Equilibrium Effects of "Financial Affirmative Action": Evidence from India

Muhammad Yasir Khan S.K. Ritadhi *

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Abstract

Can government policies relax credit constraints faced by minority citizens and affect their economic well-being? 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 – districts where the share of religious minorities exceeded 25 percent of the district population. Comparing districts within a narrow window around the population threshold in the spirit of a regression discontinuity design, we identify substantial increases in minorities' access to bank credit along both the extensive, and intensive margins. The increase in bank credit is driven by farm and consumption loans, with no evidence of a deterioration in credit quality. Exploring mechanisms, we find banks' collaboration with local self-help groups and a relaxation in collateral requirements to facilitate credit extensions to disadvantaged minority borrowers. Consistent with higher farm credit, we document an increase in farm machinery and irrigated farm holdings for minority households. Examining labor market impacts, we identify positive treatment effects for minority individuals' participation in manufacturing work. In equilibrium, we find financial affirmative action to have boosted per capita monthly household consumption of minority households by 15 percent, accounting for 60 percent of the overall consumption gap between minority and non-minority households.

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1 Introduction

Credit access for households forms a key step towards financial inclusion and inclusive development (Demirguc-Kunt and Singer, 2017). A large body of literature has documented welfare-enhancing aspects of credit access through consumption smoothing, investments in human capital, entrepreneurship, and labour market outcomes (Augsburb et al., 2015; Aydin, 2022; Breza and Kinnan, 2021; Buera et al., 2020; Cramer, 2021; Kaboski and Townsend, 2012). Access to credit, however, is not uniform across individuals and households, with gender and racial discrimination limiting credit market access for under-represented minorities (Blanchflower et al., 2003; Brock and de Haas, 2023; Fisman et al., 2020). In this paper, we ask whether affirmative action in credit allocation can alleviate discrimination against minorities in credit markets, and contribute towards their economic well-being.[1].

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 aimed at holistically improving minority well-being through a number of targeted interventions in upper primary education, hygeine and sanitation, employment generation and protection from targeted violence. The policy also 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.[2] The policy was flexibly designed and no specific targets were provided to lenders in terms of overall lending volumes.[3] 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 marginalized citizen groups.[4] To facilitate compliance with the directed credit policy, the central bank

[1] Affirmative action is a common policy lever used to address societal inequities arising from long-standing discriminatory practices. It has been extensively used to improve minority representation in politics (Pande, 2003; Jensenius, 2015; Bhavnani, 2017; Gulzar et al., 2021) and labor markets (Leonard, 1990; Miller, 2017)

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

[3] 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.

[4] 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.

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.

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). The AIDIS provides detailed information on household balance sheets, and loan-level information for outstanding household loans at the time of survey. In addition to the initial loan amount and amount outstanding, the AIDIS provides information on the source of credit, annual rate of interest, whether the loan was collateralized, and repayments made. This allows us to undertake a comprehensive analysis of the directed credit policy on minorities' access to credit along both the extensive and intensive margins, cost of credit, and repayment behaviour. 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.[5] This descriptive evidence, combined with

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

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 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 .06 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 expectedly 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).[6]

Exploiting detailed information in the AIDIS on household borrowings from non-bank sources, we identify a positive but non-signficant impact of the treatment on total household debt, and borrowings from informal sources. While not precisely estimated, the treatment effects reflect a 54 percent increase in informal borrowings, and a 30 percent increase in aggregate household debt. Related evidence points to a reallocation of minority borrowing away from non-bank financial institutions in response to increased access to bank credit. Disaggregating credit obtained from informal sources across professional

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

money lenders and community networks, we find religious minorities to have a significantly higher likelihood of having outstanding loans from both banks and community sources such as friends and relatives. Combined with the suggestive evidence of higher aggregate borrowings, our results are indicative of binding credit constraints faced by minority households. If credit constraints were not binding, we would have expected to identify a null effect of the treatment on overall household debt, and no increase in the likelihood of having loans from both bank and informal sources (Banerjee and Duflo, 2014).

We find little impact of the policy on bank credit access for non-minority households with the point estimates being positive, albeit statistically non-significant. This rules out concerns that financial affirmative action came at the cost of reduced credit access for non-minorities. Akin to minority households, there is however evidence of reallocation of credit across non-bank sources: 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. Taken together with the results for minority households, this suggests that bank credit access for minority households allowed non-minority households to substitute informal borrowings with loans from non-bank financial institutions.

Using self-reported information on households' purpose of borrowing, we show that bank credit expansion in minority concentration districts was primarily to finance farm and consumption loans. About a third of the increase in bank credit for minorities in treatment areas comprised of farm loans, while consumption loans accounted for the remainder. Along the extensive margin, we also find evidence of higher lending for human capital investments – namely bank loans taken for the purpose of health and education. There is however no evidence of increased bank lending to minorities for non-farm businesses. Self-reported information on loan repayments also show no evidence of a decline in overall credit quality, or higher delinquency for bank loans. This rules out concerns that financial affirmative action was accompanied by higher delinquencies due to mandated credit extensions to riskier borrowers.

We consider 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. If information frictions

hindered bank lending to minorities and SHGs had superior mechanisms for screening and monitoring (Banerjee and Duflo, 2010), collaboration with SHGs could have improved bank lending to creditworthy minority borrowers. 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. There is however no evidence of higher lending through bank-linked SHGs for non-minority borrowers, ruling out a secular expansion of bank lending to SHGs in minority concentration areas.

Next we explore the role of collateral. Pre-treatment descriptives showed land and real estate holdings of minority households to be 30 percent lower than non-minority households. If the lack of collateral served as a binding constraint for minority borrowing, banks could have opted to relax collateral requirements for minority borrowers. Indeed, we find a sizeable positive treatment effect on the likelihood of minority households to have an unsecured bank loan. This holds for both loans received through bank-linked SHGs, and loans obtained directly from commercial banks. Fisman et al. (2017) contends a relaxation in collateral requirements to signify an overall improvement in lenders' ability to effectively monitor and screen borrowers. An increase in collateral free lending can thereby be indicative of increased efforts by lenders to collect information about minority borrowers, in order to facilitate lending to these previously excluded groups. This would also be consistent with the absence of a worsening in credit quality in response to the directed credit policy. No comparable treatment effect however is detected for non-minority borrowers, ruling out holistic improvements in banks' information acquisition abilities in minority concentration areas.

The final channel explored is interest rates. We find limited evidence of banks reducing interest rates in response to the directed credit policy in minority concentration areas. 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). There is also no corresponding effect on the cost of borrowing for non-minorities, assuaging concerns that financial affirmative action was being undertaken through the cross-subsidization of non-minority borrowers.

Considering the aggregate effects of the directed credit policy, we first explore its impact on productive assets owned by households. In line with the increase in farm credit, we identify a positive and significant increase in households' ownership of farm machinery along both the extensive and intensive margins. This was accompanied by increased ownership of transport owned for farm purposes,

and increased values of tractors. The results point to the mechanization of farm activities in response to farm credit from banks. While overall farm holdings of minority households remained comparable across treated and control areas, we identify an increase in minority households' holdings of irrigated farm land. Broadly, the empirical results suggest that access to bank farm loans allowed minority households to engage in productive investments in farm inputs, which have the potential to yield long-term returns.

We use weekly data on labour market activities from the Primary Labour Force Survey (PLFS) undertaken in 2017 to identify the labour market impacts of the directed credit policy. While overall labour force participation, unemployment and hours worked per week remained unaffected, our paper identifies a sizeable increase in minority individuals' likelihood of engaging in manufacturing activities. Relative to 5.5 hours of manufacturing work in non-minority concentration districts, minority individuals allocated 3 additional hours to manufacturing work in minority concentration areas. This is accompanied by a corresponding reduction in time spent in trade and service activities, pointing to a reallocation of non-farm labour in response to the directed credit policy. There is no evidence of a reduction in labour supply to farm work along either the extensive or intensive margins, ruling out explanations that investments in labour saving farm technology pushed labour out from farm work (Bustos et al., 2016). The evidence instead points to an aggregate demand channel (Breza and Kinan, 2021), leading to higher demand for manufactured commodities.

We conclude our empirical analysis by identifying the impact of the directed credit policy on consumption expenditures, as a broad measure of household well-being. Relative to minority households in control districts, we identify minority households in treated districts to have 15 percent higher monthly per capita consumption, with the coefficient being significant at the 1% level. 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.

This paper makes three significant contributions to the study of affirmative action for underprivileged minority communities. First, it provides evidence of the effectiveness of an at-scale implementation of affirmative action in financial markets to address concerns about discrimination. While affirmative action has been extensively studied in labor (Leonard, 1990; Holzer and Neumark, 2000), 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.[7] Through this contribution, the paper takes forward the nascent literature on the accessibility of credit markets 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. This is a significant contribution as it provides evidence for the effectiveness of an actionable policy implemented at scale.

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). Addressing this question in financial markets, our paper makes a second contribution by showing that affirmative action policies are not necessarily accompanied by adverse effects for non-beneficiaries. Instead, our findings show how the expansion in credit facilities to minorities increase households' productive assets, manufacturing employment, and overall well-being through higher household consumption. Critically, the 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.

A third contribution of the paper is to shed light on how affirmative action policies can avoid

[7] 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).

negative consequences. A common argument for why policies intended to benefit under-represented minorities may hurt them is the presence of a "quality and fit trade-off" which is often ignored by policymakers (see, for example, a review by Arcidiacono and Lovenheim (2016) in the context of "mismatch" between students and law schools). This expected trade-off arises from a lack of information on the preferences of minorities, and one manner of addressing them is to exploit existing informal institutions that can aggregate information about minorities. In the context of India, our paper shows that banks' collaboration with local self-help groups is an effective tool in expanding credit to informationally opaque minority borrowers, without any accompanying deterioration in borrower quality.

Fourth, and finally, by offering suggestive evidence that minority households faced binding credit constraints, we also add to the large literature studying the economic impacts of credit access for credit-constrained households (Augsburb et al., 2015; Banerjee et al., 2015; Carlan and Zinman, 2011; Kaboski and Townsend, 2012). Our paper shows that targeted credit through the banking channel to religious minorities not only improves credit access along the extensive and intensive margins, but also investments in farm machinery (Field et al., 2013), and affects labor market choices of the target population. Improvements in credit access ultimately result in large increases in household consumption, equivalent to 60 percent of the consumption gap between minority and non-minority households. Consequently, our results underline the transformative role of affirmative action in bank credit for disadvantaged households.

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.[8] 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, 2007).[9] 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.[10]

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

[9] 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.

[10] 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.

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).[11] 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.[12] 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 congregations (RBI, 2007). The RBI also recommended banks to engage with self-help groups to improve their selection of underprivileged minority borrowers, and lead banks in minority concentration districts were expected to be "proactive" in this regard (RBI, 2007). Both the lead bank, and individual banks operating in minority concentration districts were advised to impart adequate training to sensitize employees to the credit needs of minority borrowers. Finally, banks were mandated to file half-yearly reports with both the RBI and the federal Ministry of Welfare, detailing the disbursement of credit to minority borrowers. This bi-annual reporting forms the sole tangible monitoring of the policy undertaken by the central bank (RBI, 2007).

2.3 Incentives for Banks

The primary incentive for banks to comply with the directed credit policy is that lending to minority borrowers would allow them to meet key regulatory targets. First, every bank operating in India is

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

[12] 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.

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).[13] 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.[14] 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 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

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

[14] 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.

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.[15] 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.

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

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%).[16]

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.[17] 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%).

3.2 Primary Labour Force Survey

We use data from the Primary Labour Force Survey to identify whether directed credit for religious minorities affected labour market outcomes. The PLFS is a nationally representative survey conducted by the NSS and provides data on labour force characteristics. It is the successor to the quinquennial employment-unemployment surveys conducted by the NSS between 1987 and 2011. We use weekly data from the PLFS which inquires individuals for their labour force participation in each of the 7 days preceding the survey. Conditional on participation, the PLFS also provides detailed information on the type of activity undertaken, as well as the hours of work accorded to that activity in each of the seven days. The PLFS provides locational identifiers at the level of district, in addition to information on individual age, educational attainment, caste and religion.

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

[17] 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.

Summary statistics of weekly labour market activities are shown in Appendix Tables C2.A and C2.B. Religious minorities comprised about 15% of working-aged individuals, and 70% of working-aged individuals were in rural areas. Less than 40% of working-aged individuals had completed secondary or higher education, and under 15% of working-aged individuals had a college degree. Overall weekly labour force participation was 55%. Conditional on participation in the labour force, individuals worked 46 hours a week, with almost 40% of the workers engaged in farm activities. 30% of the workers were engaged in trade or service activities, with manufacturing and construction work accounting for approximately 10% of workers. 12% of weekly non-farm workers reported being self-employed, and out of them, 50% worked from establishments located within their home. Amongst manufacturing workers, a third were self-employed, and three-fourths of self-employed manufacturing workers operated establishments located within their residence.

3.3 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 castes, Muslims, and other religious minorities.[18] 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

[18] 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.

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 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). We define the running variable 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 1 shows the distribution of $ShMinority_{ds}$, with the broken vertical line representing the 0.25 threshold, while Appendix Figure C1 shows that minority concentration districts are geographically distributed across eastern, northern, southern, and western India. Using $Runvar_{ds}$ from equation (1), the district-level treatment indicator – $Treat_{ds}$ is constructed to equal 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 assigned to treatment (RBI, 2007). Formally, Figure 2 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). Our specifications include state fixed effects (α), which restrict the comparison of outcomes for households located within the same state. Select household covariates such as rural or urban location, households' caste category, and whether the household has a secondary-educated member, are included in the covariate vector $\mathbf{X}$. 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. Resultantly, we estimate specification (2) after restricting the sample to 63 districts located within a bandwidth of 0.06 (share of minority population) on either side of the discontinuity threshold of 0.25. This ensures a fixed sample of districts and households being used for the empirical analysis. We however verify the robustness of our baseline results to outcome-specific MSERD bandwidths recommended by Calonico et al. (2020), as well as a host of alternate bandwidths between .04 and .09.

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

data from the AIDIS conducted in 2003. Appendix Tables B1-B6 report a statistically significant difference for only 3 of the 46 pre-treatment household characteristics. Appendix Table B2 show minority households across minority and non-minority concentration districts had comparable levels of landholdings, real estate, bank deposits and consumption. Appendix Table B4 confirms the absence of any significant differences in minority households' access to overall and bank credit across treated and control districts in the pre-treatment period. Finally, Appendix Table B7 uses data from the Basic Statistical Returns in 2001 to document that pre-treatment district financial infrastructure, financial inclusion, and credit disbursement were also comparable across minority and non-minority concentration districts.

The absence of selective sorting of districts into treatment and control status (Figure 2), combined with the overall balance of pre-treatment household and district characteristics across treated and control districts (Appendix Tables B1-B7) allow us to assign a causal interpretation to the RD coefficients estimated using equation (2).

5 Results

This section presents the key findings of our paper. 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 credit policy on household productive assets, labour market outcomes, and consumption.

5.1 Access to Credit in Minority Concentration Districts

5.1.1 Baseline Results

Figure 3 graphically compares the impact of the directed credit policy on minorities' access to credit across treated and control regions. All specifications include state fixed effects and household covariates. The sample is restricted to households in 63 districts located within our preferred bandwidth of 0.06. The horizontal axis shows the running variable, and each point in the figures show the outcome variable mean corresponding to each bin. Observations are weighted using a triangular kernel and the solid lines depict the linear fit from a local linear regression. The left panel of Figure 3 shows a sharp jump in bank credit access at the discontinuity threshold for minority households. There is also evidence of a

smaller jump in the right panel for the amount of bank credit (initial loan size) obtained by minorities.

Table 1 shows local linear regression estimates corresponding to Figure 3. Column (1) includes no other covariates with the exception of state fixed effects and identifies a positive and significant impact of the treatment on minority households' access to bank credit. Specifically, relative to observationally equivalent minority households in non-minority concentration districts, minority households in minority concentration districts are 11 percentage points more likely to have an outstanding bank loan. The corresponding treatment effect along the intensive margin in column (4) amounts to an INR 18000 increase in the value of bank loans, with both coefficients being significant at the 1% level. Columns (2) and (5) add household covariates, while columns (3) and (6) consider a quadratic polynomial in the running variable. The inclusion of household covariates causes a moderate increase in the magnitude of the extensive margin coefficient [column (2)], and a small reduction in the intensive margin coefficient. Replacing the linear polynomial in the running variable with a quadratic polynomial results in a doubling of the estimated treatment effect along both the extensive and intensive margins. The precision of the point estimates remain unaffected across both specification choices.

The point estimates are large in magnitude when compared to the outcome variable mean in observationally equivalent control districts. Approximately 11 percent of minority Muslim households had some outstanding bank loan in non-minority concentration districts, while the corresponding average bank loan amount equaled INR 18334. Considering our preferred specification in column (2) [column (5)] which includes state fixed effects, household covariates, and a linear polynomial in the running variable, the policy resulted in a near doubling in credit access for minority households along both the extensive and 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 0.3 million Muslim households in treated districts. Since the AIDIS data only collects information on outstanding loans, it is likely that the treatment effects capture a lower bound of the directed credit policy's impact on bank credit access for minorities.

In addition to bank credit, the AIDIS 2019 survey 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 bank credit is 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.[19] Alternately, if minority households faced binding credit constraints, access to cheaper sources of credit should result in an expansion in aggregate household debt for minority households in treated districts (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 institutional credit from non-bank sources.[20] While not always precisely estimated, the point estimates in Appendix Table C4 point to a reallocation in household borrowing across credit sources. First, column (1) shows that overall access to credit along the extensive margin is unaffected by the policy. 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. Column (3) points to a significant reduction in minority households' borrowing from non-bank financial institutions: minority households are 16 percentage points less likely to have an outstanding loan from these institutions. The statistically non-significant coefficient corresponding to informal borrowings [column (4)] rules out that the treatment allowed minority households to exit the informal credit market. Column (5) points to a 30 percent increase in aggregate household debt, although the coefficient is not significant at any of the conventional levels of significance. The increase in total borrowings is driven by a statistically significant increase in bank borrowings [column (6)], and a statistically non-significant 54 percent increase in informal borrowings [column (8)]. Similar to the extensive margin reduction in minority borrowings from non-bank financial institutions, we identify a INR 12600 decline in borrowings from these sources along the intensive margin (p-value .082). Overall, Appendix Table C4 points to an increase in household debt for minority households along the intensive margin (but not the extensive margin), which would be consistent with these households facing binding credit constraints.

Appendix Table C5 further disaggregates informal loans into those from professional money lenders and input suppliers, and community networks, such as friends, relatives and landlords.[21] As seen from the pre-treatment descriptives in Appendix Figure A1, the former represents relatively expensive sources of informal borrowings, while the latter are typically interest free loans.[22] Again, while the

[19] 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.

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

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

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

point estimates are noisy, columns (2) and (7) offers suggestive evidence that the increase in informal borrowing emanated through credit expansions from community networks.

We directly test for credit constraints in columns (3)-(5) by identifying whether households had both a bank loan, and a loan from informal sources. Column (3) shows a 3 percentage point increase in minority households' likelihood of having both a bank loan, and a loan from informal sources (p-value .081). This is driven by a near doubling in households' likelihood of having a loan from both banks, and community networks [column (5)]. The point estimate is also statistically significant at the 5% margin (p-value .048). Collectively, Appendix Tables C4 and C5 indicate that minority households were 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.

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 near doubling in minority households' likelihood of having a long-term bank loan. There is however a corresponding reduction in the likelihood of having a long-term loan from non-bank financial institutions [column (2)]. This again points to the substitution of credit from non-bank institutional sources with bank credit, by beneficiaries of the directed credit policy. The short-term nature of micro-credit, combined with the onset of fortnightly repayments, has been shown to serve as a major disincentive for micro-credit recipients to undertake productive investments (Field et al., 2013). Consequently, 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 investments yielding long-term returns.

5.1.2 Robustness

Appendix Table C3 shows robustness of our baseline treatment effects to alternate bandwidths and specification choices. Columns (1)-(2) and (6)-(7) show robustness to using data-driven outcome-specific MSERD optimal bandwidths recommended by Calonico et al. (2020). The optimal MSERD bandwidth of .045 is smaller than our preferred bandwidth of .06 using a linear polynomial, but of comparable magnitude when considering a quadratic polynomial (.066). The treatment effects if anything are larger when we use the MSERD outcome-specific optimal bandwidths. We extend this analysis in Figure 4 where we use coefficient plots to depict the stability of the baseline results to 10 alternate bandwidths.

The first coefficient plotted in each panel corresponds to the bandwidth of 0.04, and we re-estimate each successive specification after incrementally increasing the bandwidth by 0.005. The coefficients are stable to each of the 10 alternate bandwidths, alleviating concerns that the treatment effects were emanating from a select set of treatment and control districts. Appendix D replicates a number of our key results using outcome-specific MSERD bandwidths.

As treatment assignment varies at the level of district, our specifications include state fixed effects but not district fixed effects. Columns (3) and (8) show that the baseline findings are robust to the inclusion of select pre-treatment district covariates.[23] This in essence serves as an additional validation of the RD design: if treated and control districts are balanced within a narrow window of the discontinuity threshold, the inclusion of district covariates should not affect the point estimates. Reassuringly, the coefficient estimates in columns (3) and (8) are very comparable to the baseline estimates in columns (2) and (5) of Table 1. Columns (4) and (9) expand the sample to include the remaining two major religious minority denominations – Christians and Sikhs – and identify very similar treatment effects.

Columns (5) and (10) uses a fuzzy RD specification to account for the non-compliance in treatment assignment in 18 additional districts. As there was no reason offered on why these districts were classified as minority concentration, we exclude them from our main specifications. Including all three religious minorities, the extensive margin treatment effect using the fuzzy RD specification is substantially larger and statistically significant at the 1% level. The intensive margin coefficient is statistically non-significant (p-value .254), but positive and very similar in magnitude to the treatment effect identified in column (9). The first stage of the fuzzy RD specification is also positive, and statistically significant at the 1% level.

5.1.3 Credit Access for Non-Minorities

We now compare the impact of the directed credit policy on access to bank credit for non-minority households across treated and control districts. This informs us of the aggregate consequences of the directed credit policy. 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

[23] We include pre-treatment district-level controls for levels of urbanization, monthly per capita household consumption, share of households with access to banking services, share of households reporting no durable assets, district labour force participation, share of farm and manufacturing workers, and district bank branch density.

credit from non-minority borrowers.

Visually, Appendix Figure E1 does not suggest a reduction in bank credit for non-minority households across the discontinuity threshold. Estimates from local linear regressions in columns (1) and (5) of Appendix Table E1 confirm this: while the point estimates are positive, they are not statistically significant at any of the conventional levels of significance. This ameliorates concerns that financial affirmative action for religious minorities resulted in the crowding out of non-minorities from bank credit.

Columns (5) and (8) of Appendix Table E1 find no effect of the treatment on overall credit access for non-minorities. There is however evidence of a reallocation of credit from non-institutional to institutional sources for non-minority households: non-minority households in minority concentration districts report lower informal borrowings [columns (2) and (7)], but increased credit access from non-bank financial institutions. Along the intensive margin, the average increase in non-minority households' borrowing from non-bank financial institutions is equivalent to 70 percent of the decline in minority households' borrowings from these sources. When compared to Appendix Table C4, the coefficients suggest that access to bank credit through the directed credit policy led minority households to substitute non-bank institutional borrowings with commercial bank credit. In response, non-minority households substituted informal borrowings with loans from these sources.

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 later 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, Appendix Table E2 alleviates concerns that the expansion in minority bank credit access came through the crowding out of *Dalit* and *Adivasi* borrowers. Although the intensive margin point estimate in column (5) is negative, it is not statistically significant. Along the extensive margin [column (1)], the point estimate is positive, large, but not statistically significant. 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, Appendix Table E2 does not suggest that banks complied with the directed credit policy by reallocating credit across communities qualifying as "weaker sections."

5.1.4 Purpose of Borrowing

The AIDIS provides self-reported information on households' purpose of borrowing. We aggregate these to construct 3 broad categories of bank borrowing: farm credit, credit for non-farm businesses, and loans for household expenditures. The latter cover borrowings for education, health, housing, and other consumption purposes. Figure 5 visually depicts the treatment effects for minority households. The top row shows treatment effects along the extensive margin; the bottom row presents treatment effects along the intensive margin. There is evidence of modest positive treatment effects for farm and expenditure loans from banks. 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 along both the extensive, and intensive margins. The INR 6341 (11973) increase in bank loans for farm (expenditure) purposes is substantial, when considering that the corresponding control group means equaled INR 3910 (9168). Appendix Table C7 disaggregates expenditure loans into its three major constituents. Columns (1)-(3) identify increased bank credit access for all three expenditure loan categories: health and education, housing, and consumption. The coefficients are both statistically and economically significant. Along the intensive margin though, the increase in bank expenditure loans is driven exclusively by loans obtained for the purpose of housing. Housing loans include both loans obtained to purchase new residential properties, or augment existing properties. As both farm loans, and loans for housing purposes qualify under priority sector lending, these findings are consistent with banks' incentive to use the directed credit policy to meet their regulatory targets.

5.1.5 Credit Quality

We use self-reported data on borrowers' repayment behaviour to assess the impact of the directed credit policy on loan delinquency. 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 to be delinquent if it reported

delinquency for any of its outstanding loans. 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. While the point estimate is positive, the confidence intervals are wide enough to rule out a null effect. This assuages concerns that the expansion in minority lending came at the cost of credit quality. Columns (2) and (3) confirm that the absence of an increase in bank loan delinquency for minority households was not due to increased loan delinquencies for loans obtained from non-bank sources.

5.2 Mechanisms

Section 5.1 identified an expansion 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 pertaining to the directed credit policy for religious minorities urged lenders to collaborate with local self-help groups to extend credit to 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.

Table 3 disaggregates the source of bank credit to separately identify the treatment effect on direct bank lending to minority households, and bank lending through SHGs. While the RD coefficients are positive and significant for both channels, the treatment effects in relative terms are significantly larger for bank-linked SHGs. Thus, while under 3 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 almost 10 percentage points higher. When compared to the treatment effects in Table 3, the treatment effects in Table 1 suggests that up to 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 E3 identifies no impact of higher lending to non-minorities through bank-linked SHGs. The extensive margin coefficient is small and attenuated towards 0, while the intensive margin coefficient is negative, and not statistically 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. Columns (1)-(3) of Table 4 examines this by identifying the treatment effect on the likelihood of households obtaining an unsecured bank loan. Column (1) reports an 11 percentage point higher likelihood of minority households receiving an unsecured bank loan in treated districts, relative to a control district mean of about 4 percent. 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 E4 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 offered lower rates of interest to borrowers from religious minority groups 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 reductions to the cost of bank credit in treated districts: the point estimate is negative and attenuated towards 0. There is noisy evidence in column (5) of a 1.7 percentage point reduction in lending rates for loans issued by commercial banks (p-value .157). On the contrary, column (6) points to a 2 percentage point increase in the interest rate charged on bank-SHG loans, which again is imprecisely estimated (p-value .193). This indicates 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 E4, column (5)], suggesting that any reduction in lending rates for minority borrowers did not emanate through an overall reduction in lending rates in treated districts. This also rules out concerns about lenders cross-subsidizing minority borrowers by charging higher rates to non-minority borrowers. Surprisingly, we find a reduction in lending rates from bank-linked SHGs to non-minority households [column (6)]. The reduced lending rates however did not result in increased access to credit from this source for non-minority households.

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 also suggestive evidence of a relaxation in lending rates for loans offered directly by banks to minority borrowers. This can be through the improved selection of minority borrowers in treated districts, which would be consistent with lower collateral requirements and the absence of a deterioration in credit quality. Alternatively, it can also be an initiative by banks to reach out to minority borrowers if they believed high repayment burdens to have acted as a binding constraint for these households in accessing bank credit. However, increased borrowing from bank-linked SHGs by minority households in treated districts, despite evidence indicating higher lending rates, suggests that the cost of credit was not the primary barrier for minority borrowers in accessing bank credit.

6 Financial Affirmative Action, Household Assets, Labour Market Outcomes and Minority Well-Being

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

6.1 Productive Assets

The AIDIS provides information on households' ownership of farm and non-farm business assets. We use this information to identify whether the expansion in bank lending in minority concentration areas led to an increase in households' ownership of productive assets. In line with the increase in bank loans for farm activities, column (1) of Appendix Table C9 identifies a large, positive and significant coefficient on households' likelihood of owning farm machinery. Considering that 34 percent of Muslim households in control districts had some farm machinery, the treatment effect is equivalent to a 75 percent increase in households' likelihood of owning some farm machinery. The intensive margin

treatment effect in column (3) suggests a INR 2600 increase in the value of farm machinery owned by the average minority household in treated districts. Alike the null effect of the directed credit policy on business loans from banks to religious minorities, we identify a null effect of the treatment on non-farm machinery.

Along with the positive treatment effect for farm machinery, Appendix Table C10 identifies a higher likelihood of transport ownership for farm purposes amongst minorities in treated districts. Panel B shows this to be driven by an increase in the value of tractors owned by households, although there is no corresponding effect along the extensive margin. Overall valuation of transport equipment owned by minorities in treated districts also increase, but the point estimate is noisy (p-value .121). Collectively, the increase in farm machinery and tractors points to the mechanization of agriculture in treated districts amongst minority households.

6.2 Land and Real Estate

We next identify the treatment's impact on household land holdings and real estate. Section 5.1.4 showed Muslim households in treated districts to have higher bank loans for farm purposes, while Section 6.2 showed higher farm machinery owned by these households. In addition to investing in farm machinery, farm loan recipients could have opted to use bank credit to expand on their land holdings, or undertake 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.

Columns (1)-(3) of Appendix Table C11 shows that while aggregate farm holdings were unaffected by the directed credit policy, minority households in minority concentration districts had higher holdings of irrigated farmland [column (1), p-value .074]. Alike Section 6.1, this again points to productive investments made by disadvantaged borrowers with the potential to yield long-term returns. There is also noisy evidence of increased overall land holdings by these households [column (3), p-value .09].

Column (5) shows a near 50 percent increase in the value of real estate owned by minority households in minority concentration districts, driven almost entirely by higher valuations of residential real estate [columns (4)]. The point estimates in both instances are also statistically significant at the 5 percent level or better. There are two possible channels through which the directed credit policy could have affected real estate values. First, consistent with the increase in bank housing loans in

Appendix Table C7, minority households could have applied for housing loans and purchased homes of higher valuation. In that case, we should observe a positive treatment effect on real estate values only for religious minorities. Relatedly, bank housing loans could have been utilized to improve the quality of existing residential properties, leading to an appreciation in residential prices. Alternately, the increased availability of bank credit in minority concentration areas could have led to an overall appreciation in local asset prices through general equilibrium effects. If the latter channel is active, we would expect an appreciation of asset prices for non-minority households also.

Columns (7) and (8) offer evidence supporting the latter explanation: while the treatment effects for non-minority households are significant only at the 10% level (p-values of .078 and .089 respectively), the point estimates are comparable in magnitude to those obtained for Muslim households in columns (5) and (6).[24] In general, the intensive margin increase in bank housing loans equals about 5 percent of the appreciation in minority residential real estate values, making it unlikely that housing loans from banks could have fully accounted for this increase. The results instead suggests that minority concentration districts became more attractive residential locations following the directed credit policy.

6.3 Labour Market Outcomes

A large literature has explored the labour market impacts of credit access for households. Recent work by Breza and Kinan (2021) showed adverse labour market impacts in response to a reduction in lending by micro-finance institutions in India, both through its direct impact on business operations, and indirect effects through lower aggregate demand. Using the quasi-exogenous rollout of state-owned banks in Brazil, Fonseca and Matray (2022) shows financial deepening to widen wage inequality across workers. We examine in this regard the impact of the increased credit access on labour market outcomes for minority households.

At the outset, we list two potential channels through which the directed credit policy could have altered labour choices of minority workers in minority concentration areas. First, the increase in farm credit could have directly affected farm labour. However, as seen from Sections 6.1 and 6.2, there is evidence of increased mechanization of farm work through investments in irrigation and farm machinery, but no accompanying increase in land holdings. If households used farm credit from banks to invest in

[24] As property values of non-minority households were larger in control districts, the treatment effects in percentage terms are smaller for these households.

labour saving technology, it could have freed labour for non-farm activities – the labour push channel, seen in Bustos et al. (2016). Alternately, the mechanization of agriculture could have improved farm yields, leading to higher local aggregate demand. This in turn could have generated higher employment in local manufacturing and services to meet the additional demand for goods and services – the labour pull channel, seen in Emerick (2018). Relatedly, the increase in consumption loans from banks could also have increased local demand for goods and services, generating higher labour demand in non-farm sectors.

We use weekly employment data from the PLFS to identify the impact of the directed credit policy on labour market outcomes. Figures 6 and 7 graphically show the treatment effects. There is little impact on overall participation in the labour force, hours worked during the week or unemployment. Instead, there is evidence of a strong positive impact on manufacturing employment along both the extensive and intensive margins. There is also a corresponding upward jump at the discontinuity threshold for farm work, especially along the extensive margin.

Tables 5 and 6 present local linear regressions corresponding to Figures 6 and 7. The unit of observation is the individual, and we restrict the sample to individuals aged between 18 and 60. To account for seasonality in labour market activities, we include survey month fixed effects. Aside from that, the empirical strategy is similar to specification (2). 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 .06 around the discontinuity threshold.

The results in Tables 5 and 6 are in line with Figures 6 and 7. We detect a positive but imprecise impact of the treatment on the likelihood of farm work (p-value .149). There is a positive and statistically significant impact of the treatment on manufacturing work along both the extensive and intensive margins. Minority working-aged individuals are 6 percentage points more likely to be engaged in manufacturing work (relative to the control district mean of 10 percent), amounting to 3 additional hours of work per week (control district mean is 5.6 hours). Column (4) of Table 7 identifies a corresponding reduction in labour hours accorded to trade and service activities, the magnitude of which is very similar to the intensive margin increase in manufacturing work. Across Tables 5 and 6, the coefficients point to a reallocation of non-farm labour from trade, services and construction, to manufacturing activities. Consequently, there is little evidence to suggest that the mechanization

of farm work led to labour being pushed out from farm to non-farm activities. Instead, Tables 5 and 6 suggests higher aggregate demand as the likely channel driving the increase in manufacturing employment.

6.4 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 showed expenditure loans from banks to minority households, which could have directly increasde 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. The increased participation of minority workers in manufacturing work could also have increased household consumption if manufacturing activities offered higher returns to labour (Section 6.3). In contrast, Augsburb et al. (2015) offers a careful explanation detailing 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.

The left panel of Figure 8 shows a jump in monthly per capita consumption for minority households located to the right of the discontinuity threshold. Column (1) of Table 7 confirms this by identifying a positive treatment effect, significant at the 1% level. As the outcome of interest is logged, the coefficient identifies a 15 percent increase in monthly per capita consumption for minority households in minority concentration districts. Relative to the average consumption level in control districts, the coefficient reflects a INR 441 increase in per capita monthly household consumption. Multiplying by average household size and annualizing, the coefficient amounts to a INR 26,330 increase in aggregate household consumption. A back of the envelope calculation using the coefficient estimate in column (6) of Table 2 indicates that about 45 percent of the increase in aggregate annual household consumption in treated districts can be explained through higher expenditure loans from banks.

Column (2) of Table 7 extends the sample to Christian and Sikh households, and finds slightly smaller treatment effects, significant at the 10% level (p-value .066). Consistent with the right panel of

Figure 8, there is no impact of the directed credit policy on household consumption for non-minorities.

Section 3.3 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 running the following pooled specification, combining minority and non-minority households:

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

The outcome of interest in specification (3) is logged monthly per capita consumption of household h, residing in district d, 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 .06 around the discontinuity threshold. The coefficient estimates are weighted using NSS assigned household weights, and standard errors are clustered by district for inference.

Column (4) of Table 7 identifies a positive but statistically non-significant coefficient corresponding to β_2 (p-value .180). In column (5), we re-estimate specification (3) after replacing state fixed effects with district fixed effects. The use of district fixed effects restrict 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 with this specification. The inclusion of district fixed effects in column (6) causes a slight increase in the magnitude of β_2 estimating the differential treatment effect, and the point estimate is now statistically significant at the 10% level (p-value .083).

The point estimate in column (6) corresponding to β_2 implies a 16 percent increase in monthly per capita household consumption for minority 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 25 percent lower than non-minority households in non-minority concentration districts. Consequently, the directed credit policy resulted in a 60 percent reduction in the consumption gap between minority and non-minority households. Columns (6) and (7) extends the sample to include the remaining two major religious minority groups – Christians and Sikhs – and report very similar findings. Relative to non-minority households, minority households in minority concentration districts reported 18 percent higher monthly per capita consumption. This is equal to about 80 percent of the consumption gap between minority and non-minority households in non-minority concentration districts. Consequently, Table 7 highlights the ability of financial affirmative action to substantially reduce long-standing inequities for disadvantaged religious minorities and promoting overall household well-being.

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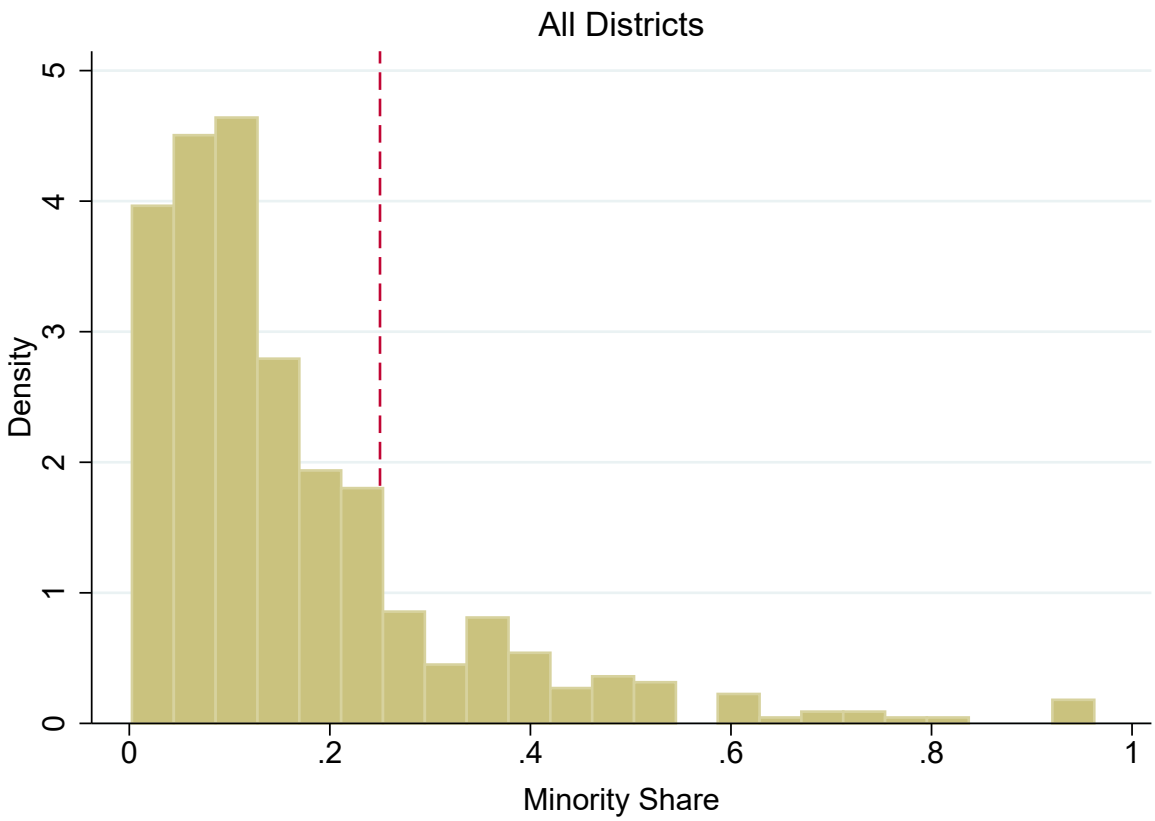
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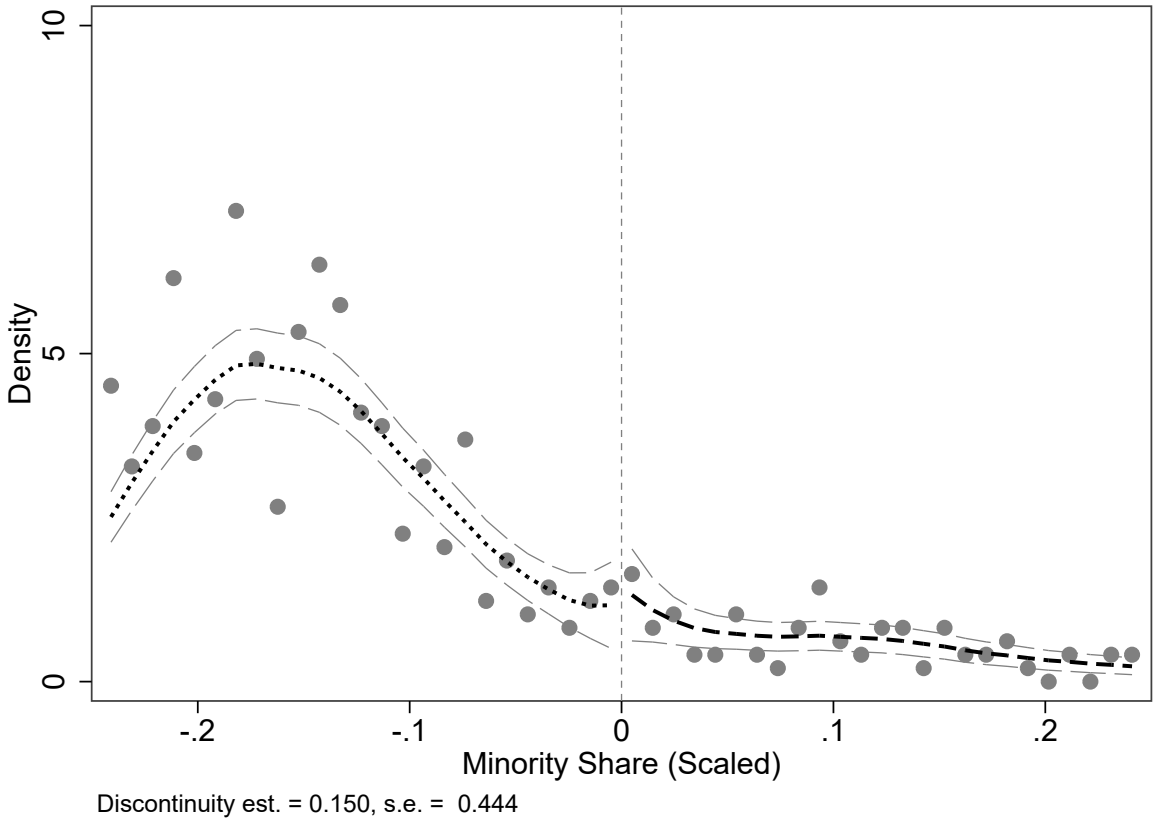
7 Figures

Figure 1: Distribution of Running Variable



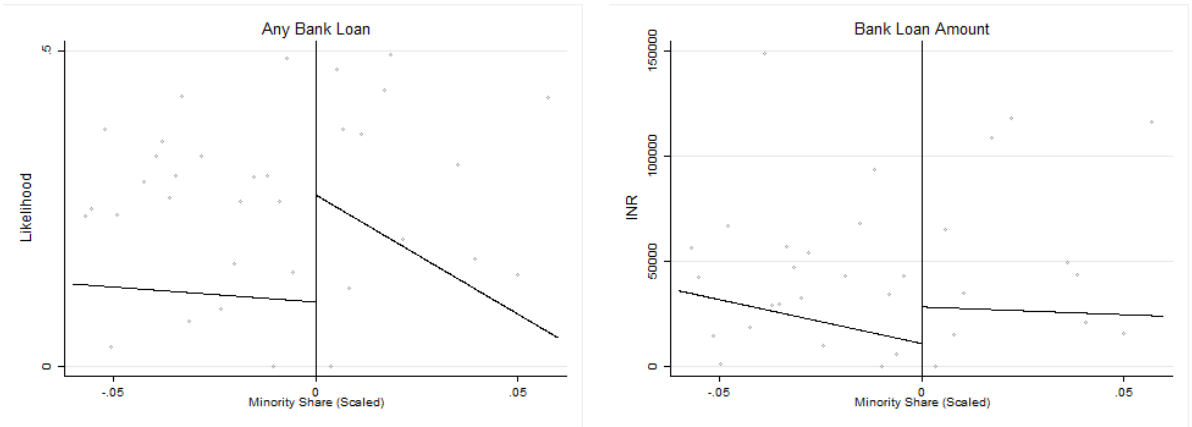
Notes: This figure shows the distribution of the share of religious minorities across districts in India. Religious minorities include the religious denominations Muslims, Christians, Sikhs, Buddhists and Zoarastrians, classified as religious minorities in the Census 2001. The population shares are computed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

Figure 2: Smoothness in Treatment Assignment at Discontinuity Threshold



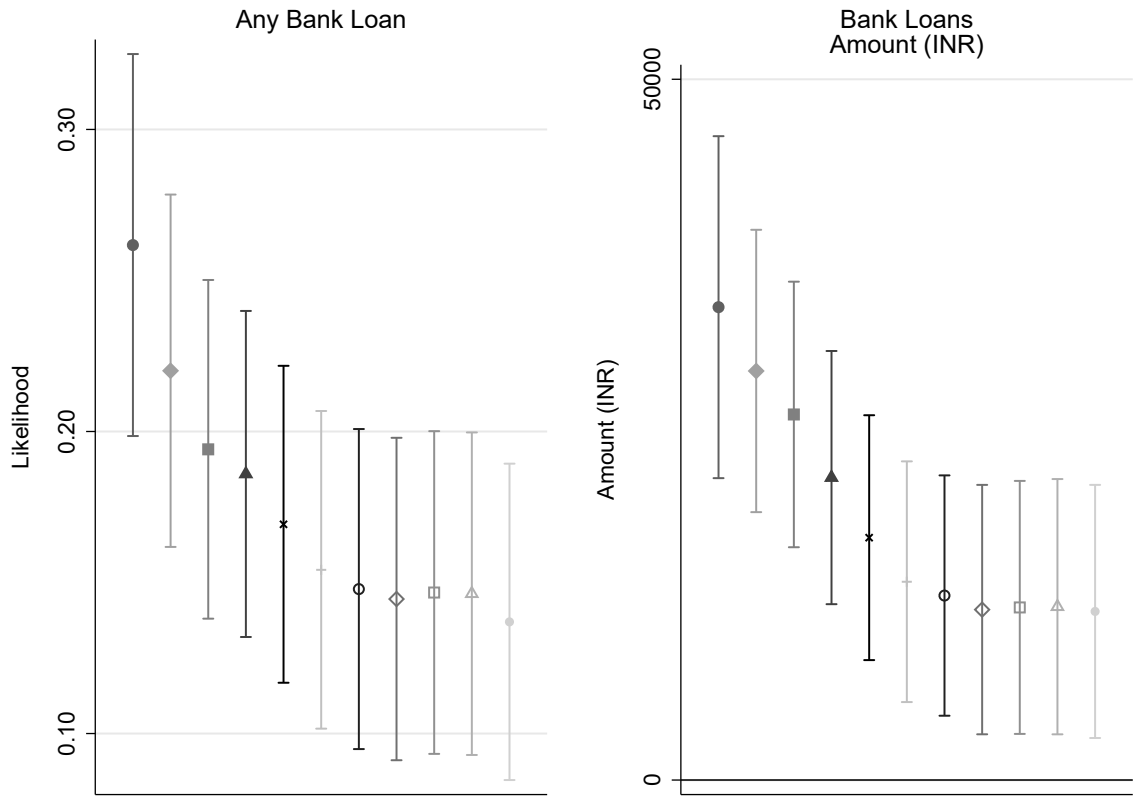
Notes: This figure presents the McCrary test (McCrary, 2008), testing for selective sorting of districts as minority concentration around the 0.25 threshold. The horizontal axis depicts the running variable – defined in equation (1). The horizontal axis shows the range of the running variable between -.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 3: Access to Bank Credit for Minority Households



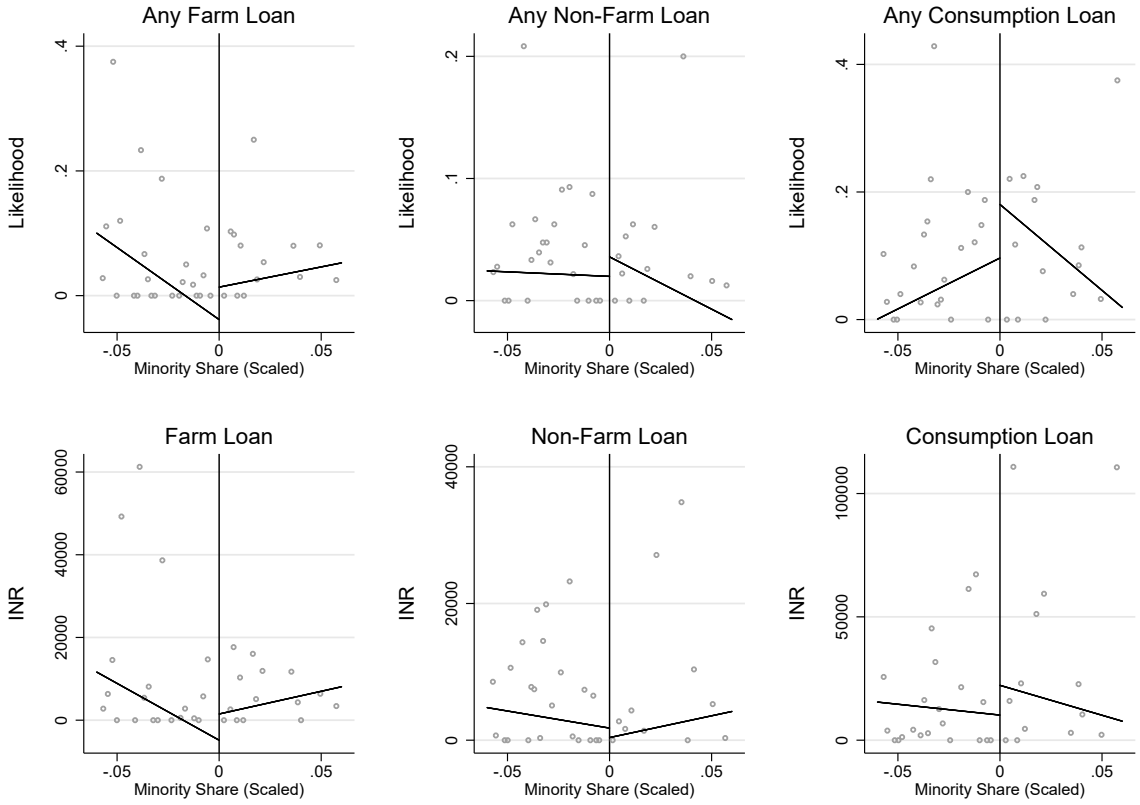
The above figures shows the treatment effect for access to bank credit for minority households. The unit of observation is the household. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). The outcome of interest in the left-panel is a dummy equaling 1 if the household has any bank loan; in the right-panel, the volume of bank loans (initial loan value). The 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 4: Access to Bank Credit for Minority Households: Robustness to Alternate Bandwidths



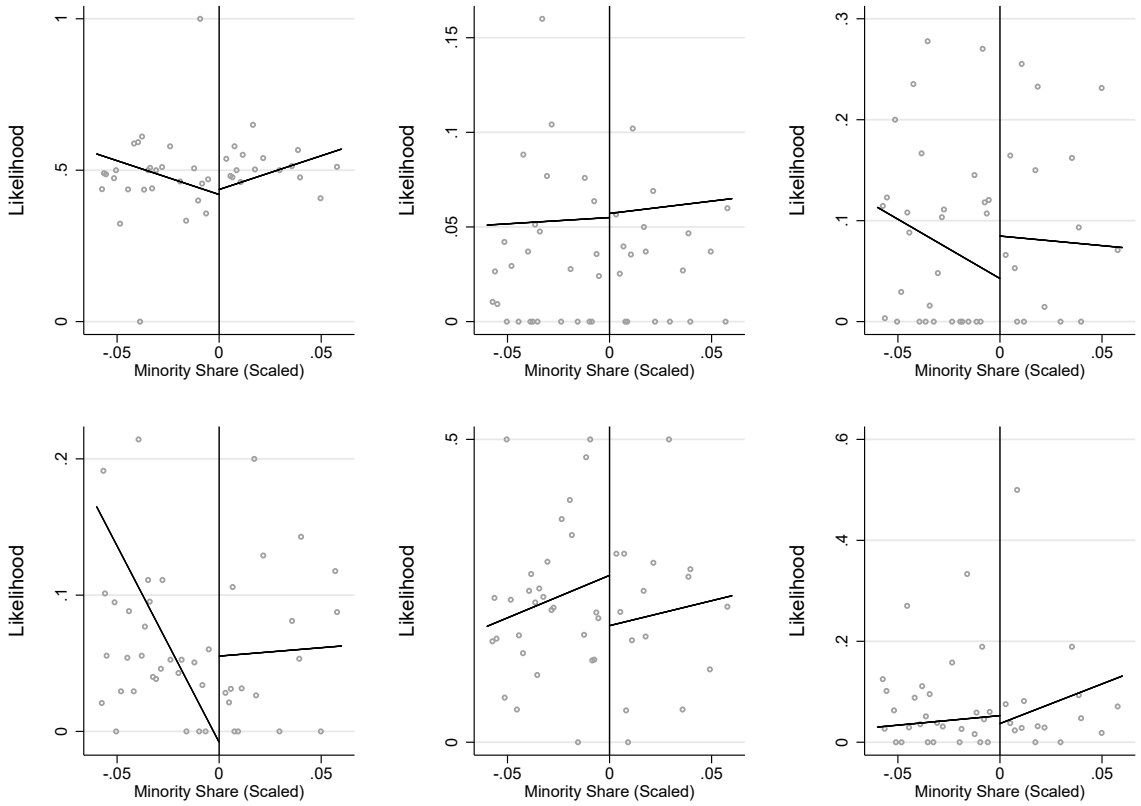
Notes: This figure shows the robustness of the baseline results to alternate bandwidths. The unit of observation is the household. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). The outcome of interest in the left-panel is a dummy equaling 1 if the household has any 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 0.005. The last point estimate is based on a bandwidth of .09. 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 5: Purpose of Bank Credit for Minority Households



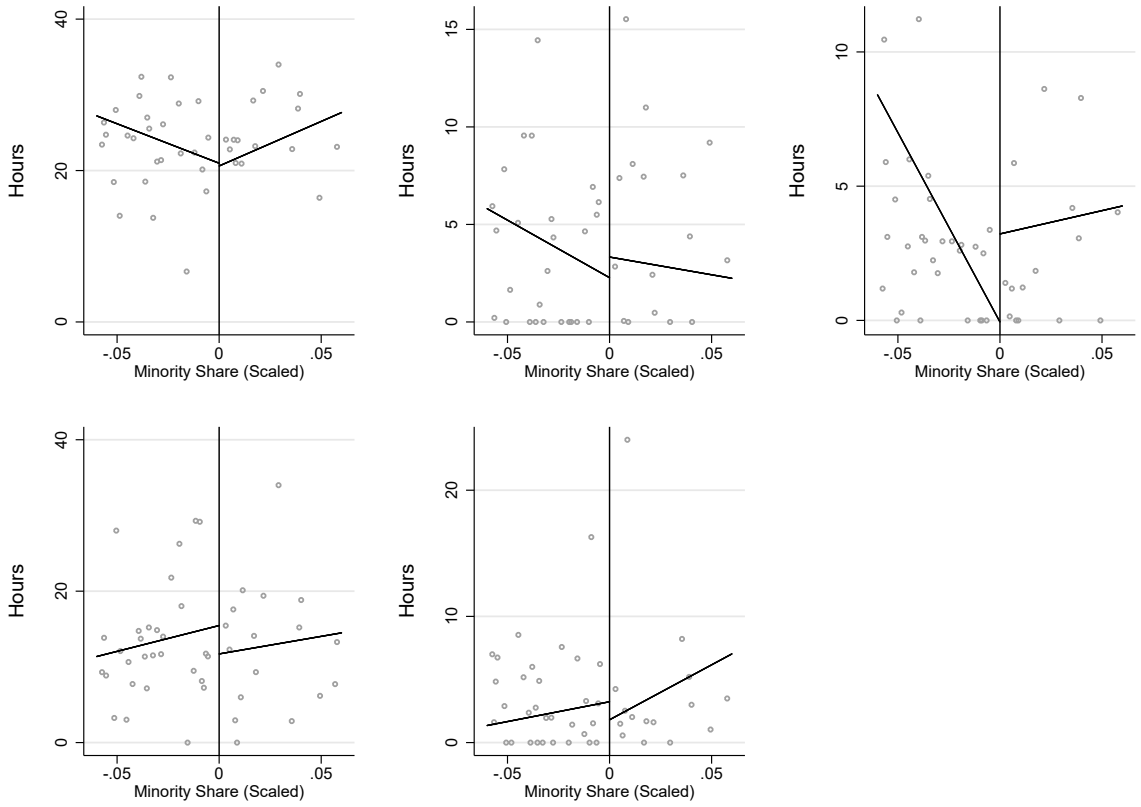
Notes: The above figure identifies the treatment effect for purpose of bank credit for minority households. The unit of observation is the household. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). The outcome of interest in the top row is a dummy equaling 1 if the household has any bank loan for the specified purpose; in the right-panel, the volume of bank loans (initial loan value) for the purpose specified. 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. *Expenditure* loans refer to loans taken for the purpose of housing, health or education, and other household expenditures. 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 6: Treatment Effect on Labour Market Outcomes: Minority Working-Age Individuals, Extensive Margin



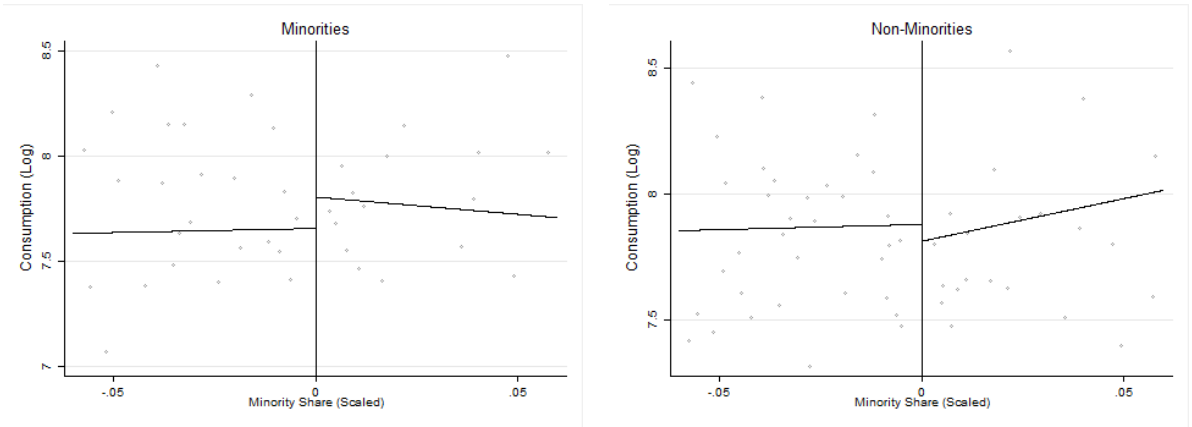
Notes: The above figure identifies the treatment effect on extensive margin labour market outcomes for minority working-age individuals. The unit of observation is the individual. Working-age individuals refer to individuals between the age of 18 and 60. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). The outcome of interest is a dummy equaling 1 if the individual engaged in any of the corresponding labour market activities, in the week preceding the survey. The horizontal lines show the linear fit from a local linear regression. The local linear regressions include state and survey month fixed effects; individual covariates such as gender, a quadratic in age, and marital status; 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 PLFS. *LFP* is a dummy equaling 1 if the individual reports participating in the labour force during the week.

Figure 7: Treatment Effect on Labour Market Outcomes: Minority Working-Age Individuals, Intensive Margin



Notes: The above figure identifies the treatment effect on intensive margin labour market outcomes for minority working-age individuals. The unit of observation is the individual. Working-age individuals refer to individuals between the age of 18 and 60. *Minority Share (Scaled)* denotes the running variable, defined in equation (1). The outcome of interest is the hours of work the individual engaged in for each of the labour market activities, in the week preceding the survey. The horizontal lines show the linear fit from a local linear regression. The local linear regressions include state and survey month fixed effects; individual covariates such as gender, a quadratic in age, and marital status; 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 PLFS. *LFP* is a dummy equaling 1 if the individual reports participating in the labour force during the week.

Figure 8: Treatment Effects on Household Consumption



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.

8 Tables

Table 1: Treatment Effect on Bank Credit: Minority Households

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Loan = 1)			Loan Amount (INR)		
Treat	.113***	.169***	.311***	18103.943***	17293.783***	34627.207***
	(.036)	(.032)	(.047)	(6364.988)	(5310.872)	(10799.202)
Observations	1836	1836	1836	1836	1836	1836
State FE	Y	Y	Y	Y	Y	Y
Household Controls	N	Y	Y	N	Y	Y
Dep Var Mean	.115	.115	.115	18334.529	18334.529	18334.529
Polynomial Fit	Linear	Linear	Quadratic	Linear	Linear	Quadratic

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)-(6), the amount of bank loans received by the household. All specifications include state fixed effects. Columns (1)-(2) and (4)-(5) control for a linear polynomial in the running variable; columns (3) and (6) control for a quadratic polynomial in the running variable. Columns (2)-(3) and (5)-(6) control for 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 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	.052***	.016	.084***	6341.136**	-1386.772	11973.199**
	(.019)	(.024)	(.027)	(2640.046)	(1922.047)	(5370.973)
Observations	1836	1836	1836	1836	1836	1836
Dep Var Mean	.033	.014	.051	3909.646	2064.700	9167.545

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. 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 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	.087***	.098***	13818.619***	3475.164***
	(.023)	(.019)	(4580.610)	(1298.123)
Observations	1836	1836	1836	1836
Dep Var Mean	.096	.024	17575.252	759.278

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. 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 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	.119**	.050**	.085***	-.003	-.017	.021
	(.046)	(.024)	(.032)	(.016)	(.012)	(.016)
Observations	1836	1836	1836	506	411	106
Dep Var Mean	.044	.035	.044	.109	.108	.109

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. 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 5: Treatment Effects on Weekly Labour Market Activities: Extensive Margin, Minority Working-Age Individuals

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Outcome = 1)					
	Labour Force Participation	Unemployed	Farm	Manufacturing	Trade and Services	Construction
Treat	.017	.002	.042	.063***	-.083***	-.016
	(.017)	(.012)	(.029)	(.013)	(.022)	(.029)
Observations	3596	3596	3596	3596	3596	3596
Dep Var Mean	.497	.048	.093	.103	.187	.060

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: Treatment Effects on Weekly Labour Market Activities: Intensive Margin, Minority Working-Age Individuals

	(1)	(2)	(3)	(4)	(5)
	Hours Worked in				
	Week	Farm	Manufacturing	Trade and Services	Construction
Treat	-.364	1.058	3.280***	-3.750***	-1.430
	(1.084)	(1.039)	(.854)	(1.421)	(1.382)
Observations	3596	3596	3596	3596	3596
Dep Var Mean	23.854	4.343	5.565	10.747	2.892

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

Table 7: 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	.149***	.129*	-.067	.066		.065	
	(.058)	(.070)	(.065)	(.136)		(.137)	
Muslim × Treat				.135	.162*		
				(.100)	(.092)		
Minority × Treat						.151	.177*
						(.099)	(.092)
Muslim				-.255***	-.250***		
				(.090)	(.079)		
Minority						-.202**	-.221***
						(.083)	(.070)
Observations	10353	14566	81649	11964	11964	12795	12795
R^2				.58	.65	.59	.68
Control Mean	2963.97	2963.97	3360.01	3360.01	3360.01	3360.01	3360.01
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 .06 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%

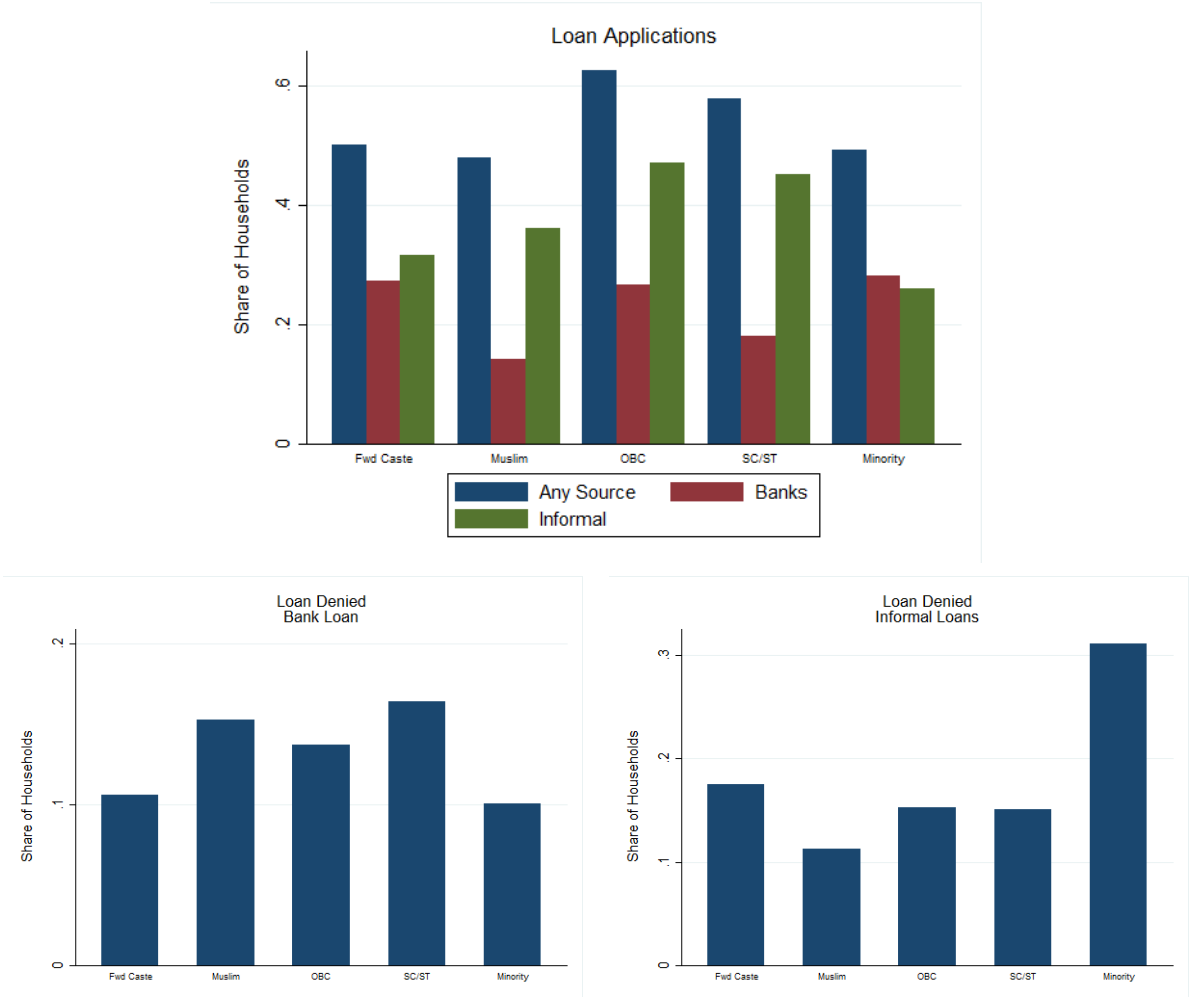
A Appendix: Pre-Treatment Desciptives

Figure A1: Pre-Treatment Household Credit Across Communities



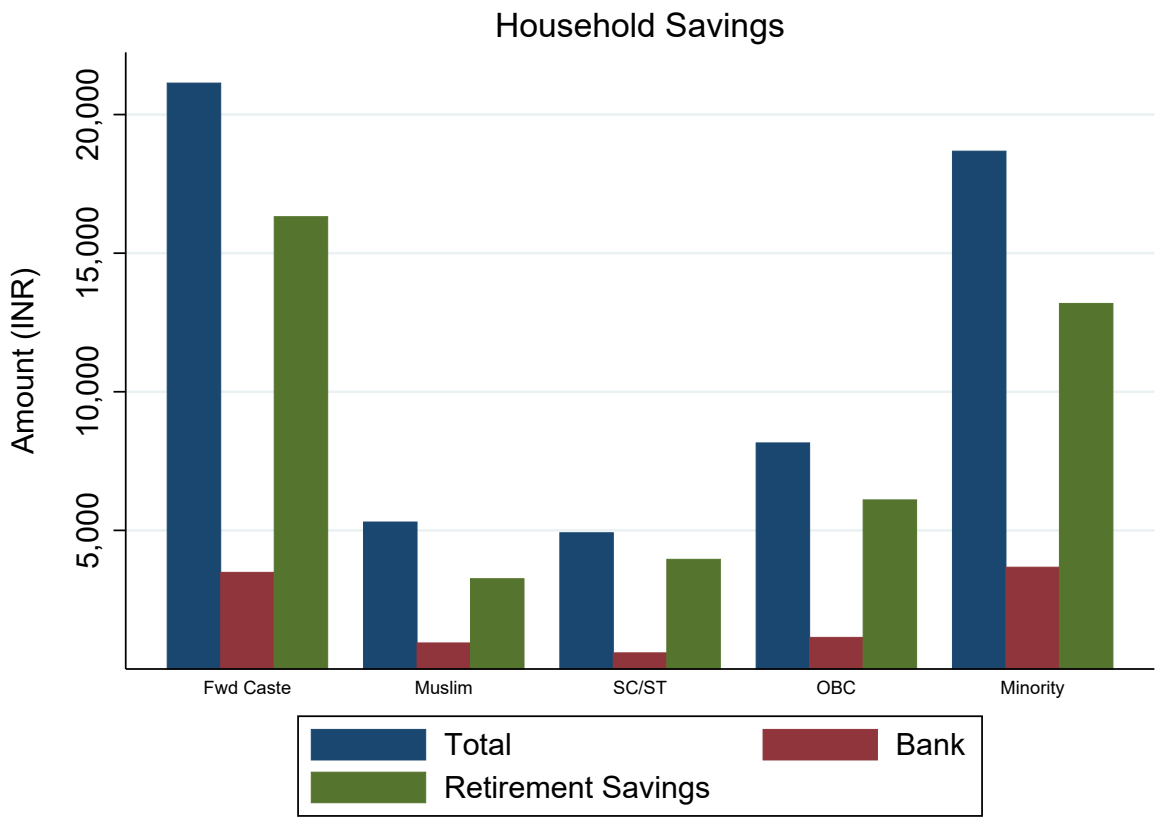
Notes: The above figure presents pre-treatment descriptive statistics for loan outcomes by community. The top-left panel shows the likelihood of households having loans from any of the above-mentioned sources; the top right-panel shows the average volume of credit received by households from each source; 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)
	Demographic Characteristics					Employment Characteristics				
	Rural	HH Size	HOH Female	Any Secondary Educated	Any Higher Educated	Any Unemployed	Any Self Employed	Any Farm	Any Manufacturing	Any Trade Services
Treat	.076	.850**	.041	.067	.019	-.013	.022	.038	-.161***	.131
	(.054)	(.401)	(.026)	(.046)	(.015)	(.022)	(.067)	(.092)	(.047)	(.080)
Observations	2056	2056	2056	2056	2056	2056	2056	2056	2056	2056
Dep Var Mean	.627	5.584	.110	.251	.049	.025	.561	.368	.206	.382

Notes: The above table shows the pre-treatment balance on household characteristics for minority households. The unit of observation is the household. The outcome of interest in column (1) is a dummy equaling 1 if the household resides in a rural area; in column (2), household size; in column (3), a dummy equaling 1 if the household head is female; in column (4), a dummy equaling 1 if the household has any member who is secondary or higher educated; in column (5), a dummy equaling 1 if the household has any member who is higher educated; in column (6), a dummy equaling 1 if any household member is unemployed; in column (7), a dummy equaling 1 if any household member is self-employed; in column (8), a dummy equaling 1 if any household member is employed in farm activities; in column (9), a dummy equaling 1 if any household member is employed in manufacturing work; in column (10), a dummy equaling 1 if any household member is engaged in trade or service activities. All specifications include state fixed effects, a linear polynomial in the running variable. Columns (2)-(10) also include household covariates. 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 Land Holdings, Real Estate, Savings and Consumption

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Land Ownership (Hectares)			Real Estate (INR)		Savings and Consumption (INR)		
	Irrigated Farm Land	Farm Land	Total Land	Residential Real Estate	Total Real Estate	Bank Deposits	Savings	Consumption
Treat	.039	.089	.081	3505.478	-1315.982	-47.627	3411.678	34.902
	(.038)	(.069)	(.064)	(12564.191)	(13987.130)	(506.100)	(3707.121)	(32.506)
Observations	2056	2056	2056	2056	2056	2056	2056	2056
Dep Var Mean	.077	.190	.213	62333.796	66617.649	1023.389	5386.305	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 B3: Pre-Treatment Balance on Productive Assets and Transport

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Machinery				Transport Assets					
	Any Farm Machinery	Farm Machinery	Any Non-Farm Machinery	Non-Farm Machinery	Any Bicycle	Bicycle Value	Any Car	Car Value	Any Transport	Transport Value
Treat	-.174***	-461.663	-.004	-257.403	.009	-112.367	-.007	-1431.608	.006	1609.724
	(.047)	(870.659)	(.018)	(319.804)	(.091)	(87.701)	(.007)	(977.715)	(.101)	(1512.196)

Notes: The above table shows the pre-treatment balance on household productive assets and transport owned for minority households. The unit of observation is the household. The outcome of interest in the odd numbered columns is a dummy equaling 1 if the household has the asset mentioned; in the even-numbered columns, the value of the asset (INR). Columns (1)-(2) pertain to farm machinery; columns (3)-(4), non-farm machinery; columns (5)-(6), bicycles; columns (7)-(8), cars; columns (9)-(10), aggregate transport value. 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 B4: Pre-Treatment Balance on Household Credit

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Any Loan = 1)			Loan Amount (INR)		
	Any Source	Bank Loan	Informal Loan	All Sources	Banks	Informal Loans
Treat	-.004	.004	.050	-5082.294	-248.192	-1593.935
	(.032)	(.015)	(.041)	(3120.060)	(1889.109)	(1604.543)
Observations	2056	2056	2056	2056	2056	2056
Dep Var Mean	.285	.027	.211	11346.187	1411.578	6002.792

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 B5: 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	-.000	.000	.001	375.850	-705.406	-34.722
	(.010)	(.007)	(.005)	(1535.201)	(619.669)	(392.296)
Observations	2056	2056	2056	2056	2056	2056
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 B6: Pre-Treatment Balance on Loan Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Cost of Credit			Pr(Any Unsecured Loan = 1)		
	Any Source	Banks	Informal Loans	Any Source	Banks	Informal Loans
Treat	-.014	.008	-.062	.051	.008	.028
	(.047)	(.012)	(.071)	(.035)	(.009)	(.040)
Observations	913	124	607	2056	2056	2056
Dep Var Mean	.181	.131	.200	.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 B7: Pre-Treatment Balance on District Banking Characteristics

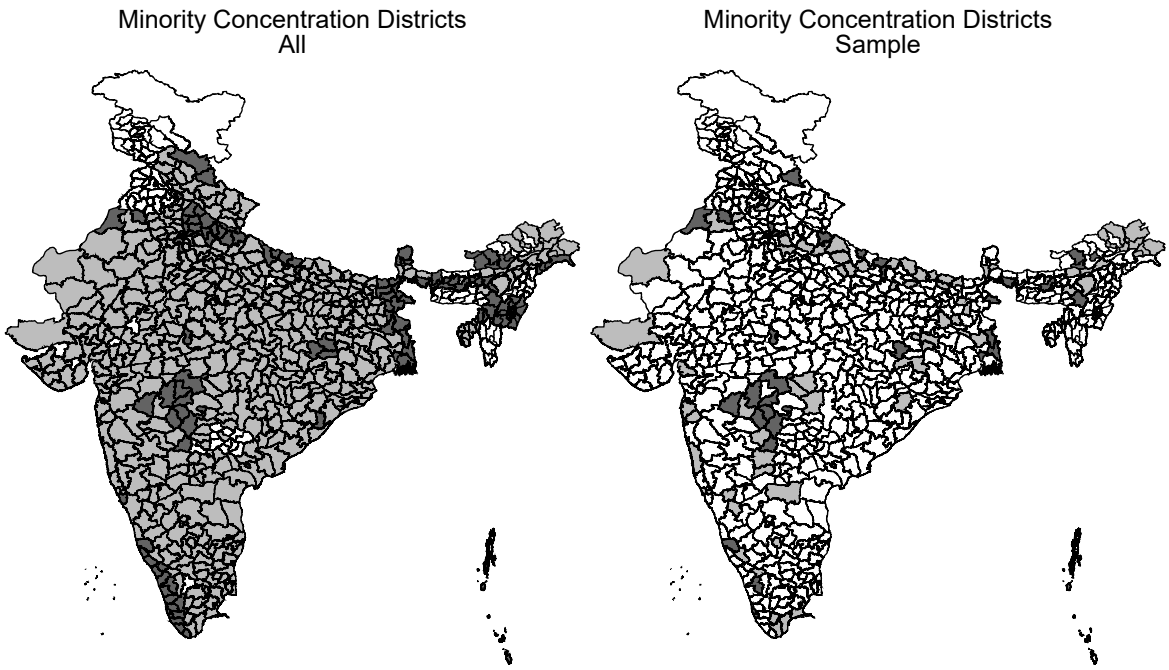
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Bank Branches			Bank Deposits		Bank Credit				
	All Banks	Government Banks	Private Banks	Deposit Accounts	Deposit Amount	Loan Accounts	Loan Amount	Farm Loan Amount	Manufacturing Loan Amount	Personal Loan Amount
Treat	77.438	77.091	.347	.953	14.215	.071	-1.328	.604	-.373	2.046
	(58.802)	(56.115)	(3.698)	(.675)	(15.461)	(.053)	(9.281)	(.600)	(4.664)	(1.376)
Observations	63	63	63	63	63	63	63	63	63	63
Dep Var Mean	118.562	110.462	8.099	.894	25.777	.113	18.214	2.230	8.549	2.407

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

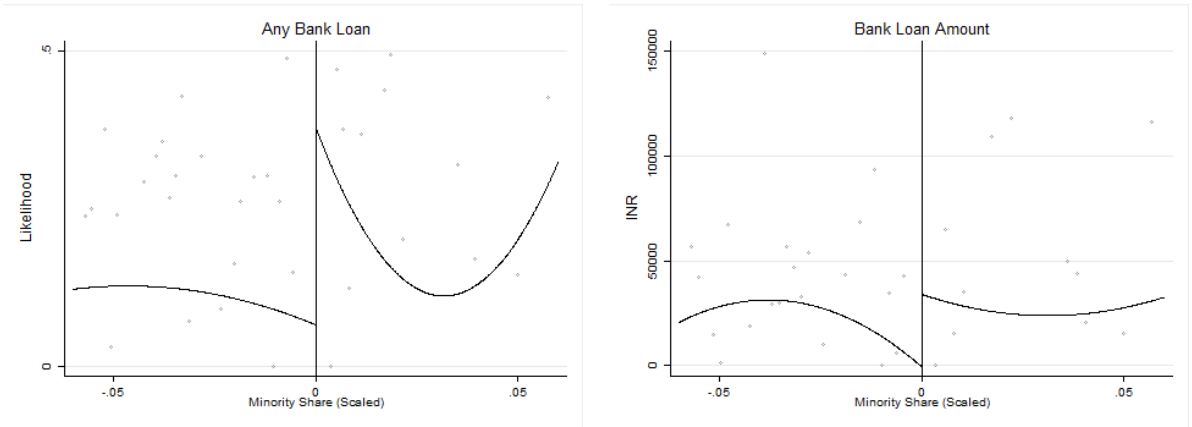
C.1 Figures

Figure C1: Geographical Distribution of Religious Minorities in India



Notes: The above figure shows the geographical distribution of minority concentration districts. The left-panel shows all minority concentration districts (darker shade); the right panel shows minority concentration districts located within a bandwidth of .07 from the discontinuity threshold, which forms our main sample.

Figure C2: Access to Bank Credit for Minority Households: Quadratic Polynomial Fit



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 quadratic polynomial fit from a second degree local polynomial regression. The local polynomial regressions include state fixed effects, household covariates and a quadratic polynomial fit 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: Treatment Effect on Bank Credit for Minority Households: Robustness of Baseline Treatment Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Pr(Loan = 1)					Loan Amount (INR)				
	MSERD Bandwidth		District Covariate	All Minorities	Fuzzy RDD	MSERD Bandwidth		District Covariate	All Minorities	Fuzzy RDD
Treat	.211***	.302***	.099***	.166***	.240***	28808.90***	35326.43***	17657.00***	18677.06***	15643.15
	(.035)	(.043)	(.029)	(.028)	(.086)	(6012.837)	(9495.72)	(5035.47)	(5834.77)	(13723.50)
Observations	1378	2058	1615	2667	3887	1378	2058	1615	2667	3887
Dep Var Mean	.128	.107	.115	.148	.166	18067.22	18249.47	18334.53	24219.28	22544.91
Bandwidth	.046	.069	.060	.060	.060	.045	.068	.060	.060	.060
Polynomial Fit	Linear	Quadratic	Linear	Linear	Linear	Linear	Quadratic	Linear	Linear	Linear

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	-.017	.169***	.021	-.160***	12709.494	17293.783***	8098.719	-12607.649*
	(.086)	(.032)	(.063)	(.045)	(15760.305)	(5310.872)	(8327.621)	(7256.772)
Observations	1836	1836	1836	1836	1836	1836	1836	1836
Dep Var Mean	.323	.115	.159	.091	42119.235	18334.529	15017.225	8398.331

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 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 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	-.047	.068	.032*	.003	.029**	-81.632	8180.351
	(.035)	(.078)	(.018)	(.013)	(.015)	(1171.679)	(8766.751)
Observations	1836	1836	1836	1836	1836	1836	1836
Dep Var Mean	.058	.103	.027	.010	.018	4401.726	10615.499

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, 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 C6: Treatment Effect on Loan Duration: Minority Households

	(1)	(2)	(3)	(4)	(5)
			Pr(Loan Duration > 1 Yr = 1)		
	Banks	Informal	Co-op. Bank	NBFC	Any Source
Treat	.119***	-.034	-.008	-.121***	-.093
	(.026)	(.055)	(.015)	(.026)	(.071)
Observations	1836	1836	1836	1836	1836
Dep Var Mean	.101	.136	.017	.054	.275

Notes: The above table shows the treatment effect on the likelihood of having a long-term loan for minority households. Long-term loans are defined as a loan whose duration exceeds 1 year. The unit of observation is the household. The outcome of interest is a dummy equaling 1 if the household has a long-term loan from the sources mentioned. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. The sample is restricted to a bandwidth of .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 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	.014**	.046**	.045***	181.604	10656.176**	1135.419
	(.006)	(.022)	(.016)	(282.767)	(5052.955)	(1599.222)
Observations	1836	1836	1836	1836	1836	1836
Dep Var Mean	.004	.020	.027	773.008	6435.760	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 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 C8: Treatment Effect on Loan Delinquency: Minority Households

	(1)	(2)	(3)
		Pr(Any Delinquent Loan = 1)	
	Banks	Informal	Any Source
Treat	.046	.144	.025
	(.058)	(.103)	(.041)
Observations	483	385	995
Dep Var Mean	.273	.518	.316

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 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 C9: Household Productive Assets: Minority Households

	(1)	(2)	(3)	(4)
	Pr(Machine = 1)		Machine Value (INR)	
	Farm	Non-Farm	Farm	Non-Farm
Treat	.255***	-.030	2594.599***	-1355.079
	(.096)	(.053)	(996.556)	(880.263)
Observations	1836	1836	1836	1836
Dep Var Mean	.341	.107	1407.542	1623.396

Notes: The above table shows the treatment effect on productive assets owned by 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 owns any machinery; in columns (3)-(4), value of machinery owned by the household. 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 C10: Household Productive Assets: Transport Owned by Minority Households

Panel A: Transport Purpose

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Any Transport = 1)			Transport Value (INR)		
	Any Transport	Farm	Non-Farm	Total Transport	Farm	Non-Farm
Treat	.013	.043***	-.117**	7085.711	1932.208	4687.236
	(.078)	(.014)	(.057)	(4564.658)	(1186.263)	(3141.209)
Observations	1836	1312	1312	1836	1836	1836
Dep Var Mean	.710	.034	.107	48160.592	4533.003	11218.113

Panel B: Transport Type

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Any Transport = 1)			Transport Value (INR)		
	Bicycles	Cars	Tractors	Bicycles	Cars	Tractors
Treat	.072	-.002	.003	85.584	3075.362	1940.043*
	(.081)	(.009)	(.003)	(224.511)	(5291.465)	(1111.982)
Observations	1836	1836	1836	1836	1836	1836
Dep Var Mean	.345	.064	.016	731.929	21565.276	3778.936

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 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 C11: Household Land and Real Estate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Minority Households						Non-Minority Households		
	Land Area (Hectares)			Real Estate Values (INR)			Real Estate Values (INR)		
	Farm Irrigated	Farm	Land	Residential Real Estate	Aggregate Real Estate	Price Per Sq. Metre	Residential Real Estate	Aggregate Real Estate	Price Per Sq. Metre
Treat	.112*	-.018	.220*	227583.095**	222946.728***	5012.379	241148.428*	211200.157*	54997.405***
	(.062)	(.053)	(.130)	(101929.75)	(73453.140)	(4517.286)	(136745.24)	(124323.33)	(16689.723)
Observations	1836	1836	1836	1836	1836	1501	10128	10128	8121
Dep Var Mean	.162	.364	.536	444965.27	472032.34	9382.540	535246.23	561639.92	79313.358

Notes: The above table shows the treatment effect on household ownership of land and real estate. The unit of observation is the household. Columns (1)-(6) show the result for minority households; columns (7)-(9) show the results for non-minority households. Columns (1)-(3) pertain to the amount of land owned by households (in hectares). Columns (4)-(9) refer to the value of real estate. *Residential Real Estate* is the value of residential buildings; *Aggregate Real Estate* is the total value of buildings. 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%

D 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 D1: Treatment Effect on Overall Credit Access 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	.249***	-.024	-.131***	63861.766***	21266.018**	-11095.426
	(.094)	(.055)	(.043)	(16033.075)	(9635.373)	(7027.124)
Observations	1354	2450	1663	1378	1378	1836
Dep Var Mean	.363	.191	.090	41007.547	10984.462	8398.331
Bandwidth	.042	.077	.057	.043	.047	.059

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 D2: 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	.069***	.039	.075***	8537.934**	-1548.841	15447.327***
	(.024)	(.026)	(.028)	(3459.339)	(1820.948)	(5435.144)
Observations	1378	1466	1931	1378	2244	1506
Dep Var Mean	.025	.021	.050	3372.215	2224.494	8271.633
Bandwidth	.044	.050	.066	.043	.071	.055

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 D3: 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	.152***	.112***	22754.942***	4171.832***
	(.027)	(.020)	(5019.067)	(1343.416)
Observations	1378	1378	1379	1498
Dep Var Mean	.096	.024	17575.252	759.278
Bandwidth	.045	.047	.048	.051

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 D4: Mechanisms: Treatment Effects on Collateral Requirements and Cost of Credit, MSERD Bandwidth

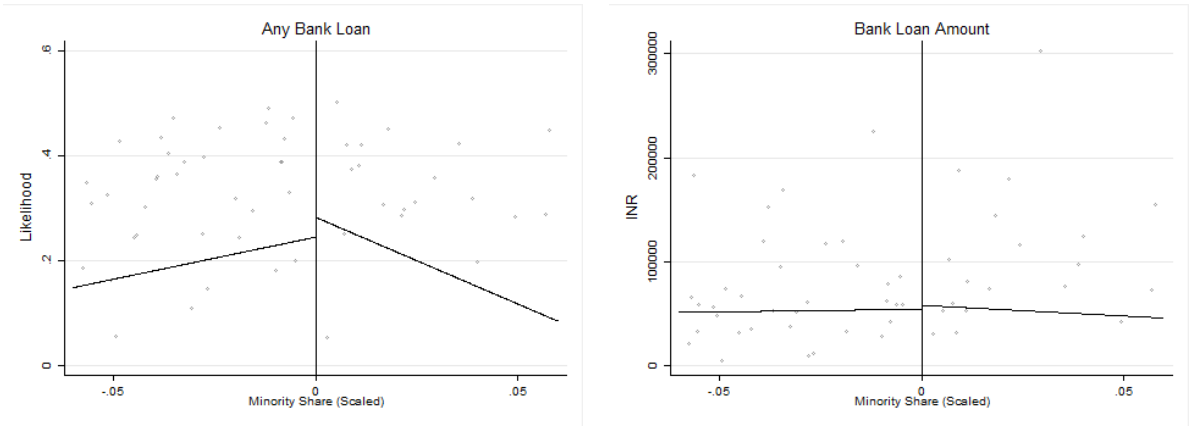
	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Unsecured Loan = 1)			Interest Rates		
	All Bank	Commercial Banks	Bank SHG	All Bank	Commercial Banks	Bank SHG
Treat	.117***	.049**	.085***	-.012	-.023**	.013
	(.041)	(.024)	(.032)	(.015)	(.011)	(.013)
Observations	1979	1931	1836	771	445	98
Dep Var Mean	.044	.035	.044	.109	.108	.109
Bandwidth	.067	.061	.059	.088	.065	.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%

E Treatment Effects for Non-Minorities

E.1 Figures

Figure E1: 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.

E.2 Tables

Table E1: Credit Access for Non-Minority Households

	(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	.037	-.067*	.078***	.030	3731.264	-14474.668***	9096.372***	253.680
	(.049)	(.038)	(.020)	(.040)	(15548.186)	(4975.667)	(2872.135)	(14131.602)
Observations	10128	10128	10128	10128	10128	10128	10128	10128
Dep Var Mean	.200	.128	.068	.327	42669.000	10695.942	8203.024	62562.257

Notes: The above table shows the treatment effect on credit access for non-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 loans received by the household from that source. *Non-Bank Financial Institutions* include co-operative banks and non-banking financial corporations. *Informal* loans include loans from professor 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. 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 E2: Credit Access for Non-Minority SC/ST Households

	(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	.062	-.098**	-.008	-.036	-3571.862	-7677.698	2453.636	-8436.622
	(.076)	(.043)	(.025)	(.062)	(8845.758)	(5524.329)	(2034.589)	(9734.123)
Observations	2606	2606	2606	2606	2606	2606	2606	2606
Dep Var Mean	.183	.151	.081	.348	23372.402	8312.398	4285.324	36565.637

Notes: The above table shows the treatment effect on credit access for non-minority *Dalit* (Scheduled Caste or SC), or *Adivasi* (Scheduled Tribe or ST) households. *Dalit* and *Adivasi* households qualify for credit extended to "weaker" sections. 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 loans received by the household from that source. *Non-Bank Financial Institutions* include co-operative banks and non-banking financial corporations. *Informal* loans include loans from professor 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. 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 E3: 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	.034	.009	4492.991	-761.727
	(.050)	(.022)	(15355.613)	(634.314)
Observations	10128	10128	10128	10128
Dep Var Mean	.164	.040	40978.332	1690.668

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 E4: 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	.016	-.008	-.002	-.067*
	(.036)	(.023)	(.022)	(.014)	(.011)	(.038)
Observations	10128	10128	10128	3319	2966	426
Dep Var Mean	.064	.036	.064	.111	.102	.111

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%