at higher rate, however at the constituency level we do not find any evidence for increase in voter registration or turnout. These results suggest minorities become politically more active when policies to uplift them improve their economic well-being.

Next, we explore what happens to the electoral outcomes of right-wing party candidates that run on an anti-minority platform. We do not find any effect of the policy on their candidacy decisions, however, their vote share appears to be affected. Concretely, the vote shares of right-wing party candidates running for office in minority concentration districts go down by 4.86 percentage points compared to the candidates from the same party running for office in the non-minority concentration districts. This result qualitatively remains similar if we analyze unconditional on there being a candidate from the

right-wing party in a constituency with an estimated decrease of 3.98 percentage points in the vote share ($p - value$ of 0.13). We further explore if the decrease in vote share of the right-wing party is concentrated in assembly constituencies that have a higher population of Muslims to understand if the minority group is more likely to act in unison to protect their interests. However, we find that the loss of vote share for the right-wing party candidates is concentrated in districts where the share of Muslims in the population is below the median share.

The effect on the vote share of the right-wing party candidates and its concentration in constituencies with below median minority population suggests that policy may resulted in increased social harmony in target districts. If that is the case, then it is likely that this had an effect on inter-religious conflict. India has a long history of violence between the majority (Hindus) and the largest minority (Muslims) (Varshney, 2003). In order to establish if there had been any change in conflict we focus on Hindu-Muslim riots. Using ACLED data from 2016 (the earliest available year) to 2019, we find that, contrary to the increased harmony argument, Hindu-Muslim riots increase in the policy districts compared to the districts with low-minority concentration. We similarly find an increase in the communal riots (largely between Hindus and Muslims) reported by NCRB during 2014 (the earliest period reporting communal riots) and 2019. Importantly, there is no corresponding increase in overall violence as we find no effect on the number of total riots of all types or the number of murders reported in the data, suggesting that the increase in violence is specific to inter-religious conflict type.

The increase in conflict suggests that the policy did not create inter-religious harmony between the majority and the largest minority group. However, it is also likely that the policy did increase harmony, while the rise in conflict is linked to the electoral calculus of the right-wing party. The timing of violence with respect to elections may help explore this possibility. Violence before elections would suggest an attempt to suppress minority turnout/boost majority turnout, while use violence after elections may be used as a tool of punishment due to the decrease in support. To explore this explanation, we split the ACLED reports in two samples, one that includes all incidents reported after the month of the election for up to twelve months, and the other includes all incidents reported up to twelve months before the election month. We find the Hindu-Muslim violence does not increase before elections, and instead, the increase in violence in the policy districts is almost exclusively coming from the post election time-period, suggesting that the rise in violence is probably strategically timed to punish the minority voters.

Based on the economic and political results, we conclude that the bank credit policy reduced economic inequality and raised social harmony between the majority and minority, but these improvements came at the cost of increased post election violence. In reaching this conclusion, the paper makes a unique contribution to a nascent literature studies the political effects of policies meant to enhance welfare of the marginalized (Chyn et al., 2024). However, unlike other papers, we first establish a direct effect on the economic well being and narrowing of inequality, and then identify a positive effect on political empowerment. Our paper also makes a unique contribution by identifying that the violence does not randomly increase due to the improved economic standing of the minority, rather it is a strategic response to electoral changes (Jha, 2013; Mitra and Ray, 2014). In this regard, the paper also contributes to the strategic use of violence in elections (Condra et al., 2018; Iyer and Shrivastava, 2018).

This paper 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, this paper explores a unique setting where an affirmative action policy was implemented in credit markets through formal banking channels that otherwise exclude a marginalized community.[8] Through this contribution, the paper takes forward the nascent literature on the accessibility of formal bank credit to minority communities. In this regard, existing studies have identified 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. However, to the best of our knowledge, none have highlighted or studied the role of affirmative action in terms of a government mandated expansion in access to formal credit for under-represented groups.

Some scholars caution against the use of affirmative action, especially through government action (Sowell, 2004), due to concerns regarding adverse negative effects (Agan and Starr, 2018). These can manifest either through a mismatch between agents who are the expected beneficiaries and the service targeted by the policy: for instance, matching students with schools (Barrow et al., 2020). Alternatively, there could be a crowding out of non-minority beneficiaries (Arcidiacono et al., 2022).

[8] 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).

Addressing this question in financial markets, our paper makes a contribution by showing that directed credit policy does not crowd out non-minorities from credit markets, suggesting that increasing bank credit access for minorities does not come at a cost to non-minorities.

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. Aggregate impacts of financial affirmative action are explored in Section 6.

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.[9] 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 policy intervention 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,

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

2007).[10] These districts were deemed such as the population share of religious minorities (as per the 2001 population census) exceeded 25% of the district's population. 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.[11]

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).[12] 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.[13] 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 across print and visual media, and engaging in information campaigns at the site of religious congrega-

[10] 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.

[11] 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.

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

[13] 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.

tions (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).[14] 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.[15] 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 SHGs to achieve their priority sector targets. This is particularly salient in light of the bank-SHG

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

[15] 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.

linkage programme unveiled in 2008, which encouraged banks to directly lend to SHGs. Consequently, the directed credit policy for religious minorities offered banks a larger pool of potential borrowers to meet their priority sector targets.

3 Data

This section describes the datasets used for the empirical analysis conducted in the paper.

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. The primary reason for households borrowing was 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.[16] 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 almost 14%, with banks charging a significantly lower rate of interest (11%) than money lenders (29%).[17]

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 the 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.[18] Nonetheless, Appendix Table C1.D shows high self-reported loan delinquency, with almost 37% 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 (28%) was lower than loans sourced from informal sources (49%).

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

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

[18] 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.

3.2 Election Data

We use data on every election to state legislative assemblies held in India between 2008 and 2019 using data compiled by the Trivedi Center for Political Data. This data includes names of each candidate, their party affiliation, and the votes they received. We use the names of candidates to classify them as minority using the machine learning algorithm from Chaturvedi and Chaturvedi (2024). The data also includes month in which the elections were held, the total number of registered voters, and the turnout on the day of elections. This data is complemented by information from the disclosures of candidates to identify their income and asset. We also complement this data to identify assembly constituencies where minority's (Muslims) share in the registered voters is below the median share across constituencies using data from Gulzar et al. (2024)[19]

3.3 Conflict Data

We use two sources of data to study conflict. First, we access data from the Armed Conflict Location and Event Dataset (ACLED) (Raleigh et al., 2010) mapped to the districts in our sample. This data covers conflict events in India starting from 2016. We use the description of events and the actors involved to identify riots that involve the majority (Hindus) and the minority (Muslims). The second data used is official crimes statistics released annually at the district level by the National Crimes Records Bureau (NCRB). These reports communal(religious) riots as a separate category from 2014. In majority of the cases the reports are at the district level, however, in few cases these reports are disaggregated to the sub-district level. We combine them all at the district and map them to our sample.

3.4 Pre-Treatment Descriptives

Prior to describing our empirical strategy, we present some descriptive evidence from the 2003 AIDIS to highlight that amongst religious minorities, Muslim households in particular faced substantial barriers to credit access and reported significantly lower financial assets. We present the descriptive analysis by disaggregating households into 5 mutually exclusive groups: Hindu forward caste, Hindu marginalized

[19] They estimate the minority's share by classifying names on the electoral roles into minority and non-minority groups using a machine learning algorithm.

castes, Muslims, and other religious minorities.[20] 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.

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 A2 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

[20] 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.

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 A2 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 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 A2 is purely descriptive, it is consistent with discrimination faced by Muslim borrowers in formal credit markets.

Appendix Figures A3 and A4 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 - A4 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. Taking cognisance 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's primary focus is to identify 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 of the form:

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

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 concentrated 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) and two household covariates: namely household size and a binary indicator for rural location. We use a triangular kernel, assigning greater weight to observations located near the discontinuity threshold. Survey weights provided in the AIDIS data are also used while estimating the regressions. 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.[21] 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 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.[22] 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.[23] 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.[24]

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 on Credit and Well-Being

This section presents the key findings of our paper on the economic well-being. We first examine how the directed credit policy affected credit access for minority households. Next we discuss three potential mechanisms explaining our findings. Subsequently, we identify the equilibrium impacts of the directed

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

[22] 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.

[23] 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.

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

credit policy on household assets, labor market outcomes, and consumption.

5.1 Access to Credit in Minority Concentration Districts

5.1.1 Baseline Results

Figure 2 graphically compares bank credit across minority households in treated and control regions. All specifications include state fixed effects and household covariates. The sample is restricted to households in 59 districts located within our preferred bandwidth of 0.058.[25] The horizontal axis shows the running variable, and 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 MSERD outcome-specific optimal bandwidths. Column (1) identifies a positive and statistically significant treatment effect, which increases 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 loans issued to minority households in minority concentration districts. The coefficient increases slightly upon the inclusion of covariates, and reflects 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. In order to

[25] We exclude districts where no Muslim household has a bank loan.

maximize our sample size, we opt to use the latter bandwidth of .058 as our preferred bandwidth in all specifications. Using the bandwidth of .058, column (3) shows that minority households saw a 18 ppt. increase in bank credit access in treated districts.

The point estimates are large when compared to the outcome variable 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 20801. Our preferred specifications in column (3) and (5) show that the policy resulted in a doubling of credit access for 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 data 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.

In addition to bank credit, the AIDIS provides an extensive break-up of credit from other institutional and non-institutional sources. We use this information to unpack whether the substantial increase in bank credit access for minority households in minority concentration districts reflects an overall expansion in credit access. Specifically, as banks are the cheapest source of credit, we would expect households to substitute non-bank sources of credit with bank credit in the absence of credit constraints.[26] 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 C4 empirically examines this by identifying the treatment effect across two other sources of household borrowing: namely, informal loans and credit from non-bank financial institutions.[27] While not always precisely estimated, the point estimates in Appendix Table C4 point to a reallocation in household borrowing across credit sources. Columns (2) and (5) replicates 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 non-bank financial institutions: 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 loan values

[26] 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.

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

(p-value .183). While column (1) shows overall credit access to be unaffected by the treatment, column (5) suggests a INR 22,00 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 treated areas. In addition to higher bank loans, the increase in household debt emanates from a statistically non-significant increase in informal loans (column (7)).

Appendix Table C5 disaggregates informal loans into those from professional money lenders and input suppliers, and community networks, such as friends, relatives and landlords.[28] As seen from the pre-treatment descriptives in Appendix Figure A1, the former represents relatively expensive sources of informal borrowings, while the latter are mostly interest free loans.[29] Columns (1) and (2) show of Appendix Table 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. Columns (7) suggests that the increase in informal loans in column (8) of Appendix Table C4 came solely through borrowings from community networks. However, the lack of precision of the point estimates limits our ability to draw any definite conclusions from these results. 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 having loans from both banks and informal sources in treated districts. Column (5) shows that this is driven by a statistically significant (p-value .017) increase in minority households having both bank loans, and loans from community networks.

Collectively, Appendix Tables C4 and C5 offers suggestive evdidence of minority households being credit-constrained. Access to directed bank credit allowed these households to substitute credit from relatively more expensive institutional sources such as NBFCs and co-operative banks. The remaining borrowing needs were accounted for by community networks, with minority households in treated areas being more prone to having outstanding loans from both banks and community networks.

The AIDIS provides qualitative indicators on loan duration, which we recode to define long-term loans as loans exceeding 1 year in duration. Column (1) of Appendix Table C6 identifies a doubling in minority households' likelihood of having a long-term bank loan. There is also a corresponding reduction in the likelihood of having a long-term loan from non-bank financial institutions [column

[28] We consider loans from landlords as part of community loans due to anecdotal evidence on extreme residential segregation along religious lines in India.

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

(3)]. This again points to the substitution of non-bank credit with cheaper bank loans by minorities in treated districts. Access to long-term bank credit at relatively low cost, combined with a flexible repayment schedule, offer households the opportunity to utilize these loans for long-term investments yielding higher returns.

5.1.2 Robustness

Appendix Table C3-C4 shows robustness of our baseline treatment effects to alternate specification 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.[30] 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.[31] Columns (1) and (4) of Appendix Table C4 show robustness to excluding new districts formed from existing minority concentration districts.[32] Columns (2) and (5) show robustness to expanding our sample to include other religious minorities – namely Christian and Sikh minority groups. Section 2 noted that an additional 13 districts were added to the original list of "minority concentration" districts, despite religious minorities accounting for less than 25% of the district population share. We exclude these districts from our main sample and use a sharp RD specification. Columns (3) and (6) show that our findings are similar if we instead use a fuzzy RD design and include these districts.[33]

All our main specifications restrict the sample to a bandwidth of .058, which is the MSERD optimal bandwidth for our intensive margin outcome variable of interest (bank loan amount). 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.

[30] 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.

[31] We use unweighted heteroskedasticity robust errors.

[32] 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.

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

5.1.3 Credit Access for Non-Minorities

We now examine aggregate consequences of the directed credit policy by comparing access to bank credit for non-minority households across treated and control districts. If banks kept the overall volume of lending fixed in minority concentration areas, the increase in bank credit to minorities documented in Section 5.1.1 would imply a reallocation of credit from non-minority borrowers.

Visually, Appendix Figure F1 shows comparable levels of bank credit access 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 conclusions. There is however evidence of a reallocation of credit from informal sources to non-bank financial institutions for non-minority households. Columns (2) and (6) show that the treatment led a significant decline in informal loans for non-minority households along both the extensive and intensive margins.[34] This was accompanied by an increase in credit obtained from non-bank financial institutions. The intensive margin increase in loans from non-bank financial institutions (column (7)) is two-thirds in magnitude to the reduction in credit from informal sources (column (6)). Combining the results with those in Appendix Table C4, the coefficients suggests that the directed credit policy led minority households to substitute non-bank institutional borrowings with commercial bank credit. On the other hand, non-minority households substituted informal credit with credit from non-bank financial institutions.

Section 2.3 noted that a key incentive to comply with the directed credit policy is that it offered banks a larger pool of borrowers to meet their regulatory targets for lending to "weaker sections". The latter include loans to female borrowers, as well as borrowers hailing from historically marginalized *Dalit* and *Adivasi* communities. While we cannot separate across male and female borrowers owing to the household-level nature of the AIDIS data, Panel B of Appendix Table F1 alleviates concerns that the expansion in minority bank credit access emanated from a crowding out of *Dalit* and *Adivasi* borrowers. Although the intensive margin point estimate in column (5) is negative, it is not statistically significant (p-value .486). Along the extensive margin [column (1)], the point estimate is positive, large,

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

but not statistically significant (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, Panel B of Appendix Table F1 does not suggest that banks complied with the directed credit policy by reallocating credit across communities qualifying as "weaker sections."

5.2 Mechanisms

Section 5.1 identified an expansion in bank credit access for minority households in minority concentration districts along both the extensive and intensive margins. We now examine three channels through which banks could have facilitated credit access for disadvantaged religious minorities.

5.2.1 Bank-SHG Linkages

The central bank in every annual policy guideline 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 minorities owing to high screening and monitoring costs, it is possible that such costs could have been ameliorated by banks lending to SHGs. If SHGs have lower costs of screening and monitoring, bank credit can be effectively targeted to creditworthy minority borrowers through SHGs (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 3 disaggregate the source of bank credit to separately identify the treatment effect on direct bank lending 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 3, 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, the treatment effects in Table 3 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 3 shows that commercial banks responded to the directed credit policy by directly lending to religious minorities, and also lending to them in collaboration with SHGs. In contrast, columns (2) and (4) of Appendix Table F2 shows 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.

5.2.2 Collateral Requirements

The second channel considered is collateral requirement by lenders. Pre-treatment descriptives in Appendix Figure A4 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 in order 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 4 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 both loans obtained directly from commercial banks, and 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. This negates any overall improvement in lenders' ability to acquire information or monitor borrowers in treated districts.

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. This would also be consistent with the findings in Section 5.1.5 which confirmed no deterioration in credit quality for loans issued to minority 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 while approving loan applications for female borrowers, leading to lower rates of acceptance. Relatedly, Table 4 suggests that relaxing collateral requirements can improve credit access for disadvantaged borrowers. This is particularly relevant when considering the large gap in real estate values across minority and non-minority households.

5.2.3 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 4 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 suggests 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 3 and 4 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.

5.3 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.

5.4 Labor Market Outcomes

A large literature has explored the labor market impacts of credit access for households. 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. We examine in this regard the impact of the increased credit access on labor market outcomes for minority households.

We list two potential channels through which the directed credit policy could have altered occupational choices of minority workers. First, higher farm credit could have directly affected farm labor. Section 6.1.1 offered evidence of 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 manufacturing and services to meet the additional demand for goods and services – the labor pull channel, evidenced in Emerick (2018). Relatedly, expenditure loans from banks could also have increased local demand for goods and services, generating higher labor

demand in non-farm sectors.

We use weekly employment data from the PLFS to identify the impact of the directed credit policy on labor market outcomes. The unit of observation is the individual, and we restrict the sample to individuals aged between 18 and 60. The empirical strategy is similar to specification (2), except that we include additional individual controls for gender, and a quadratic in age. 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.

Columns (1) and (2) of Table 5 shows that the directed credit policy did not affect overall labor force participation or the rate of unemployment for minority workers in treated districts. Column (5) also rules out the labor push channel by identifying a positive and statistically non-significant impact on the likelihood of farm work. Instead, columns (3) and (4) suggest 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 of labor. The treatment led to a 6 ppt. increase in minority workers' likelihood of being employed in the manufacturing sector, and a corresponding 6 ppt. decline in their likelihood of being employed in trade and service activities. Appendix Table D1 identifies very similar effects along the intensive margin. Column (1) shows total hours worked during the week was unaffected by the treatment; column (2) shows 5 additional hours of self-employment for minority workers in treated districts, which column (3) shows 6 hours less of wage work. There is also no impact of the treatment in column (3) on hours worked in farm activities. Instead, column (6) identifies 3 additional hours of work in the manufacturing sector.

Appendix Tables D2-D3 examine this reallocation across occupation type and sectors in greater detail. We combine here workers' occupation with their sector of employment and confirm that the treatment led to minority workers reallocating their labor from wage work in trade and services, to self-employment in manufacturing work. 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 a large scale expansion of goverment banks in India resulted in a shift away from self-employment towards wage work. To further identify the specific ectors 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 D4 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 retail or wholesale trade, it moved labor away from the transport and hospitality sectors [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, we document a reallocation of minority workers away from wage work in the transport and hospitality sectors. Exposure to directed bank credit instead pushed these workers into self-employment in textile manufacturing.

5.5 Household Consumption

We conclude our empirical analysis by identifying the treatment's effect on household consumption, as a comprehensive measure of overall household well-being. Section 5.1.4 identified a statistically significant increase in expenditure loans, which could have directly increased household consumption. Alternately, farm credit from banks for minority households resulted in increased mechanization of farm work and higher holdings of irrigated farm land, which in turn could have raised crop yields and boosted household consumption. Increased participation of minority workers in manufacturing self-employment could also have increased household consumption if manufacturing activities offered higher returns to labor (Section 6.3). In contrast, Augsburb et al. (2015) carefully details why household consumption could remain unchanged, or even reduce, despite higher access to household credit. 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 their consumption to save for the asset purchase. Ex-ante therefore, the treatment effect of increased credit access 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 6 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 6 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.

Section 3.4 noted that Muslim households had substantially lower levels of household consumption, relative to non-minority households. As a final exercise, we test 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:

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

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. The coefficient estimates are weighted using NSS assigned household weights and a triangular kernel, and standard errors are clustered by district for inference.

Column (4) of Table 6 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. Columns (7) and (8) include Christian and Sikh households and obtain very similar findings

The point estimate in column (5) corresponding to β_2 equals a 19 percent increase in monthly per capita household consumption for Muslim households in minority concentration districts, relative to non-minority households. 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. Consequently, Table 6 highlights the ability of financial affirmative action to substantially reduce long-standing inequities for disadvantaged religious minorities and promoting overall household well-being.

6 Effects on Elections and Conflict

In the previous sections, we established that credit policy was successful at increasing credit take up by minority households, resulting in improved economic welfare, and reduction in inequality. We now explore how these economic changes influence political outcomes and social harmony.

Success of the bank credit policy may affect social harmony and political outcomes in target districts through two mechanisms. First, in divided and unequal societies, changes in the economic fortunes of groups may lead people to reinforce their political and cultural identities resulting in higher political participation and increased conflict (Mitra and Ray, 2014; Bonomi et al., 2021). Second, the policy may result in social harmony if there is more economic collaboration between groups in the economy as a result of the credit policy (Jha, 2013). We investigate these two channels by using data from elections and conflict.

6.1 Electoral Effects of Policy

We first study if credit policy increase access for minority households influences electoral outcomes in the target districts. We study these effects using two outcomes, vote share and candidacy decisions, of two types of candidates: those from the right-wing party, and those that are likely Muslim.[35] While

[35] The right-wing party is known for anti-minority platform, so an empowered minority can feel more attached to their group identity and mobilize to vote against this party (Bonomi et al., 2021).

information on the party affiliation is available in the public data on elections, we do not know the religious identity of candidates. Instead, we rely on a machine learning algorithm to classify candidates into Muslim and non-Muslim groups based on their names. This algorithm is developed by **?** specifically for India, and further validated by Allie (2024).

Table 7 reports effects of credit policy on electoral outcomes using data on all candidates in Indian state elections between 2008 and 2019. First four columns study effects on the vote-shares of right-wing party candidates and Muslim candidates, while the last two columns candidacy. Each column uses state and year of election fixed effects. Focusing on column 1, the vote share of a candidate belonging to the right-wing party decreased by 4.87 percentage points in the policy district when compared to candidates of the same party in the non-policy districts. This decrease is mirrored by an increase of 3.07 percentage points (column 2) in the vote-share of candidates classified as Muslims in the policy districts compared to Muslim candidates in the non-policy districts. These effects are the result of a conditional analysis using a constituency-candidate level data, that is, the data is restricted to elections where there is a right-wing party candidate (column 1) and at least one Muslim candidate (column 2). We also report unconditional effects of the policy on vote share in columns 3 and 4 by collapsing the data to constituency level and assuming the vote share to be zero if there was no candidate with the affiliation of interest. This analysis also yield similar effects– right-wing party candidate vote share is decreased by 3.98 percentage points ($p - value$ of 0.13), and Muslim candidates' vote share increase by 3.21 percentage points. Lastly, columns 5 and 6 report effects on whether the policy changed the decision of the right-wing party and Muslim community to field candidates in elections. This analysis also uses constituency level data, where the outcome is coded as one if there is a candidate of the right-wing party and zero otherwise, similarly it is coded as one for Muslim if there is at least one Muslim candidate and zero otherwise. This analysis shows that while there was no effect on the likelihood that the right-wing party field a candidate, there is a strong positive effect on the likelihood that there was at least one Muslim candidate in a constituency as a result of the credit policy.

6.1.1 Mechanisms of Electoral Effects

The changes in vote share of the right-wing party and Muslim candidates discussed in the preceding section could be driven by three non-exclusive channels. First, Muslim voters may feel more empowered to participate in politics and vote in a coordinated manner. Second, the changes in vote shares may

not be just because of how Muslims vote but the credit policy may have also influenced how other people view Muslims in the society and, hence, vote against the party that does not agree with their view of Muslims. If they vote more in line with the interests of Muslims, that may be an evidence of increased social harmony. And third, due to improved economic standing of Muslims, they may be able to produce more qualified candidates as a group. In this section, we provide evidence that is consistent with all three of these channels.

6.1.1.1 Voter Registration and Turnout

We first explore if the main channel of electoral effects is higher participation of Muslims in politics as voters as measured by the number of voters and their turnout in elections.

If Muslims in the policy targeted districts changed their registration or turnout behavior as a result of improved economic well being, and non-minority voters did not, we should see an increase in the average voter registration and turnout, assuming all else remained the same in elections. As before, we use the sharp discontinuity at the district level to compare outcomes of assembly constituencies falling the policy districts with those falling in the non-policy districts in a regression discontinuity design. In columns 1 and 2 of Table G6, we find no evidence for increase in the number of people registered to vote and the share of registered voters who turned out to vote on the day of election, respectively. This analysis used aggregate data for all voters at the constituency level. Since it is possible that the effects on Muslims and non-Muslim voters may have gone in the opposite direction, effectively negating the policy effect, therefore, our result should be interpreted as a suggestive lack of evidence for effects on registration and turnout.

6.1.1.2 Voting Along Communal Lines

We now analyze if citizens voted along communal lines, that is, if the decrease in vote-share of right-wing party candidates and the increase in vote share of Muslim candidates is coming from constituencies with more Muslims. Since, individual voting decisions are not available, we assess shares of polled votes received by the right-wing party and Muslim candidates, separately in areas where Muslims constitute a higher share of voters and in areas where they constitute a lower share. We categorize assembly constituencies as high Muslim concentration areas if the share of their population who identify as Muslim is equal to or higher than the median share in the sample. Importantly, the reference median used is separate for constituencies that fall in the policy target districts, and those not covered by the

credit policy.

Using data from Gulzar et al. (2024) on estimated population shares of the state assembly constituencies by religion, we report the effects on vote-shares in columns 3 to 6 of Table G6.[36] Two results emerge from this analysis. First, the negative effects on the vote-share of right-wing party are not driven by areas with more Muslims. Column 3 shows that the right-wing party witnesses a large decrease in its vote share in areas where Muslims constitute a smaller population of share, whereas, column 4 shows that there is no effect on the vote share these candidates in areas where Muslims constitute larger share of the population. Second, the effects on Muslim candidates is opposite. It is only in the constituencies with higher share of Muslim population that candidates of the same religion witness an increase in their vote share.

Since the population shares are estimated it is likely there is a measurement error. We test for robustness of the results by removing districts that had more than five percentage points difference between the estimated share of Muslim and the actual share of Muslim as per the Census of India 2001. Appendix Table **??** reports the results on vote-share by Muslims as share of population are robust to removing these extreme cases.

The negative effect on the vote share of the right-wing party candidates in constituencies without high concentrations of Muslims is unexpected, as it suggests that non-Muslims likely also voted against the right-wing platform, which is an indicator of increased social harmony between the minority and majority group as a result of the credit-policy.

6.1.1.3 Inter-group Engagement

Why would the non-Muslims, and probably the majority group, vote against right-wing parties that promotes anti-Muslim sentiment? The answer to this question may lie in the special institution of bank-linked self help groups that promoted collaborative contact between Muslims and other groups. As discussed in section 5.2.1, one of the mechanisms used to improve bank credit take up by the minority households was to provide credit through the self-help groups (SHG). These groups can be formed by citizens with any affiliation and registered with the government for access to credit. In Table G8, we examine the number of different types of self-help groups the credit policy promoted.

[36] Gulzar et al. (2024) estimate the shares of Muslims from voter lists at the polling station level. They shared with us the population figures aggregated to the assembly constituency level. We are grateful to them for sharing this data.

The policy increased formation of groups that have both Muslim and non-Muslim groups as members (column 3), thus, promoting collaboration across groups that likely promoted inter-group harmony in the society.[37]

6.1.1.4 Profile of Muslim Candidates

It is likely that improved economic well being also changed the profile of candidates from the Muslim Community. For this analysis, We use data from the declaration of candidates at the time of the election and merge them with the list of candidates we have from the election data. Appendix Table G7 shows that Muslim candidates in policy districts have more net assets (column 1) and higher annual income (column 2) but They are not different in terms of age and years of education. These results reflect an improved economic well being of Muslim community in the policy districts as a result of the credit policy.

6.2 Communal Violence

The preceding section argued that the credit policy affected electoral outcomes in the policy targeted districts, by increasing the vote-shares of Muslims and decreasing the vote-share of candidates belonging to the right-wing party with anti-Muslim platform. We also found that the later effect was present only in constituencies where Muslims do not constitute a large share of the population, indicating that it was largely non-Muslim voters who turn away from the right-wing party. This result can be interpreted as indicating to improved social harmony. We now explore if the social harmony visible in elections also translates into reduced violence between the minority (Muslims) and the majority (Hindus).

Inter-group violence along the religious lines is widespread in India. While the sources of this violence can be numerous, scholars have underscored 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. Since, we have shown that the policy likely improved civic ties between the two groups, through electoral politics and formation of self-help groups, it is likely that such ties resulted in a reduction of violence in the policy districts.

To test this hypothesis, we rely on two sources of data. First, we access data on political violence from 2016 to 2019 in India from the Armed Conflict Location and Event Dataset (ACLED) (Raleigh

[37] We use a differences-in-discontinuity design for this analysis.

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 annual level for the main analysis and month 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. Second, we use official 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, it is likely to pick up on any change in rioting in minority concentration districts.

Table 8 reports the effects of credit policy on violence. Column 1 reports an increase of 0.067 riots per year between Hindus and Muslims as a result of the policy using information from ACLED. This effect is supported by analysis of the official data, as column 3 reports an increase of 4.67 religious 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 4). As a placebo, we also check the effects on murders in the NCRB data 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). Alternatively, 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 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 before and after election time periods, and run separate analysis using our regression discontinuity design specification. We eliminate the month of election from the data as we cannot

identify if the violent event happened before or after the election. Appendix Table G10, 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 election minority concentration districts. This pattern of violence suggests that it was strategically used as a response to the electoral effects. Since, we have established that the electoral effects for the right-wing party was largely arising in constituencies where Muslim concentration is low, it is then interesting to explore whether the violence was concentrated in the same constituencies. However, columns 1 and 2 of Table G11 show there is no effect on violence in constituencies with lower Muslim concentration, and a strong positive effect in constituencies with higher Muslim concentration, respectively. These results support the interpretation that violence was strategically targeted as a tool of punishment for the minority, since the increase is concentrated in constituencies where the right-wing party has fewer potential voters, and not in places where they may disillusion the potential voters even more with divisive actions.

7 Conclusion

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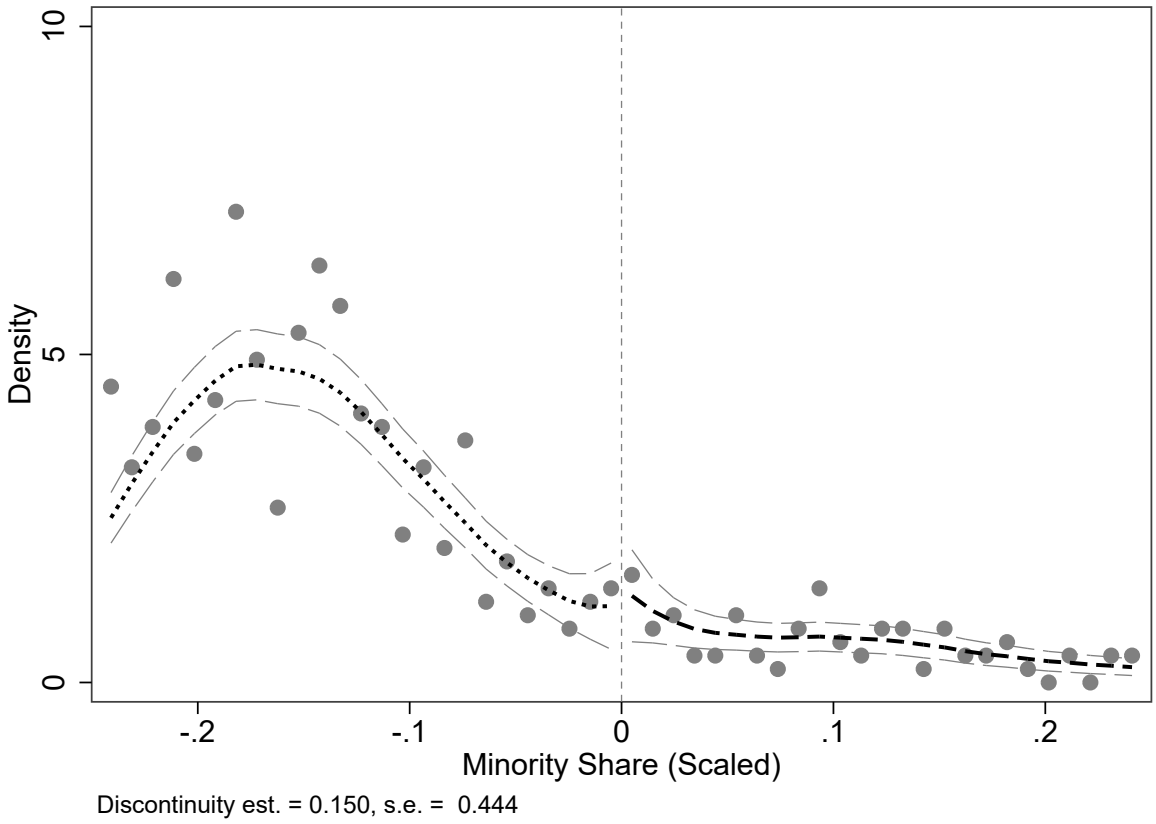
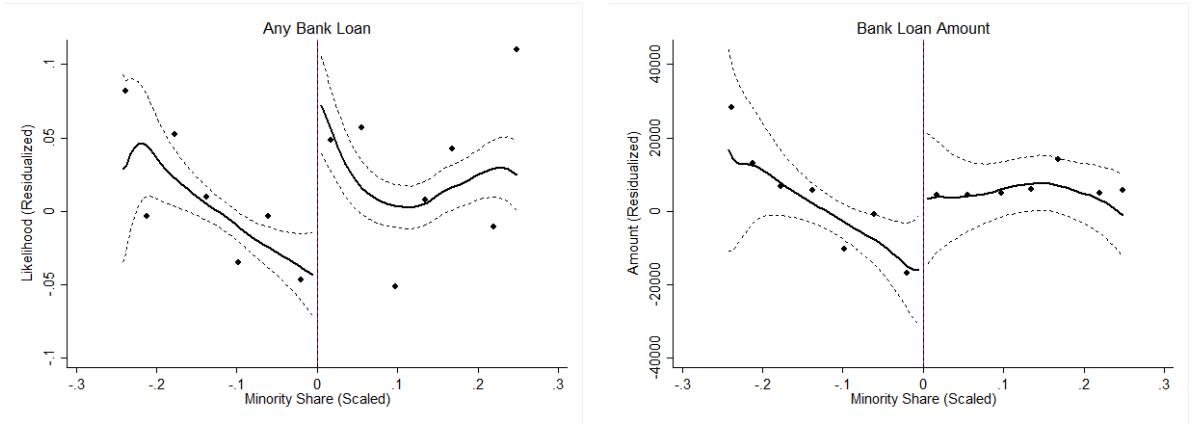
8 Figures

Figure 1: Smoothness in Treatment Assignment at Discontinuity Threshold



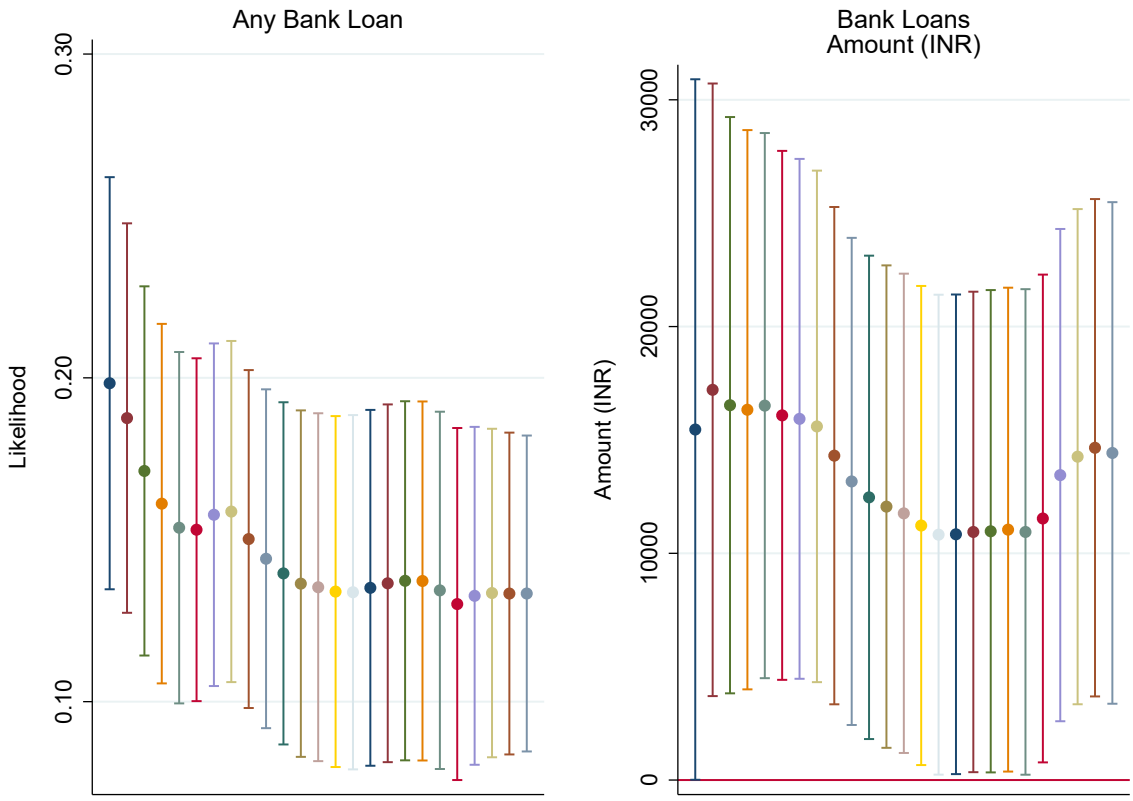
Notes: This figure presents the McCrary test (McCrary, 2008), testing for selective sorting of districts as minority concentration around the 0.25 threshold. The horizontal axis depicts the running variable – defined in equation (1). The horizontal axis shows the range of the running variable between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



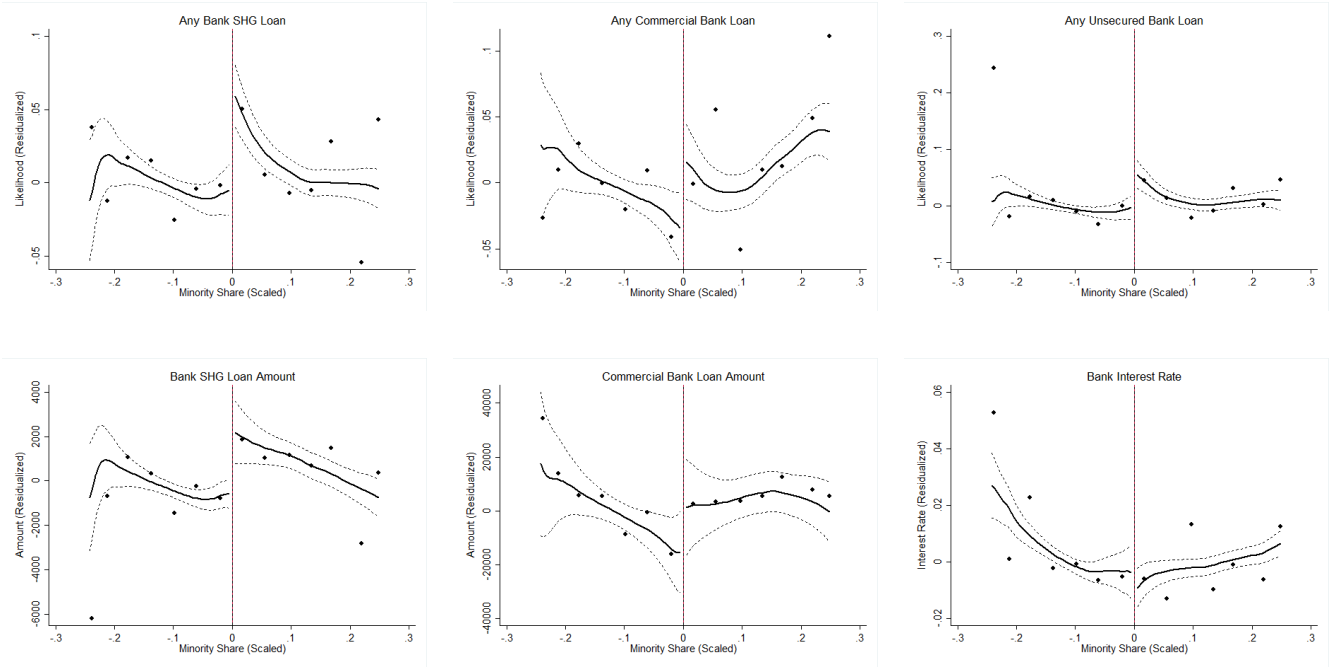
The above figures shows the treatment effect for access to bank credit for minority households. The unit of observation is the household. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). The outcome of interest in the left-panel is a dummy equaling 1 if the household has any bank loan; in the right-panel, the volume of bank loans (initial loan value). The 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



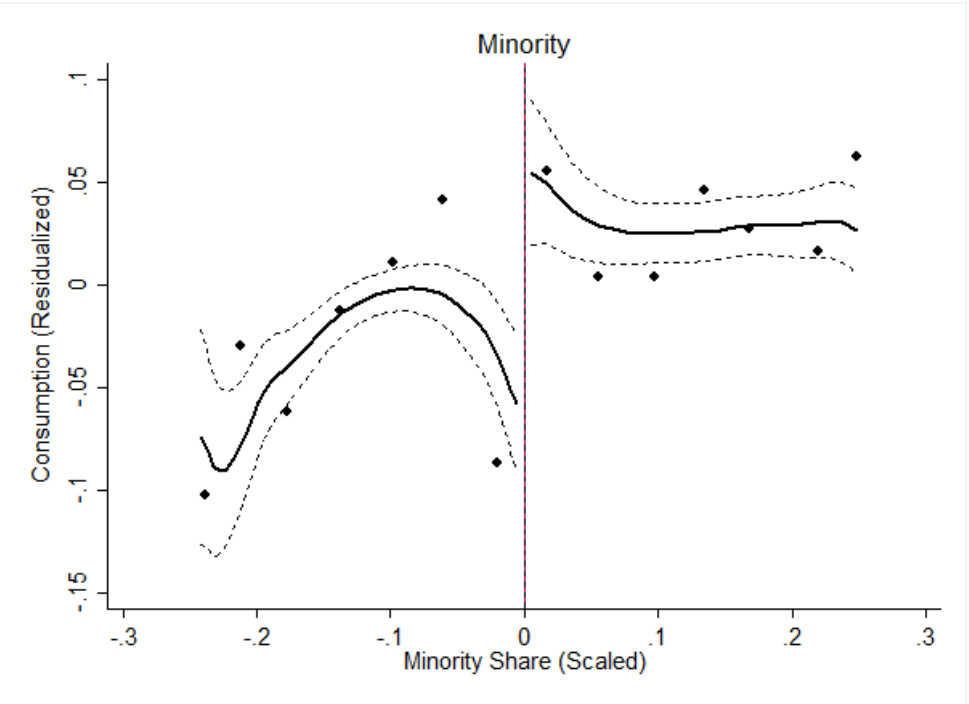
Notes: This figure shows the robustness of the baseline results to alternate bandwidths. The unit of observation is the household. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). The outcome of interest in the left-panel is a dummy equaling 1 if the household has any 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 .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 2: 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 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 controls for household size and a rural indicator. 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 3: 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 .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 4: 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 .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 5: 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 6: 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 .061 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 7: Effects on Electoral Outcomes

	(1) Right-Wing Party Vote Share	(2) Muslim Candidates Vote Share	(3) Right-Wing Party Vote Share	(4) Muslim Candidates Vote Share	(5) Any Right-Wing Candidate	(6) Any Muslim Candidates
Treat	-4.864***	3.068***	-3.987	3.208*	.048	.137**
	(1.684)	(.873)	(2.689)	(1.876)	(.048)	(.054)
Observations	857	1641	1045	1045	1045	1045
Dep Var Mean	32.824	5.791	28.480	7.873	.869	.502

This table reports the effect of credit policy on political outcomes, mentioned in column headers, comparing policy districts with non-policy districts. Columns 1 to 4 use constituency-election level data, and columns 5 and 6 used candidates level data. First two columns use candidate level data to report effects on vote share conditional on there being a candidate. Columns 3 to 5 use constituency level data reporting unconditional effect on outcomes. Each regression controls for state and year fixed effects. Standard errors in parentheses

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

Table 8: Effects on Hindu-Muslim Violence

	(1) Hindu-Muslim Riots	(2) Total Riots	(3) Communal Riots	(4) Total Riots	(5) Murders
Treat	.066**	-2.428	4.688***	-4.581	-5.711
	(.029)	(1.523)	(1.568)	(17.333)	(9.963)
Observations	248	248	326	390	195
Dep Var Mean	.057	4.200	1.534	76.348	42.549
Data Source	ACLED	ACLED	Admin	Admin	Admin
Years	2016-2019	2016-2019	2014-2019	2014-2019	2014-2019

This table reports the effect of policy on Hindu-Muslim violence using data from the Armed Conflict Location and Event Data (ACLED) (cols 1-2) and Crime Statistics from Government of India (cols 3-4). Outcomes analyzed in each regression are mentioned in the column headers. Each regression includes state and year fixed effects. Standard errors in parentheses.

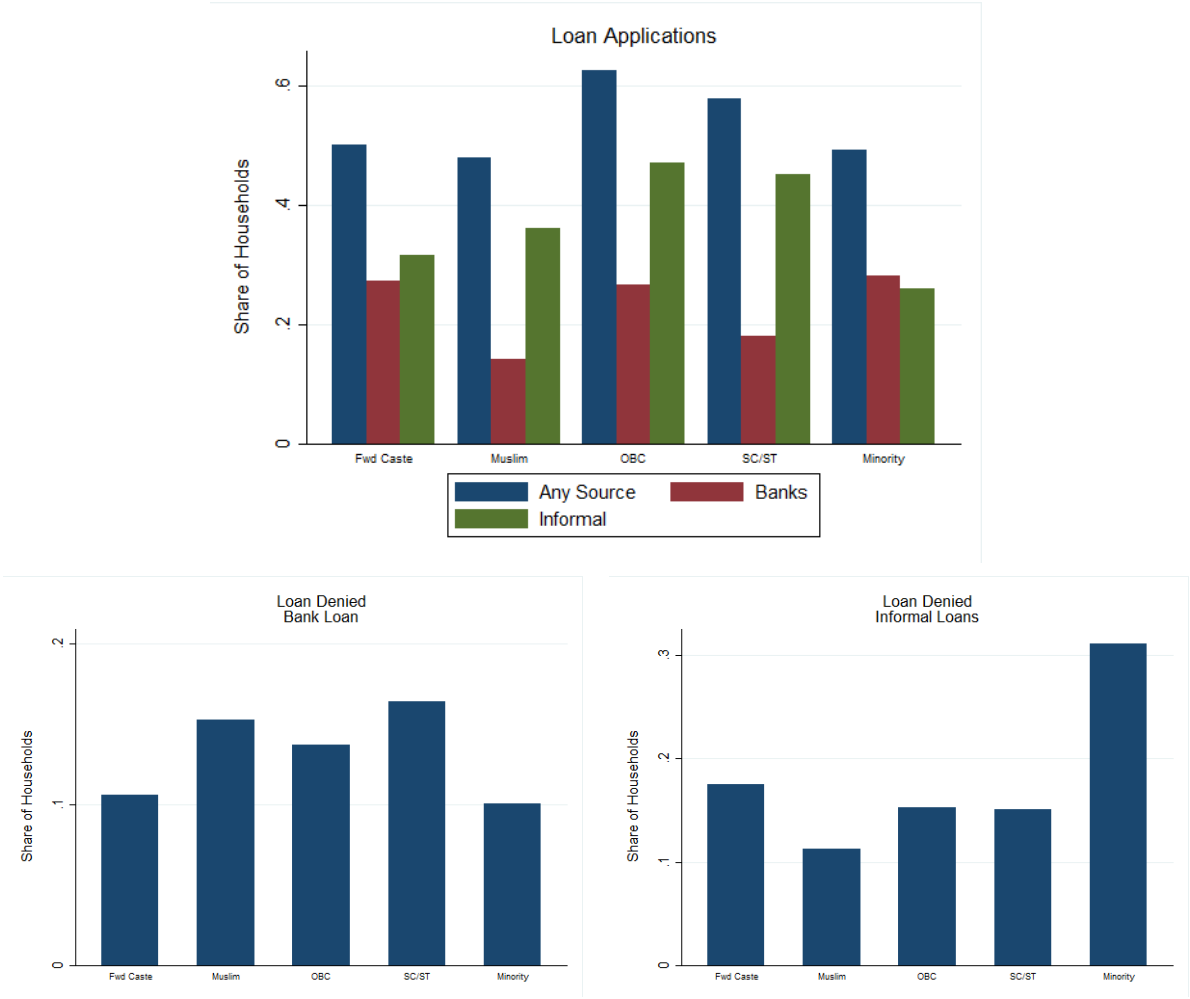
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A Appendix: Pre-Treatment Desciptives

Figure A1: Pre-Treatment Household Credit Across Communities

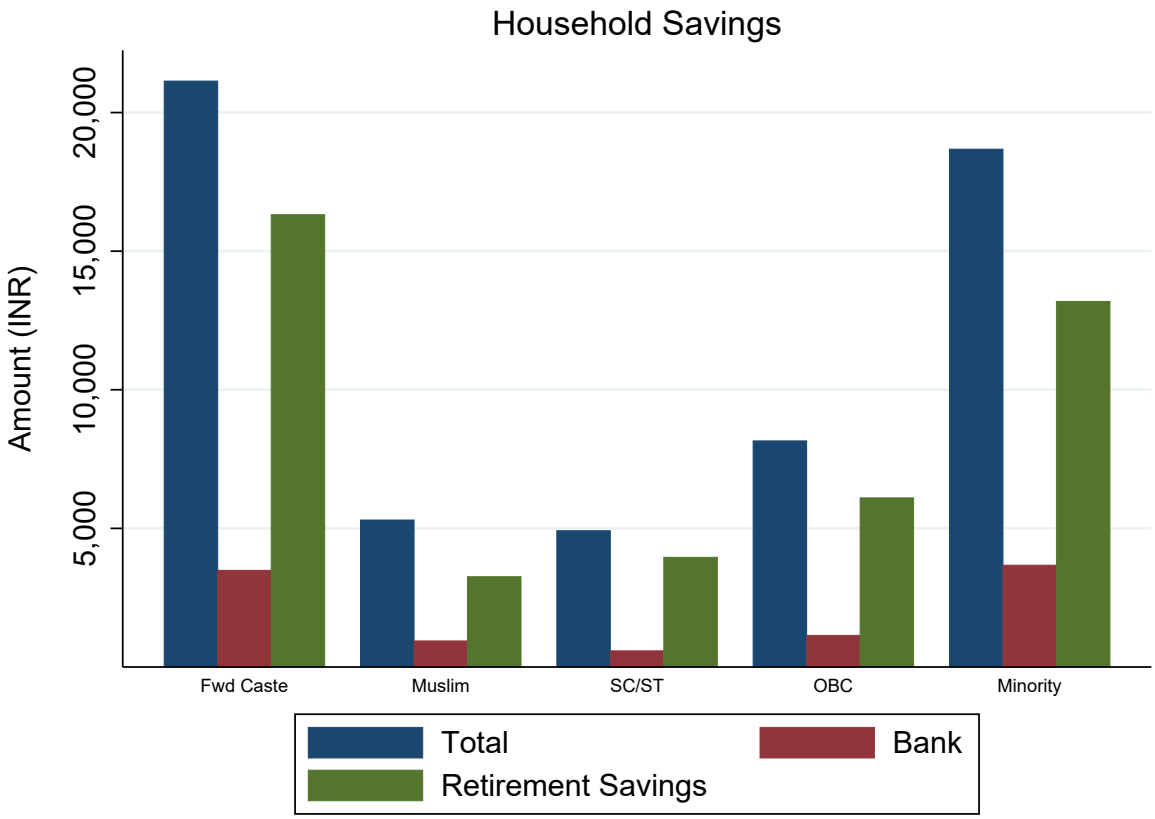
Notes: The above figure presents pre-treatment descriptive statistics for loan outcomes by community. The top-left panel shows the likelihood of households having loans from any of the above-mentioned sources; the top right-panel shows the average volume of credit received by households from each source; 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: 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 A3: 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 A4: 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.

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	Tra Se
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

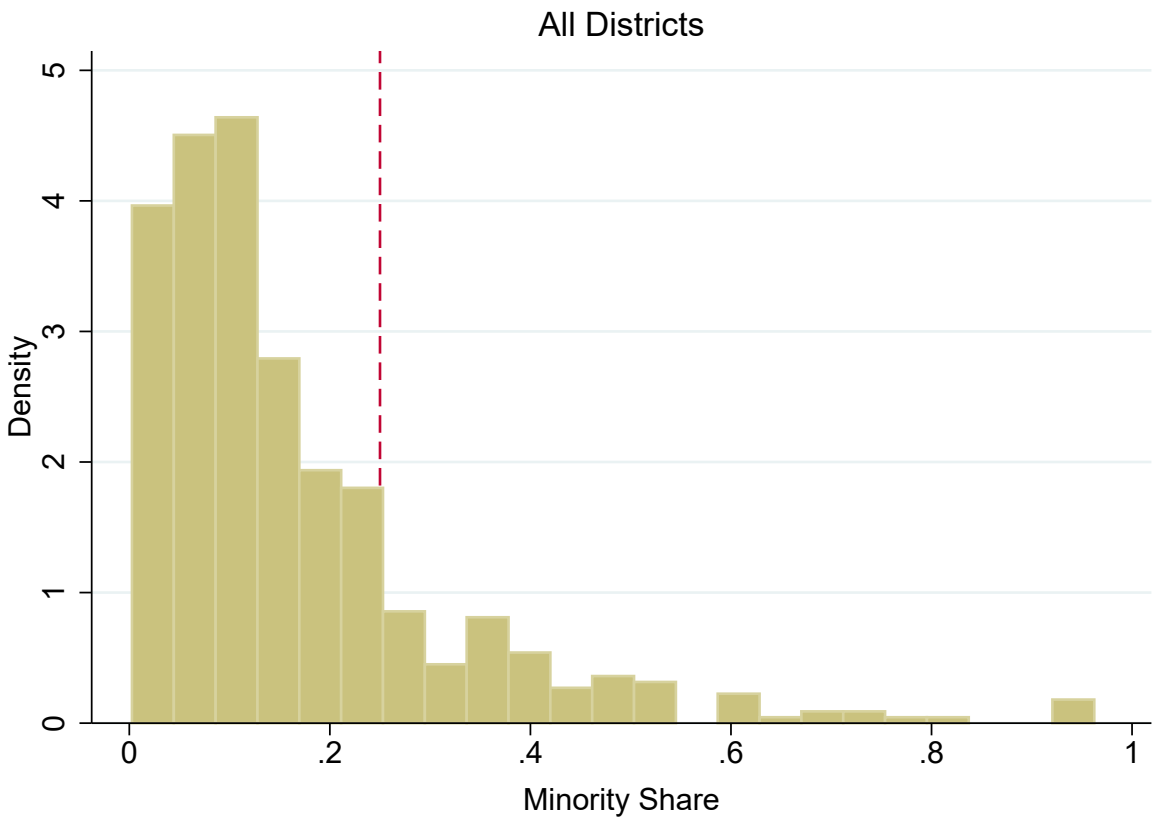
	(1)	(2)	(3)	(4)	(5)	(6)
			Per Capita Bank Credit			
	Farm Account	Farm Amount	Manufacturing Account	Manufacturing Amount	Personal Loans Account	Personal Loans Amount
Treat	.002	924.474	.001	-12897.358	-.042	-608.173
	(.009)	(1497.737)	(.001)	(22206.290)	(.059)	(3218.739)
Observations	59	59	59	59	59	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%

C Appendix: Additional Figures and Tables

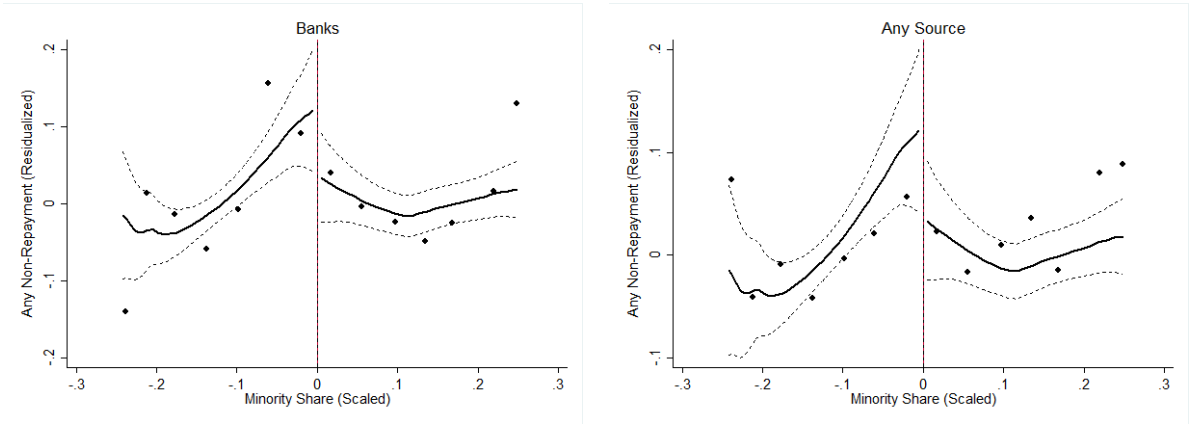
C.1 Figures

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 .06 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 Capita 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	0.117	0.321
Any community network loan	97154	0.086	0.280
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	195396	322316.7	30501	73337	203339
Total bank loan	30003	196804	312133.9	40000	75787	203339
Commercial bank loan	24553	246261	346805.9	50835	101669	285750
Bank-SHG loan	6342	46175	42281.1	20334	40000	54570
Non-bank financial institution loan	12886	132051	237190.3	28000	50835	105501
Money-lender loan	14340	116735	177972.9	21201	50835	132170
Community network loan	14214	75198	146760.7	10167	26501	71169
Farm loan	15534	145366	221153.7	32742	71169	159008
Non-Farm business loan	5555	196600	309670.0	39222	75000	205000
Expenditure loan	22540	97986	164722.3	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
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 non-bank financial institution loan	97154	0.034	0.181
Any unsecured money-lender loan	14340	0.804	0.397
Any unsecured friend/relatives loan	14214	0.911	0.285
Interest rate	60675	0.136	0.107
Bank interest rate	30003	0.106	0.050
Non-bank financial institutions interest rate	12886	0.123	0.078
Money-lender interest rate	14340	0.290	0.114
Community networks interest rate	14214	0.020	0.077
Loan delinquent	53135	0.368	0.482
Bank loan delinquent	27073	0.279	0.449
Informal loan delinquent	21466	0.490	0.500

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 loans include loans issued by co-operative banks and other non-banking financial corporations. Money-lender loans include loans obtained from input-suppliers. Community network loans include loans obtained from friends, relatives, employers and landlords. Expenditure loans include loans obtained for consumption, housing, health and education. Interest rates are weighted using loan size. Summaries are computed only for households with outstanding loans in the referred categories.

Table C2.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 C2.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 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)-(5) is a dummy equaling 1 if the household had any outstanding bank loan; in columns (6)-(10), the amount of bank loans received by the household. Columns (1)-(2) and (6)-(7) use data-driven MSERD optimal bandwidths from Calonico et al. (2020). Columns (3) and (8) extend the covariate vector to include select district covariates; columns (4) and (9) extend the sample to include all religious minorities; columns (5) and (10) use a fuzzy RD specification where we extend the sample to include all districts classified as minority concentration. Except for columns (2) and (7), all specifications include a linear polynomial in the running variable, state fixed effects, and household covariates. Columns (2) and (7) replace the linear polynomial with a quadratic polynomial. The sample in columns (3)-(5), and (8)-(10) is restricted to a bandwidth of .06 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 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)-(5) is a dummy equaling 1 if the household had any outstanding bank loan; in columns (6)-(10), the amount of bank loans received by the household. Columns (1)-(2) and (6)-(7) use data-driven MSERD optimal bandwidths from Calonico et al. (2020). Columns (3) and (8) extend the covariate vector to include select district covariates; columns (4) and (9) extend the sample to include all religious minorities; columns (5) and (10) use a fuzzy RD specification where we extend the sample to include all districts classified as minority concentration. Except for columns (2) and (7), all specifications include a linear polynomial in the running variable, state fixed effects, and household covariates. Columns (2) and (7) replace the linear polynomial with a quadratic polynomial. The sample in columns (3)-(5), and (8)-(10) is restricted to a bandwidth of .06 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 C4: 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 non-bank sources of credit for minority households. 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. 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 C5: 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, input suppliers and marketing agents. *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 sources; 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 .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 C6: 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 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 C7: 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 purpose of bank 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 direct purchase of housing. 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 C8: 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	-.045	.294***	-.119	.304**	-.107	.064
	(.066)	(.099)	(.080)	(.127)	(.165)	(.056)
Observations	465	441	174	273	210	991
Dep Var Mean	.292	.460	.337	.537	.054	.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. 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 C9: 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 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 C10: 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 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 C12: 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 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%

D Appendix: Additional Outcomes

D.1 Purpose of Borrowing

Figure **??** visually depicts the treatment effects for minority households' self-reported purpose of borrowing across three aggregate categories: farm credit, credit for non-farm businesses, and loans for household expenditures. The latter cover borrowings for education, health, housing, and other consumption purposes. The figure offers evidence of modest positive treatment effects for farm and expenditure loans from banks along the extensive margin. There is however no evidence of increased bank loans for non-farm business purposes.

Local linear regressions in Table 2 identify a positive and significant impact for farm and expenditure loans. The INR 6359 (15232) increase in bank loans for farm (expenditure) purposes is substantial, when considering that the corresponding control group mean equaled INR 3910 (11635). Overall, a third of the intensive margin increase in minority bank credit is accounted for by farm loans. Appendix Table C7 disaggregates expenditure loans into its three major constituents. Columns (1)-(3) identify positive treatment effects on the likelihood of receiving a bank loan for housing, consumption, and health and education purposes. The coefficients are both statistically and economically significant. Along the intensive margin though, the increase in bank expenditure loans is driven by loans obtained for the purpose of housing, which include both loans to purchase new residential properties, or augment existing properties. The RD estimate for health and education loans is also positive and statistically significant, but substantially smaller in magnitude than housing loans. As farm loans, education loans, and loans for housing purposes qualify under priority sector lending, higher loan disbursement in these categories would also be consistent with banks' incentive to use the directed credit policy to meet their regulatory targets.

D.2 Credit Quality

In the absence of publicly available administrative data on district-level defaults, we use self-reported data on borrowers' repayment behaviour to assess the impact of the directed credit policy on credit quality. If minority borrowers were on average riskier, and banks had avoided lending to such borrowers to mitigate credit risk, compliance with the policy would likely result in increased loan delinquency. Alternately, if financial institutions had limited information and screening mechanisms for minority borrowers, directed lending in the presence of major information asymmetries could also have worsened loan performance.

Section 3.1 noted that loans are deemed to be delinquent if no repayment had been made towards the loan for over 6 months. We extend this to classify households as delinquent if it reported delinquency for any of its outstanding loans. Visually, Appendix Figure C2 shows no evidence of a change in the likelihood of delinquent loans at the discontinuity threshold for either bank loans, or across all loan categories. If anything, the figures point to lower loan delinquency in treated areas. Column (1) of Appendix Table C8 shows that the likelihood of minority households having a delinquent bank loan was comparable across minority and non-minority concentration areas. Column (2) shows a significant increase in delinquency rates on informal loans. Upon disaggregating the source of informal borrowing into traditional money lenders [column (3)] and community networks [column (4)], we see the rise in delinquency in treated areas to originate exclusively from loans sourced through community networks. As the vast majority of these are interest-free loans, it is possible that there was a one-time repayment agreement, as opposed to repayment through installments. Consequently, the rise in delinquency observed in column (4) of Appendix Table C8 does not necessarily imply an increase in actual delinquency. Overall, the absence of an increase in delinquency for credit obtained from non-banking financial institutions [column (5)] and traditional informal sources such as money lenders assuages concerns that minority households were repaying their bank loans by defaulting on other

sources of credit.

D.3 Productive Assets

Section 5.1.4 showed that the expansion in bank credit for minority households in minority concentration areas were primarily in the form of farm and expenditure loans. The increase in the latter category was driven by housing loans. To this effect, we use the rich data on household assets from the AIDIS to identify whether the expansion in bank credit also translated into increased holding of farm assets and real estate by minority households.

D.3.1 Farm Assets

The AIDIS provides information on household land holdings, and the value of livestock, farm machinery and transport equipment owned by the household. Columns (1) and (2) of Appendix Table C9 identifies a large and statistically significant increase in minority households' ownership of farm machinery along both the extensive and intensive margins. The intensive margin increase is equivalent to 50 percent of the treatment effect for farm bank loans. Additionally, column (3) shows that minority households in treated districts were also more likely to own transport for farm activities. Columns (4) and (6) point to higher values of farm transport owned, driven by a 57% increase in the value of tractors, although the point estimates are statistically significant only at the 15% level (p-value .116)

Alongside investing in farm machinery, farm loan recipients could have expanded their land holdings, or undertaken long-term investments in the quality of agricultural land. Alternatively, the use of farm machinery could have boosted crop yields, the sales of which could have been used to increase land holdings or undertake productive investments in land quality. Columns (1)-(3) of Appendix Table C10 suggests improvements in the quality of farm land owned by minorities in minority concentration areas. Minority households saw a 9 ppt. (p-value .103) increase in the likelihood of having irrigated land, and their irrigated farm area also increased by 0.12 hectares (p-value . 110). The treatment effect on overall farm area in column (4) however is attenuated towards 0 and statistically non-significant. This is indicative of private investments in irrigation, as opposed to the acquisition of irrigated land. Columns (3) and (5) show large statistically significant increases in both the value of irrigated farm land, and overall farm land. Comparing the treatment effects in columns (3) and (5), half of the increase in aggregate farm value emanated from a higher value of irrigated farm land owned by minorities. There is however no corresponding investment in the ownership of livestock, as seen from columns (6) and (7) of Appendix Table C10. Collectively, access to farm bank credit increased minority households' ownership of farm machinery and irrigated farm lands. While overall acreage remained unchanged, there were improvements in the quality of agricultural land owned by minorities through private investments in irrigation, along with increased mechanization of agricultural activities.

D.3.2 Non-Farm Assets

In addition to farm loans, Appendix Table C7 identified a positive treatment effect on bank credit for housing purposes. These could be for the construction of new houses, or improving existing structures. Consistent with the increase in housing credit, column (1) of Appendix Table C12 identifies a weakly significant (p-value .092) increase in the value of residential buildings owned by minority households in minority concentration areas. Akin to the findings on farm land, the increase in the value of buildings is unaccompanied by any increase in residential building area. Thus, bank credit obtained by minorities for housing purpose is used to upgrade existing real estate, as opposed to purchasing additional properties. Not unsurprisingly, the positive treatment effects on farm value and residential buildings combine to yield a significant increase in the value of real estate owned by minority households in minority concentration areas [column (3)].

The treatment effect for housing values is an order of magnitude larger than the treatment effect for housing credit for banks. This leads to the question of whether the directed credit policy led to an overall appreciation of asset prices in minority concentration areas through general equilibrium effects.[1] We test this by comparing the value of real estate owned by non-minority households across minority concentration and non-minority concentration areas. In the event of an overall increase in property prices, we would expect an appreciation of asset values for non-minority households also. Columns (1)-(4) of Appendix Table F4 shows overall expenditure loans and loans for housing purposes to be unchanged across the discontinuity threshold for non-minority households. However, as seen from column (6), there is a significant (p-value .082) increase in real estate values for non-minority households. The appreciation in real estate values occur, despite noisy evidence in column (5) of a 25 percent reduction in the area of buildings owned (p-value .133). Column (7) shows a weakly significant increase in the overall value of land and buildings owned by non-minority households in minority concentration areas. The treatment effect on overall real estate for non-minorities is almost identical in magnitude to the treatment effect for buildings, implying that the increase in the value of physical assets emanated solely from the increase in building values. Resultantly, the directed credit policy had positive wealth effects for minority households, but also had an indirect spillover on the wealth of non-minority households through the appreciation of local asset prices.

D.4 Additional Labor Market Outcomes

This section explores in greater detail the impact of the directed credit policy on labor market outcomes.

Table D1: 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%

[1] Easy availability of bank credit could have incentivized households to move to minority concentration areas. Alterately, improvements in farm quality through investments in irrigation could have also made minority concentration areas more attractive.

Table D2: 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 D3: 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 D4: 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 D5: Treatment Effect on Weekly Employment by Gender: Extensive Margin

	Panel A: Males							
	(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	.065*	.072***	.202***	-.209***	.130*	-.150**	.045*	-.095*
	(.033)	(.019)	(.041)	(.038)	(.072)	(.077)	(.023)	(.055)
Observations	1705	1705	1705	1705	1705	1705	1705	1705
Dep Var Mean	.861	.081	.394	.386	.142	.631	.166	.347
	Panel B: Females							
	(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	-.043	-.066***	-.002	.025	-.076***	.096***	.063***	-.028*
	(.035)	(.019)	(.029)	(.040)	(.022)	(.036)	(.010)	(.016)
Observations	1672	1672	1672	1672	1672	1672	1672	1672
Dep Var Mean	.149	.017	.077	.056	.047	.085	.043	.034

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%

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

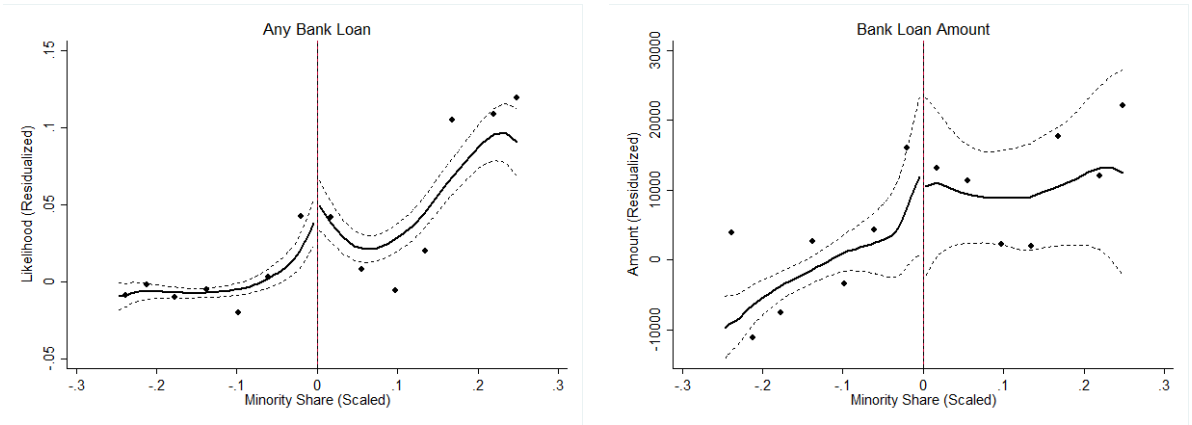
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Hours Worked in			
	Week	Self Employed	Wage Work	Farm	Non Farm	Manufacturing	Trade and Services
Treat	-.267	6.855***	-4.900***	-.541	-.044	2.587***	-.102
	(1.206)	(1.428)	(1.294)	(1.567)	(1.808)	(.954)	(2.049)
Observations	3610	2747	3889	4649	3889	2747	2540
Dep Var Mean	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

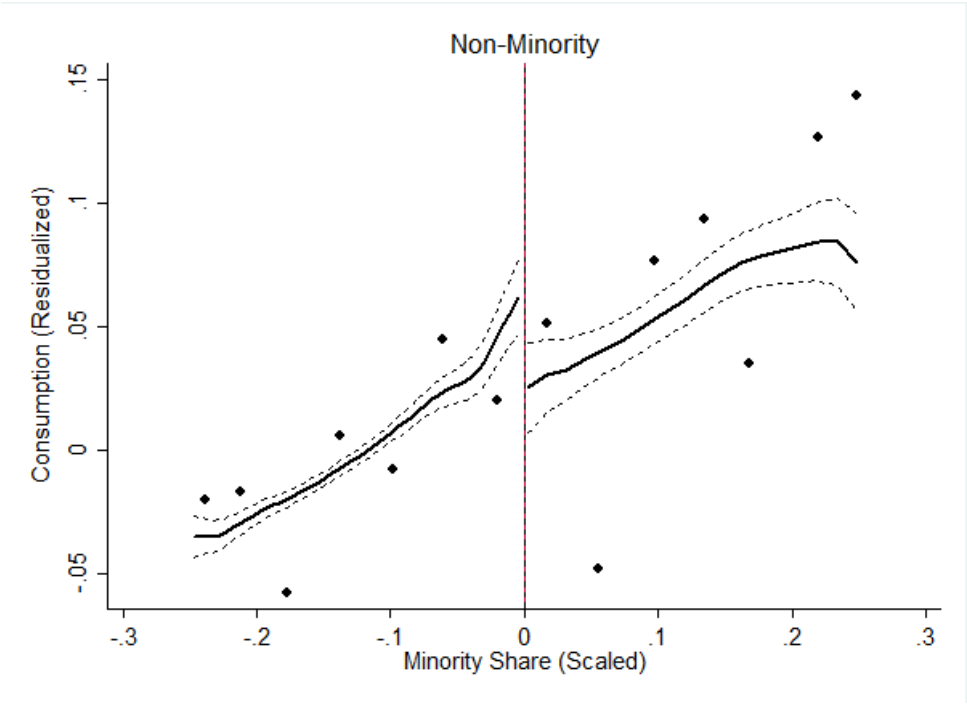
F.1 Figures

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%

Table F4: Treatment Effect on Real Estate and Household Assets: Non-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	-16.900	344313.928*	344301.179*	.019	-122.871	-.064	8458.485	-.058	4436.678
	(11.240)	(1.98e+05)	(2.04e+05)	(.046)	(1147.149)	(.098)	(13273.146)	(.045)	(10672.351)
Observations	9763	9763	9763	9763	9763	9763	9763	6978	9763
Dep Var Mean	63.692	549001.827	1527689.153	.078	2197.101	.720	61334.582	.946	50310.033

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%

G Treatment Effects for Political Outcomes

Table G5: Channels of Electoral Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	# of Voters	Turnout	Right-Wing Party Vote Share	Right-Wing Party Vote Share	Muslim Candidates Vote Share	Muslim Candidates Vote Share
Treat	4.464	.608	-10.992***	2.156	2.482	8.063***
	(8.692)	(2.021)	(2.385)	(4.041)	(2.757)	(1.991)
Observations	1045	1045	465	431	465	431
Dep Var Mean	191.836	69.545	25.633	26.279	2.951	13.718
Median and Above Muslims Population			N	Y	N	Y

This table reports the effects of credit policy on electoral outcomes mentioned in column headers. The first two columns use constituency level data to show effects on the number of voters registered in a constituency, and the turnout elections. The last four columns show the effects on vote shares by restricting sample to constituencies that have either below the median Muslim population share (columns 3 and 5) or above the median (columns 4 and 6). Standard errors clustered at the district level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table G6: Robustness of Channels of Electoral Effects

	(1) Right-Wing Party Vote Share	(2) Right-Wing Party Vote Share	(3) Muslim Candidates Vote Share	(4) Muslim Candidates Vote Share
Treat	-6.952***	1.946	6.530***	12.952***
	(2.294)	(4.542)	(1.243)	(2.029)
Observations	413	378	413	378
Dep Var Mean	24.808	26.418	2.071	14.803
Median and Above Muslims Population	N	Y	N	Y

This table checks if the effects on vote share reported in Table G6 are robust to trimming the sample of assembly constituencies based on the estimated error in population shares. For each district, we estimate Muslims as the share of district population from the constituency level shares and compare with the shares reported in 2001 census. Any district that has estimated share different by more than five percentage points is dropped from the sample for analysis reported in this table. The effects are estimated separately for samples of assembly constituencies that have Muslims' share of the population less than the median of the sample, and for samples that have the share equal or above the median. Standard errors clustered at the district level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table G7: 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 G8: Number of Self-Help Groups

	(1) Exclusively Muslim SHG	(2) Poor Muslim SHG	(3) Mix Muslim SHG	(4) Minority SHG	(5) Exclusively Minority SHG
Treat*Post	46.897**	32.669	118.057*	218.232**	46.966
	(21.915)	(21.779)	(68.070)	(103.311)	(30.559)
Observations	703	703	703	703	703
R^2	.62	.67	.46	.51	.60
Control Mean	4.09	9.15	7.44	22.93	5.72
Trends	Y	Y	Y	Y	Y
Covariates	Y	Y	Y	Y	Y

Table G9: Channels of Electoral Effects

	(1) Right-wing Party Wins	(2) Muslim Candidate Wins	(3) Congress Party Wins	(4) Congress Allies Win	(5) Right-wing Party Wins	(6) Right-wing Party Wins
Treat	-.003	.012	-.034	.019	.003	.030
	(.017)	(.008)	(.031)	(.020)	(.023)	(.019)
Observations	1045	1045	1045	1045	465	431
Dep Var Mean	.052	.002	.044	.047	.044	.060
Median and Above Muslims Population					N	Y

This table reports the effects of credit policy on electoral outcomes on the likelihood of election wins by different parties as mentioned in column headers. The last two columns show the effect on by restricting sample to constituencies that have either below the median Muslim population share (column 5) or equal and above the median (column 6). Standard errors clustered at the district level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table G10: Effects on Hindu-Muslim Violence by Election Timing

	(1) Hindu-Muslim Riots Before Elections	(2) Hindu-Muslim Riots After Elections
Treat	.005	.023***
	(.006)	(.009)
Observations	544	496
Dep Var Mean	.010	.003
Data Source	ACLED	ACLED

This table reports the effect of policy on Hindu-Muslim violence split between before and after elections using data from the Armed Conflict Location and Event Data (ACLED). Each regression includes state and year fixed effects. Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table G11: Is Violence Targeted

	(1) Hindu-Muslim Riots	(2) Hindu-Muslim Riots	(3) Total Riots	(4) Total Riots
Treat	-.001	.137***	-.297	.040
	(.003)	(.036)	(.298)	(.256)
Observations	1169	197	1169	197
Dep Var Mean	.018	.043	1.856	1.394
Median and Above Muslims Population	N	Y	N	Y

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