Does "Financial Affirmative Action" Affect Minority Well-Being? Evidence from a Directed Credit Program for Religious Minorities

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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 exploiting an unique policy intervention in India which targeted credit towards religious minorities. The policy encouraged commercial banks to expand credit in "minority concentration" districts – districts where the share of religious minorities exceeded 25% 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 minority credit access along both the extensive, and the intensive margins. The expansion in credit access is primarily driven by increased lending from bank-linked self-help groups, suggesting banks' use of local information networks to target credit to disadvantaged minority households. Credit from informal sources also increase, pointing to an overall expansion in liquidity in local credit markets. The increased credit to religious minorities is driven by consumption and non-farm business loans, and there is suggestive evidence of higher fixed capital ownership by these households. Consistent with an increase in non-farm business credit, minority workers in minority concentration districts have a disproportionately higher likelihood of being employed in the manufacturing sector, and also increase their labour supply hours in manufacturing activities.

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1 Introduction

Credit access for households forms a key step towards financial inclusion and can promote inclusive development (Demirguc-Kunt and Singer, 2017). A large body of literature has documented welfare-enhacing 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). Recent evidence in this regard has shown the importance of minority representation in financial institutions to improve lending outcomes for minority groups (Fisman et al., 2017; Frame et al., 2017). This paper brings evidence to the question of credit access for under-represented minorities by studying the impact of a directed credit intervention for religious minorities in India.

We study the Prime Minister's New 15 Point Programme for Minority Welfare (subsequently termed as "minority welfare")– a set of policy initiatives initiated in 2009 by India's federal government to improve the socio-economic conditions for religious minorities. Religious minorities, comprising of citizens from the Muslim, Christian, Sikh, Buddhist and Parsi communities, accounted for over 15 percent of the national population. Muslims made up 80% of India's religious minorities, amounting to over 130 million citizens, and form the largest religious minority in the world. 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 and discrimination. In addition, the policy also directed banks to expand credit allocation for religious minorities, which forms the key focus of this paper.

Specifically, the minority welfare policy classified select districts as "minority concentration", and directed banks to expand lending to religious minorities in these districts.[1] No specific targets were provided to banks in terms of overall lending volumes for minority borrowers. Banks were however mandated to file half-yearly reports on the quantum of credit allocated across minority groups in each minority concentration district. The annual policy guidelines issued by the central bank also directed banks to assign specific officers to oversee credit allocation for minority communities. Banks

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

were recommended to undertake local information campaigns to spread awareness about the policy, and collaborate with local self-help groups (SHGs) to identify creditworthy borrowers from religious minorities. The central bank also suggested banks to sensitize loan officers to the credit needs of religious minorities. Finally, 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.[2] The central bank expanded the definition of "weaker sections" to include religious minorities, offering banks a larger pool of potential borrowers to meet its 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 deemend "minority concentration districts" 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 as "minority concentration", and the first list of minority concentration districts was circulated in 2007, making it unlikely for districts to strategically sort themselves around the discontinuity threshold. We also use district covariates from 2004 to verify that minority and non-minority concentration districts were statistically indistinguishable around the discontinuity threshold prior to the policy intervention.

We use the RD design to compare minority credit outcomes across minority and non-minority concentration districts located within a narrow window of the discontinuity threshold. The presence of a large share of non-minorities in both treated and control districts provide us with an additional comparison group, and also allows for the testing of spillover effects or elite capture of bank credit. Our preferred empirical specification therefore compares credit access for minority and non-minority households within treated districts, in addition to comparing net treatment effects for minority households across treatment and control districts.

To empirically assess the treatment's impact on minority credit outcomes, 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

[2] Historically marginalized citizen groups here 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.

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. While the AIDIS was conducted in both 2013 and 2019, government conducted impacted evaluation regarding the policy reported limited progress on the policy objectives up to 2013 (Ministry of Minority Affairs, 2016). To this effect, we use the 2003 AIDIS survey to verify pre-treatment covariate balance and the 2019 AIDIS survey for our primary analysis. This also allows us to identify the long-term impacts of directed credit allocation towards religious minorities.

Our empirical findings identify a 25 percentage point increase in overall credit access for minority households. The effect is both statistically and economically significant, when considering that 35 percent of minority households in control districts had some outstanding loan. The increase in minority credit access emanated from both bank and non-bank sources: the likelihood of having an outstanding bank loan increased by 15 percentage points, while the likelihood of having an outstanding loan from informal sources increased by 18 percentage points. The increase in bank credit access for minorities emanates from bank-linked SHGs, as opposed to direct loans obtained from commercial banks. Lending through bank-linked SHGs essentially entail commercial banks providing financing to SHGs. The SHGs subsequently select the credit recipients and decide on the final allocation of credit. This indicates that commercial banks opted to collaborate with SHGs to identify creditworthy borrowers from minority groups, as recommended by the central bank in its annual policy guidelines on credit to minority communities.

Along with positive treatment effects for minority households, we also identify positive treatment effects for non-minority households. Relative to observationally equivalent non-minority households in control districts, non-minority households in treated districts witnessed a 13 percentage point increase in credit access, driven primarily by an increase in access to informal loans. Importantly, the difference in credit access across minority and non-minority borrowers is positive and statistically significant for overall credit, bank credit, credit from bank-linked SHGs and informal credit. There is also no difference in credit access through bank-linked SHGs for non-minorities across treated and control districts. This points to commercial banks using a specific strategy – credit issued through SHGs – to target minority borrowers in treatment districts to comply with the directed credit policy.

We obtain similar findings when considering credit access along the intensive margin. The intensive margin results identify large treatment effects for overall minority credit access, bank loans, and loans from informal sources. Again, the increase in bank credit access for minority households in treated districts emanates from bank-linked SHGs. While non-minority households in treated districts also witnessed higher loan volumes, this is driven by loans from informal sources, and we find no evidence of an increase in lending from bank-linked SHGs. As over half the households in our sample have no outstanding loans, we use an inverse hyperbolic sine transformation to identify treatment effects along the intensive margin. We verify that our results are qualitatively similar to measuring loan volumes in levels, albeit imprecisely estimated for total loans and bank loans. In terms of magnitudes, we identify an overall increase of INR 19,000 (1.4 percent of total household assets) in total loans issued to minority households in treated districts. The corresponding increase in lending from bank-linked SHGs is INR 4,600 (0.3 percent of household assets).

As the AIDIS only solicits information on outstanding loans at the time of survey and does not retroactively track all loans obtained by households over a period of time, the treatment effects likely serve as a lower bound of the policy's impact on minority credit access. Moreoever, the increase in both aggregate borrowing and borrowings from bank-linked SHGs by minority households in treated districts points to the presence of credit constraints faced by these households. In the absence of credit constraints, we would have expected minority households to substitute relatively costly informal credit using cheaper loans from bank-linked SHGs (Banerjee and Duflo, 2014).

The RD specification identifies a causal treatment effect within a narrow bandwidth of the discontinuity threshold. We use the data-driven optimal bandwidth procedure of Calonico et al. (2020) to restrict our sample to a consistent set of 92 districts, located within a bandwidth of .07 (fraction of minority population). We however demonstrate robustness to a number of alternate bandwidths. The results are also robust to using outcome-specific MSERD bandwidths. Finally, we show robustness of our baseline coefficients to the inclusion of pre-treatment district covariates. While our baseline specifications include state fixed effects, restricting the comparison of credit outcomes to households located within the same state, we also show robustness to the inclusion of district fixed effects which restrict our comparison to minority and non-minority households within the same treated district. The robustness of our results to district fixed effects indicate that non-minority and minority concentration districts did not significantly differ across unobservable district characteristics, further bolstering our

empirical design.

We exploit self-reported information on households' purpose of borrowing to show that the credit expansion in minority concentration districts was primarily to finance non-farm business and consumption loans. Importantly, we identify significant increases in lending from banks to minority households for non-farm business and consumption loans in treated districts. In contrast, the credit expansion for non-minority households was primarily for farm loans, with no impact on lending for non-farm business purposes from either bank or non-bank sources. This points to a clear divergence in bank lending in treated districts: an expansion in non-farm credit for minority households and farm loans for non-minority households.

We use additional information from the AIDIS to assess the treatment's impact on related credit outcomes. First, consistent with an expansion in credit access through SHGs, we identify a differential increase in minority households' likelihood of receiving unsecured bank credit. There is no corresponding impact for non-minorities. This points to a potential mechanism explaining our findings: if minority households had limited collateral and bank were unwilling to issue loans in the absence of collateral, a relaxation in collateral requirements by banks in response to the policy can increase credit allocation to minorities. Relatedly, we rule out that the expansion in borrowing by minorities in treated districts was through a reduction in the cost of credit. While there is evidence of a reduction in bank interest rates for non-minority households in treated districts, this is effectively reversed for minority households, implying no difference in the cost of bank credit across minority households in treated and control districts. This allows us to rule out the interest rate channel in explaining the expansion in bank credit in minority concentration districts.

Finally, we use self-reported repayment information to gauge the quality of lending in minority concentration districts. If minority borrowers were inherently riskier and limited lending by banks to these communities emanated from a rational decision to avoid a deterioration in credit quality, we would expect higher loan deliquency amongst minorities in minority concentration districts. Alternately, if banks lacked effective screening mechanisms to select creditworthy minority borrowers, or faced challenges in monitoring borrowers ex-post, a policy-mandated expansion in credit would also result in increased loan delinquency. We classify a loan to be delinquent if no repayment had been made towards that loan for at least 6 months preceding the survey. The empirical evidence suggests an overall increase in loan delinquency in minority concentration districts, driven by higher delinquencies

for loans issued directly by banks. While the point estimates are noisy, the evidence suggests a lower likelihood of non-repayment amongst minority borrowers in treatment districts.

Intriguingly, we identify no evidence of higher delinquency for loans issued by bank-linked SHGs: while the sample size is small, the evidence if anything suggests a reduction in loan delinquency amongst minority households borrowing from bank-linked SHGs in treated districts. There is also no evidence of higher delinquency for informal loans for either minority, or non-minority households in treated districts. On the whole, our findings suggest a worsening in credit quality for bank loans made in minority concentration districts to non-minority borrowers, but no such deterioration in credit quality in loans made by bank-linked SHGs to minority borrowers. This points to the information or monitoring advantages possessed by local lenders, which allows them to expand credit to marginalized borrowers, without a worsening in credit quality.

The AIDIS provides limited information on household ownership of fixed and durable assets. We use this to offer suggestive evidence of an increase in minority households' ownership of fixed capital and machinery in treated districts. The results suggest that the expansion in business loans from in minority concentration districts increased households' ownership of productive assets.

The increase in non-farm business loans and household fixed assets for minority households in treated districts leads us to examine the treatment's impact on labour market outcomes. We use cross-sectional data from the Primary Labour Force Survey (PLFS) in 2016-17 to identify the treatment's impact on weekly labour market activities. The PLFS is a nationally representative cross-sectional survey undertaken by the NSS, and provides detailed information on workers' employment status, sector of work, and type of establishment in which the worker is employed. Consistent with an increase in non-farm business loans from banks and household fixed assets, we identify an increase in minority workers' likelihood of being engaged in manufacturing activities in treated districts. The treatment effects are large, amounting to an 8 percentage point increase in the likelihood of being employed in the manufacturing sector (relative to the control district mean of 12 percent), and a 4 hour increase in manufacturing work per week. There is no corresponding increase in manufacturing work for non-minority workers; instead, we identify a higher likelihood of minority workers in treated districts being engaged in farm activities. We also find an increased likelihood of minority workers being engaged in self-employed manufacturing work in treated districts, and manufacturing activities undertaken through establishments located in the worker's home. The results suggest that the increase in business

credit for minority households in minority concentration districts allowed minority workers to increase their productive assets and engage in self-employed manufacturing work.

Related Literature

Our paper contributes to the literature studying discrimination in credit markets. Blanchflower et al. (2003) in an early work documented the presence of racial discrimination for small business credit in the U.S. More recent work by Brock and de Haas (2023) showed how gender discrimination in Turkish credit markets manifested itself through a higher likelihood of guarantors while approving loans applications from female entrepreneurs. In the Indian context, Fisman et al. (2020) showed how early-life exposure to inter-group violence reduced commercial bank lending to Muslim borrowers. While documenting the presence of racial discrimination in U.S. mortgage markets, Frame et al. (2017) showed how minority representation in financial institutions can improve credit outcomes for minority borrowers. In a similar vein, Fisman et al. (2017) showed how loan officers in banks exploit within-group soft information to increase inter-group lending in India. We add to this literature by showing how a directed credit intervention affects loan outcomes for credit-constrained religious minority households in India. Importantly, we document that banks' collaboration with local self-help groups can be effective in expanding credit to informationally opaque minority borrowers, without any accompanying deterioration in borrower quality. Our study also shows how an exansion in credit facilities to minorities increases households' productive assets and non-farm self-employment in manufacturing activities.

By documenting minority households to be credit constrained, we also add to the large literature on the economic impacts of credit access for credit-constrained households. Much of this literature has studied the role of microfinance in alleviating credit constraints for underprivileged poor households, but have found mixed evidence on overall household well-being Augsburb et al. (2015); **?**); Banerjee et al. (2015); Carlan and Zinman (2011); Kaboski and Townsend (2012). Our paper shows that targeted credit allocation through banks to religious minorities improve credit access along both the extensive and intensive margins. The increase in business loans from bank-linked SHGs results in households' ownership of productive assets, which in turn positive affects manufacturing employment for minority workers.

As the policy intervention of interest targeted credit to regions with a substantial concentration of minority groups, we also contribute to the literature studying place-based policies (see Bartik (2020) for a review). Existing literature have shown how place-based policies in the form of large-scale industrial

development contributed towards regional development and growth Busso et al. (2013); Criscuolo et al. (2019); Kline and Moretti (2014). Our paper shows how the expansion of bank credit to regions populated by underprivileged minorities can positively affect household outcomes through improved credit access and manufacturing self-employment.

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.

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 Minorities was a set of policy interventions outlined by India's federal government, which aimed to improve the socio-economic conditions of India's religious minorities – namely Muslims, Christians, Sikhs, Buddhists and Parsis.[3] 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 across the world.

The policy intervention covered the realms of education, employment, infant health, housing, sanitation and hygeine, access to credit, and protection from targeted religious discrimination and violence. The overarching policies were framed by the federal government and implemented by various departments reporting to the federal government, and financed through the federal exchequer. The initial set of policies were revised and expanded in the 2009, and renamed as the Prime Minister's New 15 Point Programme for the Welfare of Minorities.

Access to credit under the PM's minority welfare programme aimed at ensuring the "smooth flow of bank credit to minority communities" from government-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 to this effect issued a set of guidelines to commercial banks in July 2007. The most critical

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

amongst these was the notification that the federal government had classified a set of 103 districts (out of 593 districts) as "minority concentration" districts (RBI, 2007).[4] These districts were deemed such as the population share of 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. In 2009, 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 of minority concentration districts. As no rationale was provided for the inclusion of these 18 additional districts, we omit them from our analysis and focus exclusively on the 103 districts for which there was perfect compliance with the policy rule.

2.2 Monitoring by Central Bank

The RBI issued a set of instructions in 2007 to ensure banks complied with the directed credit policy for religious minorites (RBI, 2007). Most of these instructions remain in effect at the time of writing and are reiterated through annual notifications issued by the RBI (RBI, 2021).[5] 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 to assign a senior officer whose sole responsibility was to look into challenges faced by religious minorities in accessing credit.[6] This officer was also tasked with generating local awareness amongst minority communities regarding the policy and other related government schemes. The officer was also tasked with designing credit schemes to fulfill the objectives of the directed credit allocation 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 populations. This included issuing advertisements across print and visual media, and engaging in information campaigns at the site of religious congregations (RBI, 2007). Critically, the RBI recommended banks to engage with self-help groups to identify

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

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

[6] 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 in matters of financial inclusion and credit disbursement to farm activities, as well as farm and small enterprises. Lead banks are government-owned banks.

underprivileged minority borrowers (RBI, 2007). 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 also expected to impart adequate training to their officers and staff to sensitize them to credit needs of minority borrowers, in order to effectively reach out to them. 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 Commercial Banks

Every bank operating in India is mandated by the central bank to allocate at least 40 percent of its loan portfolio towards the "priority sector" (RBI, 2020). Priority sector lenders broadly includes farm credit and credit to small and micro-enterprises. Since 1991, loans for housing, education, renewable energy and self-help groups have also begun to qualify under the priority sector. If banks are unable to achieve their priority lending targets, they are expected to contribute the residual amount to a rural infrastructure development amount. As the RBI directives explicitly encouraged banks to lend to SHGs in an effort to reach out to minority borrowers, and lending to SHGs qualified under priority sector credit, it is possible that banks opted to lend to SHGs transacting with minority borrowers to serve the dual purpose of meeting their priority sector targets, while achieving compliance with the targeted lending policy in minority concentration regions. This is particularly salient in light of the bank-SHG linkage programme, unveiled in 2008, which encouraged banks to lend directly to SHGs and finance their credit needs.

Additionally, 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 the Scheduled Caste (SCs) and Scheduled Tribes (STs). With the advent of the targeted lending to minorities in minority concentration districts, the RBI expanded "weaker section" to include minority borrowers. This was inclusive of both personal loans made to minority borrowers, as well as loans made to non-registered enterprises which were owned by minorities.[7] The RBI's directives mention that banks to ascertain to ensure adequate representation of minority

[7] 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 not eligible to receive credit under this scheme, irrespective of the type of ownership.

borrowers within weaker sections.

In summary, banks now had a wider pool of potential borrowers to whom they could offer credit in order to meet their binding regulatory targets.

3 Data

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

3.1 All India Debt-Investment Survey

The primary dataset used is the All India Debt-Investment Survey. The AIDIS is a nationally representative survey, conducted decennially by the National Sample Survey Organisation (NSS) in the form of repeated cross-sections. The survey samples in excess of 100,000 households and collects extensive information on household balance sheets, including borrowings, savings and ownership of select household durable 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. Section 2 noted that while the PM's 15-Point Minority Welfare Programme was launched in 2009, implementation was poor till 2013. To this effect, we use the 2003 AIDIS survey to verify balance along household observables and outcome variables in the pre-treatment period, while the 2019 AIDIS survey is used to estimate the impact of the directed credit policy on lending outcomes of interest.

Appendix Tables A1.A–A1.D present select summary statistics from the 2019 AIDIS. In all, 42 (21) percent of households had some outstanding (bank) loan at the time of the survey. While 15 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. We separate the informal sources into two categories: a) professional money lenders and input suppliers; b) friends, relatives, employers and landlords. While 12 percent of households had outstanding loans from the

former, about 9 percent had loans from the latter. The primary reason for household borrowing was to finance consumption: while 22 percent of households reported taking loans for capital or current expenditures on non-farm businesses. Farm loans were reported by 13 percent of households.

For intensive margin measures (Appendix Table A1.C), we use the initial loan size, as opposed to the outstanding loan value at the time of the survey. This leaves our intensive margin measures unaffected by capitalized interest and debt accumulation due to non-repayment by households. Conditional on having some outstanding loan, average aggregate household loans exceeded INR 150,000 – approximately 1.6 times aggregate annual household consumption. The sum of loans obtained directly from commercial banks was larger – approximately INR 220,000. In contrast, loans from bank-linked SHG was smaller, amounting to less than INR 50,000. Informal loans equaled almost INR 100,000, driven by loans from money lenders. Loan volumes were largest for non-farm business loans, followed by consumption loans.

Appendix Table A1.D shows that the majority of households had at least one unsecured loan, primarily through informal sources. 40 percent of households reported having at least one unsecured bank loan. Most loans were obtained between 2016 and 2019 (3 years prior to the survey), and about a third of households reported at least 1 short-term loan (less than 1 year). 40 percent of households reported having loan of a longer duration (in excess of 3 years). The average annual interest rate faced by households was 14%. Household-specific interest rates are computed as the loan volume weighted average interest rate across all household loans. Expectedly, banks charged the lowest interest rates – 11%, relative to 31% charged by money lenders. Most loans obtained from friends and relatives were interest-free. The lack of administrative data on repayments makes it challenging to assess loan delinquency and borrower quality. We use the question on whether the borrower made any payments to service the loan 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 at least 6 months overdue. [8] Appendix Table A1.D shows high self-reported loan delinquency, with almost 40% of households with some outstanding borrowing reporting no repayments made on a loan in the past 6 months. While lower than informal loans, almost 40% of households borrowing from banks reported at least 1 delinquent loan.

[8] The AIDIS only informs us of the year in which the loan was obtained and not the month. To this effect, we are unable to obtain delinquency measures for loans obtained in 2019. For households obtaining loans in 2018, we can obtain comparable delinquency measures only if the survey was undertaken after June 2019.

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 the weekly data from the PLFS which asks 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 also includes locational identifiers at the level of district, as well demographic information such as age, educational attainment, caste and religion.

Summary statistics of weekly labour market activities are shown in Appendix Tables A2.A and A2.B. Religious inorities 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 less 15% of working-aged individuals had a college degree. The overall weekly labour force participation was 55%. Conditional on participation in the labour force, individuals worked 46 hours a week, with almost 40% of workers engaged in farm activities. A fifth of workers were engaged in services, with manufacturing, trade and construction activities accounting for approximately 10% of workers. 12 percent 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 home.

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. With Muslim households accounting for almost 80% of India's religious minority population, the directed credit policy was unlikely to be inframarginal. We present the descriptive analysis by disaggregating our households into 4 mutually exclusive groups: Hindu forward caste, Hindu marginalized castes, Muslims, and other religious minorities. The top-left panel of Appendix Figure A2 shows that credit from money-lenders was the primary source of credit across all four groups, but

Muslims had the lowest access to bank credit along the extensive margin. They also were more reliant on loans from informal money-lenders than forward caste Hindus, or other religious minorities. The top-right panel of Appendix Figure A2 shows that conditional on receiving a bank loan, bank loan sizes were considerably smaller for Muslim households. Aggregate bank loans for Muslim households was comparable to marginalized Hindu caste groups, and 50% smaller than forward caste Hindu households. Loans from informal sources for Muslim households also tended to be smaller. Finally, the bottom right panel of Appendix Figure A2 shows that Muslim households faced the highest cost of credit from money lenders, amounting to annual interest rates of almost 40%. Banks however did not perceive Muslim borrowers to be riskier, as seen from the comparable rates of interest charged on bank loans across all 4 groups. It is worth noting that access to credit for non-Muslim religious minorities along both the extensive and intensive margins was comparable to general caste Hindus.

It is possible that the differentially lower access to credit for Muslim borrowers from bank and non-bank sources is a manifestation of lower credit demand, as opposed to targeted discrimination by lenders. The lack of information on loan applications disallow us from separating whether the reduction in access to bank credit was due to lower demand for institutional credit or lower supply. However, the reliance of Muslim households on considerably more expensive informal sources for credit suggests that any intervention directing bank credit towards these households is likely to be welfare enhancing.

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). Our running variable is defined as:

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

$ShMinority_{ds}$ refers to the population share of religious minorities in district d, located in state s. Using $Runvar_{ds}$ from equation (1), we define the district-level treatment indicator – $Treat_{ds}$ – to equal 1 if $Runvar_{ds} > 0$ and 0 otherwise.

The treatment assignment was undertaken in 2007 and used data from the 2001 population Census. This makes it implausible for districts or states to anticipate the policy and strategically manipulate

district minority population shares to lie on either side of the treatment threshold. Indeed, we verify with the first list issued by the RBI that all districts which satisfied the treatment assignment condition were classified as "minority concentration" districts. We omit the 13 additional districts which were later assigned to the treatment, despite not meeting the treatment assignment condition. As we found no rationale to justify their inclusion, our empirical analysis excludes these districts from the sample.

We estimate the RD specification using cross-sectional linear regressions of the form:

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

The unit of observation in equation (2) is the household i, 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 in equation (1). 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 outcomes across treatment (minority concentrated) and control districts (non-minority concentrated). Our specifications include state fixed effects (α), which restrict the comparison of outcomes to households in 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 $\mathbf{X}$. All specifications are weighted using the survey weights provided in the AIDIS data. We use the data driven bandwidth selection procedure of Calonico et al. (2020) and restrict the sample to a bandwidth of .07 (share of minority population) around the discontinuity threshold of 0.25, yielding a sample of 91 districts. Standard errors are clustered by district – the level at which the treatment varies.

The directed credit policy pushed banks to increase credit allocation to minorities in minority concentration districts. Within a narrow window around the discontinuity threshold, minority concentration districts also had a significant share of non-minority households. This provides us with a natural control group to conduct a within-district comparison across minority and non-minority households. For instance, banks could have opted to expand their loan portfolios in minority concentration districts to comply with the policy directive, and any increase in credit received by minority households could reflect a secular trend in increased lending in these districts. This also allows us to test for policy spillovers, and whether any changes in banks' lending behaviour reflected a reallocation of credit from non-minority to minority borrowers. Alternately, there could also have been elite capture of bank credit

in minority concentration districts by non-minorities. This is particularly true when considering that banks faced no penalties for non-compliance with the policy directives.

Our preferred empirical specification thereby augments equation (2) by pooling data across minority and non-minority households. Resultantly, we identify the differential effect of treatment assignment on minority households using the specification:

$$Y_{ids} = \alpha_s + \beta_1 Treat_{ds} + \beta_2 Minority_{ids} \times Treat_{ds} + \gamma f(Runvar_{ds}) + \delta \mathbf{X}_{ids} + \epsilon_{ids} \tag{3}$$

Minority in equation (3) is a dummy equaling 1 if household i qualifies as a religious minority. β_1 now compares outcomes across treatment and control districts for non-minority households. β_2 compares the differential impact of the treatment for minority households in treated districts. The sum of β_1 and β_2 estimates the treatment effect for minority households in treated districts. The linear polynomial in the running variable is augmented to account for differential effects across minority and non-minority households on either side of the discontinuity threshold. We also verify the robustness of equation (3) to the inclusion of district fixed effects, whereby we compare minority and non-minority household outcomes for households located within the same district. This allows us to estimate the differential impact of the directed credit intervention for minority households after flexibly controling for both observable and unobservable district characteristics likely to be correlated with the outcome of interest.

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 AIDIS conducted in 2003. Appendix Figures A3 and **??** show that none of the outcomes of interest were significantly different across the discontinuity threshold in 2003. We also use the NSS' Employment Unemployment surveys conducted in 2004-05 to show that pre-treatment district covariates were also continuous in the running variable at the discontinuity threshold (Appendix Figures A7 and A8). The absence of any selective sorting of districts into treatment and control status, seen in Figure **??**, combined with the absence of any significant differences in outcomes and district covariates in the pre-treatment period (Appendix Figures A3-A8) allow us to assign a causal interpretation to the RD coefficients estimated from specifications (2) and (3).

5 Results

This section presents the key findings of our paper. We first examine how directed credit policy for minority concentration districts affected minority households' credit access. Subsequently, we examine labour market outcomes.

5.1 Credit Access in Minority Concentration Districts

5.1.1 Baseline Results: Extensive Margin

Figure 2 presents unconditional RD plots estimating the treatment effect on credit access for minority households. 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 linear kernel and the lines show the linear fit from a local linear regression. Discontinuity estimates and standard errors, estimated using the methodology of Calonico et al. (2020) are displayed below each figure. The sample is restricted to households in 92 districts falling within a bandwidth of .07 (fraction of minority population) around the discontinuity threshold of .25. Standard errors are clustered by district for inference.

The top-left panel of Figure 2 shows a large (albeit noisy) increase in minority households' unconditional likelihood of having some outstanding loan at the time of the survey. The middle panel of the top row documents an increased probability of having some outstanding bank loan, driven primarily by the higher likelihood of having a loan from a bank-linked SHGs (bottom-left panel). The bottom-right panel shows a large and significant increase in the likelihood of minority households having outstanding credit from informal sources. Appendix Figure A9 shows no corresponding impact for non-minority households, with all the point estimates being relatively small in magnitude and statistically non-significant.

Table 1 pools data across minority and non-minority households to identify the treatment effect on households' credit access using specification (3). The *Treat* coefficient (β_1) compares credit access for non-minority households across treatment and control districts; the interaction term, $Treat \times Minority$ (β_2), captures the differential effect across minority and non-minority households within treatment districts. All specifications include a linear polynomial in the running variable and state fixed effects, restricting the comparison of household outcomes within the same state. The net treatment effect for

minority households is captured by the sum of β_1 and β_2 and the bottom row of each panel presents p-values from testing the null of $\beta_1 + \beta_2 = 0$. The results in Panel A of Table 1 also include 4 household covariates: dummies for rural location, whether household belongs to OBC group, SC/ST group, and whether the household has any secondary educated member.

The inclusion of covariates slightly attenuates the point estimates but the coefficients remain very similar in both magnitude and precision. We henceforth focus on Panel A, which includes covariates. Column (1) of Table 1 identifies a net 29 percentage point increase in minority households' likelihood of receiving any credit in minority concentration districts. The increased credit access emanates from increased access to both bank and informal sources of credit. Like Figure 2, the increased access to bank credit is driven by an increased likelihood in receiving credit from a bank-linked SHG [column (4)], as opposed to directly receiving loans from commercial banks [column (3)].[9]

The β_1 coefficient, estimating the treatment effect for non-minority households in minority concentration areas is positive and statistically significant for overall credit access and access to informal loans.[10] The point estimate corresponding to commercial bank loans is positive, but imprecisely estimated (p-value .163). This points to an aggregate expansion in financial acess across local credit markets in treated districts. Increase access to bank-linked SHG loans however is exclusively restricted to minority households. This suggests that commercial banks accepted the recommendation of the central bank to collaborate with local SHGs, and were exploiting the screening and monitoring mechanisms available with SHGs to reach minority borrowers.

The coefficients in Table 1 point to large treatment effects as a result of the directed credit policy. 36 percent of minority households in non-minority concentration districts had some outstanding borrowings. A 29 percentage point increase in credit access thereby reflects a near doubling in credit access for minority households in treated regions. Similarly, 6 percent of minority households had some outstanding borrowings from bank-linked self-help groups, implying again a doubling in minority households' credit access from this source. Moreover, the conditional net treatment effects estimated for minority households in treated districts are qualitatively similar to the unconditional treatment effects shown in Figure 2.

[9] For commercial bank loans, the sum of $\beta_1 + \beta_2$ is statistically different from 0 at the 10 percent level and points to a 10 percentage point increase in minority households' likelihood of receiving credit from commercial banks. However, neither of the β_1 and β_2 coefficients are statistically distinguishable from 0, although the point estimates are large in magnitude.

[10] The latter is statistically significant at the 10 percent level (p-value .057).

The loan-level data in the AIDIS includes an indicator for whether a loan is securitized or not. Appendix Table A3 shows a differentially higher likelihood for minority households in treated districts to have some outstanding unsecured loan from both bank-linked SHGs and commercial banks [columns (2)-(4)]. There is however no corresponding treatment effect for non-minority households: the coefficients are positive, but attenuated towards 0 and not statistically significant. Column (5) shows an increase in the likelihood of minority households having unsecured loans from informal sources. In unreported regressions, we see this to be driven by unsecured loans from friends and relatives, as opposed to unsecured loans from professional money-lenders.

Brock and de Haas (2023) documented evidence of implicit gender discrimination in the processing of loan applications in Turkey, even when there was no explicit gender discrimination. Turkish bankers were significantly more likely to require guarantors while approving female loan applications, which were costly for borrowers. Similarly, the results in Appendix Table A3 suggest that if collateral requirements restricted access to bank credit and banks relaxed such requirements for minority borrowers, it can serve as a plausible mechanism in explaining increased credit access for minorities in minority concentration districts. This is particularly relevant when considering that Section 3.3 showed that value of real estate owned by Muslim households, accounting for 80 percent of religious minorities in India, was significantly lower than non-minority households.

The pre-treatment descriptive characteristics for household lending discussed in Section 3.3 showed that within minority communities, Muslim households were particularly disadvantaged in terms of both access to credit, loan size, and cost of informal credit. These households also had limited financial resources, pointing to binding credit constraints faced by these households. This would make Muslim households most likely to respond to the directed credit policy in minority concentration districts. We investigate this in Appendix Table A8 by re-estimating specification (3) after dropping non-Muslim minority households. β_2 now compares the difference in extensive margin loan outcomes across Hindu and Muslim households in minority concentration districts. The results are very comparable, although the double interaction coefficients do lose some precision in columns (1) and (2). This confirms that Muslim households indeed responded to the directed credit policy by expanding their borrowings from formal financial institutions.

5.1.2 Baseline Results: Intensive Margin

Figure 2 and Table 1 showed large treatment effects for minority households' credit access in minority concentration districts. Figure A10 and Appendix Table A4 presents intensive margin results corresponding to the extensive margin findings. All outcome variables are transformed using an inverse hyperbolic sine transformation. We use initial loan size to gauge intensive margin responses, which leaves them unaffected by the value of capitalized interest or delayed repayments. For households with multiple loans (or multiple loans from a source), we aggregate initial loans values to the household level.

Appendix Figure A10 shows large unconditional treatment effects for minority households for aggregate credit, loans from bank-linked SHGs, and informal credit, with no corresponding effect for non-minority households (Appendix Figure A11). Appendix Table A4 pools minority and non-minority households and estimates treatment effects conditional on state fixed effects and household covariates. The outcome of interest in column (1) is aggregate household borrowings, summed across all outstanding loans. The remaining columns correspond to total loans under each category. Panel B shows the regressions without any household covariates while Panel A adds in household covariates. The inclusion of household covariates have little effect on the point estimates and we focus on the results in Panel A including the covariates.

We identify positive coefficients on all β_2 coefficients with the exception of lending from NBFCs [column (5)]. The sum of the β_1 and β_2 coefficients are also significantly different from 0 in all instances with the exception of column (5), implying that credit allocation was significantly larger for minority households in treatment districts. Thus, not only did minority households benefit from higher credit access in minority concentration districts, but the volume of credit received too was substantially higher than observationally equivalent minority households in control regions. The coefficient estimate for non-minority households is positive across all columns, but statistically significant only for aggregate credit received [column (1)] and credit from informal sources [(6)].

We opt for an inverse hyperbolic sine transformation of our outcome variable due to the presence of both households without any borrowings (loan amount equal to 0), and large outlier loan values. Thus, while the median initial loan volume across all loans was INR 50,000, the mean initial loan volume was three times higher (INR 170,000), and a fourth of the households had loan volumes in excess of INR 140,000. Appendix Table A5 in this regard identifies the treatment effect in levels. Panel

A includes household covariates and shows that while the coefficients are directionally equivalent, the point estimates are no longer precisely estimated, with the exception of loans from bank-linked SHGs and informal loans. Columns (4) and (6) show that loans to minority households in treatment districts from bank-linked SHGs (informal sources) increased by INR almost 4600 (INR 17,000). Similar to the extensive margin coefficients, the magnitudes are large when considering that the average loan amount from bank-linked SHGs (informal sources) for minority households in control districts equaled INR 2,000 (13,100). On aggregate, column (1) of Appendix Table A5 points to a INR 20,000 increase in credit for minority households. This however needs to be interpreted with caution, as we cannot reject that the sum of the β_1 and β_2 coefficients are significantly different from 0 at any of the conventional levels of statistical significance (p-value .187).

We can use our baseline results from Tables 1 and A4 to assess whether minority households were credit constrained. Extending Banerjee and Duflo (2014) test for credit constraints, we consider a household to be credit constrained if their aggregate borrowing increases, after receiving access to cheaper sources of credit. The intuition is that if households were not facing a binding credit constraint, access to cheaper credit from financial institutions would result in a reallocation of the household's portfolio, with the household substituting relatively expensive credit from informal sources with cheaper formal credit. Both out extensive and intensive margin results however indicate that minority households in minority concentration districts were credit-constrained. Despite access to cheaper credit from bank-linked SHGs in treated districts, aggregate borrowings of minority households do increase, and there is no evidence of a reduction in borrowing from informal sources.[11] With the interest rate differential across bank and informal sources exceeding 20 percentage points, the absence of any reduction in informal credit, despite access to cheaper bank credit, points to minority households being credit constrained.

5.1.3 Robustness

We subject the baseline results in Table 1 to a number of robustness checks. Figure 2 shows robustness to alternate bandwidths. All results shown till now use our preferred optimal bandwidth of .07, based on the data-driven optimal bandwidth calculations of Calonico et al. (2020). Figure 2 shows the β_1

[11] This applies even in the case of borrowing from professional money lenders charging much higher rates of interest than banks or bank-linked SHGs.

and β_2 coefficients after estimating specification (3) for 8 alternate bandwidths between .05 and .09. The top panel shows the differential treatment effect for minority households (β_2); the bottom panel the treatment effect for non-minority households in treated districts (β_1). The coefficient plots show the coefficients to be stable to the 8 alternate bandwidths.

Appendix Table A6 re-estimates the baseline specifications using outcome-specific MSERD bandwidths, computed using the methodology of Calonico et al. (2020).[12]. We first note that with the exception of columns (4) and (6), the optimal MSERD bandwidth for the remaining outcomes is larger than our preferred bandwidth of .07. In columns (4) and (6), the optimal bandwidth is .064 – marginally smaller than our preferred bandwidth of .07. Regardless, as seen from Appendix Table A6, using the MSERD optimal bandwidths yields results which are almost identical to those estimated using the bandwidth of .07. Consequently, we continue to use our preferred bandwidth of .07 for all subsequent specifications.

Our preferred specification in equation (3) pool data across minority and non-minority households with the net treatment effect for minority households in treated districts being estimated from adding the β_1 and β_2 coefficients. Panels A and B of Appendix Table **??** shows the results to be qualitatively similar if we separately estimate local linear regressions for minority and non-minority households. All specifications are estimated using the methodology outlined in Calonico et al. (2020), and include state fixed effects and household covariates. We include a triangular kernel to assign greater weight to observations near the discontinuity threshold. While the reduction in sample size affects the precision of the coefficients, the treatment effects are very similar in magnitude to those obtained in Table 1. Panel C of Appendix Table **??** replicates the specifications in Panel A, but replaces the linear fit with a quadratic fit. While the precision declines, the results remain very similar in magnitude and direction.

As our treatment varies at the level of district, we estimate our baseline results after controling for state fixed effects and select household characteristics. Panel A of Appendix Table A7 shows the estimation of β_1 and β_2 to be unaltered by the inclusion of pre-treatment district covariates. This is what we would expect if the treatment assignment was orthogonal to district characteristics within a narrow window around the discontinuity threshold. Panel B presents our most restrictive specification including district fixed effects. Replacing state fixed effects with district fixed effects

[12] To estimate the MSERD bandwidths, we first estimate the optimal bandwidth for each outcome using the method of Calonico et al. (2020), separately for minority and non-minority households. The minimum of these two MSERD bandwidths is subsequently selected as the outcome specific optimal MSERD bandwidth.

effectively compares loan outcomes across minority and non-minority households within the same treated district. We can no longer identify the β_1 coefficient but the differential treatment effect for minority households (β_2) can still be identified. Including district fixed effects does not affect the estimation of the double-difference β_2 coefficient, indicating that treatment and control districts were comparable along both observable and unobservable dimensions in the neighbourhood of the discontinuity threshold. Appendix Table A7 thereby underlines the robustness of our empirical strategy.

5.1.4 Purpose of Borrowing

The AIDIS provides self-reported information on households' purpose of borrowing. We aggregate these to construct 4 broad categories of borrowing: farm credit, non-farm business credit, credit for health or education spending, and consumption loans. We separate these across bank and non-bank sources, with loans obtained from bank-linked SHGs being classified under bank lending. Table 2 uses specification (3) to identify that bank lending for minority households in minority concentration districts was undertaken primarily for non-farm business and consumption purposes. With 5 (3) percent of minority households having outstanding loans for non-farm business purposes (from banks), the treatment effects in Table 2 are again large in magnitude.

In contrast, the aggregate extensive margin increase in lending for non-minority households in Table 1 was primarily driven by an increase in farm loans [column (1)], and bank-originated farm loans [column (2)]. The interaction term estimating heterogeneity for minority households in treated districts are negative in both instances, but not statistically different from 0. This points to an overall expansion in farm credit access in minority concentration districts from both bank and non-bank sources, for both minority and non-minority households.

Appendix Table A9 shows the intensive margin treatment effects corresponding to Table 2. Panel A considers an inverse hyperbolic sine transformation of loan amounts; Panel B shows the specification in levels. The intensive margin coefficients in Panel A are very similar to the extensive margin coefficients. The double interaction coefficients in Panel B remain positive, but are not precisely estimated. Nonetheless, the coefficients signify an INR 4,000 increase in overall business loans, and an INR 3,000 increase in business loans from banks.

5.1.5 Cost of Credit

We information on annual interest rates associated with each loan to identify the whether the directed credit policy in minority concentration areas affected the cost of credit. We aggregate loan-specific interest rates to the level of households by calculating loan-volume weighted averages across all loans obtained by the household. A priori, the cost of bank credit can both increase or decrease as a result of the policy intervention. If banks perceive minority borrowers to be risky, and interest rates capture credit risk, banks could have increased lending to minority borrowers in response to the policy, but charged an interest premium to cover the increased riskiness of their loans. Alternatively, if the cost of credit served as a constraint for minority borrowers in accessing bank loans, banks could have opted to lower lending rates for minority borrowers in order to comply with the policy.

Column (1) of Table 3 points to an overall 3 percentage point increase in lending rates in treated districts for non-minority households. The standard errors are sufficiently wide to disallow us from rejecting the null of a zero effect at conventional levels of significance (p-value .138). There is also no evidence of a differential effect across minority households. There is however evidence of a large reduction in the interest rates on bank loans for non-minority households in treated districts. This emanates from a reduction in interest rates on loans issued by bank-linked SHGs [column (4)], as opposed to loans issued by commercial banks [column (3)]. Importantly though, the β_2 coefficient is positive and statistically signficant across both columns (2) and (4) (p-values .007 and .082). This implies that while the cost of credit from bank-linked SHGs declined for non-minority borrowers in treated districts, the reduction in lending rates was not extended to minority borrowers. This would be consistent with bank-linked SHGs perceiving minority borrowers to be riskier. Overall, while credit access for minority borrowers in minority-concentration districts increased along both the extensive and intensive margins, this was not accompanied by lower costs of lending. Finally, column (5) shows that the aggregate increase in lending rates in treated districts seen in column (1) was driven by an increase in interest rates on informal loans. There is no evidence of a differential effect for minority borrowers in treated districts: if anything, the evidence points to lending rates being unchanged for minority borrowers in minority concentration districts.

5.1.6 Loan 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 were avoiding lending to such borrowers in an effort to mitigate credit risk, compliance with the policy would likely result in higher loan delinquency. Alternately, if financial institutions had limited information and screening mechanisms for minority borrowers, directed lending in the presence of major information asymmetries can also worsen loan performance.

Section 3.1 lists our primary self-reported measure of loan performance: no repayment for at least 6 months. In addition, we also use self-reported information on writeoffs. We classify a household to be delinquent if it reports any of its outstanding loans to be delinquent. Table 4 documents an aggregate increase in delinquency in minority concentration districts. This is concentrated amongst bank loans, and loans issued to non-minority borrowers. Column (3) of Table 4 reports a near 30 percentage point increase in delinquency (Panel A), and an accompanying 12 percentage point increase in writeoffs [Panel B] for commercial bank loans for non-minority borrowers in treated districts. There is no evidence of a differential effect across minority borrowers, although the interaction terms are negative. Intriguingly, the increase in deliquency and writeoffs are not observed for loans obtained from bank-linked SHGs or informal sources [columns (4) and (5)]. The β_1 coefficients are attenuated towards 0 and not statistically significant. With limited information on lenders, we cannot pinpoint whether this is due to improved screening mechanisms or monitoring capabilities of SHGs and informal lenders. Broadly though, the evidence suggests that local financial institutions were more effective in preventing loan delinquencies in minority concentration districts. From a policy perspective, Table 4 suggests that commercial banks should consider expanding their linkages with self-help groups to undertake lending to minority borrowers. Increased lending through SHGs to informationally opaque marginalized borrowers leaves credit quality unaffected, despite transactions with a greater number of borrowers and an increase in the volume of credit disbursed. Involving SHGs to lend to minority borrowers also does not involve an increase in the cost of credit.

5.1.7 Household Productive Assets

We conclude our empirical analysis of the AIDIS data by estimating the treatment's impact on households' ownership of productive assets. The AIDIS provides information on real estate holdings by

households, and also any ownership of machinery or fixed assets for productive purposes. The results in Appendix Table A10 are noisy but offer suggestive evidence of an increase in households' ownership of machinery and productive assets along both the extensive and intensive margins. There is limited evidence of an increase in productive assets for non-minority households in minority concentration districts, but an aggregate increase in household machinery for minority households by approximately INR 1,500 (p-value .039), and productive assets by INR 4,000 (p-value .086). The magnitude of increase in productive assets is strikingly similar to the quantum of business loans received by minority households from banks in treated districts. While the coefficients in Appendix Table A10 should be interpreted with caution owing to the large standard errors, it offers suggestive evidence that the increase in non-farm business loans from banks to minority borrowers in minority concentration districts was used to increase the ownership of productive business assets by minority households.

5.2 Labour Market Outcomes

Section 5.1 showed that directed credit allocation for minorities in minority concentration districts substantially increased access to bank credit for minority households, particularly from bank-linked SHGs. The increased credit was primarily used to finance consumption and engage in non-farm business activities. We now examine using the PLFS data whether the increase in business credit and household ownership of productive assets affected labour market outcomes for minority households.

We use specification (3) to estimate the treatment effect on labour market outcomes along the extensive margin. Unlike Section 5.1, the unit of observation now is the individual. We augment specification (3) with month dummies to account for seasonality in labour market outcomes. In addition to the existing household covariates, we also account for individual covariates such as gender, a quadratic in age, and dummies for educational qualifactions. Table 5 presents the baseline treatment effects: Panel A includes both household and individual covariates in addition state and month dummies; Panel B excludes all covariates with the exception of state and month fixed effects. All specifications are restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. The covariates have little effect on the coefficient magnitudes but do improve their precision, and we focus our discussion on the results in Panel A.

Column (1) includes all working-aged individuals aged between 20 and 60 and identifies no treatment effect on labour force participation for minority and non-minority individuals. The point

estimates corresponding to both the β_1 and β_2 coefficients are negative, incicating lower labour force participation rates in treated districts, but the confidence intervals are too wide to reject a null effect. As the treatment left unaffected workers' labour force participation choices, columns (2)-(7) restrict the sample to working-aged individuals who also participated in the labour force.

Consistent with an increase in farm credit for non-minorities in minority concentration districts, we see an increased likelihood of non-minority workers being engaged in farm activities [column (3)], albeit significant at the 10% level (p-value .074). The differential effect for minority workers is however negative, and effectively reverses the positive treatment effect for non-minority workers. Instead, we identify a large and statistically significant impact on the likelihood of minority workers being employed in manufacturing activities, with the double interaction coefficient being statistically significant at the 5% level. The remaining coefficients, with the exception of a reduction in non-minority workers' likelihood of being engaged in trading activities, are statistically non-significant. The double interaction coefficients for minority workers' participation in services, and the likelihood of being unemployed, are both negative and large in magnitude, but imprecisely estimated.

The PLFS reports the hours of work allocated to each activity during the week, which we use to identify the treatment's impact on labour market outcomes along the intensive margin. The results in Appendix Table A11 are very similar to the extensive margin findings in Table 5 – while aggregate hours worked during the week remains unaffected [column (2)], non-minority workers in treated districts spent 5 additional hours working in farm activities. This positive treatment effect is however reversed for minority workers. Instead, minority workers spent 4 additional hours in manufacturing work during the week [column (4)]. Considering an 8 hour work day, this amounts to an additional half a days labour in manufacturing work, or a 50 percent increase relative to minority workers in control districts. While the double interaction term is not precisely estimated, column (6) points to a 5 hour decline in service activities for minority workers in treated districts, pointing a reallocation from services to manufacturing work for minorty workers in treated districts.

Section 5.1 showed that the treatment had a causal impact on non-farm business loans for minority households along both the intensive and the extensive margins. Section 5.1.7 also presents evidence of an increase in minority households' ownership of machinery and fixed assets. The results in Table 5 and Appendix Table A11 would be consistent with the above findings if the increase in manufacturing work is driven by self-employed workers, running proprietary or partnership enterprises, which qualified

for bank credit under the directed credit policy. We test this by using the PLFS data on workers' location of employment. For each worker, the PLFS enquires about the usual location of the workplace. Location of workplace is broadly classified into the worker's own establishment located within their home, the worker's own establishment in a location distinct from their home, and establishments owned by the employer. We exploit this information to identify whether the treatment affected minority workers' likelihood of being self-employed, and working in enterprises operated from their own home.

The odd numbered columns in Appendix Table A12 are dummies equaling 1 if the worker operates his or her own enterprise; the even numbered columns in Appendix Table A12 equal 1 if the worker operates their enterprise from the premises of their home. Column (1) reports an aggregate increase in the likelihood of being self-employed for minority workers, while columns (3) and (5) confirm that the increase in self-employment is present for non-farm activities, and manufacturing work. The even-numbered columns support the explanation that the increased likelihood of self-employment extended to workers operating establishments from their home. The increase in non-farm and manufacturing self-employment, as well as operating home enterprises is restricted to minority workers. The β_1 coefficients remain small and statistically indistinguishable from 0. The findings of Appendix Table A12 is consistent with the explanation that the expansion in business credit from banks in minority concentration districts increased minority households ownership of productive assets, and boosted manufacturing work undertaken from self-employed home-operated establishments.

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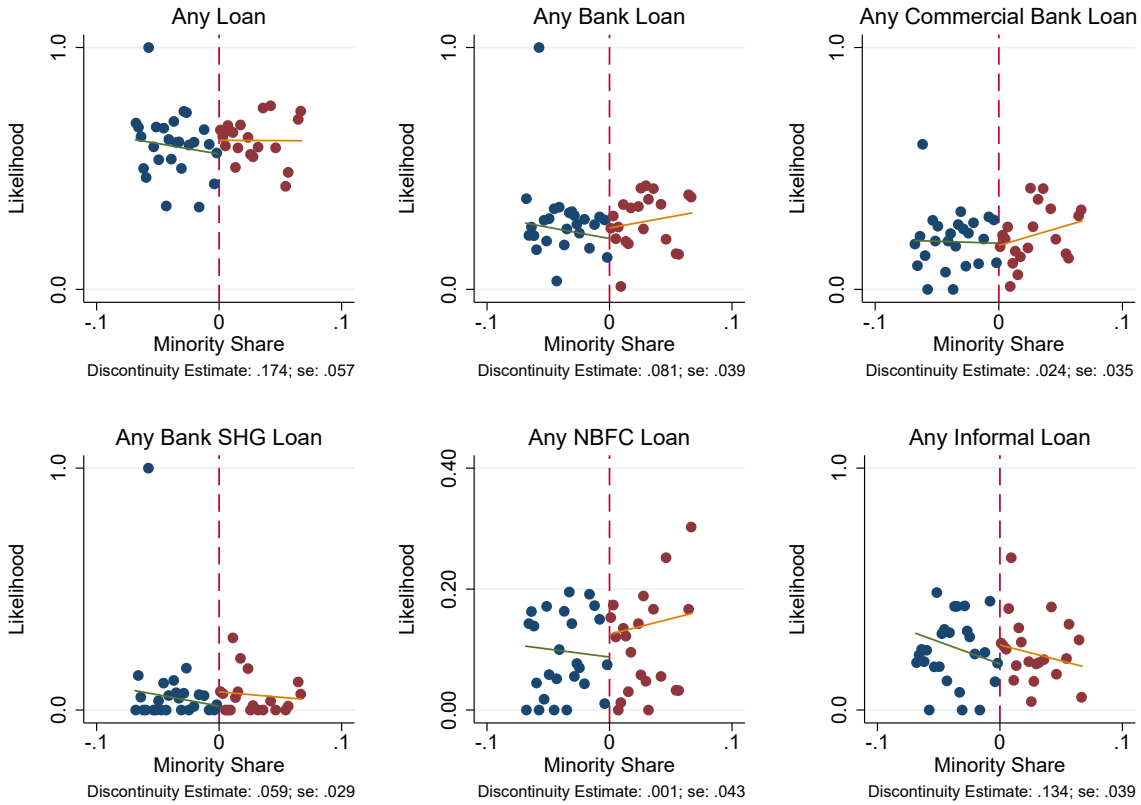
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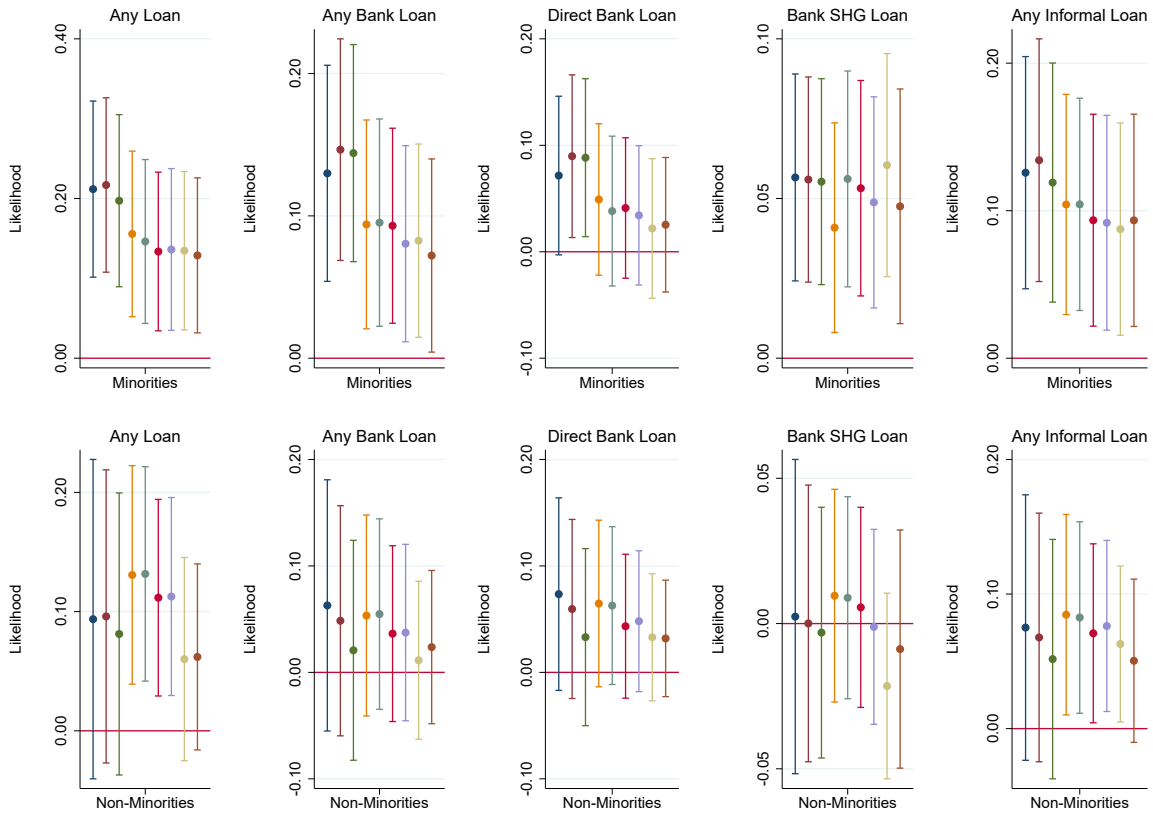
6 Figures

Figure 2: Minority Credit in Minority Concentration Districts: Extensive Margin, RD Plots



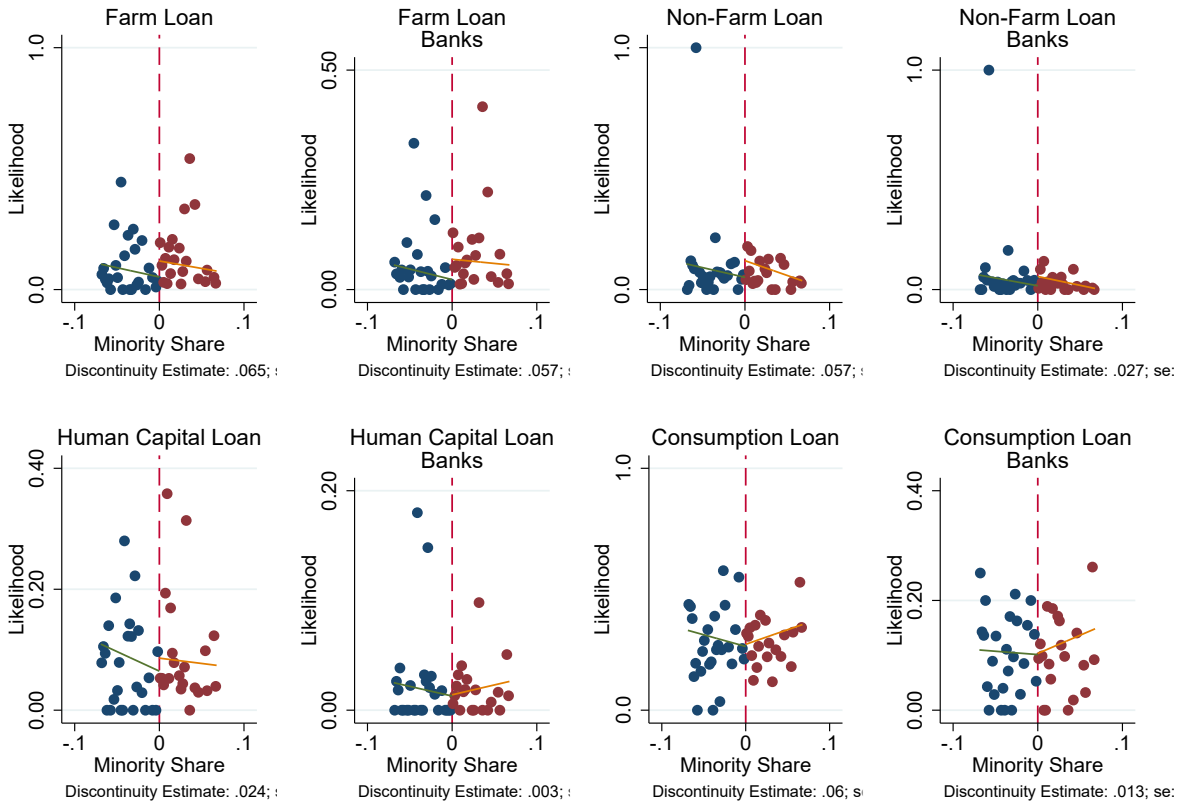
Notes: This figure shows unconditional RD plots comparing extensive margin credit access for minority households across treatment and control districts. The unit of observation is the household. The outcome of interest is a dummy, equal to 1 if the household has any outstanding loan in the categories mentioned. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. Standard errors are clustered by district.

Figure 2: Minority Credit in Minority Concentration Districts: Extensive Margin, RD Plots



Notes: The above figure shows robustness of the baseline results in Table 1 to alternate bandwidths. The unit of observation is the household. The outcome of interest is a dummy equaling 1 if the household has an outstanding loan for each category noted. Each point denotes the coefficient estimate, with the vertical lines representing 95% confidence intervals. The coefficients are estimated using specification (3) and include state fixed effects, a linear polynomial in the running variable and household covariates. The coefficient estimates in the bottom row estimate the treatment effect for non-minority households (β_1); those in the top row estimate the differential treatment effect for minority households (β_2). The first coefficient in each figure is estimated using a bandwidth of .05 around the discontinuity threshold; the last coefficient uses a bandwidth of .09. The bandwidth is expanded by 0.5 for each subsequent coefficient. Standard errors are clustered by district.

Figure 3: Minority Credit in Minority Concentration Districts: Purpose of Borrowing, Extensive Margin, RD Plots



Notes: This figure shows unconditional RD plots comparing the purpose of borrowing along the extensive margin for minority households across treatment and control districts. The unit of observation is the household. The outcome of interest is a dummy, equal to 1 if the household has any outstanding loan in the categories mentioned. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. The first and third columns consider loans from any source for the purposes mentioned; the second and third columns consider loans received directly from banks or bank-linked SHGs. *Human Capital* loans refer to loans taken to finance education or health expenditures. Standard errors are clustered by district.

7 Tables

Table 1: Minority Credit in Minority Concentration Districts: Extensive Margin

			Panel A: Controls			
	(1)	(2)	(3)	(4)	(5)	(6)
				Pr(Loan = 1)		
	Any Source	Banks	Commercial Bank	Bank-Linked SHG	NBFCs	Informal
Treat	.132**	.055	.063	.009	.030	.083*
	(.054)	(.054)	(.045)	(.021)	(.024)	(.043)
Minority × Treat	.146**	.095**	.038	.056***	-.040	.104**
	(.062)	(.044)	(.042)	(.020)	(.036)	(.043)
Observations	20786	20786	20786	20786	20786	20786
R^2	.13	.10	.07	.13	.05	.15
$\beta_1 + \beta_2 = 0$	.000	.016	.032	.025	.657	.001
Control Mean	.35	.15	.10	.06	.07	.16
			Panel B: No Controls			
	(1)	(2)	(3)	(4)	(5)	(6)
				Pr(Loan = 1)		
	Any Source	Banks	Commercial Bank	Bank-Linked SHG	NBFCs	Informal
Treat	.151**	.059	.062	.014	.034	.095**
	(.062)	(.055)	(.043)	(.022)	(.024)	(.048)
Minority × Treat	.143**	.095**	.039	.054***	-.048	.111**
	(.069)	(.042)	(.040)	(.020)	(.037)	(.048)
Observations	20786	20786	20786	20786	20786	20786
R^2	.12	.09	.06	.13	.05	.14
$\beta_1 + \beta_2 = 0$	.000	.022	.037	.028	.754	.001

Notes: The above table shows the treatment effect on credit access along the extensive margin. The unit of observation is the household. The outcome of interest is a dummy equaling 1 if the household had any outstanding credit from the sources mentioned. *Bank* in column (2) refers to loans received directly from commercial banks, or through bank-linked SHGs. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Minority \times Treat$ estimates the differential treatment effect for minority households. All specifications include state fixed effects and a linear polynomial in the running variable. Coefficient estimates in Panel A also include household covariates. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority households in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 2: Minority Credit in Minority Concentration Districts: Purpose of Borrowing, Extensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Loan = 1)							
	Farm Loan		Non-Farm Loan		Human Capital Loan		Consumption Loan	
	Any Source	Bank Loan	Any Source	Bank Loan	Any Source	Bank Loan	Any Source	Bank Loan
Treat	.165***	.073**	-.008	-.007	.028	.004	.074*	.013
	(.044)	(.036)	(.011)	(.007)	(.020)	(.009)	(.038)	(.038)
Minority × Treat	-.040	-.003	.059**	.032*	.033	.004	.120***	.074**
	(.050)	(.032)	(.023)	(.016)	(.021)	(.010)	(.041)	(.031)
Observations	20786	20786	20786	20786	20786	20786	20786	20786
R^2	.17	.10	.08	.10	.05	.04	.07	.06
$\beta_1 + \beta_2 = 0$	.013	.042	.010	.077	.024	.359	.000	.059
Control Mean	.07	.03	.05	.03	.06	.01	.18	.06

Notes: The above table shows the treatment effect for households' purpose of borrowing along the extensive margin. The unit of observation is the household. The outcome of interest is a dummy equaling 1 if the household had any outstanding credit for the purposes mentioned. The outcome of interest in the odd-numbered columns consider loans from any source; in the even-numbered columns, loans obtained directly from banks or bank-linked SHGs. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Minority \times Treat$ estimates the differential treatment effect for minority households. All specifications include state fixed effects and a linear polynomial in the running variable. Coefficient estimates in Panel A also include household covariates. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority households in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 3: Cost of Credit in Minority Concentration Districts

	(1)	(2)	(3)	(4)	(5)
	Annual Interest Rate				
	All Sources	Banks	Commercial Bank	Bank-Linked SHG	Informal
Treat	.031	-.026**	-.030	-.010	.133***
	(.020)	(.010)	(.025)	(.011)	(.044)
Minority × Treat	.006	.031***	.042*	.008	-.048
	(.020)	(.011)	(.024)	(.011)	(.055)
Observations	12722	5917	1091	4980	4922
R^2	.14	.26	.41	.32	.24
$\beta_1 + \beta_2 = 0$	.171	.726	.622	.890	.186
Control Mean	.15	.11	.11	.10	.19

Notes: The above table shows the treatment effect on annual interest rates for loans. The unit of observation is the household. The outcome of interest is loan volume weighted interest rates from the sources mentioned. *Bank* in column (2) refers to loans received directly from commercial banks, or through bank-linked SHGs. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Minority \times Treat$ estimates the differential treatment effect for minority households. All specifications include state fixed effects and a linear polynomial in the running variable. Coefficient estimates in Panel A also include household covariates. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority households in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 4: Credit Quality in Minority Concentration Districts

			Panel A: Non-Repayment		
	(1)	(2)	(3)	(4)	(5)
			Pr(Non-Repayment = 1)		
	Any Loan	Banks	Commercial Bank	Bank-Linked SHG	Informal
Treat	.298***	.258**	.287**	.024	-.148
	(.098)	(.121)	(.116)	(.115)	(.140)
Minority × Treat	-.067	-.127	-.052	-.051	.077
	(.076)	(.109)	(.129)	(.110)	(.121)
Observations	11653	5073	4350	952	3894
R^2	.29	.38	.47	.27	.20
$\beta_1 + \beta_2 = 0$	.031	.432	.185	.827	.710
Control Mean	.36	.24	.31	.09	.59
			Panel B: Writeoffs		
	(1)	(2)	(3)	(4)	(5)
			Pr(Writeoff = 1)		
	Any Loan	Bank	Commercial Bank	Bank-Linked SHG	Informal
Treat	.063***	.112***	.121***	-.005	.029
	(.021)	(.040)	(.042)	(.004)	(.018)
Minority × Treat	-.069	-.016	-.020	.011	-.011
	(.055)	(.013)	(.018)	(.012)	(.011)
Observations	11839	4706	4016	799	3571
R^2	.05	.07	.08	.05	.03
$\beta_1 + \beta_2 = 0$	.900	.011	.017	.487	.071
Control Mean	.01	.00	.00	.01	.00

Notes: The above table shows the treatment effect on household delinquency. The unit of observation is the household. The outcome of interest in Panel A is a dummy equaling 1 if the household had any delinquent loan for each source; in Panel B, a dummy equaling 1 if the household received a writeoff on a loan for each source. *Bank* in column (2) refers to loans received directly from commercial banks, or through bank-linked SHGs. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Minority \times Treat$ estimates the differential treatment effect for minority households. All specifications include state fixed effects and a linear polynomial in the running variable. Coefficient estimates in Panel A also include household covariates. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority households in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

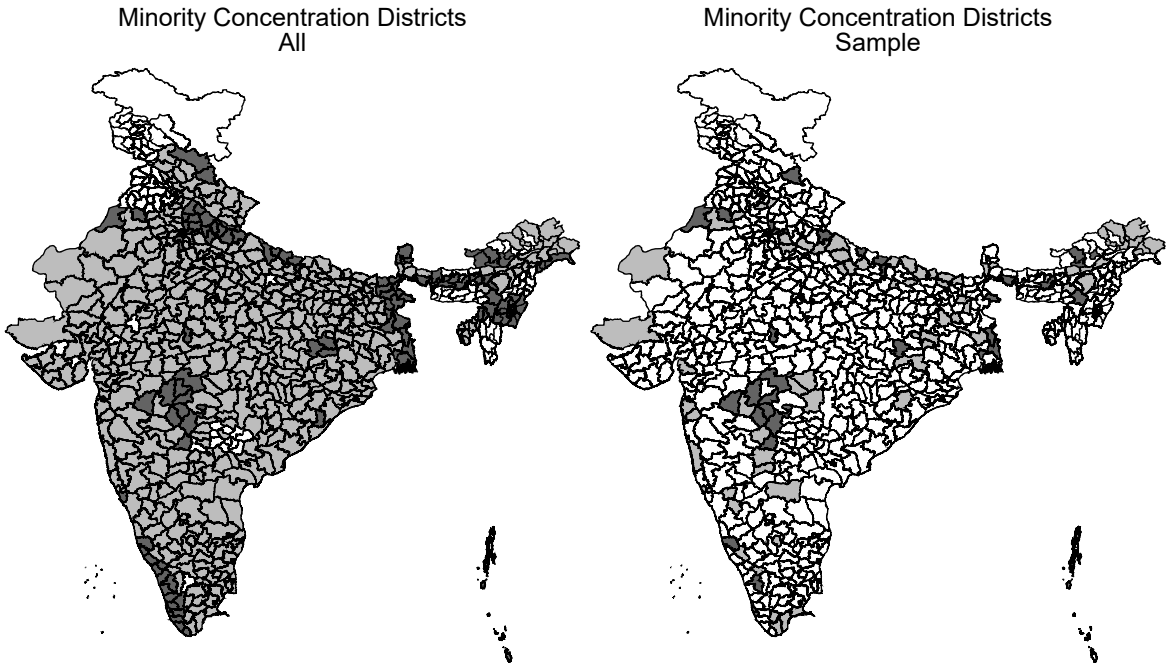
Table 5: Labour Market Outcomes in Minority Concentration Districts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Pr(Outcome = 1)			
	Labour Force Participation	Unemployed	Farm	Manufacturing	Trade	Construction	Services
Treat	-.049	-.020	.127*	-.036	-.033**	-.021	-.005
	(.034)	(.020)	(.071)	(.037)	(.016)	(.022)	(.027)
Minority × Treat	-.034	-.019	-.100	.123**	.045	.027	-.062
	(.040)	(.039)	(.079)	(.053)	(.038)	(.047)	(.062)
Observations	44760	24406	24406	24406	24406	24406	24406
R^2	.49	.11	.38	.07	.05	.06	.16
$\beta_1 + \beta_2 = 0$	.121	.367	.770	.103	.747	.899	.271
Control Mean	.52	.10	.28	.14	.12	.12	.22

Notes: The above table shows the treatment effect on labour market outcomes along the extensive margin. The unit of observation is the working-aged individual. Working-aged individuals are individuals aged between 18 and 60 years. Columns (2)-(7) restrict the sample to working aged individuals who participated in the labour force during the week prior to the survey. 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)-(7), a dummy equal to 1 if the individiual was engaged in the activity referred. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority individuals; $Minority \times Treat$ estimates the differential treatment effect for minority individuals. All specifications include state fixed effects, a linear polynomial in the running variable, and individual demographic and education controls. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority individuals in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

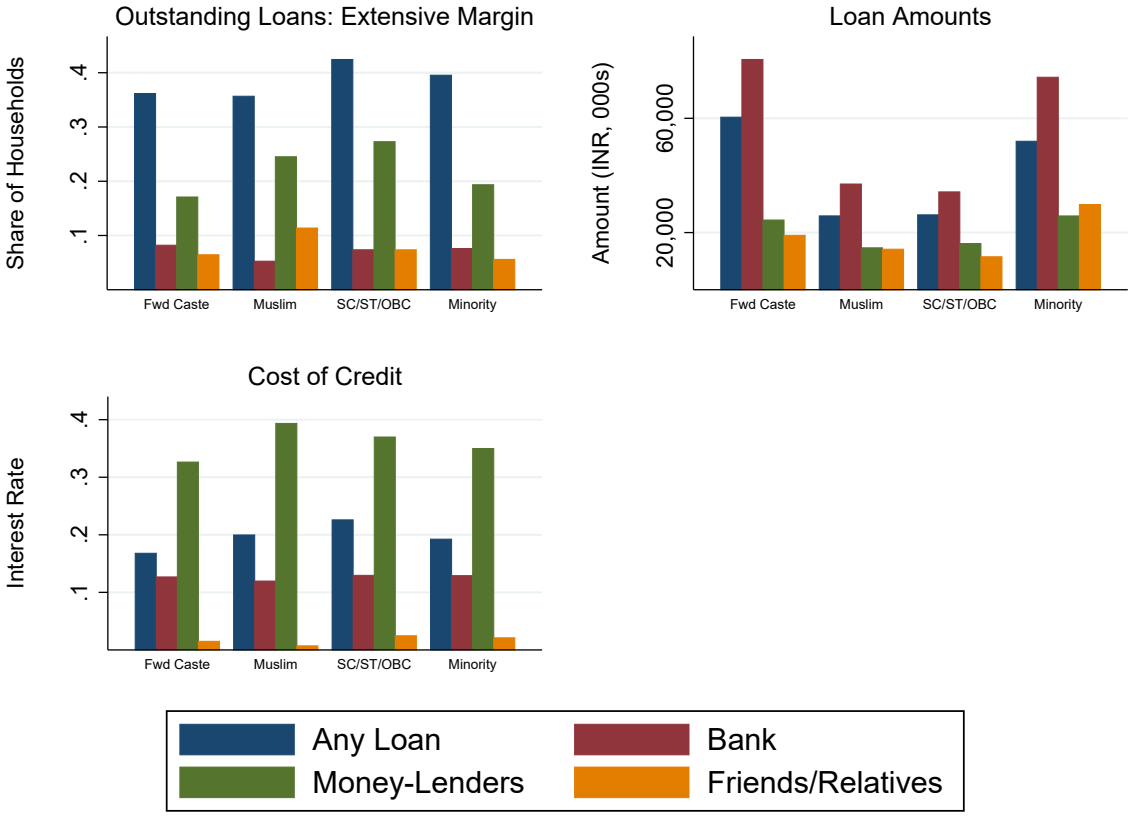
A Figures

Figure A1: Pre-Treatment Household Credit Across Communities



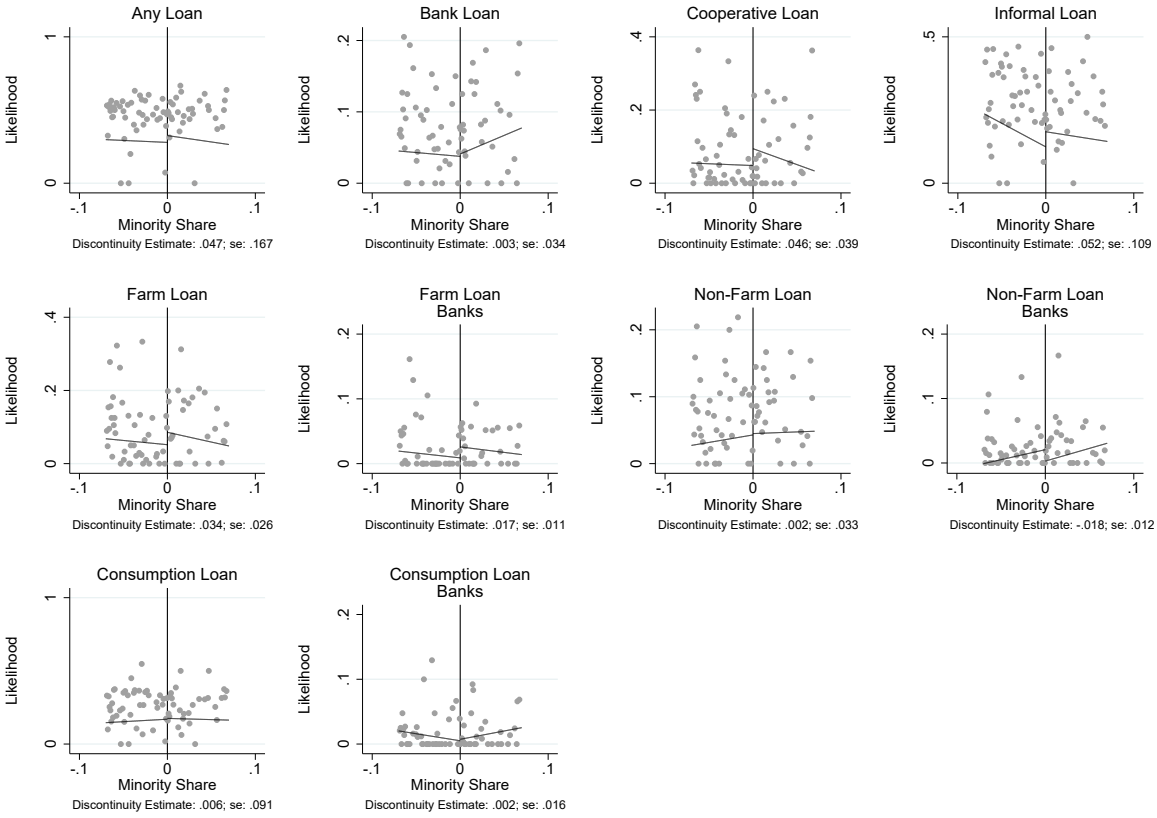
Notes: The above figure shows the geographical distribution of minority concentration districts. The left-panel shows all minority concentration districts (darker shade); the right panel shows minority concentration districts located within a bandwidth of .07 from the discontinuity threshold, which forms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

Figure A2: Pre-Treatment Household Credit Across Communities



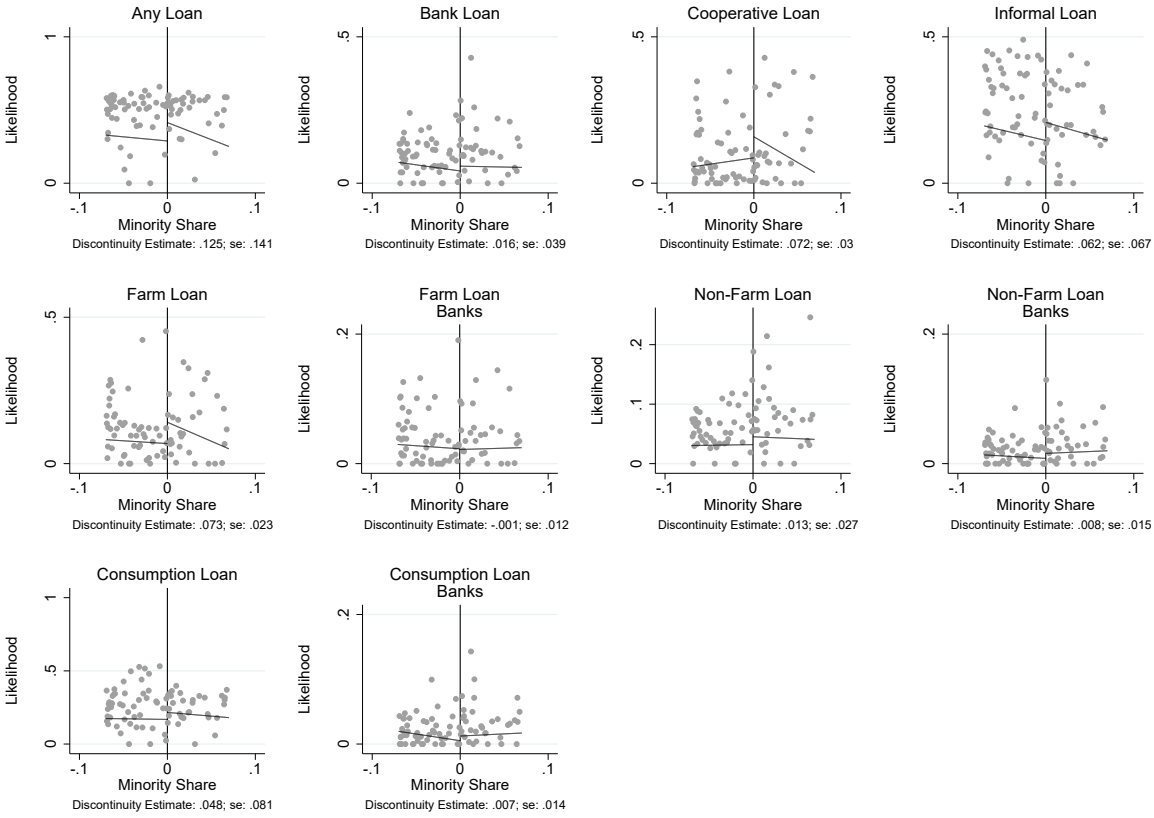
Notes: The above figure presents pre-treatment descriptive statistics for loan outcomes by community. The top-right panel shows the likelihood of households having loans from any of the above-mentioned sources; the top left-panel shows the average volume of credit received by households from each source;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

Figure A3: Pre-Treatment Balance in Minority Credit Outcomes: Extensive Margin



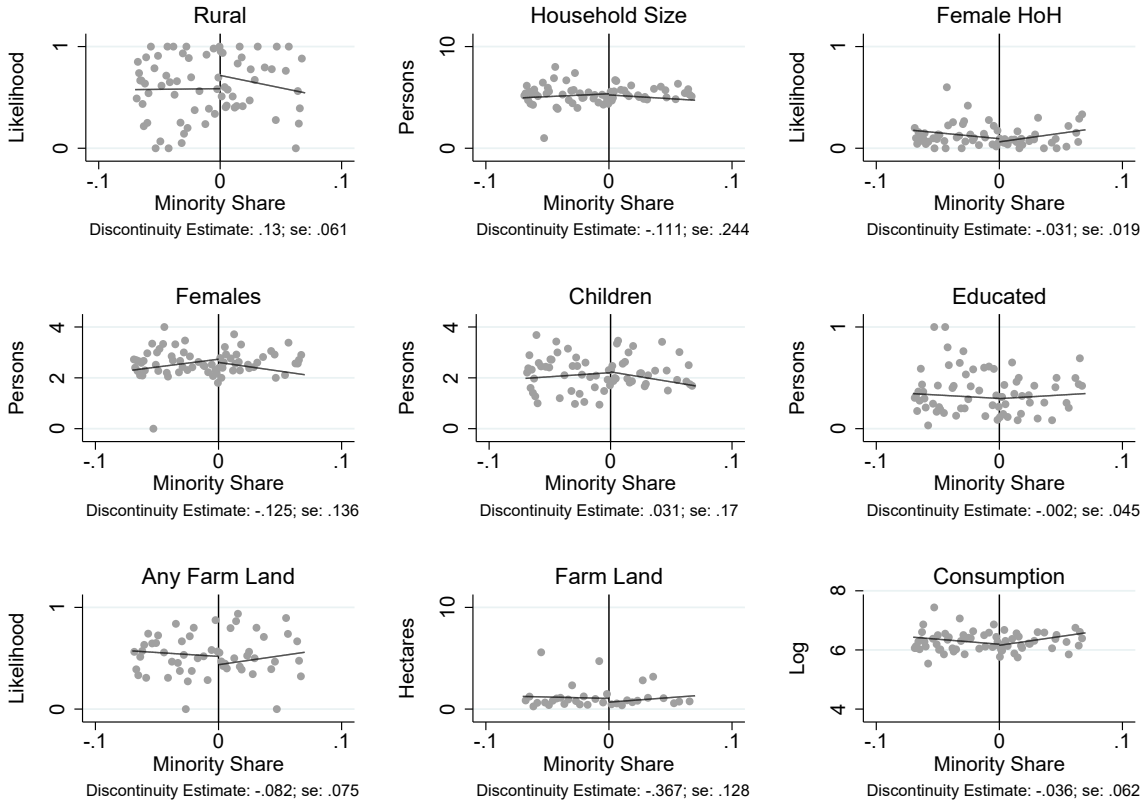
Notes: This figure shows unconditional RD plots showing balance in pre-treatment extensive margin loan outcomes for minority households across treatment and control districts. The unit of observation is the household. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. Standard errors are clustered by district.

Figure A4: Pre-Treatment Balance in Non-Minority Credit Outcomes: Extensive Margin



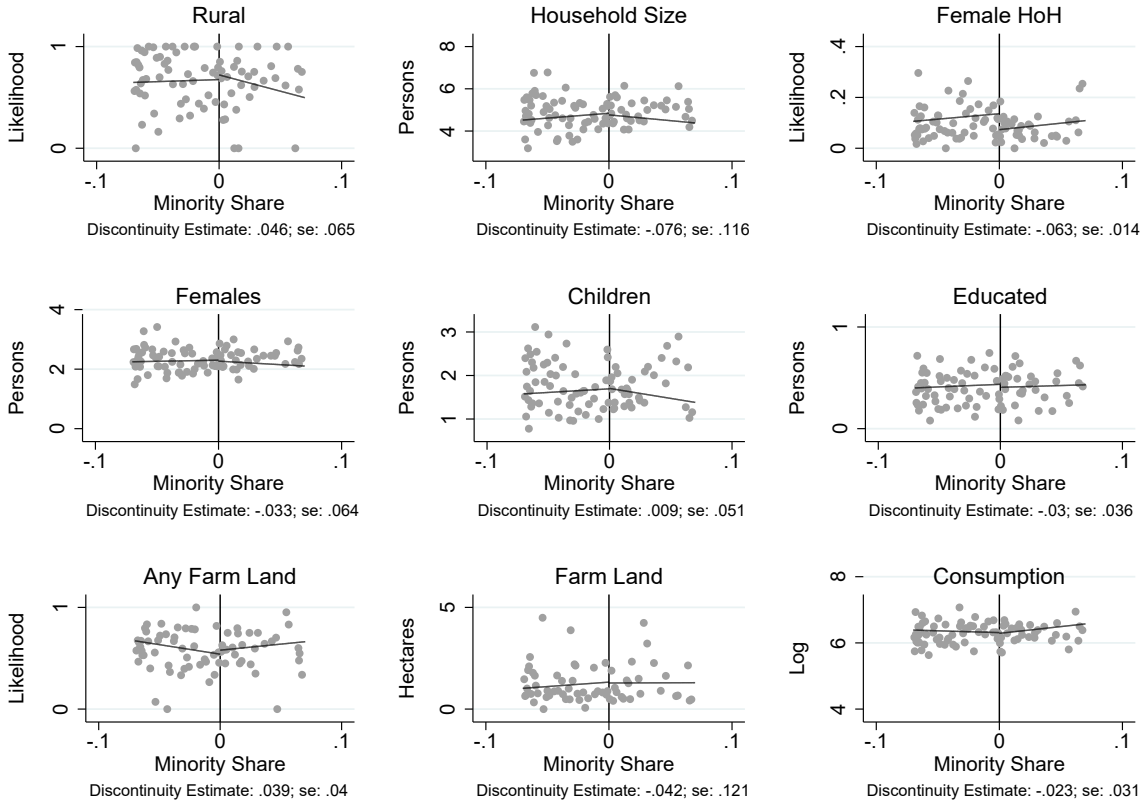
Notes: This figure shows unconditional RD plots showing balance in pre-treatment loan outcomes along the extensive margin for non-minority households across treatment and control districts. The unit of observation is the household. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. Standard errors are clustered by district.

Figure A5: Pre-Treatment Balance in Minority Household Characteristics



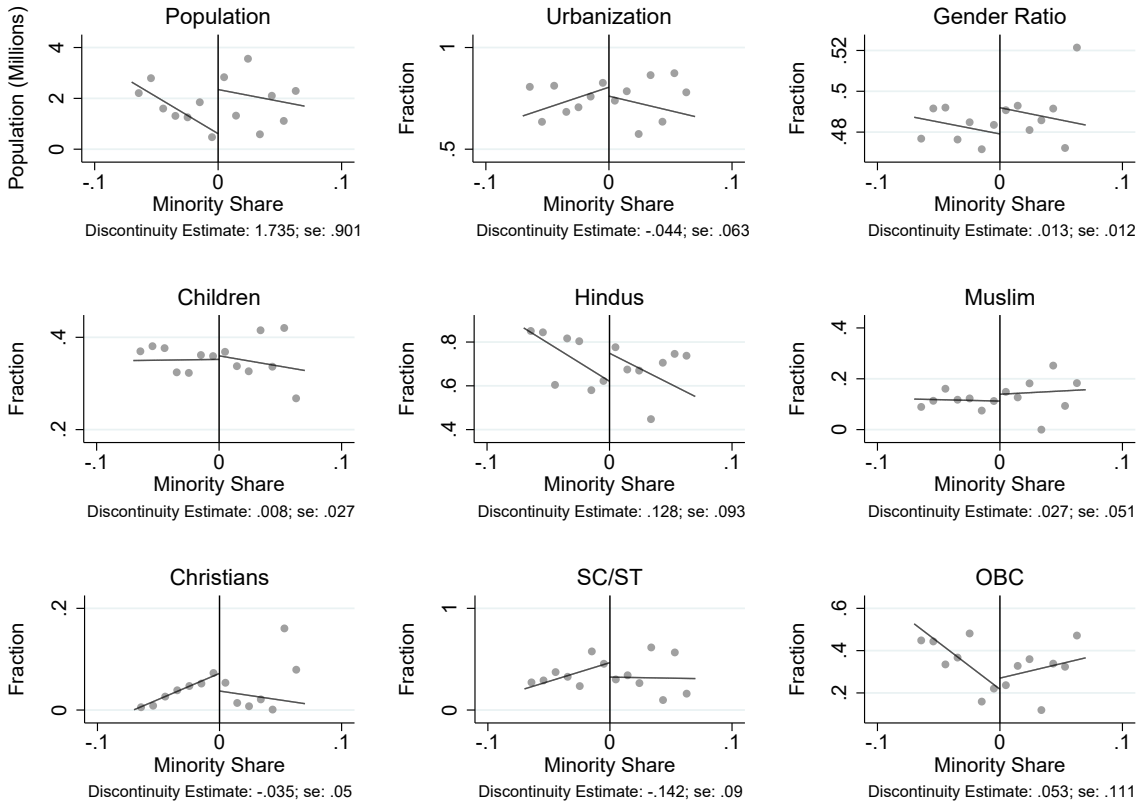
Notes: This figure shows unconditional RD plots showing balance in pre-treatment household characteristics for minority households across treatment and control districts. The unit of observation is the household. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. Standard errors are clustered by district.

Figure A6: Pre-Treatment Balance in Non-Minority Household Characteristics



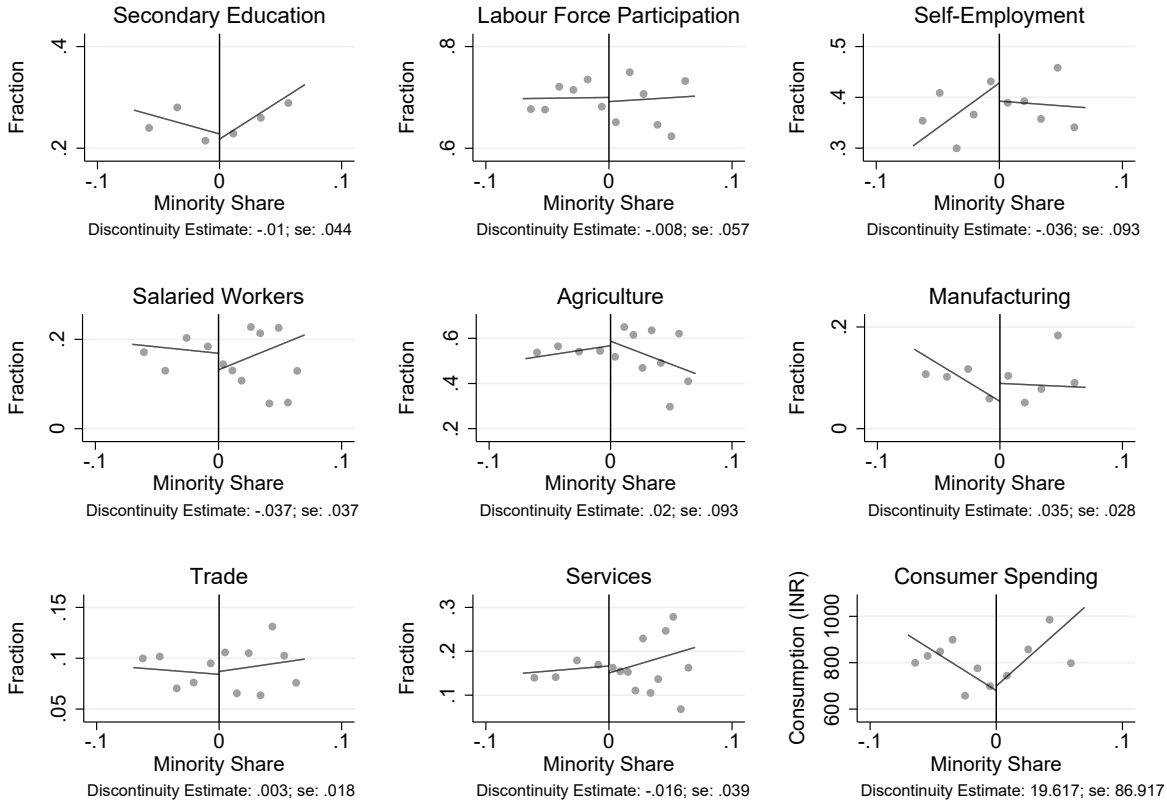
Notes: This figure shows unconditional RD plots showing balance in pre-treatment household characteristics for non-minority households across treatment and control districts. The unit of observation is the household. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. Standard errors are clustered by district.

Figure A7: Pre-Treatment Balance in District Demographic Characteristics



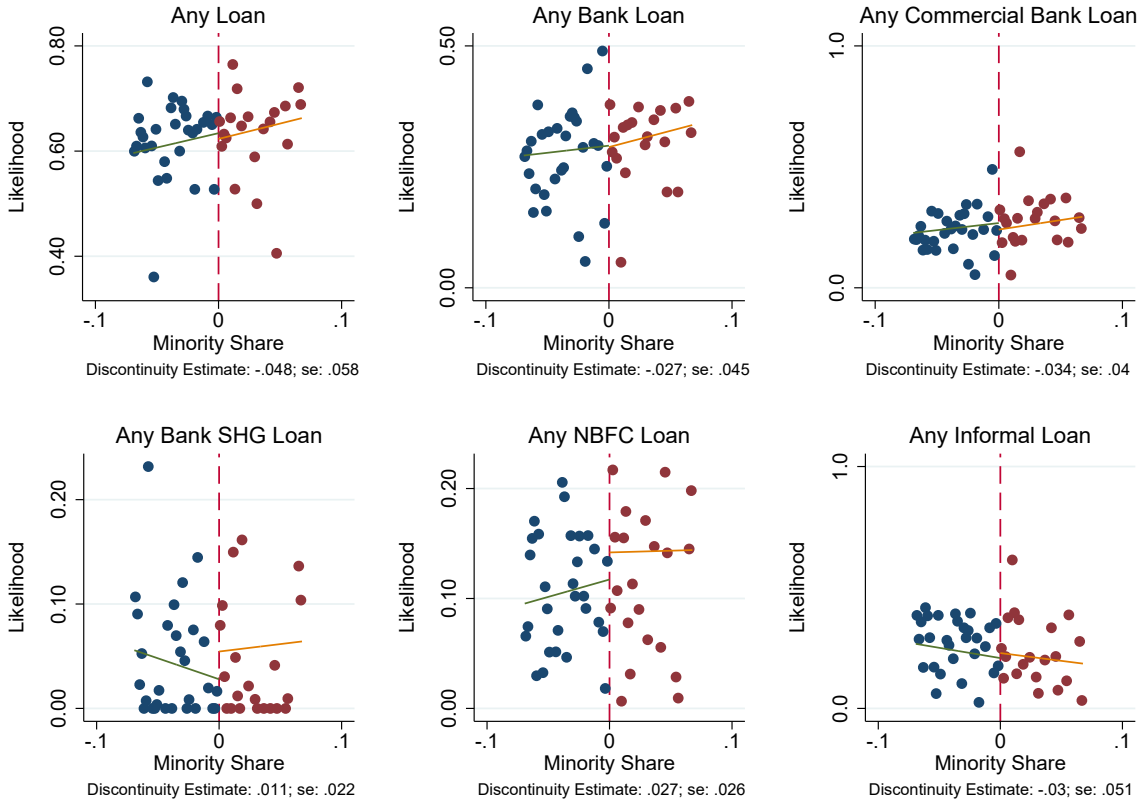
Notes: This figure shows unconditional RD plots showing balance in pre-treatment district demographic characteristics across treatment and control districts. The unit of observation is the household. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. Standard errors are clustered by district.

Figure A8: Pre-Treatment Balance in District Economic Characteristics



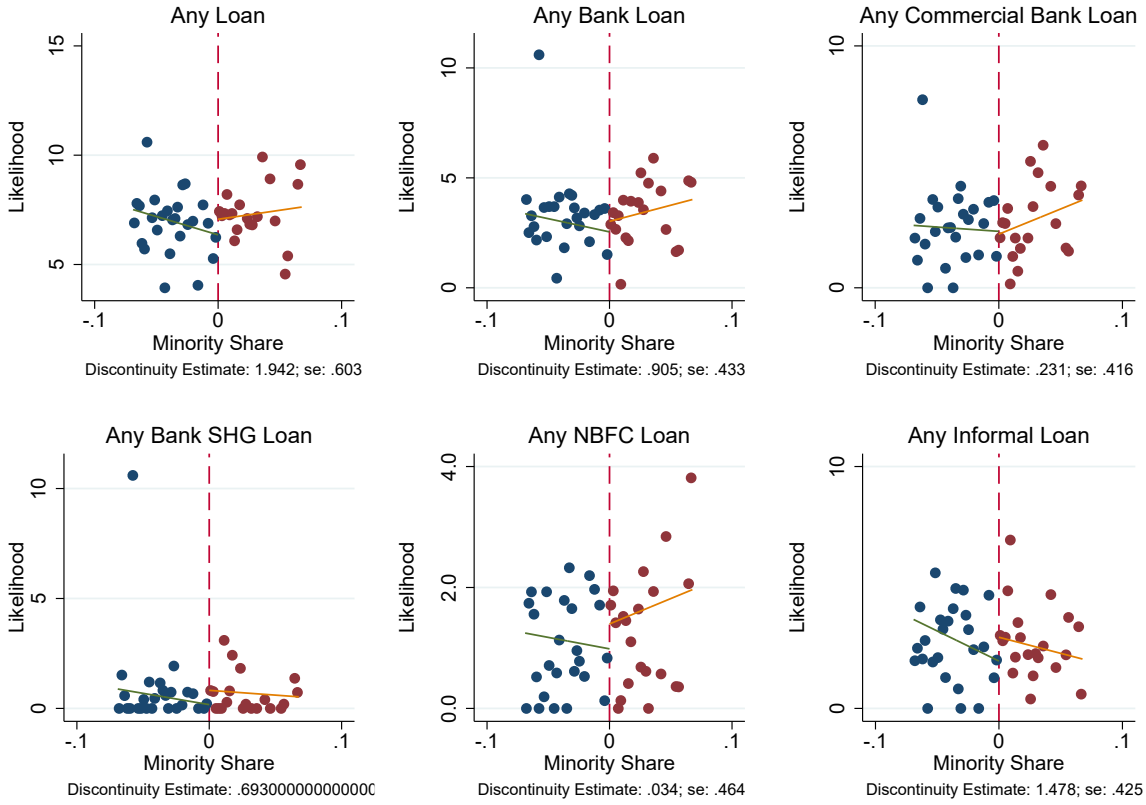
Notes: This figure shows unconditional RD plots showing balance in pre-treatment household characteristics for non-minority households across treatment and control districts. The unit of observation is the household. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. Standard errors are clustered by district.

Figure A9: Minority Credit in Minority Concentration Districts: Extensive Margin, Non-Minorities, RD Plots



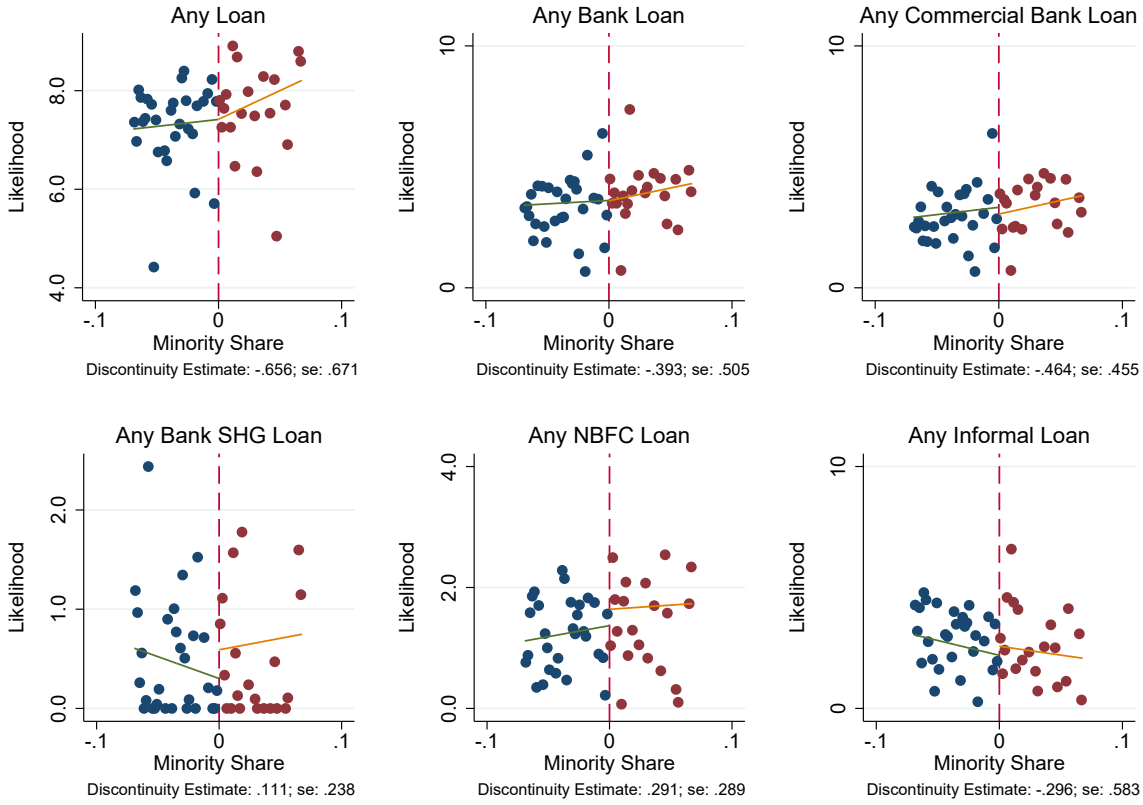
Notes: This figure shows unconditional RD plots comparing extensive margin credit access for non-minority households across treatment and control districts. The unit of observation is the household. The outcome of interest is a dummy, equal to 1 if the household has any outstanding loan in the categories mentioned. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. Standard errors are clustered by district.

Figure A10: Minority Credit in Minority Concentration Districts: Intensive Margin, RD Plots



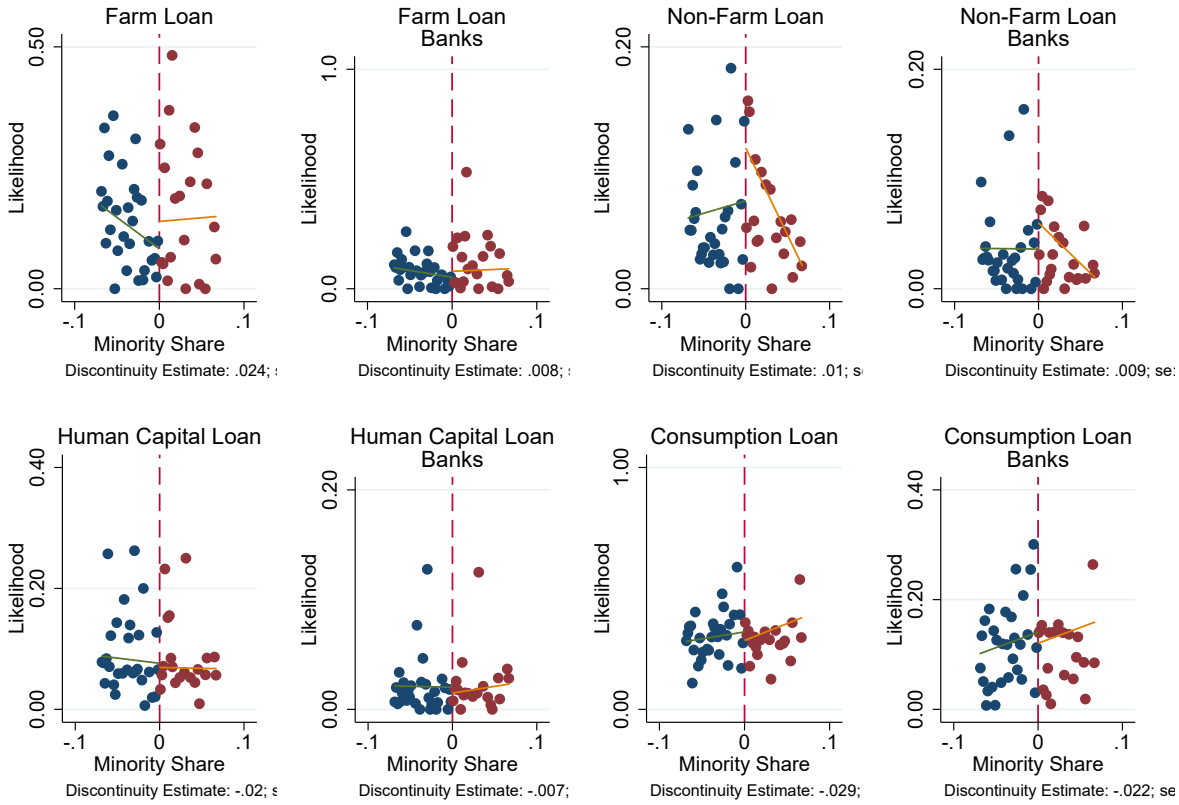
Notes: This figure shows unconditional RD plots comparing intensive margin credit access for minority households across treatment and control districts. The unit of observation is the household. The outcome of interest the volume of credit received in the categories mentioned, inverse hyperbolic sine transformed. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. Standard errors are clustered by district.

Figure A11: Minority Credit in Minority Concentration Districts: Intensive Margin, RD Plots



Notes: This figure shows unconditional RD plots comparing intensive margin credit access for non-minority households across treatment and control districts. The unit of observation is the household. The outcome of interest is the volume of credit received in the categories mentioned, inverse hyperbolic sine transformed. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. Standard errors are clustered by district.

Figure A12: Minority Credit in Minority Concentration Districts: Purpose of Borrowing, Extensive Margin, RD Plots



Notes: This figure shows unconditional RD plots comparing extensive margin credit access for minority households across treatment and control districts. The unit of observation is the household. The outcome of interest is a dummy, equal to 1 if the household has any outstanding loan in the categories mentioned. Each point denotes the outcome variable mean corresponding to each bin along the horizontal axis. The vertical broken line represents the discontinuity threshold of 0 – districts where minorities comprise 25% of the district population. The horizontal axis to the right of the vertical broken represent treated units; to the left, control units. The horizontal lines represent linear fits. A triangular kernel, in addition to household specific weights, is used to compute the household means. The sample is restricted to the 92 districts located within a bandwidth of .07 around the discontinuity threshold. Discontinuity estimates and standard errors, computed as per Calonico et al. (2020), are presented below each figure. Standard errors are clustered by district.

B Tables

Table A1.A: Summary Statistics: Manufacturing Establishments

	N	Mean	SD
Rural	99698	0.668	0.471
Minority	99698	0.153	0.360
Muslim	99698	0.116	0.320
SC/ST	99698	0.286	0.452
OBC	99698	0.449	0.497
Any Secondary Educated Individual	99698	0.614	0.487
Any Higher Educated Individual	99698	0.234	0.423
Farm	99698	0.368	0.482
Non-Farm Self-Employed	99698	0.157	0.364
Non-Farm Casual Work	99698	0.183	0.387
Non-Farm Salaried Work	99698	0.200	0.400

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

Table A1.B: Summary Statistics: Household Credit, Extensive Margin

	N	Mean	SD
Any Loan	99698	0.421	0.494
Any Bank Loan	99698	0.213	0.409
Any Commercial Bank Loan	99698	0.151	0.358
Any Bank SHG Loan	99698	0.074	0.261
Any NBFC Loan	99698	0.092	0.289
Any Informal Loan	99698	0.195	0.396
Any Money-Lender Loan	99698	0.115	0.319
Any Friends/Relatives Loan	99698	0.085	0.279
Any Farm Loan	99698	0.131	0.338
Any Non-Farm Business Loan	99698	0.039	0.193
Any Health/Education Loan	99698	0.059	0.237
Any Consumption Loan	99698	0.223	0.416

Notes: This table shows the summary statistics for household credit along the extensive margin. Bank SHG loans are loans issues by bank-linked SHGs.

Table A1.C: Summary Statistics: Intensive Margin Household Credit

	N	Mean	SD	P25	P50	P75
Total Loan	62991	156716	188855.7	30000	70000	200000
Total Bank Loan	28445	170871	236943.2	35000	70000	200000
Commercial Bank Loan	22702	219098	263209.1	50000	100000	300000
Bank SHG Loan	6541	45125	40475.5	20000	40000	50000
NBFC Loan	11582	96202	150054.7	25000	45000	90000
Informal Loan	28386	94505	144822.1	15000	43000	100000
Money-Lender Loan	14569	108413	151212.6	20000	50000	125000
Friends/Relatives Loan	14578	69190	121765.7	10000	25000	70000
Farm Loan	15891	135352	188310.5	30000	70000	150000
Non-Farm Business Loan	5961	167083	229804.7	37000	70000	200000
Health/Education Loan	8525	93658	145264.7	20000	50000	100000
Consumption Loan	32381	147690	224744.6	25000	50000	150000

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 refers to loans issued by bank-linked self-help groups.

Table A1.D: Summary Statistics: Loan Characteristics

	N	Mean	SD
Loan issued within 3 yrs	62610	0.885	0.320
Loan issued before 5 yrs	62610	0.052	0.221
Loan issued between 3 and 5 yrs	62610	0.163	0.369
Short-term loan	62991	0.280	0.449
Long-term loan	62991	0.382	0.486
Any unsecured loan	62991	0.620	0.485
Any unsecured bank loan	28445	0.387	0.487
Any unsecured informal loan	28386	0.851	0.356
Any unsecured money-lender loan	14569	0.804	0.397
Any unsecured friend/relatives loan	14578	0.912	0.283
Interest Rate	62618	0.141	0.124
Bank Interest Rates	28445	0.109	0.052
Informal Interest Rate	28386	0.190	0.198
Money-Lender Interest Rate	14569	0.313	0.167
Friends/Relatives Interest Rate	14578	0.021	0.087
Loan Delinquent	50245	0.387	0.487
Bank Loan Delinquent	12303	0.383	0.486
Informal Loan Delinquent	10490	0.639	0.480

Notes: This table shows the summary statistics for other loan characteristics. Bank SHG loans are loans issues by bank-linked SHGs. Interest rates are weighted using loan size. Summaries are computed only for households with outstanding loans in the referred categories.

Table A2.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 A2.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 A3: Unsecured Loans in Minority Concentration Districts

	(1)	(2)	(3)	(4)	(5)
	Pr (Unsecured Loan = 1)				
	Any Source	Banks	Commercial Bank	Bank-Linked SHG	Informal
Treat	.132**	.025	.015	.017	.043
	(.054)	(.037)	(.029)	(.018)	(.032)
Minority × Treat	.146**	.070***	.035**	.031*	.102**
	(.062)	(.022)	(.015)	(.018)	(.042)
Observations	20786	20786	20786	20786	20786
R^2	.13	.07	.04	.08	.08
$\beta_1 + \beta_2 = 0$	.000	.043	.092	.055	.002
Control Mean	.21	.05	.02	.03	.12

Notes: The above table shows the treatment effect on households' likelihood of receiving an unsecured loan. The unit of observation is the household. The outcome of interest is a dummy equaling 1 if the household had an unsecured loan from the sources mentioned. *Bank* in column (2) refers to loans received directly from commercial banks, or through bank-linked SHGs. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Minority \times Treat$ estimates the differential treatment effect for minority households. All specifications include state fixed effects and a linear polynomial in the running variable. Coefficient estimates in Panel A also include household covariates. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority households in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table A4: Minority Credit in Minority Concentration Districts: Intensive Margin, Inverse Hyperbolic Sine Transformation

			Panel A: Controls			
	(1)	(2)	(3)	(4)	(5)	(6)
	Loan Amount (Inverse Hyperbolic Sine Transformed)					
	All Sources	Banks	Commercial Bank	Bank-Linked SHG	NBFCs	Informal
Treat	1.606**	.620	.726	.078	.350	1.035**
	(.625)	(.629)	(.526)	(.225)	(.283)	(.465)
Minority × Treat	1.666**	1.104**	.427	.665***	-.445	1.116**
	(.743)	(.542)	(.530)	(.221)	(.419)	(.468)
Observations	20786	20786	20786	20786	20786	20786
R^2	.13	.10	.07	.13	.05	.16
$\beta_1 + \beta_2 = 0$	.000	.023	.048	.018	.795	.000
Control Mean	44703.69	22143.74	20127.72	2016.02	6273.37	13109.54
			Panel B: No Controls			
	(1)	(2)	(3)	(4)	(5)	(6)
	Loan Amount (Inverse Hyperbolic Sine Transformed)					
	All Source	Banks	Commercial Bank	Bank-Linked SHG	NBFCs	Informal
Treat	1.792**	.644	.698	.137	.396	1.168**
	(.693)	(.626)	(.504)	(.240)	(.279)	(.512)
Minority × Treat	1.621**	1.098**	.435	.647***	-.544	1.182**
	(.809)	(.524)	(.507)	(.217)	(.423)	(.505)
Observations	20786	20786	20786	20786	20786	20786
R^2	.12	.09	.06	.13	.05	.15
$\beta_1 + \beta_2 = 0$	.000	.017	.046	.017	.678	.000

Notes: The above table shows the treatment effect on credit access along the intensive margin. The unit of observation is the household. The outcome of interest is the total credit received by the household from the sources mentioned. *Bank* in column (2) refers to loans received directly from commercial banks, or through bank-linked SHGs. Outcomes of interest are transformed using an inverse hyperbolic sine transformation. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Minority \times Treat$ estimates the differential treatment effect for minority households. All specifications include state fixed effects and a linear polynomial in the running variable. Coefficient estimates in Panel A also include household covariates. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority households in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table A5: Minority Credit in Minority Concentration Districts: Intensive Margin

			Panel A: Controls			
	(1)	(2)	(3)	(4)	(5)	(6)
			Loan Amount			
	All Sources	Banks	Commercial Bank	Bank-Linked SHG	NBFCs	Informal
Treat	8034.45	1195.05	1258.45	-63.40	2985.97	8525.12**
	(12664.63)	(10673.44)	(10288.67)	(782.97)	(2614.26)	(3404.80)
Minority × Treat	11704.13	6998.11	2353.34	4644.77***	-8992.29	8785.88*
	(16522.76)	(13548.01)	(13689.49)	(1469.55)	(6746.98)	(4597.90)
Observations	20786	20786	20786	20786	20786	20786
R^2	.10	.05	.05	.08	.03	.11
$\beta_1 + \beta_2 = 0$	.187	.447	.732	.000	.273	.000
Control Mean	44703.69	22143.74	20127.72	2016.02	6273.37	13109.54
			Panel B: No Controls			
	(1)	(2)	(3)	(4)	(5)	(6)
			Loan Amount			
	All Source	Banks	Commercial Bank	Bank-Linked SHG	NBFCs	Informal
Treat	3929.51	-3249.98	-3404.20	154.21	2429.98	8813.27**
	(14577.09)	(12451.61)	(12240.68)	(787.86)	(2496.08)	(3593.41)
Minority × Treat	10384.81	6035.51	1332.93	4702.58***	-10051.25	8952.36*
	(16963.92)	(13874.07)	(14180.73)	(1646.66)	(6351.47)	(4523.16)
Observations	20786	20786	20786	20786	20786	20786
R^2	.08	.04	.03	.08	.03	.10
$\beta_1 + \beta_2 = 0$	.331	.801	.851	.001	.164	.000

Notes: The above table shows the treatment effect on credit access along the intensive margin. The unit of observation is the household. The outcome of interest is the total credit received by the household from the sources mentioned. *Bank* in column (2) refers to loans received directly from commercial banks, or through bank-linked SHGs. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Minority \times Treat$ estimates the differential treatment effect for minority households. All specifications include state fixed effects and a linear polynomial in the running variable. Coefficient estimates in Panel A also include household covariates. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority households in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table A6: Robustness to MSERD Bandwidth: Minority Credit in Minority Concentration Districts, Extensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)
				Pr(Loan = 1)		
	Any Source	Banks	Commercial Bank	Bank-Linked SHG	NBFCs	Informal
Treat	.109**	.036	.049	.013	.043*	.055
	(.049)	(.050)	(.042)	(.024)	(.025)	(.044)
Minority × Treat	.143**	.093**	.038	.047**	-.025	.110**
	(.059)	(.041)	(.042)	(.020)	(.036)	(.045)
Observations	22612	22229	21050	17736	23637	17736
R^2	.13	.10	.07	.09	.07	.10
Bandwidth	.077	.075	.071	.064	.081	.065
$\beta_1 + \beta_2 = 0$	.000	.039	.064	.058	.591	.003
Control Mean	.34	.15	.10	.04	.08	.14

Notes: The above table shows robustness of the baseline results to using outcome-specific MSERD bandwidths. The unit of observation is the household. The outcome of interest is a dummy equaling 1 if the household had any outstanding credit from the sources mentioned. *Bank* in column (2) refers to loans received directly from commercial banks, or through bank-linked SHGs. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Minority \times Treat$ estimates the differential treatment effect for minority households. All specifications include state fixed effects and a linear polynomial in the running variable. Coefficient estimates in Panel A also include household covariates. *Bandwidth* refers to the outcome specific MSERD bandwidth. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority households in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table A7: Minority Credit in Minority Concentration Districts: Extensive Margin

			Panel A: District Controls			
	(1)	(2)	(3)	(4)	(5)	(6)
			Pr(Loan = 1)			
	Any Source	Banks	Commercial Bank	Bank-Linked SHG	NBFCs	Informal
Treat	.149**	.020	.038	.006	.050*	.121***
	(.061)	(.054)	(.047)	(.019)	(.026)	(.040)
Minority × Treat	.139**	.097**	.041	.053**	-.044	.098**
	(.063)	(.045)	(.044)	(.020)	(.037)	(.045)
Observations	20661	20661	20661	20661	20661	20661
R^2	.13	.11	.07	.14	.05	.16
$\beta_1 + \beta_2 = 0$	.000	.043	.071	.038	.842	.001
Control Mean	.35	.15	.10	.06	.07	.16
			Panel B: District Fixed Effects			
	(1)	(2)	(3)	(4)	(5)	(6)
			Pr(Loan = 1)			
	Any Source	Banks	Commercial Bank	Bank-Linked SHG	NBFCs	Informal
Minority × Treat	.139**	.089*	.035	.053**	-.037	.103**
	(.064)	(.045)	(.046)	(.020)	(.037)	(.044)
Observations	20786	20786	20786	20786	20786	20786
R^2	.14	.12	.08	.14	.06	.17

Notes: The above table shows the robustness of the baseline results to controling for pre-treatment district covariates and district fixed effects. The unit of observation is the household. The outcome of interest is a dummy equaling 1 if the household had any outstanding credit from the sources mentioned. *Bank* in column (2) refers to loans received directly from commercial banks, or through bank-linked SHGs. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Minority \times Treat$ estimates the differential treatment effect for minority households. Coefficient estimates in Panel A include state fixed effects, pre-treatment district covariates and household covariates; in Panel B, district fixed effects and household covariates. A linear polynomial in the running variable is included in both panels A and B. The penultimate row in Panel A shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority households in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table A8: Minority Credit in Minority Concentration Districts: Exclude Non-Muslim Religious Minorities

	Panel A: Extensive Margin					
	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Loan = 1)					
	Any Source	Banks	Commercial Bank	Bank-Linked SHG	NBFCs	Informal
Treat	.120**	.048	.059	.006	.026	.092**
	(.051)	(.054)	(.045)	(.022)	(.024)	(.045)
Muslim × Treat	.092	.088*	.034	.052**	-.098	.124**
	(.082)	(.052)	(.048)	(.025)	(.061)	(.049)
Observations	19360	19360	19360	19360	19360	19360
R^2	.13	.10	.07	.13	.05	.15
$\beta_1 + \beta_2 = 0$	.001	.053	.068	.092	.212	.000
Control Mean	.34	.13	.09	.05	.07	.17
	Panel B: Intensive Margin					
	(1)	(2)	(3)	(4)	(5)	(6)
	Loan Amount					
	All Sources	Banks	Commercial Bank	Bank-Linked SHG	NBFCs	Informal
Treat	1.501**	.553	.686	.049	.309	1.148**
	(.591)	(.631)	(.528)	(.234)	(.280)	(.489)
Muslim × Treat	1.029	1.046	.398	.627**	-1.113	1.291**
	(.990)	(.641)	(.596)	(.275)	(.693)	(.537)
Observations	19360	19360	19360	19360	19360	19360
R^2	.13	.10	.07	.13	.05	.16
$\beta_1 + \beta_2 = 0$	.450	.311	.551	.001	.147	.000
Control Mean	40697.34	15990.12	15990.12	1760.16	5709.44	14238.04

Notes: The above table shows robustness of the baseline results to excluding non-Muslim religious minorities. The unit of observation is the household. The outcome of interest in Panel A is a dummy equaling 1 if the household had any outstanding credit from the sources mentioned; in Panel B, the amount of credit received (inverse hyperbolic sine transformed). *Bank* in column (2) refers to loans received directly from commercial banks, or through bank-linked SHGs. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Muslim \times Treat$ estimates the differential treatment effect for Muslim households. All specifications include state fixed effects and a linear polynomial in the running variable. Coefficient estimates in Panel A also include household covariates. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority households in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table A9: Minority Credit in Minority Concentration Districts: Purpose of Borrowing, Intensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Loan Amount							
	Farm Loan		Non-Farm Loan		Human Capital Loan		Consumption Loan	
	Any Source	Bank Loan	Any Source	Bank Loan	Any Source	Bank Loan	Any Source	Bank Loan
Treat	20311.58***	10611.01**	-428.46	-44.75	4378.57**	980.86	-2407.95	-9005.64
	(5045.42)	(4689.03)	(3481.58)	(2835.69)	(1704.59)	(793.08)	(8011.36)	(7514.14)
Minority × Treat	-5581.05	-2488.47	4452.59	3085.78	-2729.69	-2062.79	13877.58	11136.91
	(6457.65)	(3880.66)	(3590.56)	(2793.79)	(2069.89)	(1787.32)	(11109.44)	(10645.15)
Observations	20786	20786	20786	20786	20786	20786	20786	20786
R^2	.11	.07	.02	.01	.01	.01	.05	.04
$\beta_1 + \beta_2 = 0$	.014	.045	.178	.166	.371	.420	.220	.774
Control Mean	7616.15	4739.23	5701.50	3107.51	4718.43	1574.68	20059.73	8954.96

Notes: This table credit disbursement across underbanked and non-underbanked districts in the post-treatment period. The unit of observation is the district. Panel A considers private bank credit; panel B credit from government banks. Outcome variables are transformed using an inverse hyperbolic sine transformation. Odd-numbered columns show bank credit along the intensive margin (outstanding loans); even-numbered columns, bank credit along the extensive margin (total loan accounts). The outcome of interest in columns (1) and (2) is total credit; in columns (3) and (4), farm credit; in columns (5) and (6), manufacturing credit; in columns (7) and (8), loans to the priority sector; and columns (9) and (10), loans to micro and small enterprises. All specifications include district and year fixed effects, in addition to a linear polynomial in the running variable and district covariates. The sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Control variable means for outstanding loan amounts are in millions. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table A10: Household Productive Assets in Minority Concentration Districts

	(1)	(2)	(3)	(4)	(5)	(6)
	Machinery			Productive Assets		
	Any Machinery	Amount	Amount (IHS)	Any Productive Asset	Amount	Amount (IHS))
Treat	.009	.146	511.187	.026	.224	643.096
	(.029)	(.248)	(529.480)	(.040)	(.372)	(1589.824)
Minority × Treat	.065	.541	1096.757	.103*	.813	3497.464
	(.051)	(.463)	(758.192)	(.057)	(.532)	(2109.364)
Observations	20786	20786	20786	20786	20786	20786
R^2	.04	.04	.03	.06	.06	.05
$\beta_1 + \beta_2 = 0$	.117	.107	.039	.037	.074	.086
Control Mean	.12	1412.94	1412.94	.17	3768.18	3768.18

Notes: The above table shows the treatment effect household ownership of productive assets. The unit of observation the household. The outcome of interest in columns (1)-(3) refer to physical machinery owned by households; in columns (4)-(6), total productive assets owned by households. The outcome of interest in columns (1) and (3) are dummies equaling 1 if the household owns any machinery/productive assets. The outcomes of interest in columns (3) and (6) are inverse hyperbolic sine transformation of the amount of machinery/productive assets owned by households. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Minority \times Treat$ estimates the differential treatment effect for minority households. All specifications include state fixed effects, a linear polynomial in the running variable, and household covariates. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority households in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table A11: Labour Market Outcomes in Minority Concentration Districts

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours Worked During the Week					
	Total	Farm	Manufacturing	Trade	Construction	Services
Treat	-3.994	5.542**	-3.473*	-2.543**	-1.054	-1.531
	(2.729)	(2.770)	(2.003)	(.969)	(1.278)	(1.327)
Minority × Treat	1.530	-5.307	7.871***	2.914	.416	-3.960
	(2.816)	(4.253)	(2.755)	(2.294)	(2.533)	(3.271)
Observations Observations	24406	24406	24406	24406	24406	24406
R^2	.17	.35	.07	.05	.05	.17
$\beta_1 + \beta_2 = 0$	.500	.958	.105	.878	.804	.083
Control Mean	47.80	13.50	7.90	7.15	6.29	12.20

Notes: The above table shows the treatment effect on labour market outcomes along the intensive margin. The unit of observation are workers. Workers are individuals aged between 18 and 60 years who also participate in the labour force during the week. The outcome of interest in column (1) is total hours of work undertaken during the week; in columns (2)-(6), hours of work engaged in for each activity referred. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority households; $Minority \times Treat$ estimates the differential treatment effect for minority individuals. All specifications include state fixed effects, a linear polynomial in the running variable, and individual demographic and education controls. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority individuals in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table A12: Non-Farm Self-Employment in Minority Concentration Districts

	(1)	(2)	(3)	(4)	(5)	(6)
	All Activities		Non-Farm Work		Manufacturing Work	
		Self-Employed		Self-Employed		Self-Employed
	Self-Employed	Home	Self-Employed	Home	Self-Employed	Home
Treat	-.009	.007	-.002	.013	.013	.022
	(.026)	(.022)	(.024)	(.020)	(.014)	(.014)
Minority × Treat	.105**	.087***	.114***	.087***	.059	.058*
	(.046)	(.032)	(.042)	(.030)	(.039)	(.035)
Observations	24406	24406	24406	24406	24406	24406
R^2	.07	.07	.07	.06	.06	.07
$\beta_1 + \beta_2 = 0$	.033	.012	.007	.004	.049	.019
Control Mean	.12	.06	.12	.06	.03	.02

Notes: The above table shows the treatment effect on type of non-farm employment. The unit of observation is workers. Workers are individuals aged between 18 and 60 years and participating in the labour force. The outcome of interest in column (1) is a dummy equaling 1 if the worker is self-employed; in column (2), a dummy equaling 1 if the worker is self-employed and working from their own home; in column (3) is a dummy equaling 1 if the worker is engaged in non-farm self-employment; in column (4), a dummy equaling 1 if the worker is engaged in non-farm self-employment from their own home; in column (5), a dummy equaling 1 if the worker is engaged in manufacturing self-employment; in column (6), a dummy equaling 1 if the worker is engaged in manufacturing self-employment from their own home. Employment outcomes refer to employment in the 7 days preceding the survey. The coefficients are estimated using specification (3). *Treat* refers to the treatment effect for non-minority workers; $Minority \times Treat$ estimates the differential treatment effect for minority workers. All specifications include state fixed effects, a linear polynomial in the running variable, and individual demographic and education controls. The penultimate row shows p-values from testing the null hypothesis of $\beta_1 + \beta_2 = 0$. Control means refer to the unconditional mean for minority workers in control districts. The sample is restricted to a bandwidth of .07 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%