Religion and Credit Market Participation: Evidence from Muslims in India

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Abstract

Does Islam's prohibition on interest-bearing transactions reduce Muslim demand for formal credit? Exploiting a pair of bank branch expansion policies as exogenous supply-side shocks in India, I find that increased banking access raised loan demand among non-Muslim households and firms, but had no significant impact on Muslim borrowers. The policy widened the bank credit gap between Muslim and non-Muslim households in the treated districts by 6.2 percentage points. While reluctance to borrow from banks on religious grounds is prevalent among Muslims, demand for interest-bearing credit is significantly lower in areas with greater local adherence to Islamic norms. I find no evidence that lender discrimination, differences in underlying demand, or limited bank accessibility can account for this divergence, and establish that Muslims self-select out of conventional banking on account of religious identity. The findings have implications for financial inclusion policy in economies where Shariah-compliant alternatives remain limited or unavailable.

JEL classification: G21, G41, Z12

Keywords: Religion, Islamic economics, behavioural finance, financial inclusion

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1 Introduction

Religious beliefs, norms, and institutions shape the economic lives of individuals, their labour supply, consumption, human capital investment, and aggregate growth. Nonetheless, its influence on household finance remains relatively underexplored, despite evident disparities in credit market participation across religious, ethnic, and racial lines. In the United States, Black and Hispanic households borrow at significantly lower rates than white households (Blanchflower et al., 2003). In sub-Saharan Africa, ethnic minorities exhibit systematically lower rates of bank account ownership and formal borrowing (Beck et al., 2007) and Muslims across the world are significantly underbanked. Promoting financial inclusion among such disadvantaged groups has long been a policy goal, yet little attention has been paid to whether this exclusion can itself be a self-selected outcome driven by group identity. This paper provides causal evidence on how identity primed by religious obligations affects the demand for formal credit among Muslims in India.

Muslims across the world are disproportionately excluded from the formal financial sector, and 7 per cent explicitly cite religious belief as the reason for their low credit uptake (Gallup, 2011). We observe a similar pattern for Muslims in India, where despite making up a significant share of the urban population, institutional borrowing is nearly half that of non-Muslims. Previous research has attributed this gap to cultural proximity (Fisman et al. 2017), perceived discrimination (Fiorin et al., 2025) and systemic neglect (Sachar Committee Report, 2006). However, the existing literature does not isolate the role of preferences prompted by religious beliefs, specifically Islam's prohibition on interest-bearing transactions as a determinant of voluntary financial exclusion. Building on the identity economics framework (Akerlof and Kranton, 2000), I examine whether Muslims who adhere to Islamic law deviate systematically from standard borrowing behaviour and whether this deviation can explain the observed credit gap between Muslims and non-Muslims in India.

To identify the effect of Islam's prohibition on interest-bearing transactions on the financial decisions of households and firms, I exploit a pair of bank branch expansion policy reform authorized by

the Reserve Bank of India between 2005-2009[1]. Given strong reverse causality between religiosity and economic outcomes, the bank branch expansion policy provides an ideal setting to assess the causal impact of religious practices on economic behaviour. Since bank loans entail interest payments which are prohibited in Islam, I examine whether this expansion in banking services altered the demand for bank credit among Muslim borrowers and whether this observed difference can be attributed to their religious adherence.

The banking sector in India until the early 2000s was characterized by regional disparity and structural hindrances. In fact till before the policy was implemented about 13% of districts in India did not have a private bank branch. To redress this inequality, the Reserve Bank of India mandated the bank branch authorization policy, first implemented in 2005 and then amended in 2009, which substantially expanded the spread of bank offices across the country. The policy classified districts as underbanked or overbanked based on whether a district's population-per-branch exceeded the national average, providing a quasi-natural threshold for a regression discontinuity design. The 2009 amendment further restricted the set of qualifying underbanked districts to those located in the underbanked states, and it is this criterion that constitutes the primary source of exogenous variation exploited in this paper.

To begin with, the policy reform was unanticipated and centrally mandated, reducing the risk of manipulation. The running variable is determined by the district level population from the 2001 Census, which predates the policy by several years, and the McCrary density test confirms no manipulation around the threshold. The treated and control districts around the cutoff are comparable along relevant parameters, facilitating clear identification of the policy's effects on Muslim borrowing behaviour.

To empirically assess this, I use data from the 59th and 70th rounds of the All India Debt and Investment Survey (AIDIS) and the Sixth Economic Census, which provide detailed information on the borrowing portfolio of households and firms along with the religious and demographic profile

[1]Although a regression discontinuity design using the 2005 bank branch expansion policy was first developed by Young (2017), this paper primarily exploits the 2009 amendment to the policy which has not been previously used.

of borrowers. For the religiosity index, I use a novel dataset on the density of religious institutions at the district and sector level. Finally, drawing on RBI data on the district and sub-district level spread of bank branches, I construct a measure of bank accessibility based on proximity to borrowers.

To begin with I find that these policies significantly expanded bank branch networks in financially backward districts in India. Till before 2005, an average underbanked district in India had about 90 bank branches. By 2013, the year relevant to our analysis, this number rose to 135. There was also a sharp increase in the number of accounts opened, amount deposited and the volume of loan intake in the underbanked districts. Next, employing a fuzzy regression discontinuity design, I observe that the policy increased the incidence of bank borrowing and share of bank loans among non-Muslim households in treated districts by 36–39%, with effects especially pronounced in rural areas. Farm-based establishments owned by non-Muslims in treated districts became 80% more likely to rely on bank credit. While Hindu households increased borrowing in response to the policy, Muslim households did not, widening the bank credit gap between the two groups in the treated district by 6.2 percentage points.

The results are robust to alternative specifications. A difference-in-discontinuities design, which pools the pre-policy 59th round of the AIDIS (2002–03) with the post-policy 70th round (2012–13), confirms that prior to the policy, Muslims (non-Muslims) in the treatment and control group around the threshold exhibit similar borrowing patterns and the Diff-in-Disc estimates closely follow the RD estimate. The baseline estimates remain stable across a range of bandwidth choices, including MSE-optimal and two-sided MSE-optimal bandwidths, and are robust to triangular kernels. Placebo cutoff tests at artificial thresholds of ± 1000 and ± 2000 find no spurious discontinuities which lends further credibility to the identifying assumption. Finally no other policy uses the same population-per-branch cutoff, ruling out the possibility that the discontinuity captures the effect of a concurrent intervention rather than the branch expansion policy.

Next, I examine the role of religiosity in shaping borrowing behaviour. Religious institutions have historically been more prevalent in areas with a strong Muslim presence, making them a useful proxy for local levels of religious adherence. Using the number of religious institutions as a measure of religiosity, I augment the baseline regression discontinuity specification with a complete set of interactions terms to estimate heterogeneous treatment effects. Consistent with our main hypothesis, I find that credit market participation among Muslim households in urban areas is significantly lower in the more religious districts.

To identify the channels through which an expansion in banking services affects formal credit uptake, I exploit the fuzzy RD setup and examine four supply-side mechanisms. First, the policy relieved binding credit constraints. Overall indebtedness among non-Muslim households in treated districts rose without any offsetting decline in borrowing from other sources, indicating that additional bank loans represent new credit rather than a substitution away from alternative borrowing. Second, entry by commercial banks in underbanked districts lowered the average cost of borrowing by approximately three percentage points, inducing households at the margin to take up formal credit they would not otherwise have demanded. Third, the policy coincided with RBI notifications that relaxed collateral requirements on small loans, raising take-up of collateral-free loans and collateral-free agricultural loans among non-Muslim households by 5.2 and 6.3 percentage points respectively. Although banks sought to meet priority-sector lending targets, default rates among marginal borrowers show no significant difference between treated and control districts, ruling out concerns about risky lending. The policy therefore expanded formal credit access to creditworthy but previously excluded borrowers. Muslim households in treated districts experienced the same improvements across all four margins of bank access, yet their borrowing behaviour remained unaffected.

Although the ineffectiveness of the policy for Muslim households could partly reflect self-selection, it is crucial to establish that the policy itself was not biased against any particular group. Muslims in India face discrimination, residential segregation, and occupational stratification, which may underpin structural differences between the two communities and further constrain their access

to formal credit. To address this concern, in Section 8 I rule out alternative channels that could confound the analysis.

One potential concern is that differences in occupation structure across groups drive differential borrowing needs. I address this by analysing the impact of the policy by purpose of borrowing for each group. Additional loans were taken by non-Muslim households for both consumption and productive purposes, with increased loan demand concentrated among agricultural households. No such effects are observed for Muslim households. To rule out lender bias, I employ Becker's outcome test and find no significant difference in default rates among marginal borrowers between Muslims and non-Muslims in the treated districts relative to the control group. Additionally, to ensure that Muslim borrowers had adequate physical access to banks, I assess bank branch density at the sub-district level weighted by the population share of Muslims in that area. I find no effect of the policy on demand for bank loans by Muslim households even in areas with greater bank branch density.

Religious beliefs, laws, cultural practices, and institutions significantly shape individual preferences, choices, actions, and values. These factors also influence the legal and institutional frameworks within countries, creating complex interactions between religion and economic development. This paper contributes to the existing body of work exploring the impact of religion on economic outcomes (Becker et al. 2021). It closely aligns with previous work examining the impact of Islamic traditions, including the Hajj pilgrimage (Clingingsmith et al., 2009) and Ramadan fasting (Almond and Mazumder, 2011; Campante and Yanagizawa-Drott, 2015), on economic development and subjective well-being, and health outcomes respectively. A related strand examines religiously framed moral appeals and their effect on delinquency rates (Bursztyn et al., 2019). While the prohibition on interest is a foundational principle of Islam, this paper provides some of the first causal evidence on how religious beliefs shape preferences and financial decision-making.

More broadly, a growing body of work documents how identity concerns generate frictions in various markets that produce deviations from standard economic behaviour. Identity can shape labour supply (Oh 2023), consumption (Atkin et al. 2021), and political expression (Bursztyn et al. 2020),

with the common thread being that individuals incur real economic costs to maintain congruence between rational behaviour and identity-prescribed norms. Islam's prohibition on interest-bearing transactions creates a direct conflict between a religiously mandated obligation and participation in conventional credit markets. I show that Muslim households in India respond to this conflict through self-exclusion from formal borrowing, forgoing access to credit even when supply-side constraints are relaxed. This extends the identity-economics framework to household finance, and establishes religiously motivated self-exclusion as a driver of the Muslim credit gap in India.

Third, this paper contributes to the literature on financial inclusion by identifying demand-side religious preferences as a binding constraint on formal borrowing that supply-side interventions alone cannot resolve. The existing literature explains low credit market participation among marginalized groups through supply-side rigidities: geographic inaccessibility (Burgess and Pande, 2005), collateral constraints (Stiglitz and Weiss, 1981), financial illiteracy (Lusardi and Mitchell, 2014), and lender discrimination (Blanchflower et al., 2003). Here we see that even when these barriers are relaxed by an exogenous policy shock, Muslim households do not respond. The findings suggest that when religious beliefs shape financial decision-making, supply-side interventions alone may not suffice to close this gap. Furthermore, the expansion of conventional banking infrastructure may generate unequal gains across groups, even when the policy is aimed at redressing the needs of the underbanked population.

Finally, these results add to our understanding of poor banking outcomes for Muslims across the world. While use of Islamically permissible financial products can help mitigate financial exclusion (Karlan et al. 2014), these services are limited in many developing countries. In the absence of viable Shariah-compliant financial alternatives, low credit uptake and limited savings can have far-reaching implications. The results emphasise the need to understand financial behaviours prompted by religion when designing inclusive economic policies.

The remainder of the paper is organised as follows. Section 2 describes the context and the policy rule. Section 3 presents the identification strategy and Section 4 the data. Section 5 presents

the main results, followed by a discussion of the mechanisms in Section 6. Section 7 examines the heterogeneous impact of religiosity, and Section 8 addresses alternative channels. Section 9 presents robustness checks and Section 10 concludes.

2 Background and Policy

2.1 The Prohibition on Interest in Islamic Law

Islamic jurisprudence prohibits 'Riba', a term whose literal meaning, 'increase' or 'excess' maps closely onto the economic concept of interest. The prohibition appears explicitly in multiple verses of the Quran and is reiterated in the Hadith, the recorded sayings and practices of the Prophet Muhammad. Muslim jurists across all major schools of legal thought have historically interpreted riba to encompass any predetermined, contractually fixed return on a loan, regardless of the agent's identity or the purpose of the transaction. This interpretation stands in contrast to the usury prohibitions found in other Abrahamic traditions: orthodox Jewish law (Halakha) restricts interest-bearing transactions primarily among co-religionists, while medieval Christian canon law was gradually relaxed in the early modern period. Shariah law by contrast, treats any interest component as categorically impermissible, with no analogous relaxation over time. Standard loan contracts, which fix a predetermined rate of interest as the price of credit, fall unambiguously within the scope of riba and are therefore categorically impermissible. In the absence of Shariah-compliant alternatives, a Muslim borrower that adheres to Islamic law confronts a choice: forgo bank credit entirely, or violate foundational religious obligation.

Although sparse, there is some evidence to suggest adherence to this norm. Muslims across the world are thoroughly underbanked. Compared to 44% non-Muslims only 24% of them have a bank account. In the Middle-East and North African region, where they make up 93-94% of the total population, only 18% of them own a bank account (Gallup, 2011). Drawing on a sample of 65,000 adults across 64 countries with significant Muslim populations, Demirgüç-Kunt et al. (2013) confirm that Muslims are significantly less likely than non-Muslims to own a bank account or save at

formal financial institutions. Gallup (2011) reports that 7 per cent of Muslims worldwide explicitly cite religious belief as the reason for their limited engagement with formal credit, and the scale of the global Islamic finance market valued at approximately $3.2 trillion attests to a widespread preference for Shariah-compliant financial arrangements. Nonetheless, the aggregate alone cannot establish the extent to which low formal credit uptake reflects voluntary religious abstention rather than supply-side constraints.

2.2 Credit Market Participation of Muslims in India

Muslims constitute roughly 14 per cent of India's population and form its largest religious minority. On average, they are the poorest religious group in the country, recording the lowest consumption levels and asset ownership rates, and the lowest rates of formal credit market participation (AIDIS 2019–20). According to the 70th round of the AIDIS data (2012-13), 37.3 per cent of them do not even have a bank account, well above the national average of 27.4 per cent. Compared to 29 per cent of non-Muslim households only 24 per cent of Muslim households have at least one outstanding loan. Among indebted households, the incidence of repeated borrowing stands at 14.6 per cent for Muslims, compared to 22 per cent for non-Muslims. Additionally, only 4.7 per cent of Muslim households and less than 2 per cent of economic establishments owned by them borrow from banks or rely on them for their primary credit needs. The disparity is most pronounced in urban areas, where bank borrowing by non-Muslims is nearly double that of Muslims, a pattern that has persisted consistently over two decades.

The prohibition on riba not only shapes borrowing choices but also saving portfolios, and the two effects are mutually reinforcing. Since fixed returns on bank deposits are equally impermissible, Muslims face a lower opportunity cost of informal lending, which in turn sustains a supply of interest-free loans. Approximately 8.4 per cent of Muslim households hold at least one loan from friends or family, and intra-community loans account for around 34 per cent of their loan portfolios. However, such loans are severely capped by both their size and purpose. Only 11 per cent of these loans are taken for business investment, while 48 per cent are used to meet household expenditures.

Furthermore, the average size of loan for a Muslim household stands at approximately INR 86,000, compared to INR 137,000 for a non-Muslim household.

While the absence of an interest burden provides relief for consumption-smoothing loans among poor households, reliance on informal credit severely constrains productive investment. Informal intra-community loans are small, predominantly consumed rather than invested, and unlikely to substitute for formal credit in supporting business expansion or investment. To the extent that the prohibition on interest reduces productive borrowing and not merely speculative or financially distressed borrowing, it may contribute to the perpetuation of income gaps between Muslims and non-Muslims in India. While quantifying this gap lies beyond the scope of this paper, a better understanding of the welfare implications of such norms will require further inquiry into how supply-side innovations in Islamic finance and community lending networks might facilitate alternative credit arrangements.

The next section outlines the RBI's branch authorization policy, which this paper exploits as a source of exogenous variation in formal credit access to estimate its effect on the demand for bank loans among Muslim and non-Muslim households.

2.3 Policy Rule

The circular on branch authorization policy released by the Reserve Bank of India (RBI) in 2005 implemented the branch expansion policy which I will utilise in this paper. The banking sector in India under the supervision of RBI restricts banks from freely setting up new branches or closing and shifting existing ones. Before 2005, private banks were required to acquire licences to set up new branches and get permission before closing or shifting an existing branch. The reform in 2005 altered the regulatory environment by incentivising banks to set up new branches in financially backward districts. Licenses to open new branches in highly sought markets were issued depending on the bank's performance in the underbanked areas. A district was assigned the underbanked status if the population per bank branch in that district exceeded the national population per branch. Secondly, the RBI introduced the Annual Branch Expansion Plan (ABEP) following which

banks had to submit an elaborate plan of proposed network changes (opening, closing, and shifting of bank branches) in the following year to the RBI. In addition, private banks were required to ensure that at least 25% of their branching network worked in centres with a population of less than a hundred thousand people and maintain 40% of their outstanding credit as loans to the priority sector.

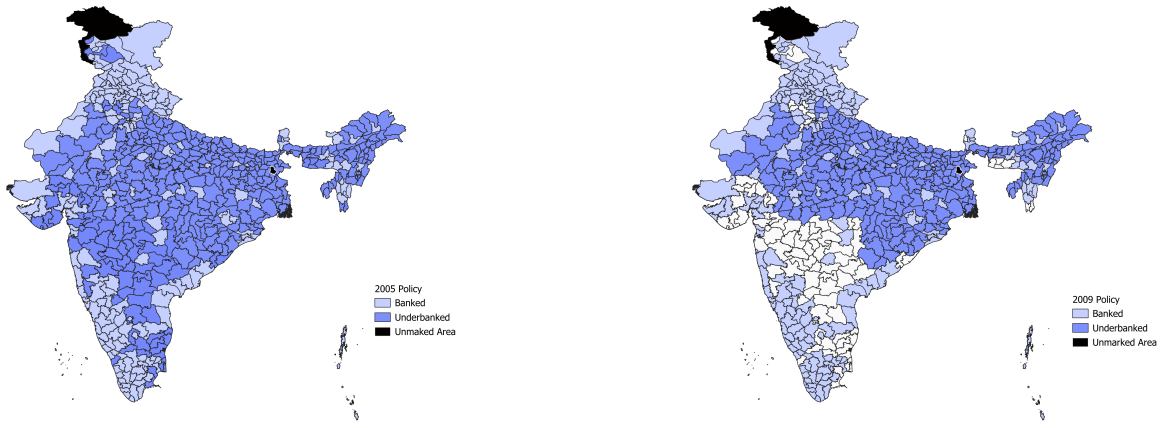
Although the policy rule here was not as strictly binding as the nationalization of private sector banks between 1969-1980, the incentive to expand banking activities in profitable centres prompted banks to set up new branches in the underbanked districts. Here, however, banks had the liberty to choose amongst the many underbanked districts and the exact location of the branch within an underbanked district. Moreover, banks in underbanked districts were not allowed to shift or close their branch unless the given centre had another operating commercial bank. Banks could only shift their branches to centres with low population groups or other centres within the underbanked district. Finally, in 2008 RBI made the metropolitan cities ineligible to qualify as underbanked centres, and in December 2009, RBI mandated that to open a new branch in tier 1 or tier 2 centres banks may have to plan their expansion in such a manner that at least one-third of their branches opened in a financial year are in the underbanked districts in the underbanked states. Thus greater emphasis was now on the financially backward districts in underbanked states. These set of rules acted coherently to ensure the expansion of banking activities in the underbanked districts.

3 Identification Strategy

3.1 Empirical Specification

The rule employed by the RBI for identifying districts as underbanked in the 2005 bank branch policy reform was based on a simple district and national-level estimate of population per branch and it yields a clear quasi-natural experiment to employ the regression discontinuity techniques. Underbanked districts were identified using the population per branch in that district relative to the national population per branch. To obtain the national average, the total population of India as

Figure 1: Underbanked and Banked Districts under the Branch Expansion Policy



(a) **(b)**

(a) Map of India depicting districts based on 2001 Census borders. Underbanked and banked districts as per the 2005 policy rule. (b) Map of India depicting districts based on 2001 Census borders. Underbanked and banked districts as per the 2009 policy rule.

per census 2001 was divided by the total number of scheduled commercial bank branches operating in the country in 2005-2006. A corresponding figure was calculated for each district to be compared to this national average. The above algorithm induces a cutoff, treating the population per branch in each district less the national average as the running variable. Districts with population per branch above the national average were designated the underbanked status and districts below the cutoff were classified as overbanked.

Let us define the running variable Z_d as:

Z_d= $Branch_d$ - $\overline{Branch}$

Where Z_d is the population per branch in district d less the national population per branch. If $Z_d > 0$ the district is underbanked. Here $Branch_d$ is the population per branch in that district and $\overline{Branch}$ is the national average.

Here I have a single running variable corresponding to two policy reforms. In 2005 when the policy was first mandated, districts all over the country where the population per branch was above the national cutoff qualified as underbanked. In 2009, RBI modified the policy such that now only

the underbanked districts in the underbanked states qualified as underbanked. Therefore I define two dummy variables corresponding to the two policy reforms.

As per the 2005 policy let UB_d be the treatment dummy where

UB_d=1 if the district is assigned the underbanked status

UB_d=0 otherwise

As per the 2009 policy let UB_{ds} be the treatment dummy where

UB_{ds}=1 if the district is assigned the underbanked status and it also belongs to the underbanked states

UB_{ds}=0 otherwise

A state is classified as underbanked if its population per branch office exceeds the national average. My main results focus on the 2009 policy. An examination of the RBI's list of underbanked districts reveals imperfect compliance with the assignment rule: four (five) districts assigned the underbanked (overbanked) status have a population per branch below (above) the threshold. Given this non-compliance with the assignment rule, I employ a fuzzy regression discontinuity design.

To estimate the effects of branch expansion policy on borrowing by households and firm, I use the local polynomial approach proposed by Calonico et al. (2014), which yields point estimators with optimal properties (Cattaneo et al., 2020; Cattaneo and Titiunik, 2022). My primary specification includes linear regressions on either side of the cutoff, uniform kernels and a fixed bandwidth. Following the fuzzy regression discontinuity approach I have specified below the two-stage least squares model

$$UB_{ds} = \alpha_0 + \alpha_1 D_d + \alpha_2 Z_d + \alpha_3 Z_d * D_d + \alpha_4 S_s + \mu_i \tag{1}$$

$$Y_i = \beta_0 + \beta_1 \widehat{UB}_{ds} + \beta_2 Z_d + \beta_3 Z_d * D_d + \beta_4 X_i + \beta_5 S_s + \epsilon_i \tag{2}$$

Here UB_{ds} is the treatment dummy and it equals 1 if the district is underbanked and belongs to an underbanked state. Y_i is the outcome of interest measured for subject i and I run the specification separately for Muslims and non-Muslims. The binary instrument D_d equals one when the average

population per branch is above the cutoff. Z_d and $Z_d{}^*D_d$ reflect the first order polynomials below and above the cutoff, X_i are the household level controls. and S_s denotes the state dummy.

Next I use equation (3), to calculate the differential impact of the policy on Muslims and non-Muslim households. I estimate the following specification.

$$\begin{aligned} Y_i = \gamma_0 + \gamma_1 D_d + \gamma_2 Muslim_i * D_d + \gamma_3 Z_d + \gamma_4 Z_d * D_d + \gamma_5 Muslim_i \\ + \gamma_6 Muslim_i * Z_d + \gamma_7 Muslim_i * Z_d * D_d + \gamma_8 S_s + \eta_i \end{aligned} \tag{3}$$

Where the new terms are $Muslim_i$, an indicator equal to one if the borrower is Muslim, together with its interaction with the running variable Z_d and with the underbanked assignment indicator D_d. Equation (3) is the reduced form of the interacted specification: the outcome is regressed directly on the assignment indicator and its interactions rather than on the instrumented treatment, so that γ_1 measures the intention-to-treat effect of the policy for non-Muslim borrowers and γ_2 the differential effect between Muslim and non-Muslim households. While the per-group effects are estimated by fuzzy two-stage least squares, the fully interacted 2SLS analogue of equation (3) is weakly identified. Compliance with the assignment rule is near-perfect within the Muslim subsample, which renders the interacted treatment term nearly collinear with its instrument. I therefore estimate the reduced form of the interacted equation to recover the gap. Since compliance is high within both groups, the reduced-form and the fuzzy estimates are very close in magnitude. Finally, to avoid over-rejection in RD designs arising from parametric specifications with interaction terms (Hsu and Shen, 2017), I estimate the main effects from separate group-specific regressions using equation (2), and employ equation (3) solely to quantify the resulting gap in borrowing.

3.2 Identification Assumption

The validity of the regression discontinuity design requires that districts cannot manipulate their assignment to the underbanked or overbanked groups. Since the running variable was determined by population figures from the 2001 Census, anticipatory behaviour or selective sorting around the threshold is unlikely. To verify this formally, I conduct the McCrary density test. The estimated

density of the running variable is smooth around the cutoff (Figure A1), and the formal manipulation test fails to reject the null hypothesis of no discontinuity in the density of the running variable.

Next, I check whether the baseline controls vary smoothly across the threshold. Since the analysis compares Muslim and non-Muslim borrowers in treated and control districts, I verify that borrowers within each religious group are balanced across the cutoff. Importantly, the RD identifying assumption does not require that Muslim and non-Muslim individuals are similar to one another only that each group is balanced across the threshold. Table A1 reports RD estimates of covariate discontinuities within a narrow bandwidth around the cutoff, estimated separately for the overall sample and by religious group. The covariates include individual characteristics of the household head (age, sex, marital status, and education), household-level characteristics (household size, land ownership, monthly per-capita consumption expenditure, and primary occupation), and district-level characteristics (population shares of Muslims, Scheduled Castes, and Scheduled Tribes).

For the 2009 policy, I find no statistically significant discontinuity in most household and individual characteristics, with the exception of marginal differences in occupation and the incidence of borrowing. The Muslim population share is approximately 11 percentage points higher in the control group and although the RD design controls for smooth differences across the threshold, I run a covariate-adjusted specification to absorb any residual imbalance. Finally, I find no significant discontinuity in the pre-policy probability of holding a bank loan for either Muslims or non-Muslims, which alleviates concerns that pre-existing differences in credit market participation are driving the results.

4 Data Source

I have collected data on the district-wise spread of scheduled commercial banks and other banking parameters from the Basic Statistical Returns (BSR), volumes 30, 34, 39, and 42, published by the Reserve Bank of India. To ensure baseline parity, I use Census data on the size of the Muslim, Scheduled Caste, and Scheduled Tribe populations across treatment and control districts. I also

use the NSS 2003 survey for household-level data on education, employment, and consumption. To check for any pre-existing differences in access to banking infrastructure, I use the Master Office File (MOF), which records the exact location of bank branches at the community development block (CD block) level.

I estimate the primary outcomes using two data sources. To analyse household borrowing behaviour, I use the 59th and 70th round of All-India Debt and Investment Survey (AIDIS), which contains detailed information on household asset and debt portfolios, including the source and purpose of borrowing, and the amount of outstanding credit. To examine firm-level credit demand, I draw on the Sixth Economic Census, which covers credit access among establishments owned by Muslims and non-Muslims across treatment and control districts. Finally, I construct a novel district-level dataset on the number of mosques and madrasas using the WAQF Assets Management System of India (WAMSI), which I use as a proxy for religiosity. Table 1 reports summary statistics for Muslim and non-Muslim households, for all districts and for districts within the estimation bandwidth.

5 Results

5.1 Effect on Bank Branch Expansion

The branch expansion policy played a crucial role in the growth of bank branches in the underbanked regions of the country. Although banks had the liberty to choose the exact location of their branches, the policy effectively ensured an equitable distribution of branches and bridged the gap in access to credit. In 2005, about 79 districts in India, 53 of which were underbanked did not have a private bank branch. By 2013, this number had decreased to just 8 districts, 4 of which belonged to the underbanked group.

Chowdhury and Ritadhi (2022) studied the impact of the 2005 bank branch expansion policy, revealing a 48-73% higher growth in the total number of bank branches opened in underbanked

districts between 2006 and 2010. Using differences in discontinuity specification, they found a 10-20% higher annual growth in the number of private bank branches in these districts post-policy.

Next, I examine the impact of the 2009 modification to the policy on banking outcomes. Before 2005, an underbanked district in an underbanked state had, on average, about 90 bank offices. This number increased to 108 in 2010 and 135 in 2013. Controlling for pre-policy levels of the outcome variables and state fixed effects, treated districts now have 8.7 additional private bank branches (Table 2), and the size of outstanding credit is about 28.3% higher. The increase in outstanding credit likely reflects the policy's emphasis on priority sector lending and meeting credit targets in underbanked regions. These estimates are stable across alternative bandwidths, kernels and polynomial orders (Table A9; Figure A17). Figure A3 plot the corresponding discontinuities in each banking-sector outcome. Overall, the branch authorization policy effectively widened the banking network across the country.

The expansion of private bank branches in underbanked districts and a reduction in the average cost of borrowing serve the credit needs of households and firms. By running separate regressions for Muslim and non-Muslim households and establishments, I assess whether improved banking access translates differentially into formal credit uptake based on religious identity.

5.2 Effect on Households

I estimate the effect of the 2009 bank branch expansion policy on household borrowing using the 70th round of the All India Debt and Investment Survey (AIDIS, 2012–13). The unit of observation is the household, although about 31 per cent of loans in the sample were taken for commercial purposes. I report results for the 2009 policy throughout the main text, since the 70th round of the AIDIS was conducted in 2012–13 and therefore most of the borrowing reported by the households are in the years just preceding the survey. The sample is restricted to a bandwidth of 3700 around the population-per-branch cutoff, yielding 179 districts[2]. The baseline specification is a local-linear

[2]I fix a common bandwidth across both groups, chosen to lie between their group-specific MSE-optimal windows, so that the credit gap is identified by comparing Muslims and non-Muslims over the same estimation window rather than differing effective populations. The estimates are stable across alternative bandwidth choices (Table A10).

regression discontinuity design with a uniform kernel and fixed bandwidth. All specifications include state fixed effects, and standard errors are clustered at the district level. I run sensitivity checks using triangular kernels, quadratic polynomials in the running variable, and data-driven MSE and CER-optimal bandwidths.

One important caveat concerns the contemporaneous minority credit programme. As part of a 15-point welfare programme announced in 2009, commercial banks were encouraged to expand lending to religious minorities in districts where their population share exceeded 25 per cent. In addition to direct credit, commercial banks were asked to collaborate with local self-help groups to reach borrowers from minority communities. The programme was poorly implemented in the initial years following the announcement and to avoid any confounding effect of this policy on my results, I restrict the sample to loans taken before 2012.

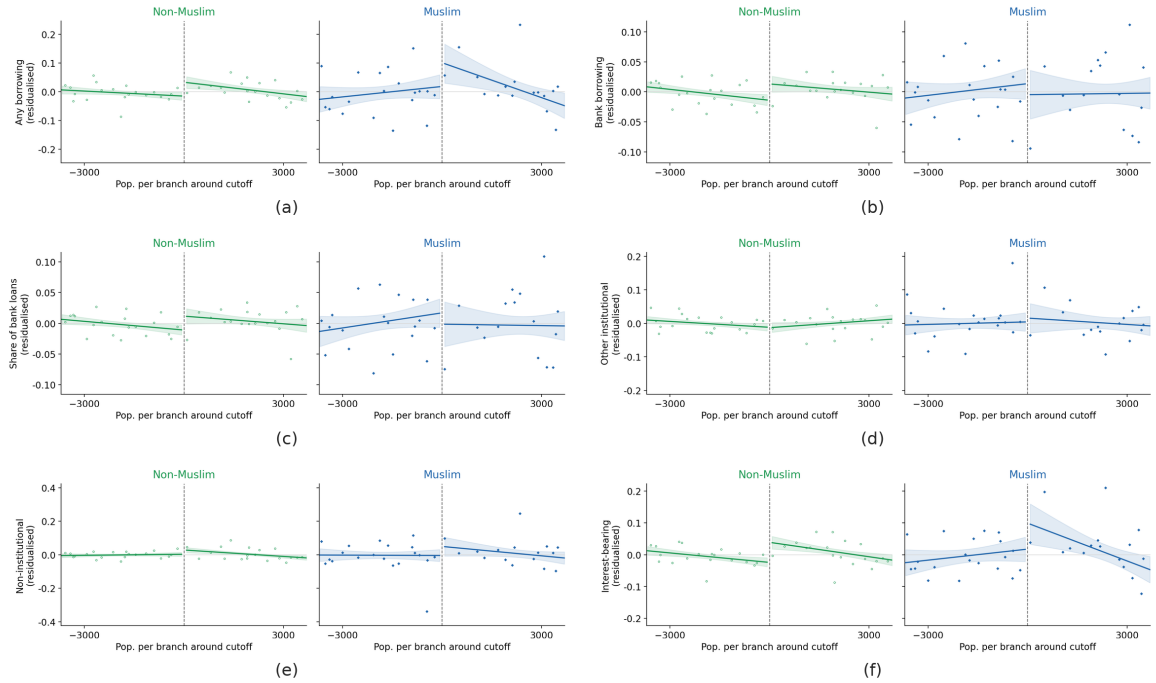
5.2.1 Non-Muslim Households

Table 3 summarises the treatment effects of the 2009 bank branch expansion policy. To begin with, the policy had a significant impact on the incidence of indebtedness and for non-Muslims this increase in borrowing was largely financed by bank loans. Incidence of bank borrowing and the share of bank loans for non-Muslim borrowers increased by 3.9 and 3.2 percentage points respectively. Consistent with this shift towards formal credit, the probability of taking at least one interest-bearing loan increased by 9.0 percentage points. I find no impact of the policy on the incidence of borrowing from any other institutional or non-institutional agency, thus households are not merely substituting alternative credit with bank loans. The results are robust to the inclusion of household-level covariates, specifically the age, sex, educational attainment and primary occupation of the household head, family size, and land ownership. Figure 2 plots the discontinuity in each borrowing outcome separately for non-Muslim and Muslim households, with the covariate-adjusted counterparts in Figure A23.

Given that the policy was targeted towards semi-urban centres with low population size, Table A2 examines whether the treatment effects vary by region. Among non-Muslim households in the rural

sector, the incidence of bank borrowing and the share of bank loans increased by 6.6 and 5.4 percentage points respectively, with no offsetting decline in borrowing from any other source. The 2009 bank branch expansion policy was thus effective in both expanding the extensive margin of bank borrowing and shifting the composition of household debt towards formal credit among non-Muslim households, with greater effects observed in rural areas consistent with the policy's design. The corresponding discontinuities for the rural sub-sample are plotted in Figure A4, with the covariate-adjusted counterparts in Figure A24.

Figure 2: Effect of the 2009 Policy on Household Borrowing



Discontinuity at the underbanked cutoff in each household borrowing outcome: (a) any borrowing, (b) bank borrowing, (c) share of bank loans, (d) borrowing from other formal sources, (e) borrowing from informal sources, and (f) interest-bearing borrowing, matching columns (1)–(6). Outcomes are residualised on state fixed effects; points are binned means and the solid lines local linear fits estimated separately on either side of the cutoff, with shaded 95 per cent confidence intervals. Data source: NSS AIDIS 70th round.

5.2.2 Muslim households

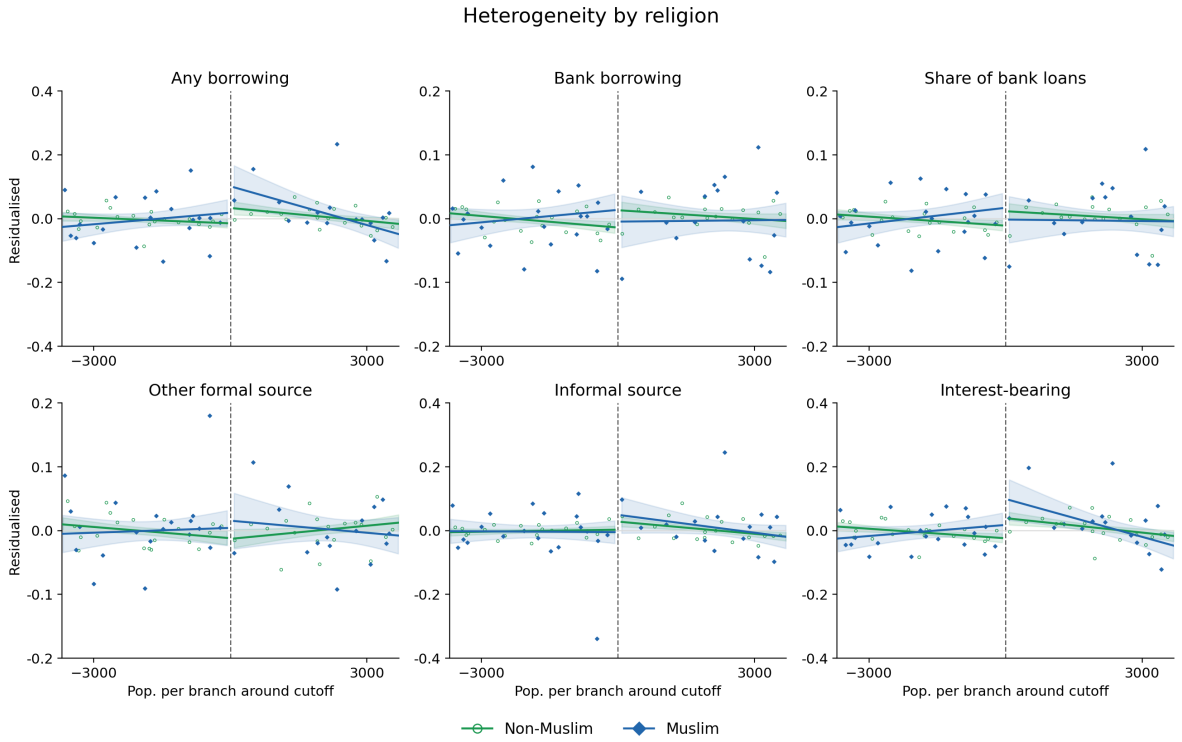
For Muslim households, the policy had no discernible effect on either the incidence of bank borrowing or the share of bank loans (Table 3). The rise in overall indebtedness was driven entirely by non-institutional sources. This pattern holds in the rural sub-sample, where the policy was directly targeted. The increase in interest-bearing loans among Muslim households reflects an

increase in informal loans, not bank credit, and since the policy did not directly stimulate informal lending, it would be fallacious to attribute this result to the branch expansion policy. The results remain consistent once we control for household characteristics.

5.2.3 Heterogeneity by religion

Table 4 formalises the between-group comparison using equation (3), which includes a full set of interactions between the underbanked indicator, the Muslim dummy, and the running variable. Figures 3 and A5 plot the corresponding reduced-form discontinuities for the full and rural samples respectively. The policy increased the incidence of bank borrowing among non-Muslim households by 3.7 percentage points. Muslim households, however, did not respond to the policy, and the coefficient on the interaction term $Muslim_i * D_d$ is negative and significant. Taken together, the policy widened the bank credit gap between the two groups by 6.2 percentage points, or 56 per cent relative to the control group mean. Figures A7 and A8 plot the reduced-form discontinuities underlying the mechanism estimates for the full and rural samples respectively. The same pattern holds for the share of household borrowing met by bank loans, where the gap widens by 5.6 percentage points. In the rural sub-sample, the interaction term is of similar sign, though the estimate is insignificant, owing to the smaller sample of rural Muslim households.

Figure 3: Heterogeneity by Religion in Household Borrowing



Discontinuity at the underbanked cutoff in each household borrowing outcome, plotted separately by religion. The six panels are any borrowing, bank borrowing, share of bank loans, borrowing from other formal sources, borrowing from informal sources, and interest-bearing borrowing. Outcomes are residualised on state fixed effects; the solid lines are local linear fits estimated separately on either side of the cutoff, with shaded 95 per cent confidence intervals. Data source: NSS AIDIS 70th round.

5.3 Effect on Firms

To estimate the impact of the branch expansion policy on firms, I use the sixth round of the Economic Census (2013), which enumerates data on the economic establishments operating in India. Of the 58.5 million establishments recorded, 59.5 per cent were located in rural areas and agriculture accounted for 34.7 per cent of establishments, followed by retail trade (22.2 per cent) and manufacturing (15.6 per cent). In urban areas, retail trade and manufacturing were the dominant sectors at 35.1 and 20.6 per cent respectively.

While the branch expansion policy aimed to redress regional disparity and an inequitable distribution of bank branches, even prior to the policy, banks were operational across most districts. Credit constraints were therefore unlikely to bind for large and medium-scale enterprises with sufficient working capital to access formal finance regardless of branch density. The relevant margin

here is therefore small firms, those that plausibly depend on local banking infrastructure to meet their borrowing needs. An average establishment in the Economic Census data employs 2.24 workers; I therefore restrict the sample to firms hiring less than three workers. These firms are usually small in size with limited capacity and come within the ambit of the policy's targeted recipients. All establishments belonging to this category are under self-proprietorship and are heavily self-financed.

Although no effect is observed for either of the policy overall, I find a positive treatment effect once we restrict our analysis to agricultural-based establishments. Table 5 reports the impact of the policy on bank borrowing, and the corresponding discontinuities are plotted in Figure A6, with the covariate-adjusted counterparts in Figure A25. Following the 2009 bank branch expansion policy, farm-based businesses are now 80% more likely to rely on bank credit. This effect is driven by firms that hire at least one worker, which is not surprising given that 41 million firms do not hire workers and 66.4% of them are residence-based. They're typically too small to qualify for or demand formal credit.

In contrast, when examining the impact of the policy on establishments owned by Muslims, I find no significant treatment effect. Even after restricting the analysis to agricultural-based establishments in the rural areas or controlling for the firm characteristics, the policy had no impact.

Thus the branch expansion policy significantly increased bank borrowing among non-Muslim households and firms, with effects concentrated in rural areas. No comparable effect is observed for Muslim borrowers. In the next section, I examine the mechanisms through which the branch expansion policy increased credit uptake, how the consequent credit gap between the two groups varies with local religiosity, and whether it reflects voluntary abstention from interest-bearing credit or other barriers to formal borrowing.

6 Mechanism

A natural question concerns the mechanism through which expanded banking access translates into greater formal credit uptake. This section identifies four supply-side channels through which

branch expansion can stimulate credit demand: an easing of credit constraints, a reduction in the average cost of borrowing, relaxed collateral requirements, and an expansion in the pool of eligible borrowers. While non-Muslim borrowers responded positively across these channels, the response is near-uniformly absent for Muslim borrowers, documented across all four margins. This is once again consistent with the idea that exclusion from formal borrowing is driven by religious preferences rather than by structural impediments to credit access.

6.1 Credit Constraints

An important precondition for branch expansion to raise borrowing is that households and firms have unmet demand for credit, either through binding constraints or latent willingness to borrow at prevailing terms. Previous work in the literature provides direct evidence that small firms in India experience binding credit constraints (Banerjee and Duflo, 2014). One way to detect whether borrowers face such constraints or are simply shifting from informal to formal sources is by assessing whether households and firms increase their total indebtedness following the policy. If borrowers in treated districts were already meeting their credit needs through non-institutional lenders and merely substituted towards cheaper bank loans, aggregate indebtedness should be unaffected. In contrast, if the policy relieved binding credit constraints, total borrowing should rise. Among non-Muslim households in treated districts, overall indebtedness, the incidence of bank borrowing, and the share of bank loans for indebted households rise significantly, while borrowing from other formal or informal sources shows no statistically significant decline (Table 3). This pattern is inconsistent with mere credit substitution and indicates that a substantial share of the additional bank loans represents new credit rather than a reshuffling of the existing debt portfolio.

6.2 Cost of Borrowing

A second channel through which branch expansion may increase formal borrowing is a reduction in the average cost of credit. Informal financial intermediaries including agricultural money lenders and professional money lenders typically charge interest rates substantially above those prevailing at commercial banks. To the extent that limited bank presence previously constrained households

to high-cost informal lenders, entry by commercial banks in underbanked districts may lower the prevailing cost of borrowing. Consistent with this mechanism, the average interest rate on interest-bearing loans taken by non-Muslims is approximately 3 percentage points (Table 6) lower in treated districts. This reduction in the cost of loans would not only lead treated households to borrow more but would also induce households at the margin to take up formal loans they would not otherwise demand. Muslims in treated districts pay higher interest rates relative to those in control districts, which confirms that they do not take advantage of the reduction in the cost of formal borrowing.

6.3 Collateral Requirements

The third channel operates through the relaxation of collateral requirements. The 2009 bank branch expansion policy coincided with two RBI notifications that significantly relaxed collateral requirements on small loans. In August 2009, under the Swarnajayanti Gram Swarozgar Yojana (SGSY), the exemption threshold on secondary collateral security was raised from INR 50,000 to INR 100,000 for individual loans and from INR 500,000 to INR 1,000,000 for group loans. Subsequently, in June 2010, the RBI waived all security requirements for agricultural loans up to INR 100,000. These regulatory changes would be expected to benefit primarily low-income households for whom collateral constraints previously constituted a barrier to formal credit. Among non-Muslim households in treated districts, the take-up of collateral-free loans of up to INR 100,000 increased by 5.2 percentage points, and collateral-free agricultural loans increased by 6.3 percentage points (Table 6). No significant effects are observed for Muslim households along either margin.

6.4 Risky Borrowing

A fourth channel concerns the possibility that the policy expanded borrower eligibility at the extensive margin, by inducing banks to accept applications from borrowers whom they would previously have screened out. As banks sought to meet the RBI's priority-sector lending targets in underbanked districts, they may have extended credit to riskier applicants who were previously excluded. If so, the policy could have increased default rates in the treated group, as loans were now being offered to borrowers with poor eligibility. This prediction is not borne out empirically. Defining

default as the amount outstanding on the survey date as a share of the original amount borrowed, and focusing on short-term and medium-term loans taken at least one year and four years, respectively, before the survey date, I find no significant difference in default rates between treated and control districts.

Taken together, the evidence suggests that the policy did not expand the pool of risky borrowers and that the increase in formal credit uptake reflects an increase in demand by creditworthy households that were previously either credit-constrained, deterred by high borrowing costs, or excluded by collateral requirements. Muslim households in treated districts faced the same reduction in borrowing costs, relaxation of collateral requirements, and improved access to new bank branches yet no comparable effects are observed for them. The ineffectiveness of the policy for Muslim borrowers is therefore difficult to explain through supply-side factors alone. Table A3 reports the corresponding estimates for the rural sub-sample.

7 Effect of Religiosity

Although religion can determine economic behaviour and outcomes, its effect may not be entirely homogeneous. Whether Islam's prohibition on interest-bearing transactions can affect banking behaviour depends on one's adherence to their religion. As per the 7th round (2017-2022) of the World Values Survey Data, Muslims in India report a higher level of religiosity as compared to non-Muslims. 83.7 per cent of them identify themselves as religious as compared to 79.1 per cent of non-Muslims. 94.4 per cent of Muslims believe in God and 30 to 35 per cent of them believe in heaven and hell. Furthermore about 30 per cent of Muslims attend religious services more than once a week and 37.4 per cent of them, following the daily obligatory prayers in Islam, offer prayers several times a day. In this section I incorporate the heterogeneous impact of religiosity on borrowing behaviour. Since individual-level data on religiosity is not available, I use district- and sector-level counts of religious institutions, specifically the number of mosques and madrasas (Islamic schools), as proxies for local religious intensity. This approach is well-established in the economics of religion literature. Bazzi et al. (2020b) use village-level counts of mosques and Islamic boarding schools from Indonesia

to measure local Islamic institutional presence, and find that a higher density of such institutions is associated with stronger Islamist political outcomes and lower agricultural productivity. In related work on the same setting, Bazzi et al. (2020a) treat local Islamic institutions as both measures of and channels for religious intensity, in their study of how schooling shapes religiosity in Indonesia. A similar approach is taken by Brooke and Masoud (2016), who use mosque density as a direct measure of religious infrastructure in their study of Islamist electoral support in Egypt.

The distribution of these institutions, particularly mosques and madrasas, across India is closely linked to both the historical spread of Islam and the contemporary demographic profile of Muslims. Islam entered the Indian subcontinent primarily through two avenues. First, through trade routes connecting the Indian subcontinent with the Arab world. Trade relations between India and the Arab world have a long history, with Malabar and Kochi, now parts of Kerala, serving as key hubs for Arab and Persian traders. The Cheraman Juma Mosque in Kerala is believed to be the first mosque in India, established by the last ruler of the Chera dynasty, who converted to Islam and facilitated its spread in the Malabar region. Second, the spread of Islam was significantly influenced by Muslim rulers and dynasties, starting with the Delhi Sultanate and continuing through the Mughal era. By the end of the seventeenth century, Mughal rule extended across Kabul, Kashmir, Delhi, Bengal, Odisha, Gujarat, Rajasthan, and parts of the Deccan. During this period, numerous mosques were built, and Islamic education was promoted. It is not surprising that contemporary Muslim populations are notably concentrated in regions with a rich Islamic history and large number of Islamic institutions. States with a high density of such institutions, namely mosques and madrasas, include Uttar Pradesh, Rajasthan, West Bengal, and Haryana, regions formerly ruled by Muslim kings, as well as Kerala, Karnataka, Gujarat, and Maharashtra, regions that were central to trade between India and the Arab world.

To estimate the treatment effect heterogeneity, more precisely the effect of moving from areas with fewer to those with more religious institutions, I augment the baseline RD with full interactions between the treatment indicator and the number of religious institutions. Following Calonico et al. (2015), this specification allows for a clean separation of the average treatment effect from

its heterogeneous component. One concern with this variable is that the number of religious institutions may itself independently impact borrowing outcomes through channels other than one's own religious adherence. However, since the empirical specification in equation (4) includes this count both as a control and interacted with the treatment indicator and the running variable, any residual direct effect on borrowing behaviour is absorbed by the level term. Since data on religious institutions are available at the district level and separately by sector, I estimate the specification separately for urban and rural households. The RD setup is specified below:

$$\begin{aligned} Y_i^j = \beta_0 + \beta_1 D_d + \beta_2 Z_d + \beta_3 Z_d * D_d + \beta_4 RI_{dr} + \beta_5 RI_{dr} * D_d \\ + \beta_6 RI_{dr} * Z_d + \beta_7 RI_{dr} * D_d * Z_d + \beta_8 P_{dr} + \beta_9 S_s + \epsilon_i \end{aligned} \tag{4}$$

RI_{dr} is the number of religious institutions in a particular sector r in a district d and P_{dr} is the size of the Muslim population in that area. As in equation (3), the fully interacted 2SLS analogue is weakly identified, and I therefore estimate the reduced form equation, regressing the outcome on the underbanked assignment indicator and its interactions. Here β_1 gives the intent-to-treat effect of the policy for the baseline group and β_5 captures how this effect varies with religiosity, that is, the differential effect of the policy as the number of religious institutions increases by one standard deviation.

Table 7 reports the heterogeneity estimates for Muslim households, and Figure A9 plots the corresponding coefficients. For urban Muslim households, the baseline RD estimate β_1 is statistically insignificant across all borrowing outcomes, consistent with the main results in Table 3. However, the interaction term β_5 is negative and significant for the incidence of bank borrowing and the share of bank loans. Bank borrowing and the share of bank loans decline by 3.9 and 3.8 percentage points respectively for a one standard deviation increase in the number of religious institutions. This confirms that in more religiously dense urban districts, the policy is even less effective at inducing formal bank credit among Muslim households.

For rural Muslim households β_1 is positive and significant for overall indebtedness and the likelihood of taking an interest-bearing loan, nonetheless, the RD estimate and the estimate on the interaction

term for the two main outcome variables remain insignificant. Thus, while the local presence of religious institutions appears to impose a meaningful constraint on formal borrowing among Muslim households in urban areas, no such pattern is observed among rural households.

8 Alternative Channels

Muslims in India are economically disadvantaged and face discrimination in credit markets, residential segregation, and occupational rigidity. Given these structural differences, the differential impact of the branch expansion policy on Muslim and non-Muslim borrowers need not reflect religious preferences alone. It is possible that Muslims, by virtue of their occupational profile or inherently lower demand for formal credit, were less likely to benefit from the policy, or that lender bias continued to constrain their access to bank credit even after branch expansion. It is also possible that new branches were systematically located away from Muslim-concentrated neighbourhoods, leaving them with inadequate geographical access to banking services. I address each of these concerns below and find no evidence that lender discrimination, differences in loan demand, or accessibility to banks can account for the observed difference.

8.1 Lender Discrimination

In this section I analyse whether supply-side discrimination drives the null impact of the bank branch expansion policy on Muslim borrowers. Muslims in India have poor formal credit market participation. Fisman et al. (2017), exploiting the quasi-random rotation of loan officers across branches of an Indian bank, show that cultural proximity between officers and borrowers significantly expands credit access, raises loan sizes, and lowers collateral requirements. Muslim borrowers are thus disadvantaged in the majority of interactions where they face officers from a different social group. A response to a Right to Information (RTI) query revealed that in 2015–16 the share of Muslim recipients in priority-sector lending was marginally above 2 per cent, far below their population share of approximately 14 per cent. If lenders in underbanked districts subject Muslim applicants to harsher scrutiny, even after the branch expansion policy, the null effect on Muslim

borrowing could reflect constrained supply rather than absent demand. This section addresses that concern through two complementary tests: a conditional loan approval analysis and an outcome test for taste-based lender discrimination.

8.1.1 Loan Approval Rates

A first pass at the discrimination question comes from examining conditional loan approval rates. A standard benchmark test for discrimination (Blanchflower et al. 2003) compares approval rates across groups after controlling for observed markers of creditworthiness of the applicant. Using the second round of the India Human Development Survey (IHDS-II, 2011–12), I find that Muslim households are significantly less likely to receive a loan conditional on applying for one. Furthermore, conditional on household characteristics, Muslims are 5.4 percentage points less likely to apply for a bank loan in the first place. Although anticipated discrimination may itself discourage loan applications (Fiorin et al., 2025), a gap of this size is difficult to reconcile solely with perceived lender-discrimination. Crucially, however, when the analysis is restricted to districts within the narrow bandwidth around the policy threshold, neither the gap in loan approval nor the gap in loan application between Muslim and non-Muslim households in the treated districts relative to the difference between the two groups in the control district remains statistically significant (Table A4).

8.1.2 Outcome Test

To address the endogeneity concerns in the benchmark tests, I implement Becker's (1957, 1993) outcome test for taste-based discrimination. The intuition of the outcome test is straightforward: if lenders are prejudiced against a particular group, they will apply a more stringent screening threshold to members of that group. As a result, the marginal borrower for the discriminated group should exhibit higher creditworthiness (Butler et al. 2023). Conversely, if the screening threshold is uniform across groups, default rates at the margin should be equalized.

Using the AIDIS dataset, I define default as the amount outstanding on the survey date as a share of the amount borrowed originally. I restrict the sample to short-term loans (due within 12 to 18 months) borrowed at least one year prior to the survey date, and to medium-term loans (due within

three to five years) borrowed at least four years prior to the survey date, allowing enough time for repayment to have come due. To isolate marginal borrowers, those most likely to be affected by lender screening, I focus on loans below INR 100,000, a threshold that coincides with the collateral exemption limits introduced by the RBI alongside the 2009 branch expansion policy. The outcome test yields no evidence of taste-based discrimination against Muslim borrowers (Table A4), and Figure A10 plots the corresponding reduced-form coefficients. The religion of the borrower has no significant effect on the probability of default on bank loans, and is robust to the inclusion of household-level controls.

8.2 Demand for loans

In this section, I examine the demand for credit among Muslim and non-Muslim households and firms. If there are inherent differences in borrowing needs between the two groups, these may help explain the differential impact of the policy. Using economic census data, I first analyse loan demand across various occupational groups. In rural areas, Muslim-owned firms are primarily engaged in manufacturing (24.2%), retail trade (29.9%), and agriculture-related activities (21.2%). In contrast, for non-Muslim-owned firms, agriculture is the dominant sector (40.6%), followed by retail trade (22.9%) and manufacturing (15.8%). When we focus on the underbanked districts near the policy threshold, approximately 10.5% of Muslim-owned firms and 14.5% of non-Muslim-owned firms operate in agriculture or agriculture-related activities. In other rural occupations, the shares are 9% and 8.5% for Muslim and non-Muslim owned firms. Around the threshold, significant policy effects are observed for agricultural businesses owned by non-Muslims, while no comparable effects are found for Muslim-owned firms (Table A5). No significant treatment effects are detected in any other occupational category.

Next, I look at the AIDIS dataset to examine household demand for consumption loans. Approximately 33.5% of non-Muslim households and 31.8% of Muslim households in India have taken at least one loan to meet their consumption needs. Most borrowing is directed towards medical expenses, housing, and other household expenditures. As an outcome of the policy, bank loans taken

for consumption purposes by non-Muslim households in rural India increase by 1.9 percentage points (Table A5; Figure A11). No such effects are observed for Muslim households.

Regardless of a household's primary occupation or the intended purpose of the loan, I observe positive and statistically significant treatment effects for non-Muslim borrowers. In contrast, the policy appears to have had no measurable impact on Muslim households or firms. Thus the differential effects of the bank branch expansion policy cannot be explained by differences in occupational structures or disparities in loan demand between the two groups.

8.3 Accessibility to Banks

In this section, I study whether the ineffectiveness of the policy for Muslim households and firms can be attributed to impediments in physical accessibility to banks. The Sachar Committee Report (2006) identified several Muslim-concentrated districts as informal negative zones with poor formal credit options. Asher et al. (2026) show that public amenities are systematically located away from neighbourhoods where Muslim and scheduled caste communities are predominantly found. This compels me to examine the exact location of new branches at the sub-district level and inspect whether Muslim households had sufficient access to them.

To evaluate this, I use the RBI Master Office File (MOF), which records the precise geographic location of every scheduled commercial bank branch opened between 2006 and 2013. I define p_i as the probability of a borrower obtaining a loan in sub-district i, where p_i is measured as the number of bank branches opened between 2006 and 2013 divided by the total population in subdistrict i. Variation in p_i is driven primarily by differences in the number of new branches across sub-districts, with greater expansion corresponding to higher branch availability per capita. Next, I multiply this probability by the size of the Muslim population in sub-district i, denoted m_i, and sum across all sub-districts to obtain the expected probability of a Muslim borrower obtaining a bank loan in district j, $E(m_j)$.

Table A6 and Figure A12 report the impact of the policy separately across districts with relatively higher and lower bank accessibility for Muslim households, defined by whether the expected probability $E(m_j)$ lies above or below the median. The incidence of bank borrowing and the share of bank loans among Muslim households is 10.6% (7.4%) and 8.9% (6.6%) in districts above (below) the median. Consistent with the baseline results, there is no evidence of a positive policy effect on the two primary indicators of bank borrowing, even in districts where Muslims had relatively greater access to banks. Given that districts above the median also have a higher share of Muslims, it is not surprising that institutional borrowing is significantly lower in these areas, accompanied by greater reliance on non-institutional sources of credit. Overall, the results suggest that improved access to banking services for Muslim households does not necessarily translate into higher uptake of bank loans.

9 Robustness Checks

To address concerns regarding any prior differences between the two groups and demonstrate the robustness of my results to alternative specifications, I formally run a difference-in-discontinuities model and a series of robustness and placebo checks.

9.1 Difference-in-discontinuities (DIF-IN-DISC) approach

A key concern with the finding that branch expansion raised bank borrowing among non-Muslim households and firms but had no comparable effect on Muslim borrowers is that the two groups may have been on different pre-existing trajectories at the threshold even before the policy was implemented. If Muslim and non-Muslim borrowers were already diverging in their credit uptake prior to the policy, the cross-sectional RD comparison would conflate this differential trend with the causal effect of branch expansion. To address this, I implement a difference-in-discontinuities (Diff-in-Disc) model as a sensitivity check following Grembi et al. (2016). The estimator pools pre-policy and post-policy data and identifies the treatment effect as the change in the RD discontinuity from before to after the policy, estimated separately for each group. Any existing difference in

unobservables on either side of the threshold for each group is absorbed by the pre-policy period discontinuity estimate and differenced out. I pool the 59th round of the AIDIS (2002–03) as the pre-policy period with the 70th round (2012–13) as the post-policy period, and estimate the following specification separately for Muslim and non-Muslim households:

$$\begin{aligned} Y_i = \delta_0 + \delta_1 D_d + \delta_2 Post_t * D_d + \delta_3 Z_d + \delta_4 Z_d * D_d + \delta_5 Post_t \\ + \delta_6 Post_t * Z_d + \delta_7 Post_t * Z_d * D_d + \delta_8 X_i + \delta_9 S_s + \eta_i \end{aligned} \tag{5}$$

Equation (5) is estimated using the local polynomial regression on the subsample of observations that lie within the bandwidth. The specification here is estimated in reduced form, with the outcome regressed on the underbanked assignment indicator D_d and its interaction terms, for the reason discussed in Section 3.1. $Post_t$ is an indicator equal to one in the post-policy period and all other variables are as defined previously. The coefficient δ_1 captures any discontinuity at the threshold in the first period, while δ_2, the parameter of interest, identifies the policy-induced change in the discontinuity, after controlling for any group-specific pre-existing gap.

The identification rests on three assumptions. First, districts cannot manipulate their population-per-branch ratio or selectively sort themselves into either group, a possibility ruled out by the McCrary density test discussed in Section 3.2. Second, potential outcomes should be continuous at the cutoff prior to the implementation of the policy. Consistent with this, both main banking outcomes yield non-significant RD estimates for Muslims and non-Muslims alike in 2003, as captured by δ_1. Third, the effect of any confounding factor must be constant over time across the treated and control groups. This assumption cannot be formally tested, as AIDIS data disaggregated by the religious group of the borrower is only available from 2003 onward. If the results obtained in Section 5 are not an outcome of any pre-existing variation or unobserved factors, then δ_1 should be statistically indistinguishable from zero for both the groups, and δ_2 should reproduce the baseline RD estimates.

We see this in Tables A7 and A8; Figures A13 and A14 plot the pre- and post-policy discontinuities for non-Muslim and Muslim households in the full sample, and Figures A15 and A16 do so for the

rural sector. For both Muslim and non-Muslim households, the pre-policy discontinuity in bank borrowing outcomes is small and statistically insignificant. For non-Muslim borrowers, the Diff-in-Disc estimate is statistically significant for the incidence of bank borrowing and for both the incidence of bank borrowing and the share of bank loans in the rural sector. Furthermore, it closely mirrors the RD estimates obtained in Section 5. For Muslim households, δ_2 is negative although insignificant across all columns, consistent with the absence of any positive impact of the policy.

9.2 Robustness and Placebo Tests

To assess the robustness of the main RD estimates, I conduct several sensitivity checks (Tables A10 and A11). The corresponding specification curves for the household outcomes are plotted in Figures A18 (full sample) and A19 (rural sector). First, I examine the stability of the estimates across alternative bandwidth choices, including the MSE-optimal bandwidth of Calonico et al. (2014), that separately optimizes bandwidths on each side of the cutoff, and the coverage-error-rate optimal (CER) bandwidth of Calonico, Cattaneo, and Farrell (2020), which minimises the coverage error of the robust confidence interval. Under the MSE-optimal bandwidth, the effects on bank borrowing and interest-bearing loans for non-Muslim households remain positive, though marginally insignificant, while the bank borrowing coefficients for Muslim households are negative across all bandwidth choices. Under the CER-optimal bandwidth, however, these effects for non-Muslim households are positive and statistically significant, with bank borrowing and the share of bank loans rising by 3.7 and 3.1 percentage points respectively, while the bank borrowing coefficients for Muslim households remain negative. Second, I assess sensitivity to the choice of polynomial order and kernel. The results are robust to a triangular kernel. They do not hold under a quadratic polynomial in the running variable, a pattern commonly observed in settings with limited observations near the threshold (Gelman and Imbens, 2019). The rural sub-sample closely mirrors the main specification. The policy has a positive and significant effect on bank borrowing among non-Muslim households, while the effects for Muslim households remain insignificant across all specifications.

Finally, I conduct a placebo cutoff analysis by re-estimating the baseline specification at four artificial thresholds, ± 1000 and ± 2000 on either side of the true cutoff, for banking-sector outcomes

(Table A12; Figure A20) and for household borrowing outcomes overall and in the rural subsample (Tables A13 and A14; Figures A21 and A22). The estimates are uniformly insignificant at ±1000 and at the +2000 threshold, for both Muslim and non-Muslim households, indicating no spurious discontinuities in that range. A small number of estimates attain significance at the −2000 threshold, most consistently on interest-bearing credit, alongside bank borrowing for Muslim households and the broader borrowing margins for non-Muslims. While this might appear to strain the design, the estimates there retain the sign of the baseline, negative for Muslims and positive for non-Muslims, which is precisely what the overlap in the estimation window would predict. These are likely to reflect a residual effect on account of the estimation window overlapping the true cutoff, rather than a spurious discontinuity at this placebo threshold. Taken together, these checks lend further credibility to the identifying assumption that potential outcomes vary smoothly at the true threshold and support the validity of the findings.

10 Conclusion

This paper provides causal evidence that religious identity shapes financial decision-making in economically meaningful ways. Exploiting a pair of bank branch expansion policies as exogenous supply-side shocks, I show that improved access to formal banking significantly raised credit uptake among non-Muslim households and firms, but had no comparable effect on Muslim borrowers. The bank credit gap between Muslims and non-Muslims in the underbanked districts widened by 6.2 percentage points, and this divergence persists even after ruling out lender discrimination, differential demand, and unequal physical access to banking infrastructure as alternative explanations. The heterogeneity analysis further shows that this gap in formal borrowing is most pronounced in districts with a higher density of religious institutions, consistent with the view that stronger adherence to Islamic norms actively discourages engagement with interest-bearing credit. These findings carry broad implications. First, they establish that financial exclusion need not be purely a supply-side phenomenon. Even when credit is made cheap, accessible, and collateral-free, religiously motivated preferences can sustain a persistent gap in formal credit uptake. This has direct relevance for financial inclusion policy in economies with large Muslim populations, where expanding branch networks

or reducing borrowing costs may be insufficient without complementary supply of Shariah-compliant financial products. Second, the results speak to the identity economics literature by demonstrating that religious norms can generate economically significant deviations from standard borrowing behaviour, with consequences that extend beyond individual welfare to productive investment and business growth. Several important questions remain open. This paper does not examine the saving side of the household balance sheet, though the same prohibition that deters bank borrowing also discourages interest-bearing deposits, implying a lower opportunity cost of informal lending within Muslim communities. Whether the availability of intra-community interest-free loans attenuates or reinforces religious abstention from formal credit is a question that warrants further investigation. More broadly, the welfare implications of religiously motivated financial exclusion, particularly its effects on productive investment, business expansion, and inter-generational mobility remain an important area for future research.

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Tables

Table 1: Household Summary Statistics

	Non-Muslim		Muslim	
	All districts	$[-3700, +3700]$	All districts	$[-3700, +3700]$
	(1)	(2)	(3)	(4)
Panel A: Household Head Characteristics				
Years of education	5.64	5.63	4.61	4.78
	(3.78)	(3.78)	(3.34)	(3.43)
Rural self-employed (yes/no)	0.36	0.33	0.30	0.25
	(0.48)	(0.47)	(0.46)	(0.43)
Urban self-employed (yes/no)	0.10	0.11	0.17	0.22
	(0.30)	(0.31)	(0.37)	(0.41)
Rural worker (yes/no)	0.18	0.17	0.14	0.11
	(0.38)	(0.38)	(0.34)	(0.31)
Urban worker (yes/no)	0.15	0.15	0.11	0.12
	(0.35)	(0.35)	(0.31)	(0.32)
Urban casual labour (yes/no)	0.10	0.11	0.12	0.14
	(0.29)	(0.32)	(0.32)	(0.35)
Panel B: Asset Ownership				
Owns land (yes/no)	0.85	0.82	0.85	0.84
	(0.36)	(0.38)	(0.36)	(0.36)
Owns building (yes/no)	0.84	0.81	0.85	0.84
	(0.37)	(0.39)	(0.35)	(0.37)
Owns livestock (yes/no)	0.38	0.37	0.35	0.34
	(0.49)	(0.48)	(0.48)	(0.47)
Value of physical assets (Rs)	1,460,557	1,229,940	923,034	923,780
	(13,200,000)	(3,577,693)	(2,358,244)	(2,243,916)
Value of financial assets (Rs)	54,057	53,725	24,648	26,986
	(282,763)	(269,172)	(120,872)	(135,437)
Debt–asset ratio	0.54	0.75	0.57	1.13
	(17.08)	(11.48)	(12.80)	(14.37)
Panel C: Borrowing Portfolio				
Own a bank account (yes/no)	0.74	0.76	0.63	0.71
	(0.44)	(0.43)	(0.48)	(0.45)
Any borrowing (yes/no)	0.29	0.30	0.24	0.24
	(0.45)	(0.46)	(0.43)	(0.43)
Borrowed from bank (yes/no)	0.07	0.07	0.05	0.06
	(0.26)	(0.26)	(0.21)	(0.23)
Bank borrowing, mean (yes/no)	0.06	0.06	0.04	0.05
	(0.23)	(0.23)	(0.19)	(0.21)
Other institutional (yes/no)	0.11	0.12	0.07	0.07
	(0.31)	(0.33)	(0.26)	(0.26)
Non-institutional (yes/no)	0.16	0.16	0.14	0.14
	(0.36)	(0.37)	(0.35)	(0.35)
Interest-bearing loan (yes/no)	0.25	0.27	0.18	0.18
	(0.43)	(0.44)	(0.38)	(0.39)
Amount borrowed (Rs)	137,411	151,597	86,248	79,700
	(868,208)	(1,197,748)	(412,395)	(281,881)
Amount outstanding (Rs)	53,407	59,695	31,260	29,691
	(418,672)	(593,131)	(174,591)	(137,670)
Observations	97099	29274	13701	3056

Survey-weighted means with standard deviations in parentheses. Data source: NSS AIDIS 70th round. Columns (1) and (3) cover all districts; columns (2) and (4) restrict to districts within a bandwidth of $[-3700, +3700]$ around the cutoff. Rupee amounts are rounded to the nearest whole rupee. The number of observations varies slightly across rows due to missing values; for example, in the all-districts columns land ownership has 97,061 (non-Muslim) and 13,695 (Muslim) observations, and the debt–asset ratio has 95,306 and 13,310, respectively.

Table 2: Impact on the banking sector

	All banks (number)	Private banks (number)	No. of accounts (log no.)	Amount deposited (log no)	Credit outstanding (log no.)
	(1)	(2)	(3)	(4)	(5)
A. Main specification	3.246	8.729*	0.085	0.100	0.283*
	(10.533)	(4.613)	(0.082)	(0.096)	(0.149)
Observations	179	178	179	179	179
Control group mean	222.086	33.990	2064034	96446.12	73304.22

Impact on the Banking sector. Data source: RBI basic statistical returns and Census 2001. This table presents regression discontinuity estimates of the effect of the 2009 branch expansion policy on aggregate banking outcomes. Standard errors are clustered at the district level. The specification includes state fixed effects and controls for the outcome variable in the pre-policy period. * p<0.10, ** p<0.05, *** p<0.01.

Table 3: Treatment effect of the 2009 policy on households

	Any borrowing	Bank borrowing	Share of bank loans	Other formal source	Informal source	Int-bearing
	(1)	(2)	(3)	(4)	(5)	(6)
			Non-Muslim			
A. Main specification	0.068***	0.039**	0.032*	0.000	0.034	0.090***
	(0.025)	(0.019)	(0.017)	(0.019)	(0.022)	(0.027)
B. Covariate adjustment	0.068***	0.043**	0.037**	0.001	0.029	0.091***
	(0.024)	(0.019)	(0.017)	(0.019)	(0.021)	(0.027)
Observations	29274	29274	29274	29274	29274	29274
Control group mean	0.431	0.111	0.096	0.176	0.206	0.373
			Muslim			
A. Main specification	0.089*	-0.031	-0.033	0.011	0.069*	0.088*
	(0.046)	(0.036)	(0.030)	(0.034)	(0.038)	(0.048)
B. Covariate adjustment	0.100**	-0.019	-0.027	0.016	0.077**	0.102**
	(0.044)	(0.035)	(0.030)	(0.033)	(0.037)	(0.047)
Observations	3056	3056	3056	3056	3056	3056
Control group mean	0.410	0.104	0.091	0.126	0.228	0.295
p-value from NM=M	0.646	0.033	0.016	0.808	0.341	0.963

Main results using 2009 policy. Data source: NSS AIDIS 70th round. This table presents regression discontinuity estimates from the main estimating equation of the effect of the 2009 branch expansion policy on borrowing outcomes. Standard errors are clustered at the district level. All specification includes state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: Treatment effect of the 2009 policy by Religion

	Any borrowing	Bank borrowing	Share of bank loans	Other formal source	Informal source	Int-bearing
	(1)	(2)	(3)	(4)	(5)	(6)
			Overall			
A. Underbanked	0.068**	0.039*	0.032	-0.000	0.034	0.089***
	(0.029)	(0.023)	(0.021)	(0.022)	(0.026)	(0.030)
B. Underbanked*Muslim	0.037	-0.064**	-0.057**	0.033	0.036	0.042
	(0.051)	(0.032)	(0.029)	(0.045)	(0.049)	(0.064)
C. Muslim	0.005	0.004	0.005	-0.021	0.009	-0.037
	(0.035)	(0.018)	(0.017)	(0.021)	(0.030)	(0.033)
Observations	32330	32330	32330	32330	32330	32330
Control group mean	0.429	0.111	0.095	0.172	0.208	0.365
			Rural			
A. Underbanked	0.078**	0.062***	0.050***	0.016	0.014	0.099**
	(0.037)	(0.019)	(0.017)	(0.024)	(0.031)	(0.038)
B. Underbanked*Muslim	0.106	-0.039	-0.033	0.095	0.040	0.118
	(0.093)	(0.049)	(0.047)	(0.058)	(0.070)	(0.095)
C. Muslim	-0.018	0.042	0.042	-0.042	-0.071*	-0.042
	(0.059)	(0.032)	(0.029)	(0.033)	(0.042)	(0.053)
Observations	17373	17373	17373	17373	17373	17373
Control group mean	0.416	0.084	0.067	0.171	0.230	0.349

Results using 2009 policy and equation 3. Data source: NSS AIDIS 70th round. The table presents heterogeneous effects of the 2009 branch expansion policy by religion. To ensure identification, the table presents reduced-form estimates, where the outcome is regressed on the underbanked assignment indicator and its interactions rather than the instrumented treatment, for the reason discussed in Section 3.1. The point estimates may therefore differ slightly from the fuzzy RD estimates in Tables 3a and 3b. Standard errors are clustered at the district level. All specifications include state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table 5: Treatment effect of the policy on firms

	Overall	Rural	Farm-based
	(1)	(2)	(3)
	Non-Muslim		
A. Main specification	0.001	0.001	0.012**
	(0.004)	(0.007)	(0.006)
B. Covariate adjustment	0.001	0.001	0.012*
	(0.004)	(0.007)	(0.006)
Observations	2628683	1127061	353534
Control group mean	0.025	0.027	0.015
	Muslim		
A. Main specification	0.010	0.032	-0.006
	(0.010)	(0.029)	(0.013)
B. Covariate adjustment	0.011	0.033	-0.006
	(0.010)	(0.029)	(0.013)
Observations	281806	82137	14230
Control group mean	0.028	0.041	0.024
p-value from NM=M	0.298	0.323	0.306

Main results for economic establishments. Data source: Economic Census 6th round. This table presents regression discontinuity estimates from the main estimating equation of the effect of the 2009 branch expansion policy on the major source of finance for firms. Standard errors are clustered at the district level. All specifications include state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table 6: Mechanism

	Avg. interest rate	Collateral-free loan	Collateral-free agri loan	Loan default rate
	(1)	(2)	(3)	(4)
		Non-Muslim		
RD estimate	−2.991**	0.052**	0.063***	−0.024
	(1.262)	(0.021)	(0.019)	(0.048)
Observations	20,893	18,500	5,816	1,753
Control group mean	19.867	0.063	0.073	0.979
		Muslim		
RD estimate	5.652**	0.025	−0.171	0.006
	(2.565)	(0.040)	(0.141)	(0.119)
Observations	1,713	1,845	288	169
Control group mean	20.550	0.055	0.080	0.959

Mechanisms underlying the 2009 branch expansion policy. Data source: NSS AIDIS 70th round. This table presents fuzzy RD estimates of the effect of the 2009 branch expansion policy categorised by credit market mechanisms. Column (1) reports the average interest rate on bank loans, columns (2) and (3) report the likelihood of receiving a collateral-free bank loan for all borrowers and agricultural households respectively; column (4) reports the default rate on short- and medium-term bank loans. Standard errors are clustered at the district level. All specifications include state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table 7: Treatment effect by religiosity

	Any borrowing	Bank borrowing	Share of bank loans	Other formal source	Informal source	Int-bearing
	(1)	(2)	(3)	(4)	(5)	(6)
			Urban			
A. RD estimate	-0.080	-0.022	-0.021	-0.030	-0.068	0.002
	(0.067)	(0.034)	(0.030)	(0.063)	(0.060)	(0.062)
B. RD x Religiosity	-0.060	-0.039**	-0.038**	-0.038	-0.004	-0.063
	(0.044)	(0.019)	(0.017)	(0.027)	(0.037)	(0.044)
Observations	1711	1711	1711	1711	1711	1711
Control group mean	0.422	0.092	0.082	0.139	0.236	0.303
			Rural			
A. RD estimate	0.206*	0.121	0.106	-0.017	-0.017	0.225*
	(0.121)	(0.075)	(0.066)	(0.062)	(0.087)	(0.126)
B.RD x Religiosity	-0.559***	-0.030	0.003	-0.387***	-0.567***	-0.259
	(0.201)	(0.139)	(0.121)	(0.112)	(0.144)	(0.208)
Observations	1253	1253	1253	1253	1253	1253
Control group mean	0.390	0.125	0.105	0.105	0.214	0.281

Heterogeneity effects of the 2009 branch expansion policy by the number of religious institutions. Data source: NSS AIDIS 70th round and WAMSI. This table presents regression discontinuity estimates of the 2009 branch expansion policy with complete interaction between the underbanked assignment indicator and the number of religious institutions. To ensure identification, the estimates are reduced-form, with the outcome regressed on the assignment indicator and its interactions, for the reason discussed in Section 3.1. Standard errors are clustered at the district level. All specifications include state fixed effects and district-level Muslim population. * p<0.10, ** p<0.05, *** p<0.01.

11 Appendix

11.1 Tables

Table A1: Baseline Test

	Overall	Non-Muslim	Muslim
	(1)	(2)	(3)
Panel A: Household characteristics (AIDIS 59th Round)			
Age	−0.578	−0.615	−0.853
	(0.560)	(0.598)	(1.147)
Education	0.045	−0.083	0.572**
	(0.135)	(0.145)	(0.250)
Observations	42,451	38,571	3,876
Male	−0.001	0.001	−0.022
	(0.012)	(0.012)	(0.030)
Ever married	0.016	0.022*	−0.051
	(0.013)	(0.013)	(0.034)
Self-employed in agriculture (rural)	−0.075**	−0.079**	−0.079
	(0.035)	(0.036)	(0.055)
Self-employed in non-agriculture (rural)	0.074***	0.085***	−0.081
	(0.022)	(0.022)	(0.058)
Other (rural)	0.013	0.014	−0.003
	(0.014)	(0.014)	(0.019)
Self-employed (urban)	−0.039*	−0.033*	−0.017
	(0.023)	(0.020)	(0.077)
Salaried (urban)	0.035	0.024	0.135***
	(0.025)	(0.025)	(0.047)
Other (urban)	−0.008	−0.010	0.044
	(0.013)	(0.013)	(0.035)
Household size	0.028	0.101	−0.122
	(0.100)	(0.095)	(0.242)
Land operated	−0.002	−0.007	−0.038
	(0.039)	(0.042)	(0.057)
MPCE	−27.040	−32.213	−25.550
	(34.147)	(36.209)	(43.268)
Observations	42,461	38,580	3,877
Panel A (cont.): Borrowing outcomes (AIDIS 59th Round)			
Incidence of borrowing (yes/no)	0.030	0.024	0.057
	(0.021)	(0.021)	(0.046)
Incidence of bank borrowing (yes/no)	−0.014	−0.018	−0.019
	(0.013)	(0.013)	(0.031)
Share of bank loans	−0.010	−0.015	−0.000
	(0.012)	(0.012)	(0.030)
Observations	42,461	38,580	3,877
Panel B: District characteristics (Census 2001)			
Muslim population share	−0.111***		
	(0.034)		
SC population share	0.017		
	(0.021)		
ST population share	−0.008		
	(0.068)		
Observations	179		

Baseline covariate balance test. Panel A uses NSS AIDIS 59th round data and presents regression discontinuity estimates of the effect of the 2009 branch expansion policy on pre-policy household characteristics and selected borrowing outcomes, estimated for the overall sample and separately for Non-Muslim and Muslim households. Occupation categories are indicator variables for NSS household type. Panel B uses Census 2001 data and presents RD estimates for district-level population shares. Effective number of observations reported. Standard errors are clustered at the district level. All specifications include state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table A2: Treatment effect of the 2009 policy on rural households

	Any borrowing	Bank borrowing	Share of bank loans	Other formal source	Informal source	Int-bearing
	(1)	(2)	(3)	(4)	(5)	(6)
			Non-Muslim			
A. Main specification	0.076**	0.066***	0.054***	0.015	0.008	0.103***
	(0.032)	(0.017)	(0.015)	(0.022)	(0.025)	(0.033)
B. Covariate adjustment	0.087***	0.074***	0.062***	0.025	0.005	0.115***
	(0.032)	(0.017)	(0.015)	(0.022)	(0.025)	(0.033)
Observations	16119	16119	16119	16119	16119	16119
Control group mean	0.418	0.081	0.065	0.176	0.231	0.354
			Muslim			
A. Main specification	0.278***	0.073	0.042	0.090*	0.146**	0.218**
	(0.077)	(0.046)	(0.043)	(0.053)	(0.059)	(0.096)
B. Covariate adjustment	0.275***	0.076	0.044	0.082	0.147**	0.213**
	(0.078)	(0.051)	(0.047)	(0.051)	(0.059)	(0.097)
Observations	1254	1254	1254	1254	1254	1254
Control group mean	0.390	0.125	0.105	0.105	0.214	0.281
p-value from NM=M	0.022	0.832	0.882	0.228	0.056	0.289

Main results using 2009 policy. Data source: NSS AIDIS 70th round. This table presents regression discontinuity estimates from the main estimating equation of the effect of the 2009 branch expansion policy on borrowing outcomes for rural households. Standard errors are clustered at the district level. All specification includes state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table A3: Mechanism (rural sector)

	Avg. interest rate	Collateral-free loan	Collateral-free agri loan	Loan default rate
	(1)	(2)	(3)	(4)
		Non-Muslim		
RD estimate	−3.791**	0.061***	0.063***	−0.131**
	(1.552)	(0.022)	(0.019)	(0.052)
Observations	11,532	11,640	5,816	1,051
Control group mean	20.314	0.044	0.043	1.001
		Muslim		
RD estimate	−6.346*	0.096	−0.171	−0.042
	(3.378)	(0.084)	(0.141)	(0.148)
Observations	723	790	288	105
Control group mean	17.967	0.056	0.115	0.342

Mechanisms underlying the 2009 branch expansion policy, rural sector. Data source: NSS AIDIS 70th round. This table presents fuzzy RD estimates of the effect of the 2009 branch expansion policy categorised by credit market mechanisms. Column (1) reports the average interest rate on bank loans, columns (2) and (3) report the likelihood of receiving a collateral-free bank loan for all borrowers and agricultural households respectively; column (4) reports the default rate on short- and medium-term bank loans. Standard errors are clustered at the district level. All specifications include state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table A4: Lender Discrimination

	Default rate	Loan applied	Loan rejected
	(1)	(2)	(3)
A. Underbanked	-0.008	0.016	0.005
	(0.076)	(0.044)	(0.081)
B. Underbanked*Muslim	0.218	-0.057	0.312
	(0.263)	(0.063)	(0.222)
C. Muslim	-0.047	-0.041	-0.023
	(0.096)	(0.039)	(0.083)
Observations	1283	7404	1737
Control group mean	1.005	0.235	0.145

Results using the 2009 policy and the specification $Y_i = \gamma_0 + \gamma_1 D_d + \gamma_2 Muslim_i * D_d + \gamma_3 Z_d + \gamma_4 Z_d * D_d + \gamma_5 Muslim_i + \gamma_6 Muslim_i * Z_d + \gamma_7 Muslim_i * Z_d * D_d + \gamma_8 S_s + \eta_i$, where Y_i is the relevant outcome variable in each column. To ensure identification, the estimates are reduced-form, with the outcome regressed on the underbanked assignment indicator and its interactions. Data source: column (1) uses the NSS AIDIS 70th round; columns (2) and (3) use IHDS round 2. Column (1) uses the ratio of amount outstanding to amount borrowed as a proxy for default, restricted to borrowers with a short/medium-term bank loan; columns (2) and (3) report effects on the probability of a household applying for a loan and of a loan application being rejected. Standard errors are clustered at the district level. All specifications include state fixed effects and a local linear control in the running variable. * p<0.10, ** p<0.05, *** p<0.01.

Table A5: Treatment effect by demand for loans

	Consumption loans	Agricultural loans	Non-agricultural loans
	(1)	(2)	(3)
		Non-Muslim	
A. Overall	-0.001	0.012*	-0.001
	(0.013)	(0.006)	(0.005)
Observations	29274	353534	2252895
Control group mean	0.075	0.015	0.026
B. Rural sector	0.019*	0.013*	-0.004
	(0.011)	(0.007)	(0.007)
Observations	16119	310178	799260
Control group mean	0.034	0.016	0.032
		Muslim	
A. Overall	-0.010	-0.006	0.012
	(0.018)	(0.013)	(0.010)
Observations	3056	14230	265929
Control group mean	0.057	0.024	0.028
B. Rural sector	0.018	-0.006	0.031
	(0.031)	(0.015)	(0.032)
Observations	1254	11169	70028
Control group mean	0.050	0.027	0.044

Results categorised by demand for loans. Data source: NSS AIDIS 70th round and Economic census 6th round. This table presents regression discontinuity estimates from the main estimating equation of the effect of the 2009 branch expansion policy on bank borrowing for households and firms categorised by the purpose of borrowing. Standard errors are clustered at the district level. All specification includes state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table A6: Treatment effect by accessibility to banks

	Any borrowing	Bank borrowing	Share of bank loans	Institutional source	Non-institutional source	Int-bearing
	(1)	(2)	(3)	(4)	(5)	(6)
			Overall			
A. Above median	0.086	-0.088***	-0.083***	-0.069***	0.185***	0.049
	(0.053)	(0.030)	(0.024)	(0.016)	(0.052)	(0.041)
Observations	2556	2556	2556	2556	2556	2556
B. Below median	0.114*	0.021	0.011	0.096**	-0.003	0.128*
	(0.058)	(0.029)	(0.026)	(0.041)	(0.043)	(0.070)
Observations	2322	2322	2322	2322	2322	2322
Control group mean	0.410	0.104	0.091	0.126	0.228	0.295

Results using 2009 policy grouped by accessibility to banks. Data source: NSS AIDIS 70th round. This table presents regression discontinuity estimates from the main estimating equation of the effect of the 2009 branch expansion policy on borrowing outcomes. Treated districts are categorised by their accessibility to banks. Standard errors are clustered at the district level. All specification includes state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table A7: Diff-in-disc

	Any borrowing	Bank borrowing	Share of bank loans	Other formal source	Informal source	Int-bearing
	(1)	(2)	(3)	(4)	(5)	(6)
			Non-Muslim			
A. Treat	0.026	-0.009	-0.005	-0.011	0.033	0.042
	(0.032)	(0.016)	(0.015)	(0.023)	(0.036)	(0.035)
B. Diff-in-disc estimate	0.039	0.037*	0.027	-0.006	0.008	0.048
	(0.038)	(0.020)	(0.018)	(0.033)	(0.038)	(0.042)
Observations	68117	68117	68117	68117	68117	68117
			Muslim			
A. Treat	0.182***	0.033	0.039	0.002	0.142**	0.165***
	(0.062)	(0.043)	(0.039)	(0.046)	(0.058)	(0.062)
B. Diff-in-disc estimate	-0.111	-0.072	-0.080	0.019	-0.083	-0.057
	(0.104)	(0.054)	(0.050)	(0.062)	(0.074)	(0.085)
Observations	6963	6963	6963	6963	6963	6963

Results using Diff-in-Disc approach. Data source: NSS AIDIS 70th and 59th round. This table presents difference in discontinuity estimates of the 2009 branch expansion policy using equation 5. To ensure identification, the estimates are reduced-form, with the outcome regressed on the assignment indicator and its interactions, for the reason discussed in Section 3.1. All specification includes state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table A8: Diff-in-disc (rural sector)

	Any borrowing	Bank borrowing	Share of bank loans	Other formal source	Informal source	Int-bearing
	(1)	(2)	(3)	(4)	(5)	(6)
			Non-Muslim			
A. Treat	0.004	-0.018	-0.013	-0.016	0.007	0.027
	(0.033)	(0.019)	(0.018)	(0.027)	(0.039)	(0.041)
B. Diff-in-disc estimate	0.055	0.066***	0.050**	0.007	0.005	0.066
	(0.044)	(0.022)	(0.021)	(0.037)	(0.046)	(0.051)
Observations	40448	40448	40448	40448	40448	40448
			Muslim			
A. Treat	0.291***	0.075	0.073	0.067	0.136*	0.263***
	(0.099)	(0.058)	(0.049)	(0.073)	(0.082)	(0.091)
B. Diff-in-disc estimate	-0.194	-0.080	-0.096	0.026	-0.119	-0.094
	(0.172)	(0.085)	(0.076)	(0.116)	(0.120)	(0.152)
Observations	3096	3096	3096	3096	3096	3096
p-value from NM=M	0.460	0.334	0.174	0.546	0.643	0.894

Results using Diff-in-Disc approach. Data source: NSS AIDIS 70th and 59th round. This table presents difference in discontinuity estimates of the 2009 branch expansion policy using equation 5. To ensure identification, the estimates are reduced-form, with the outcome regressed on the assignment indicator and its interactions, for the reason discussed in Section 3.1. Standard errors are clustered at the district level. All specification includes state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table A9: Robustness checks: banking sector outcomes

	All banks (number)	Private banks (number)	No. of accounts (log no.)	Amount deposited (log no.)	Credit outstanding (log no.)
	(1)	(2)	(3)	(4)	(5)
A. MSE-optimal bandwidth	4.511	15.516**	0.141	0.111	0.653**
	(16.762)	(7.525)	(0.092)	(0.128)	(0.265)
Observations	110	109	146	142	118
B. Two-sided MSE-optimal bandwidth	7.018	8.963*	0.220***	0.056	0.229**
	(9.109)	(4.727)	(0.068)	(0.074)	(0.107)
Observations	214	148	234	247	228
C. CER-optimal bandwidth	0.854	17.955**	0.183	0.349	0.847**
	(14.962)	(8.368)	(0.153)	(0.221)	(0.340)
Observations	73	72	109	107	89
D. Two-sided CER-optimal bandwidth	7.324	13.186***	0.233***	0.090	0.449***
	(10.570)	(4.966)	(0.082)	(0.089)	(0.159)
Observations	168	136	169	176	155
E. Triangular kernel	6.215	13.112**	0.135	0.169	0.493**
	(11.906)	(5.606)	(0.091)	(0.127)	(0.207)
Observations	179	178	179	179	179
F. Quadratic polynomial	8.358	11.263	0.243	0.358	0.763*
	(21.005)	(9.134)	(0.203)	(0.269)	(0.420)
Observations	179	178	179	179	179

Robustness checks using 2009 policy. Data source: RBI basic statistical returns and Census 2001. This table presents regression discontinuity estimates from alternative specifications of the effect of the 2009 branch expansion policy on aggregate banking outcomes. Rows A and B use MSE-optimal and two-sided MSE-optimal bandwidths, and rows C and D use CER-optimal and two-sided CER-optimal bandwidths; all four use a uniform kernel with data-driven bandwidths. Row E uses a triangular kernel with a fixed bandwidth. Row F uses a quadratic polynomial in the running variable with a uniform kernel and fixed bandwidth. In column (2), outcome-specific data-driven bandwidth selection is infeasible because private-bank presence exhibits little variation on the underbanked side of the cutoff; rows A–D of column (2) therefore apply the bandwidths selected for column (1). Standard errors are clustered at the district level. All specifications include state fixed effects and controls for the outcome variable in the pre-policy period. * p<0.10, ** p<0.05, *** p<0.01.

Table A10: Robustness checks: loan outcomes

	Any borrowing	Bank borrowing	Share of bank loans	Other formal source	Informal source	Int-bearing
	(1)	(2)	(3)	(4)	(5)	(6)
			Non-Muslim			
A. MSE-optimal bandwidth	0.033	0.022	0.017	−0.004	0.031	0.053**
	(0.020)	(0.015)	(0.013)	(0.019)	(0.020)	(0.024)
Observations	43754	42558	43009	29633	34191	42752
B. Two-sided MSE-optimal bandwidth	0.047**	0.024	0.019	0.019	0.010	0.062***
	(0.020)	(0.015)	(0.013)	(0.014)	(0.016)	(0.022)
Observations	50341	47526	47808	49355	42821	51228
C. CER-optimal bandwidth	0.068***	0.037**	0.031*	0.008	0.017	0.093***
	(0.024)	(0.019)	(0.016)	(0.023)	(0.025)	(0.027)
Observations	31794	31077	31794	20801	24229	31738
D. Two-sided CER-optimal bandwidth	0.047*	0.034*	0.022	−0.017	0.016	0.070**
	(0.026)	(0.018)	(0.015)	(0.015)	(0.020)	(0.028)
Observations	37194	35815	37676	40268	32775	39824
E. Triangular kernel	0.041*	0.017	0.014	0.001	0.015	0.074***
	(0.024)	(0.020)	(0.017)	(0.020)	(0.025)	(0.027)
Observations	29274	29274	29274	29274	29274	29274
F. Quadratic polynomial	0.009	−0.001	−0.004	−0.001	0.007	0.044
	(0.034)	(0.031)	(0.027)	(0.026)	(0.036)	(0.038)
Observations	29274	29274	29274	29274	29274	29274
			Muslim			
A. MSE-optimal bandwidth	0.026	−0.146***	−0.144***	−0.008	0.076**	0.010
	(0.044)	(0.023)	(0.021)	(0.037)	(0.035)	(0.046)
Observations	2123	1794	1794	2657	2171	1832
B. Two-sided MSE-optimal bandwidth	0.012	−0.031	−0.026	−0.004	0.037	0.022
	(0.044)	(0.025)	(0.024)	(0.024)	(0.031)	(0.040)
Observations	4161	6384	6289	5197	6209	3881
C. CER-optimal bandwidth	0.011	−0.100***	−0.106***	0.016	0.106***	0.053
	(0.047)	(0.018)	(0.018)	(0.043)	(0.032)	(0.048)
Observations	1645	1353	1335	1799	1645	1519
D. Two-sided CER-optimal bandwidth	0.008	−0.005	−0.016	0.003	0.054	0.099**
	(0.046)	(0.025)	(0.023)	(0.029)	(0.035)	(0.044)
Observations	3524	5288	5205	3747	4867	3245
E. Triangular kernel	0.060	−0.066***	−0.066***	−0.006	0.076**	0.061
	(0.039)	(0.025)	(0.022)	(0.035)	(0.031)	(0.044)
Observations	3056	3056	3056	3056	3056	3056
F. Quadratic polynomial	0.034	−0.125***	−0.127***	0.003	0.115**	0.035
	(0.059)	(0.038)	(0.032)	(0.060)	(0.045)	(0.057)
Observations	3056	3056	3056	3056	3056	3056

Robustness checks using 2009 policy. Data source: NSS AIDIS 70th round. This table presents regression discontinuity estimates from alternative specifications of the effect of the 2009 branch expansion policy on borrowing outcomes. The first set of results corresponds to non-Muslim households and the second set to Muslim households. Rows A and B use MSE-optimal and two-sided MSE-optimal bandwidths, and rows C and D use CER-optimal and two-sided CER-optimal bandwidths; all four use a uniform kernel with data-driven bandwidths. Row E uses a triangular kernel with a fixed bandwidth. Row F uses a quadratic polynomial in the running variable with a uniform kernel and fixed bandwidth. Standard errors are clustered at the district level. All specifications include state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table A11: Robustness checks (loan outcomes: rural sector)

	Any borrowing	Bank borrowing	Share of bank loans	Other formal source	Informal source	Int-bearing
	(1)	(2)	(3)	(4)	(5)	(6)
			Non-Muslim			
A. MSE-optimal bandwidth	0.026	0.040***	0.031**	0.008	0.006	0.058**
	(0.027)	(0.014)	(0.013)	(0.022)	(0.023)	(0.029)
Observations	22932	20844	22087	16440	18543	22932
B. Two-sided MSE-optimal bandwidth	0.042*	0.040***	0.032***	0.032*	−0.022	0.070***
	(0.023)	(0.013)	(0.012)	(0.018)	(0.019)	(0.024)
Observations	32217	28242	29731	30893	29184	32997
C. CER-optimal bandwidth	0.070**	0.066***	0.052***	0.007	−0.014	0.095***
	(0.031)	(0.016)	(0.015)	(0.026)	(0.026)	(0.033)
Observations	16384	14991	15987	12172	13629	16384
D. Two-sided CER-optimal bandwidth	0.038	0.040**	0.039***	−0.015	−0.012	0.065**
	(0.029)	(0.016)	(0.013)	(0.018)	(0.024)	(0.029)
Observations	25075	20679	23314	24701	22237	26431
E. Triangular kernel, $h = 3700$	0.035	0.052***	0.046***	0.006	−0.030	0.075**
	(0.030)	(0.016)	(0.014)	(0.021)	(0.024)	(0.033)
Observations	16119	16119	16119	16119	16119	16119
F. Quadratic polynomial, $h = 3700$	−0.023	0.031	0.025	−0.010	−0.052	0.017
	(0.041)	(0.027)	(0.024)	(0.030)	(0.032)	(0.041)
Observations	16119	16119	16119	16119	16119	16119
			Muslim			
A. MSE-optimal bandwidth	0.186**	−0.025	0.007	0.101***	0.174***	0.155*
	(0.082)	(0.057)	(0.053)	(0.039)	(0.057)	(0.087)
Observations	781	781	879	812	781	894
B. Two-sided MSE-optimal bandwidth	0.076	0.050	0.022	0.003	0.050	0.134**
	(0.057)	(0.049)	(0.047)	(0.031)	(0.052)	(0.062)
Observations	1621	1640	1632	1674	1581	1664
C. CER-optimal bandwidth	0.094	−0.091	−0.158***	0.131**	0.044	0.097
	(0.105)	(0.064)	(0.055)	(0.053)	(0.062)	(0.107)
Observations	528	528	697	638	526	697
D. Two-sided CER-optimal bandwidth	0.173**	0.053	0.028	0.022	0.151***	0.165**
	(0.073)	(0.050)	(0.045)	(0.036)	(0.052)	(0.080)
Observations	1183	1398	1295	1483	1227	1261
E. Triangular kernel, $h = 3700$	0.229***	0.022	−0.011	0.084*	0.149***	0.206**
	(0.070)	(0.047)	(0.045)	(0.043)	(0.050)	(0.083)
Observations	1254	1254	1254	1254	1254	1254
F. Quadratic polynomial, $h = 3700$	0.046	−0.051	−0.105	0.187**	0.073	0.072
	(0.161)	(0.079)	(0.077)	(0.095)	(0.092)	(0.184)
Observations	1254	1254	1254	1254	1254	1254

Robustness checks using 2009 policy. Data source: NSS AIDIS 70th round. This table presents regression discontinuity estimates from alternative specifications of the effect of the 2009 branch expansion policy on borrowing outcomes for rural households. The first set of results corresponds to non-Muslim households and the second set to Muslim households. Rows A and B use MSE-optimal and two-sided MSE-optimal bandwidths, and rows C and D use CER-optimal and two-sided CER-optimal bandwidths; all four use a uniform kernel with data-driven bandwidths. Row E uses a triangular kernel with a fixed bandwidth of 3700. Row F uses a quadratic polynomial in the running variable with a uniform kernel and fixed bandwidth of 3700. Standard errors are clustered at the district level. All specifications include state fixed effects. * p<0.10, ** p<0.05, *** p<0.01.

Table A12: Placebo cutoff tests: banking sector

	All banks (number)	Private banks (number)	No. of accounts (log no.)	Amount deposited (log no.)	Credit outstanding (log no.)
	(1)	(2)	(3)	(4)	(5)
A. Placebo cutoff +1000	4.762	5.060	0.383**	−0.084	0.014
	(18.460)	(7.407)	(0.172)	(0.137)	(0.226)
Observations	183	155	183	183	183
B. Placebo cutoff −1000	−52.291	−36.743	0.582	0.760	1.658
	(155.670)	(66.530)	(0.984)	(1.442)	(2.257)
Observations	175	159	175	175	175
C. Placebo cutoff +2000	77.611	49.558	0.628	−0.094	0.577
	(81.957)	(35.633)	(0.779)	(0.408)	(0.786)
Observations	176	125	176	176	176
D. Placebo cutoff −2000	−22.935	−9.367	0.024	0.260	0.146
	(50.525)	(20.058)	(0.179)	(0.279)	(0.311)
Observations	173	137	173	173	173

Placebo cutoff tests for the banking sector using the 2009 policy. Data source: RBI basic statistical returns and Census 2001. This table presents fuzzy regression discontinuity estimates at four artificial cutoffs (±1,000 and ±2,000 around the true threshold) for aggregate banking outcomes. All specifications use a linear polynomial, uniform kernel, bandwidth of 3700, state fixed effects, and control for the outcome variable in the pre-policy period. Observations are the effective number of observations within the estimation window. Standard errors are clustered at the district level. * p<0.10, ** p<0.05, *** p<0.01.

Table A13: Placebo cutoff tests

	Any borrowing	Bank borrowing	Share of bank loans	Other formal source	Informal source	Int-bearing
	(1)	(2)	(3)	(4)	(5)	(6)
			Non-Muslim			
A. Placebo cutoff +1000	0.063	0.067	0.063	0.013	−0.044	0.077
	(0.076)	(0.051)	(0.045)	(0.057)	(0.062)	(0.088)
Observations	29607	29607	29607	29607	29607	29607
B. Placebo cutoff −1000	−0.081	−0.384	−0.267	0.558	−0.672	0.206
	(1.098)	(1.312)	(0.981)	(1.741)	(2.121)	(1.200)
Observations	28311	28311	28311	28311	28311	28311
C. Placebo cutoff +2000	0.411	0.234	0.283	−0.081	0.112	0.367
	(0.278)	(0.178)	(0.176)	(0.163)	(0.188)	(0.315)
Observations	27366	27366	27366	27366	27366	27366
D. Placebo cutoff −2000	0.256**	0.061	0.055	0.241**	0.007	0.272**
	(0.107)	(0.065)	(0.056)	(0.094)	(0.083)	(0.108)
Observations	27329	27329	27329	27329	27329	27329
			Muslim			
A. Placebo cutoff +1000	−0.057	0.093	0.071	0.037	−0.127	0.030
	(0.103)	(0.092)	(0.081)	(0.074)	(0.114)	(0.091)
Observations	3304	3304	3304	3304	3304	3304
B. Placebo cutoff −1000	−0.385	−0.214	−0.323	0.081	−0.389	0.549
	(0.852)	(0.416)	(0.399)	(0.435)	(0.776)	(0.770)
Observations	2920	2920	2920	2920	2920	2920
C. Placebo cutoff +2000	0.209	−0.063	−0.032	−0.062	0.047	−0.064
	(0.271)	(0.161)	(0.143)	(0.196)	(0.255)	(0.243)
Observations	3302	3302	3302	3302	3302	3302
D. Placebo cutoff −2000	−0.210	−0.262***	−0.238***	−0.052	0.057	−0.283**
	(0.155)	(0.097)	(0.087)	(0.099)	(0.121)	(0.135)
Observations	3054	3054	3054	3054	3054	3054

Placebo cutoff tests using 2009 policy. Data source: NSS AIDIS 70th round. This table presents regression discontinuity estimates at four artificial cutoffs (±1,000 and ±2,000 around the true threshold) for Non-Muslim and Muslim households. All specifications use a linear polynomial, uniform kernel, bandwidth of 3700, and state fixed effects. Standard errors are clustered at the district level. * p<0.10, ** p<0.05, *** p<0.01.

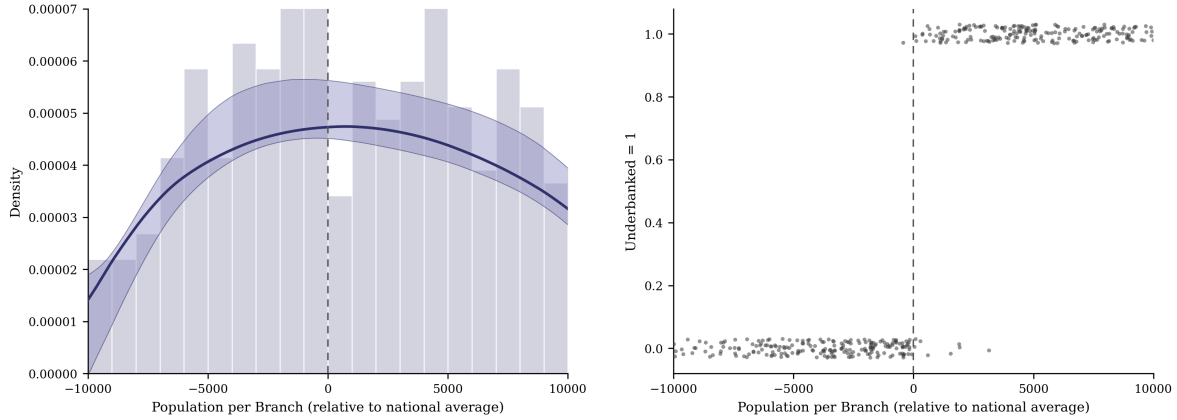
Table A14: Placebo cutoff tests (rural sector)

	Any borrowing	Bank borrowing	Share of bank loans	Other formal source	Informal source	Int-bearing
	(1)	(2)	(3)	(4)	(5)	(6)
			Non-Muslim			
A. Placebo cutoff +1000	−0.042	−0.069	−0.071	0.043	−0.044	−0.046
	(0.111)	(0.064)	(0.060)	(0.086)	(0.082)	(0.124)
Observations	17416	17416	17416	17416	17416	17416
B. Placebo cutoff −1000	−0.103	−2.772	−2.500	2.336	−0.593	0.558
	(2.607)	(14.403)	(12.940)	(12.223)	(3.693)	(3.796)
Observations	15218	15218	15218	15218	15218	15218
C. Placebo cutoff +2000	0.673	0.404*	0.429*	0.018	0.195	0.579
	(0.460)	(0.244)	(0.244)	(0.204)	(0.260)	(0.455)
Observations	17012	17012	17012	17012	17012	17012
D. Placebo cutoff −2000	0.192	0.062	0.045	0.294**	−0.078	0.227
	(0.131)	(0.065)	(0.054)	(0.133)	(0.117)	(0.141)
Observations	14123	14123	14123	14123	14123	14123
			Muslim			
A. Placebo cutoff +1000	−0.057	0.100	0.072	−0.159*	0.036	−0.086
	(0.121)	(0.096)	(0.095)	(0.081)	(0.097)	(0.142)
Observations	1363	1363	1363	1363	1363	1363
B. Placebo cutoff −1000	−0.116	−0.161	−0.303	0.439	−0.168	0.555
	(0.572)	(0.394)	(0.353)	(0.325)	(0.522)	(0.568)
Observations	1208	1208	1208	1208	1208	1208
C. Placebo cutoff +2000	1.191*	0.417	0.372	−0.328	0.597	0.040
	(0.688)	(0.367)	(0.326)	(0.366)	(0.500)	(0.513)
Observations	1378	1378	1378	1378	1378	1378
D. Placebo cutoff −2000	−0.083	−0.227	−0.238*	−0.120	0.299*	−0.162
	(0.205)	(0.148)	(0.131)	(0.136)	(0.157)	(0.151)
Observations	1170	1170	1170	1170	1170	1170

Placebo cutoff tests using 2009 policy, rural sector. Data source: NSS AIDIS 70th round. This table presents regression discontinuity estimates at four artificial cutoffs (±1,000 and ±2,000 around the true threshold) for Non-Muslim and Muslim households. All specifications use a linear polynomial, uniform kernel, bandwidth of 3700, and state fixed effects. Standard errors are clustered at the district level. * p<0.10, ** p<0.05, *** p<0.01.

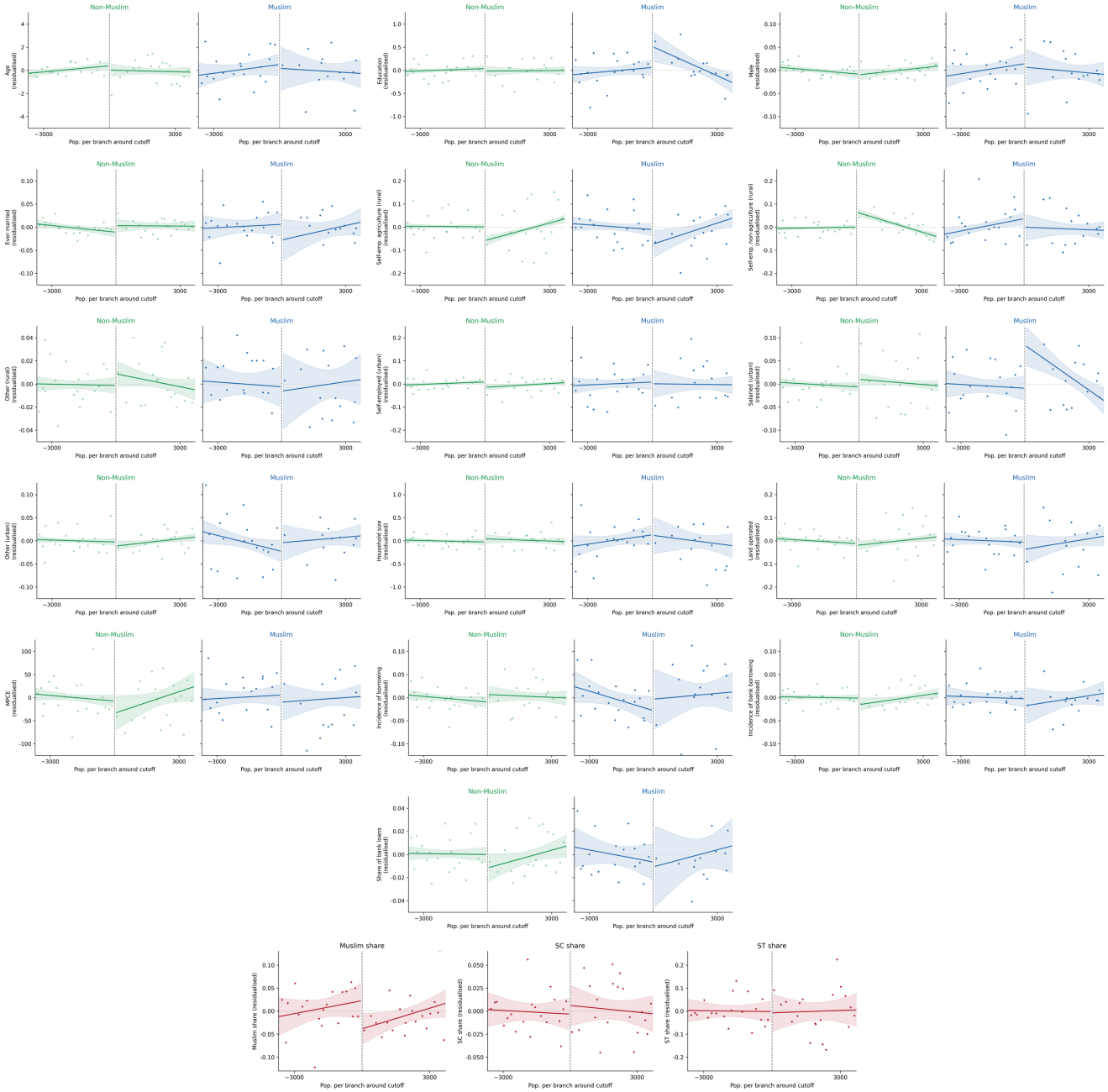
11.2 Figures

Figure A1: McCrary Density Test and Fuzzy RD Design for the 2009 Policy



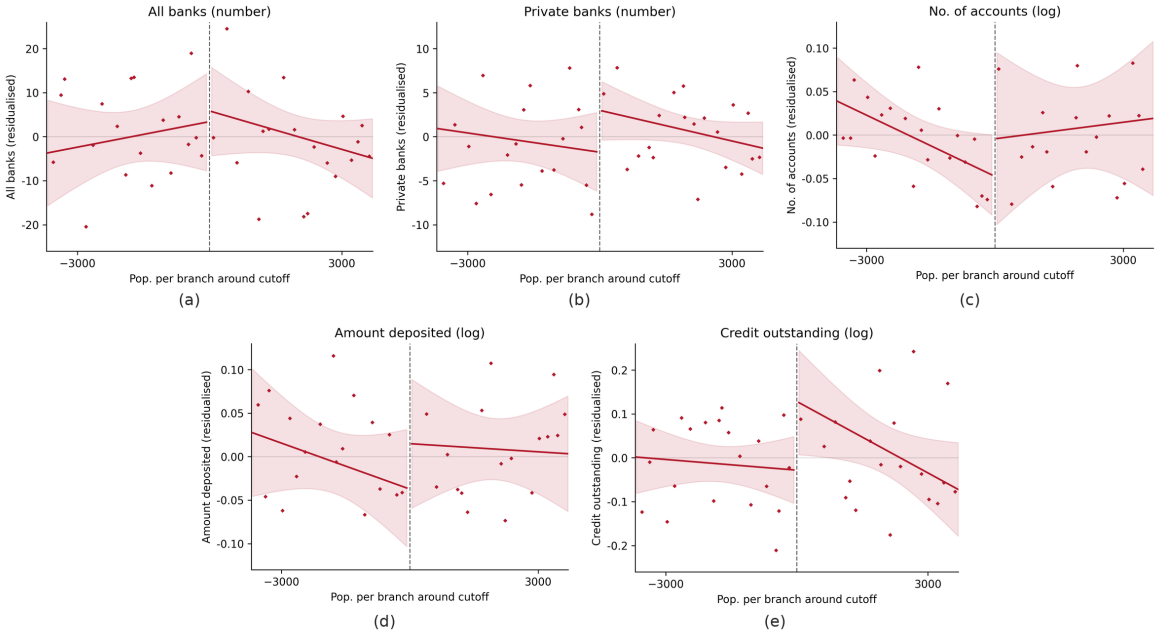
The left panel plots the McCrary (2008) density test for the district-level population-per-branch running variable, normalised so that the underbanked cutoff lies at zero. The histogram shows the raw density of the running variable and the solid line the local-polynomial density estimate with its shaded 95 per cent confidence band; the manipulation test does not reject continuity of the density at the cutoff. The right panel plots the first stage of the fuzzy design, with the probability that a district is classified as underbanked on the vertical axis against the running variable on the horizontal axis. The sharp jump at the cutoff confirms a strong first stage. Data source: BSR (RBI), 2005–06.

Figure A2: Baseline Covariate Balance Test



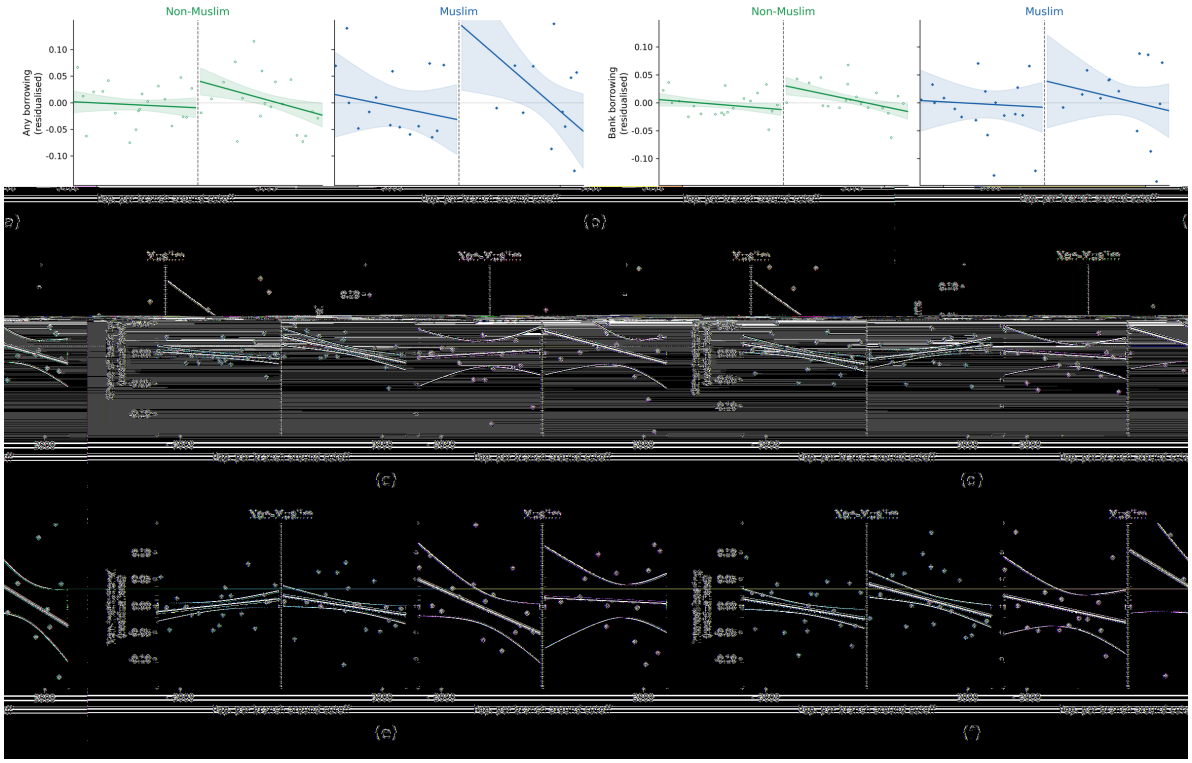
Baseline covariate balance around the underbanked cutoff. Each sub-plot regresses a pre-policy covariate on the running variable (residualised on state fixed effects), with binned means and local linear fits estimated separately on either side of the cutoff (shaded 95 per cent confidence intervals; the cutoff is normalised to zero). The sixteen two-panel plots (Panel A, AIDIS 59th round) show household characteristics and pre-policy borrowing outcomes; the three single plots (Panel B, Census 2001) show the district-level Muslim, Scheduled Caste, and Scheduled Tribe population shares. Data sources: NSS AIDIS 59th round and Census 2001.

Figure A3: Effect of the 2009 Policy on the Banking Sector



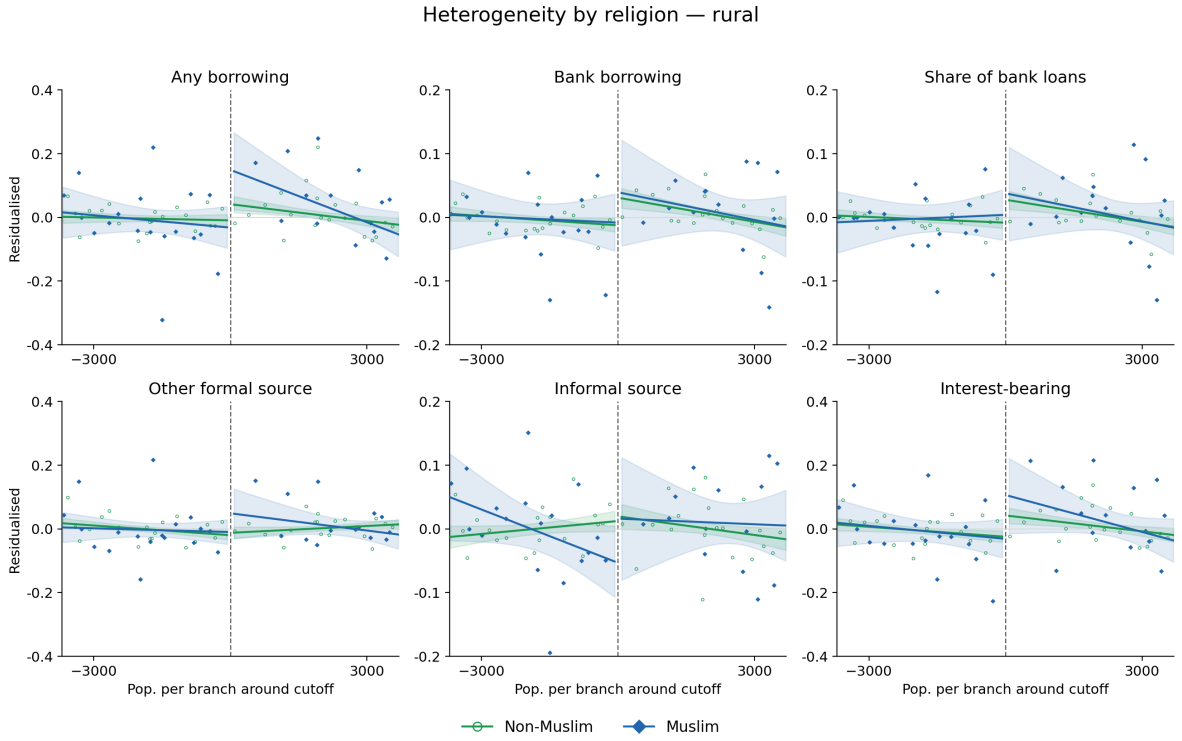
Discontinuity at the underbanked cutoff in banking-sector outcomes across all districts: (a) number of bank branches (all banks), (b) number of private bank branches, (c) number of deposit accounts (log), (d) amount deposited (log), and (e) credit outstanding (log). Each outcome is residualised on state fixed effects and its pre-policy value; points are binned means and the solid lines local linear fits estimated separately on either side of the cutoff, with shaded 95 per cent confidence intervals. Data source: RBI Basic Statistical Returns.

Figure A4: Effect of the 2009 Policy on Household Borrowing (Rural Sector)



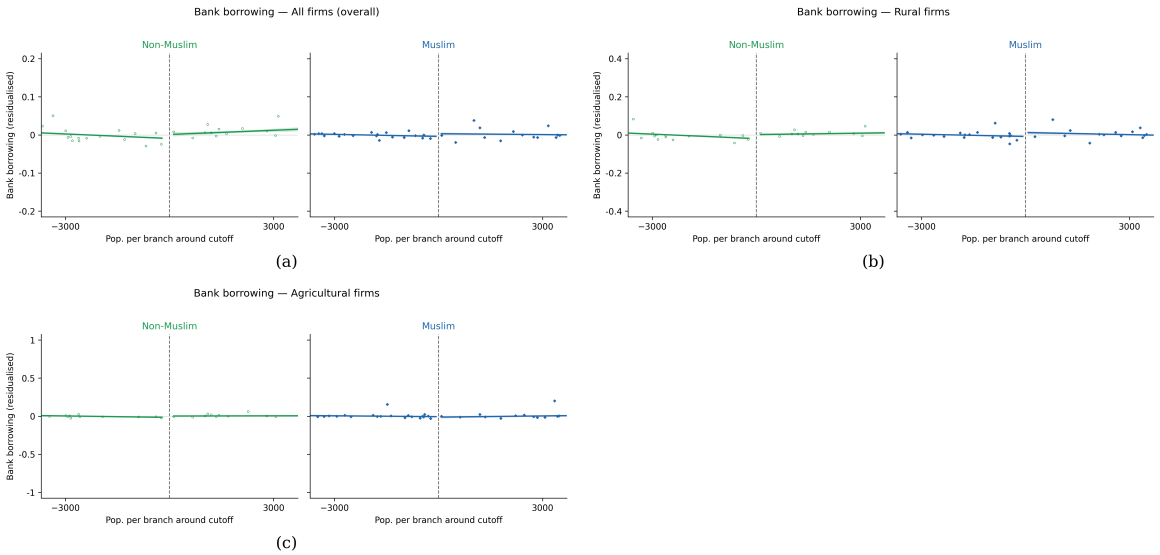
Discontinuity at the underbanked cutoff in each household borrowing outcome in the rural sector: (a) any borrowing, (b) bank borrowing, (c) share of bank loans, (d) borrowing from other formal sources, (e) borrowing from informal sources, and (f) interest-bearing borrowing, matching columns (1)–(6). Outcomes are residualised on state fixed effects; points are binned means and the solid lines local linear fits estimated separately on either side of the cutoff, with shaded 95 per cent confidence intervals. Data source: NSS AIDIS 70th round.

Figure A5: Heterogeneity by Religion in Household Borrowing (Rural Sector)



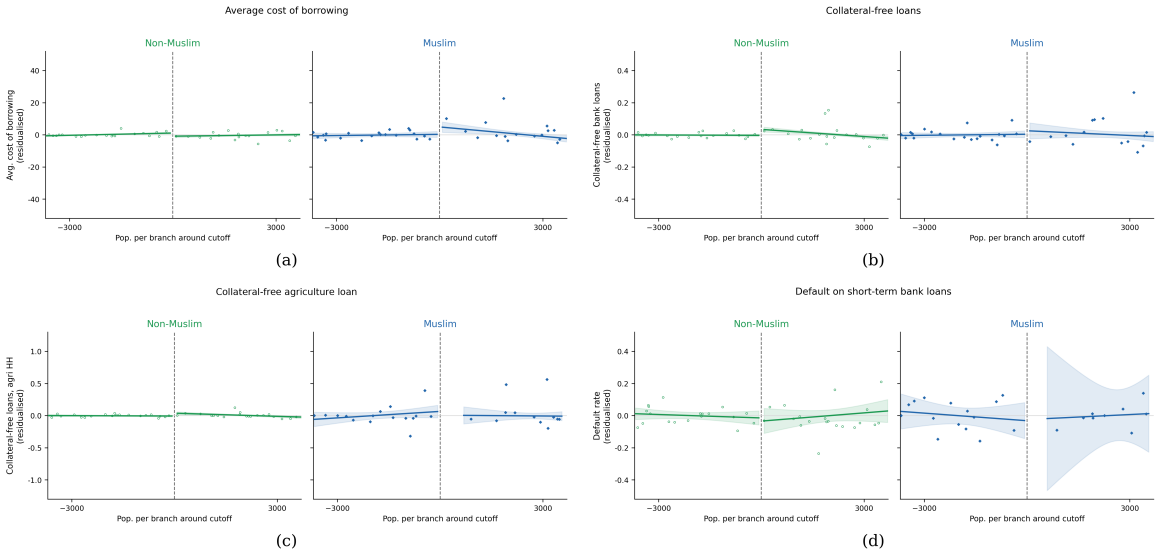
Discontinuity at the underbanked cutoff in each household borrowing outcome in the rural sector, plotted separately by religion. The six panels are any borrowing, bank borrowing, share of bank loans, borrowing from other formal sources, borrowing from informal sources, and interest-bearing borrowing. Outcomes are residualised on state fixed effects; the solid lines are local linear fits estimated separately on either side of the cutoff, with shaded 95 per cent confidence intervals. Data source: NSS AIDIS 70th round.

Figure A6: Effect of the 2009 Policy on Firm Bank Borrowing



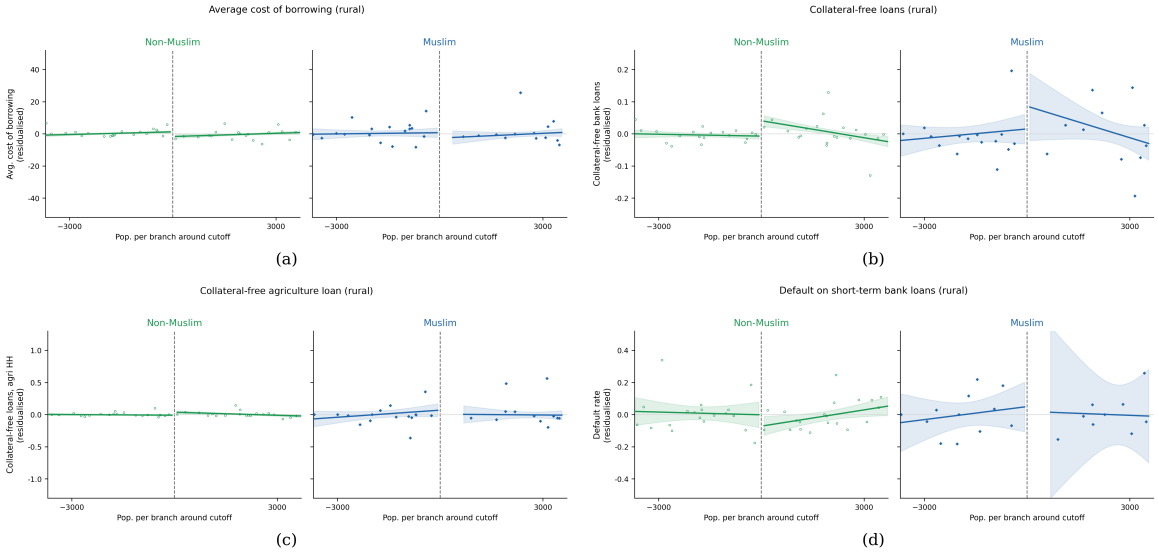
The figure plots the discontinuity at the underbanked cutoff in firm bank borrowing among (a) the establishment sample as a whole, (b) establishments in the rural sector, and (c) farm-based establishments. The outcome variable is a dummy equal to one if the establishment reports a commercial bank as a major source of finance and is residualized on state fixed effects. Data source: Economic Census 6th round.

Figure A7: Effect of the 2009 Policy on Credit-Market Mechanisms



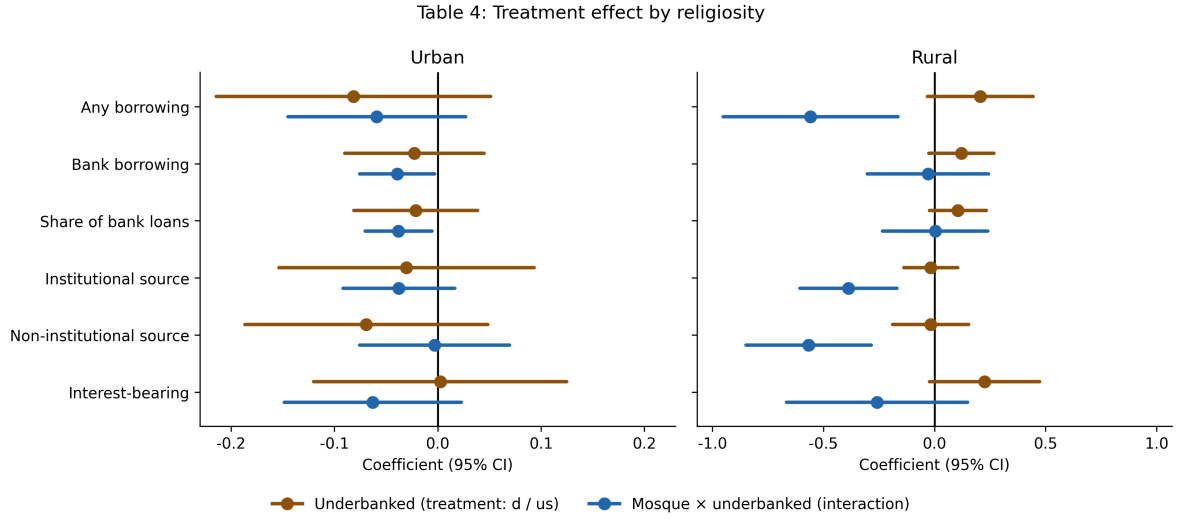
The figure plots the discontinuity at the underbanked cutoff in the four credit-market mechanisms underlying the 2009 branch expansion policy, corresponding to the estimates in Table 6: (a) the average interest rate on the household's bank loans, (b) a dummy equal to one if the household received a collateral-free bank loan, (c) a dummy equal to one if an agricultural household received a collateral-free bank loan, and (d) the default rate on the household's short- and medium-term bank loans. Each outcome is residualised on state fixed effects. Data source: NSS AIDIS 70th round.

Figure A8: Effect of the 2009 Policy on Credit-Market Mechanisms (Rural Sector)



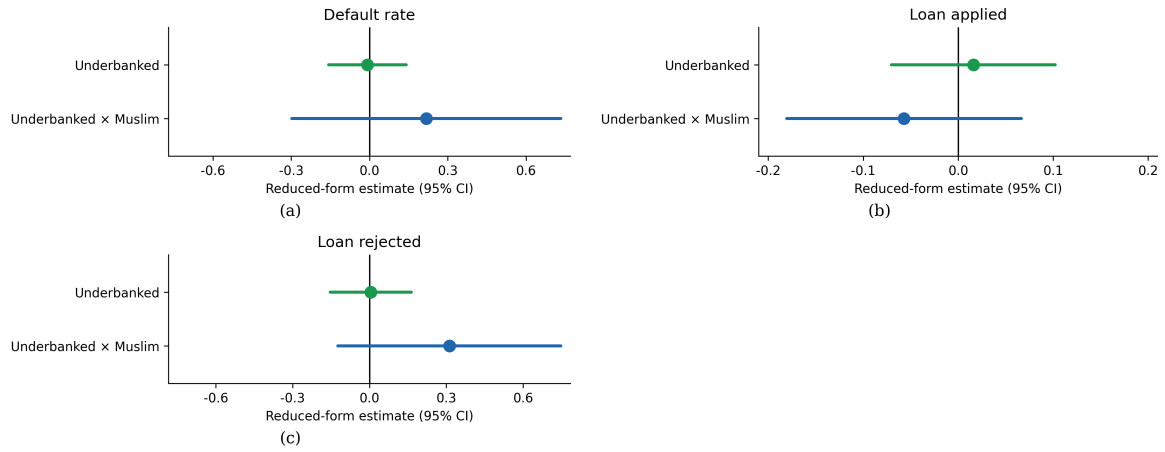
The figure plots the discontinuity at the underbanked cutoff in the four credit-market mechanisms underlying the 2009 branch expansion policy among households in the rural sector, corresponding to the estimates in Table A3: (a) the average interest rate on the household's bank loans, (b) a dummy equal to one if the household received a collateral-free bank loan, (c) a dummy equal to one if an agricultural household received a collateral-free bank loan, and (d) the default rate on the household's short- and medium-term bank loans. Each outcome is residualised on state fixed effects. Data source: NSS AIDIS 70th round.

Figure A9: Effect of the 2009 Policy on Muslim Household Borrowing by Religiosity



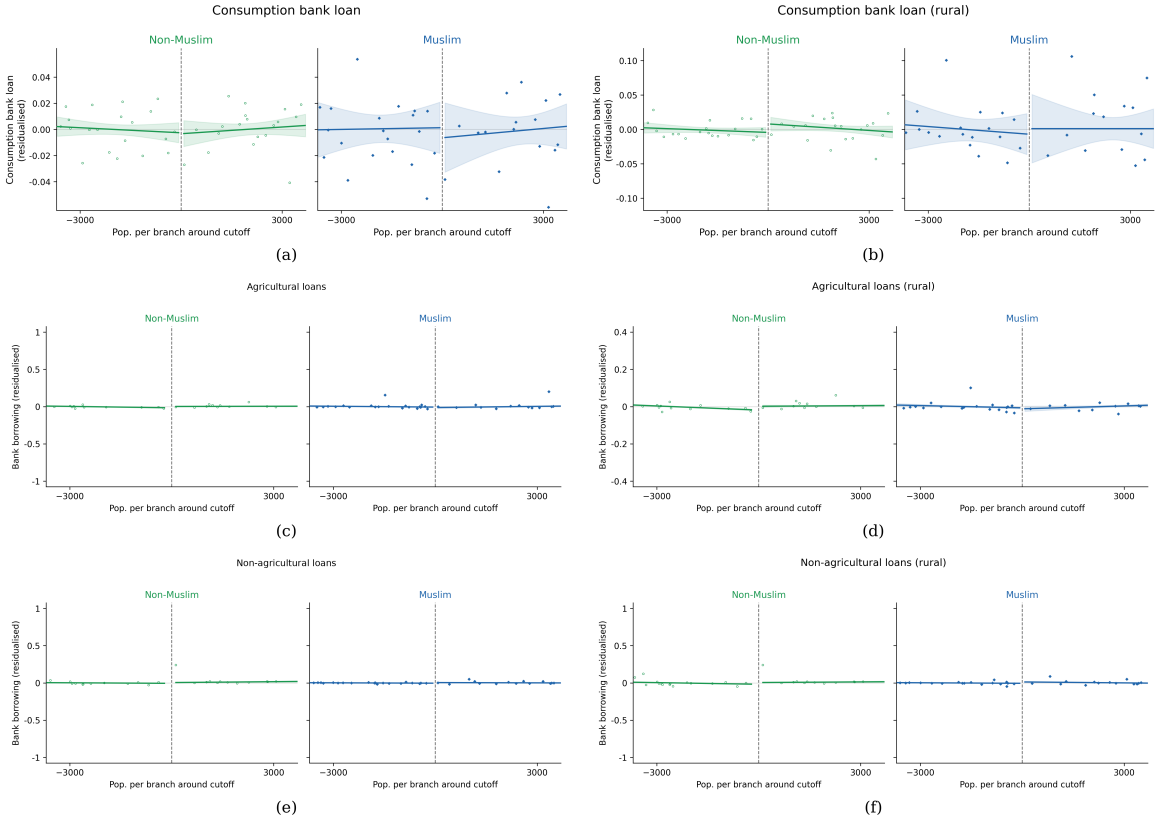
The figure plots the regression discontinuity estimates from Table 7, which augments the baseline specification with a full set of interactions between the underbanked assignment indicator and the district–sector count of religious institutions (mosques and madrasas), estimated for Muslim households separately for the urban (left panel) and rural (right panel) sectors. For each of the six borrowing outcomes, the two markers report the baseline treatment effect of the policy (β_1, the coefficient on the underbanked indicator) and the differential effect associated with a one standard deviation increase in the number of religious institutions (β_5, the mosque × underbanked interaction), as labelled in each panel; in the online colour version β_1 is shown in brown and β_5 in blue. Points are the point estimates and horizontal bars the 95 per cent confidence intervals; the vertical line marks zero. Standard errors are clustered at the district level and all specifications include state fixed effects and the district-level Muslim population. Data sources: NSS AIDIS 70th round and WAMSI.

Figure A10: Lender Discrimination: Reduced-Form Estimates



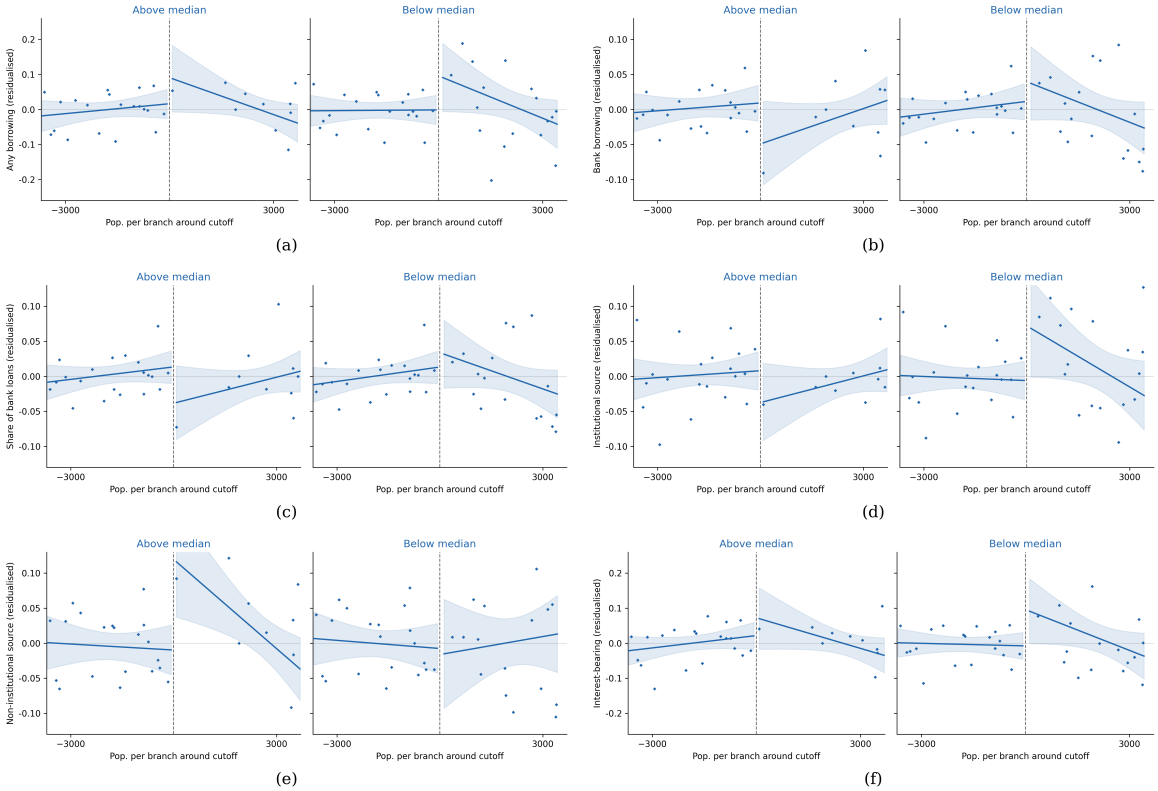
The figure plots the reduced-form regression discontinuity estimates from Table A4 for the three lender-discrimination outcomes: (a) the loan default rate, (b) the probability of applying for a loan, and (c) the probability of a loan application being rejected. In each panel, the two markers report the coefficient on the underbanked assignment indicator (γ_1) and the coefficient on its interaction with the Muslim indicator (γ_2, underbanked × Muslim), as labelled; in the online colour version γ_1 is shown in green and γ_2 in blue. Points are the point estimates and horizontal bars the 95 per cent confidence intervals; the vertical line marks zero. Default (panel a) is measured as the ratio of amount outstanding to amount borrowed for short- and medium-term bank loans. Standard errors are clustered at the district level and all specifications include state fixed effects and a local linear control in the running variable. Data sources: NSS AIDIS 70th round (panel a) and IHDS round 2 (panels b and c).

Figure A11: Effect of the 2009 Policy on Bank Borrowing by Purpose



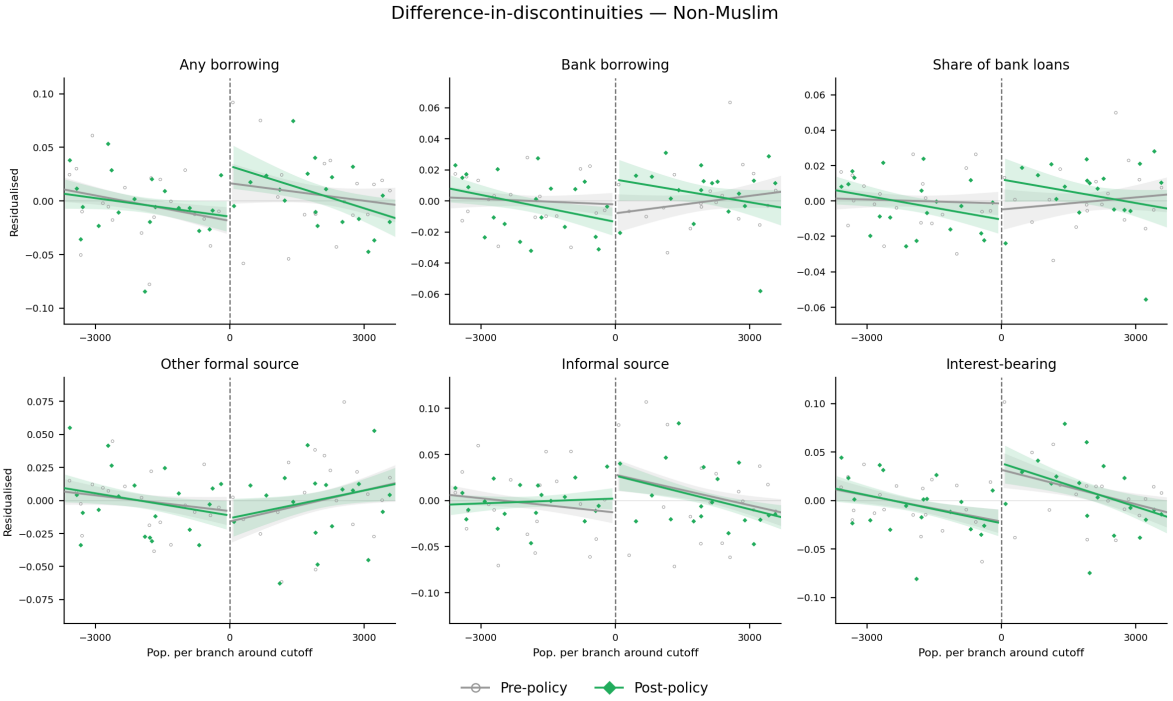
The figure plots the discontinuity at the underbanked cutoff in bank borrowing by purpose of loan, corresponding to the estimates in Table A5: (a) consumption loans and (b) consumption loans in the rural sector; (c) agricultural loans and (d) agricultural loans in the rural sector; (e) non-agricultural loans and (f) non-agricultural loans in the rural sector. Each outcome is a dummy equal to one if the household has an outstanding bank loan of the relevant purpose and is residualised on state fixed effects. Data source: NSS AIDIS 70th round.

Figure A12: Effect of the 2009 Policy on Muslim Household Borrowing by Bank Accessibility



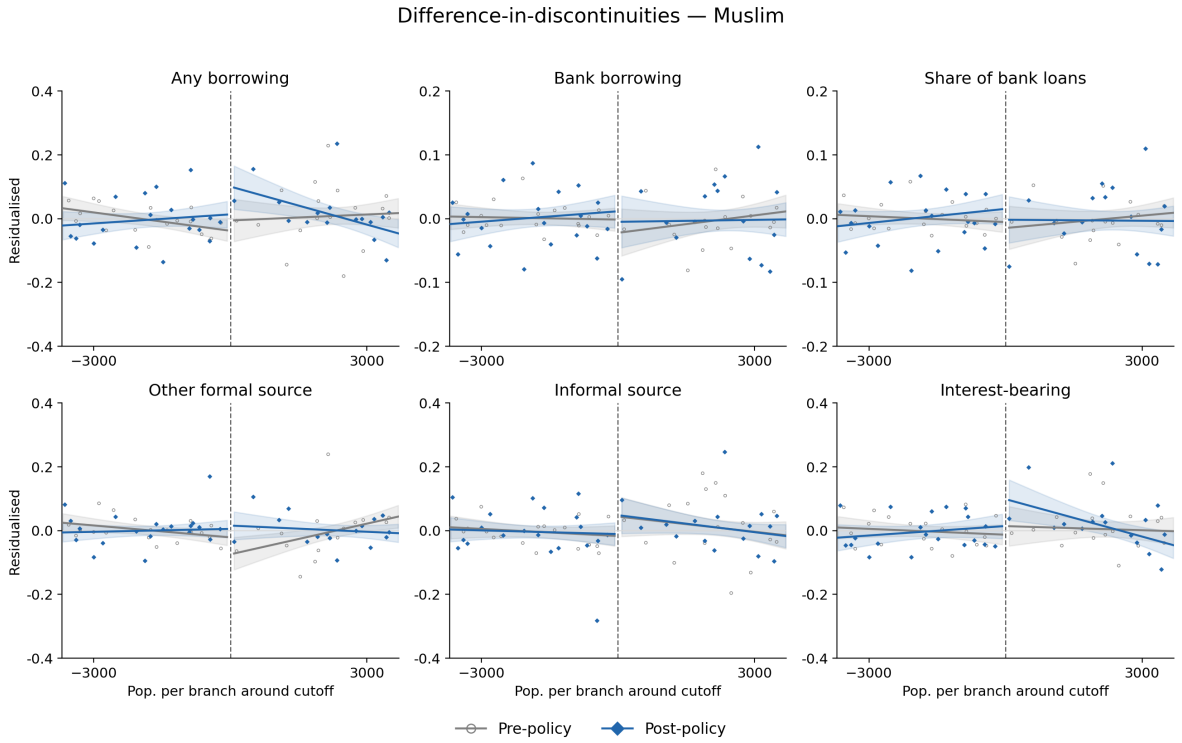
The figure plots the discontinuity at the underbanked cutoff in each borrowing outcome among Muslim households, separately for districts with above-median (left sub-panel) and below-median (right sub-panel) bank accessibility, corresponding to the estimates in Table A6. The six outcomes are (a) any borrowing, (b) bank borrowing, (c) share of bank loans, (d) borrowing from institutional sources, (e) borrowing from non-institutional sources, and (f) interest-bearing borrowing. Each outcome is residualised on state fixed effects; the solid lines are local linear fits estimated separately on either side of the cutoff, with shaded 95 per cent confidence intervals. Data source: NSS AIDIS 70th round.

Figure A13: Difference-in-Discontinuities: Non-Muslim Households



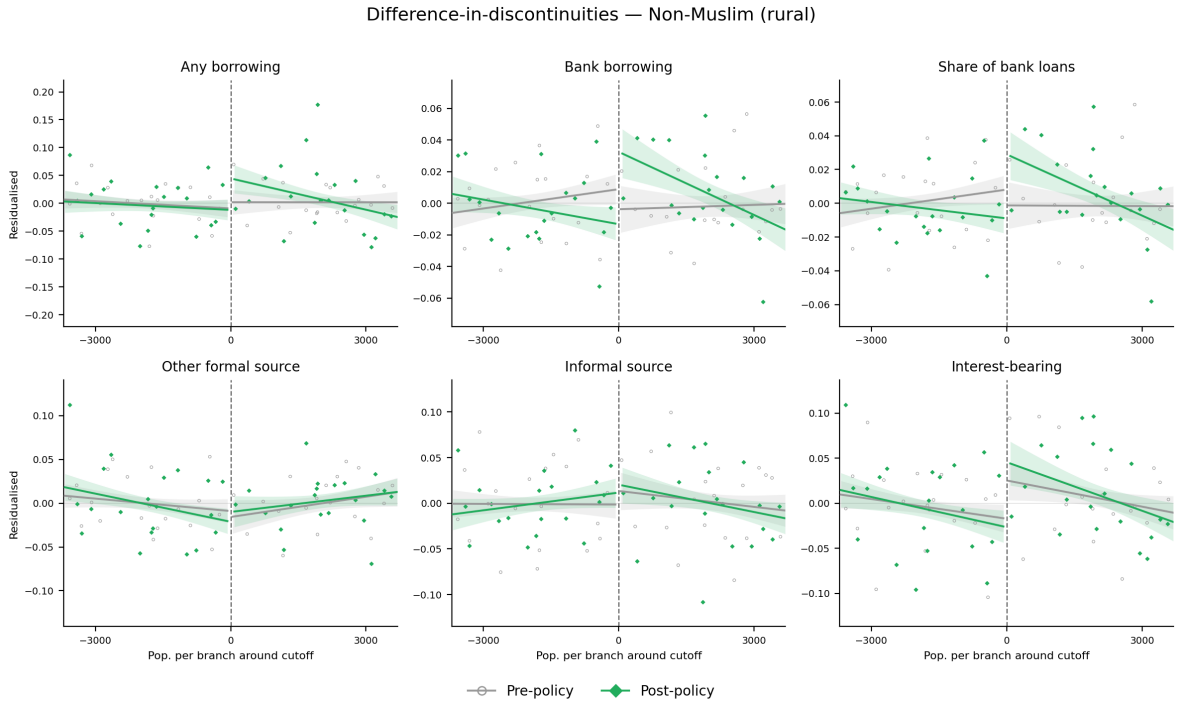
The figure plots pre- and post-policy regression discontinuities in each borrowing outcome for non-Muslim households, corresponding to the difference-in-discontinuities estimates in Table A7. Each outcome is residualised on state fixed effects, and the solid lines are local linear fits estimated separately on either side of the cutoff, with shaded 95 per cent confidence intervals. The six panels are any borrowing, bank borrowing, share of bank loans, other formal source, informal source and interest-bearing borrowing. Data sources: NSS AIDIS 59th and 70th rounds.

Figure A14: Difference-in-Discontinuities: Muslim Households



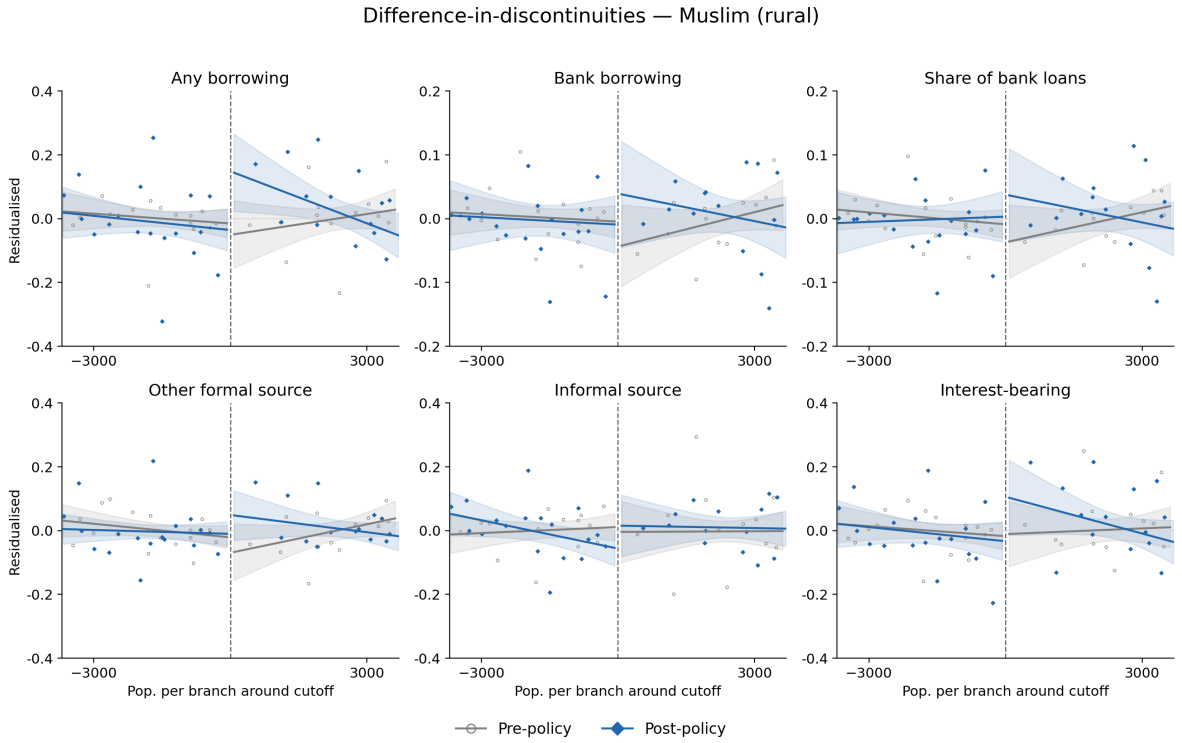
The figure plots pre- and post-policy regression discontinuities in each borrowing outcome for Muslim households, corresponding to the difference-in-discontinuities estimates in Table A7. Each outcome is residualised on state fixed effects, and the solid lines are local linear fits estimated separately on either side of the cutoff, with shaded 95 per cent confidence intervals. The six panels are any borrowing, bank borrowing, share of bank loans, other formal source, informal source and interest-bearing borrowing. Data sources: NSS AIDIS 59th and 70th rounds.

Figure A15: Difference-in-Discontinuities: Non-Muslim Households (Rural Sector)



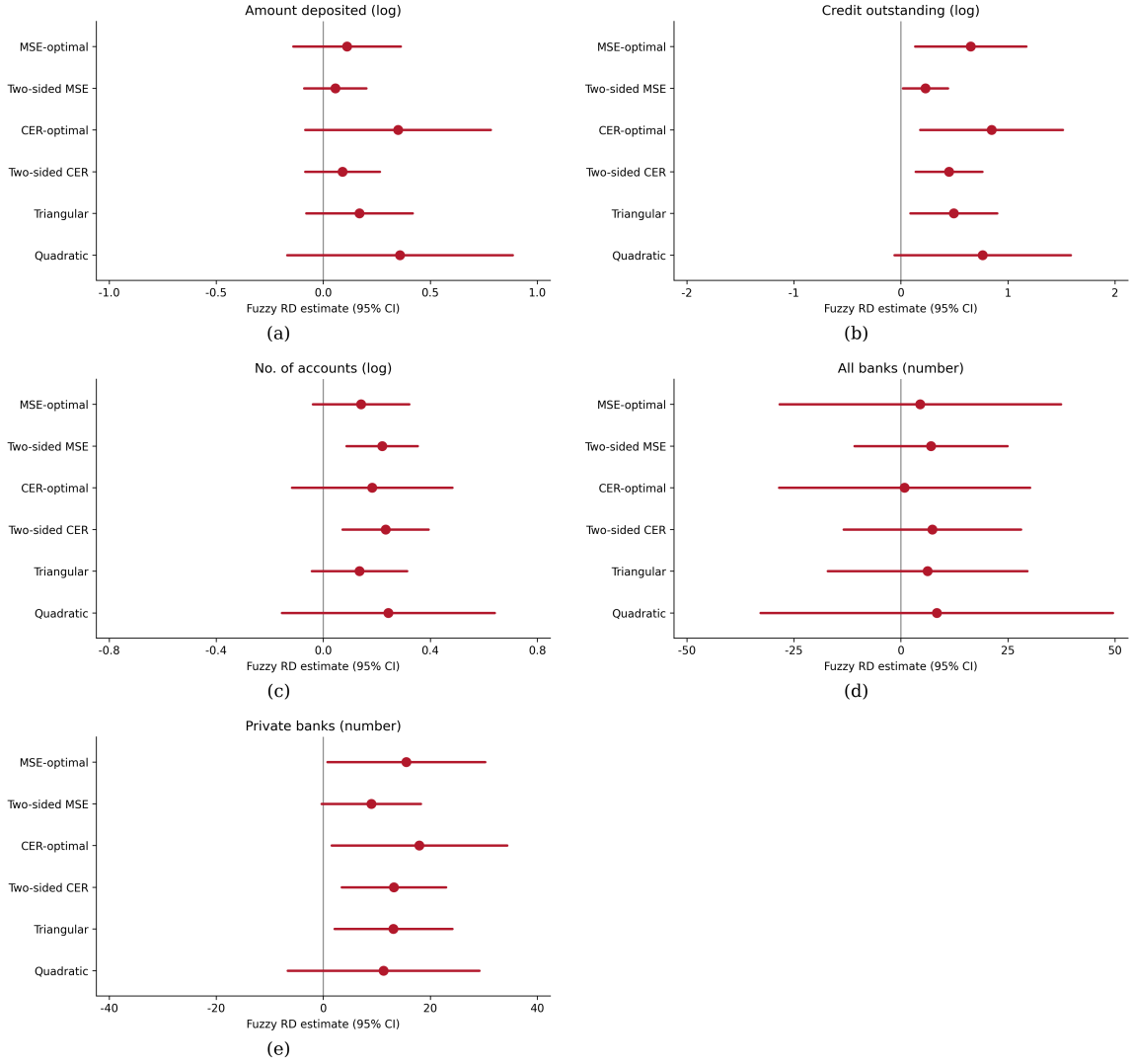
The figure plots pre- and post-policy regression discontinuities in each borrowing outcome for non-Muslim households among households in the rural sector, corresponding to the difference-in-discontinuities estimates in Table A8. Each outcome is residualised on state fixed effects, and the solid lines are local linear fits estimated separately on either side of the cutoff, with shaded 95 per cent confidence intervals. The six panels are any borrowing, bank borrowing, share of bank loans, other formal source, informal source and interest-bearing borrowing. Data sources: NSS AIDIS 59th and 70th rounds.

Figure A16: Difference-in-Discontinuities: Muslim Households (Rural Sector)



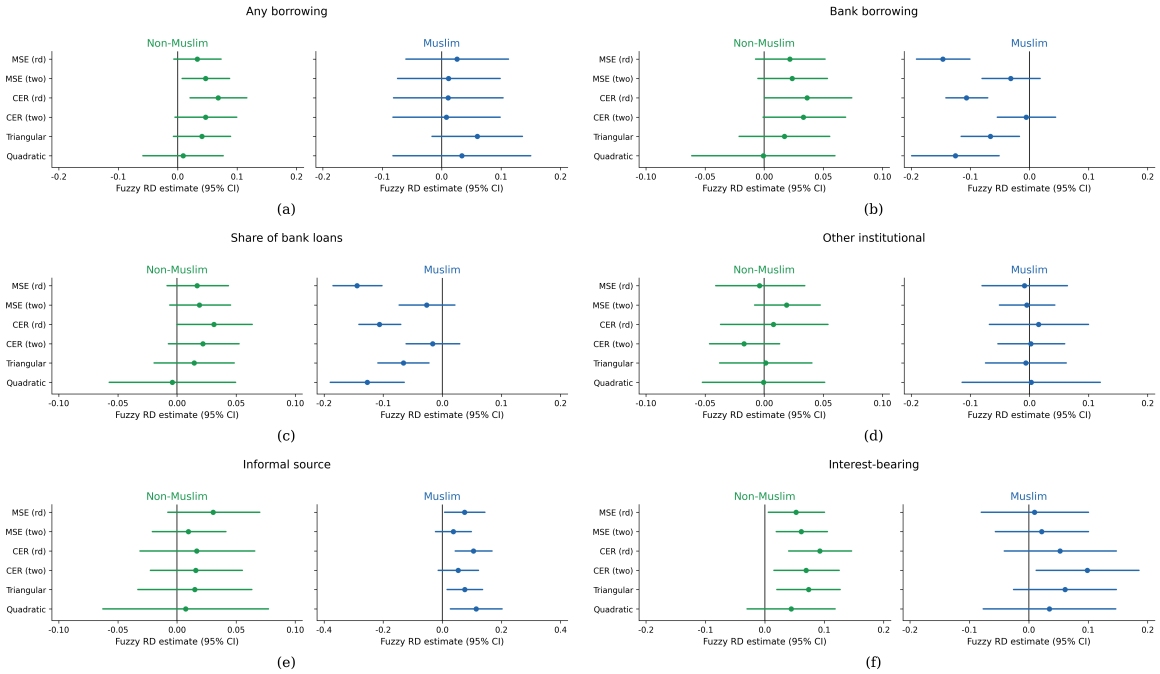
The figure plots pre- and post-policy regression discontinuities in each borrowing outcome for Muslim households among households in the rural sector, corresponding to the difference-in-discontinuities estimates in Table A8. Each outcome is residualised on state fixed effects, and the solid lines are local linear fits estimated separately on either side of the cutoff, with shaded 95 per cent confidence intervals. The six panels are any borrowing, bank borrowing, share of bank loans, other formal source, informal source and interest-bearing borrowing. Data sources: NSS AIDIS 59th and 70th rounds.

Figure A17: Robustness of the Banking-Sector Estimates



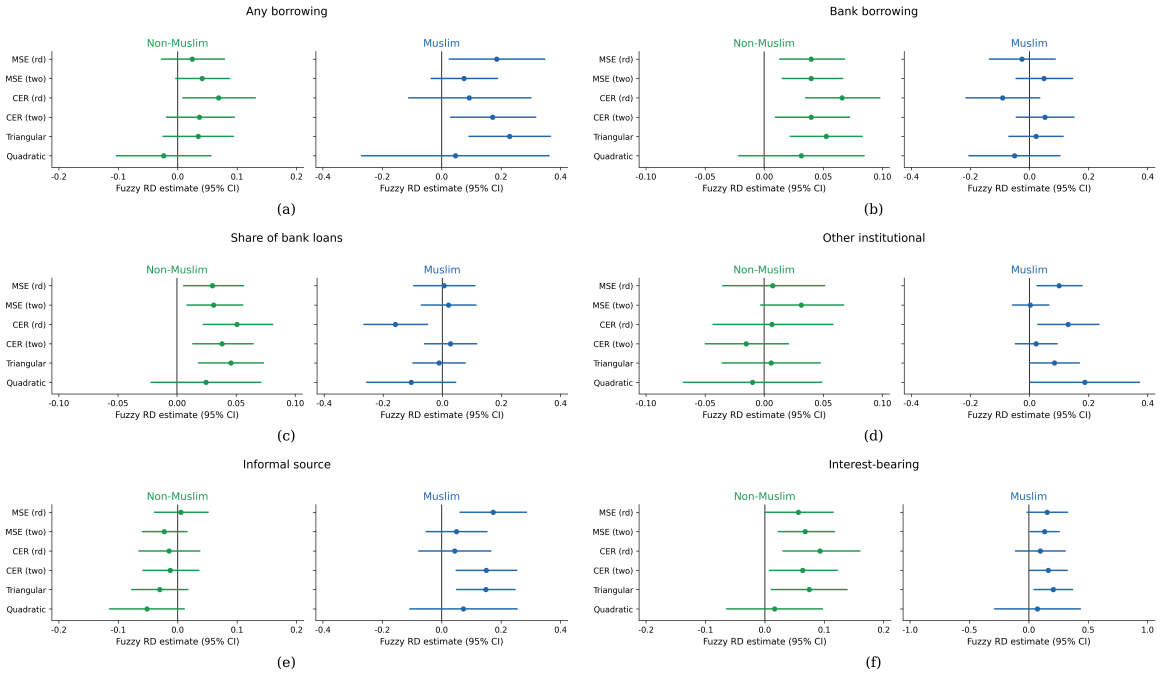
The figure plots fuzzy regression discontinuity estimates of the effect of the 2009 policy on the five banking-sector outcomes in Table A9: (a) the log amount deposited, (b) the log credit outstanding, (c) the log number of accounts, (d) the number of all bank branches, and (e) the number of private bank branches. Within each panel, the six rows report estimates under alternative bandwidth and specification choices: the MSE-optimal and CER-optimal bandwidths (each one- and two-sided), a triangular kernel, and a quadratic local polynomial in the running variable. Markers are the point estimates and horizontal bars the 95 per cent confidence intervals; the vertical line marks zero. Data source: Reserve Bank of India, Basic Statistical Returns.

Figure A18: Robustness of the Household Estimates



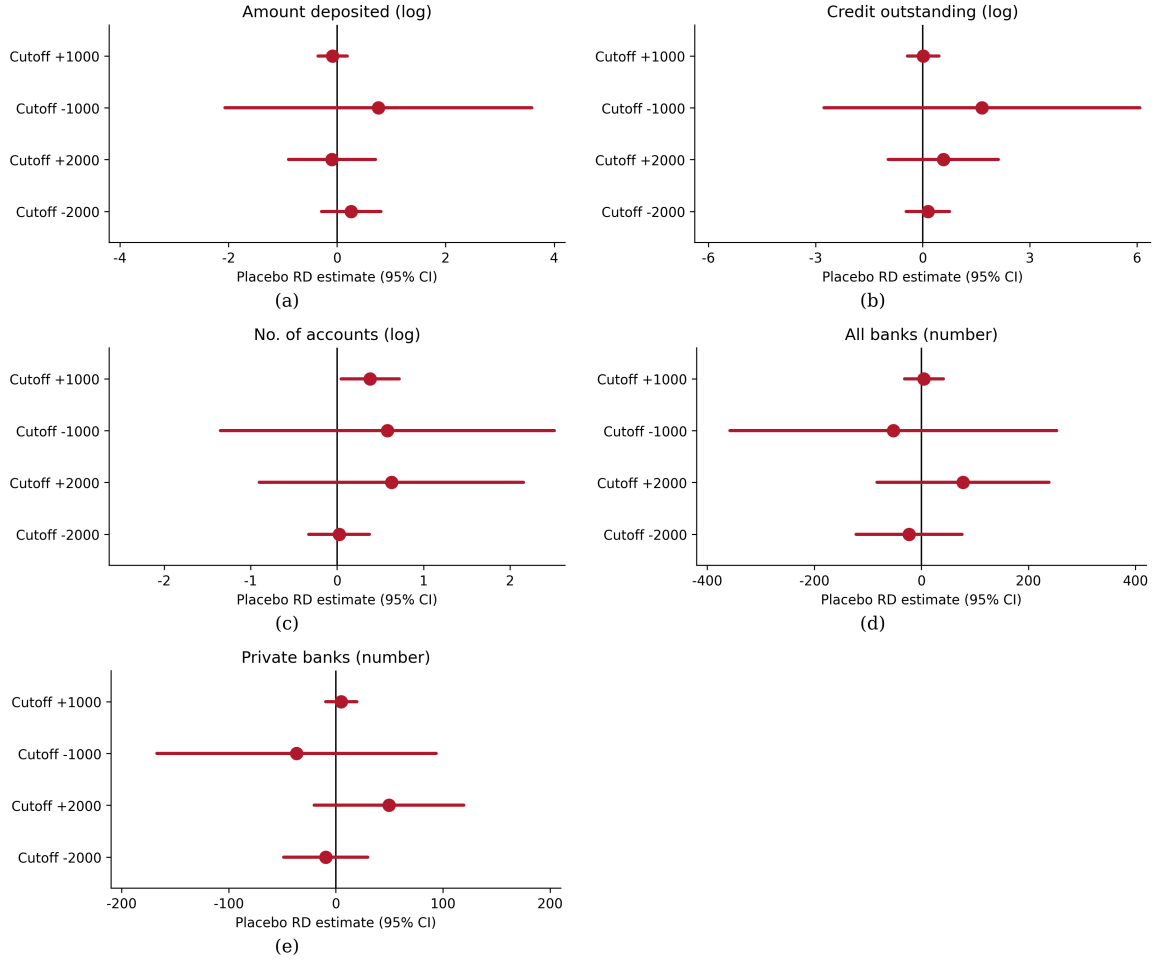
The figure plots fuzzy regression discontinuity estimates of the effect of the 2009 policy on the six household borrowing outcomes in Table A10 for the full sample of households, separately for non-Muslim (green, open circles) and Muslim (blue, filled diamonds) households: (a) any borrowing, (b) bank borrowing, (c) share of bank loans, (d) borrowing from other institutional sources, (e) borrowing from informal sources, and (f) interest-bearing borrowing. Within each sub-panel, the six rows report estimates under alternative bandwidth and specification choices: the MSE-optimal and CER-optimal bandwidths, each one-sided (rd) and two-sided (two), a triangular kernel, and a quadratic local polynomial in the running variable. Markers are the point estimates and horizontal bars the 95 per cent confidence intervals; the vertical line marks zero. Data source: NSS AIDIS 70th round.

Figure A19: Robustness of the Household Estimates (Rural Sector)



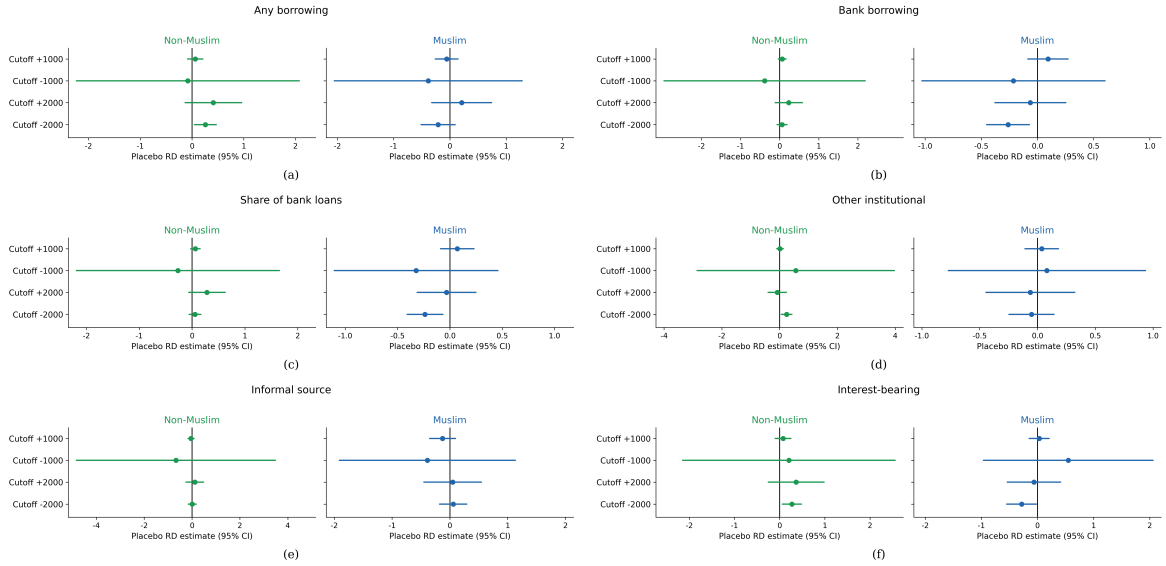
The figure plots fuzzy regression discontinuity estimates of the effect of the 2009 policy on the six household borrowing outcomes in Table A11 for households in the rural sector, separately for non-Muslim (green, open circles) and Muslim (blue, filled diamonds) households: (a) any borrowing, (b) bank borrowing, (c) share of bank loans, (d) borrowing from other institutional sources, (e) borrowing from informal sources, and (f) interest-bearing borrowing. Within each sub-panel, the six rows report estimates under alternative bandwidth and specification choices: the MSE-optimal and CER-optimal bandwidths, each one-sided (rd) and two-sided (two), a triangular kernel, and a quadratic local polynomial in the running variable. Markers are the point estimates and horizontal bars the 95 per cent confidence intervals; the vertical line marks zero. Data source: NSS AIDIS 70th round.

Figure A20: Placebo Tests: Banking-Sector Estimates



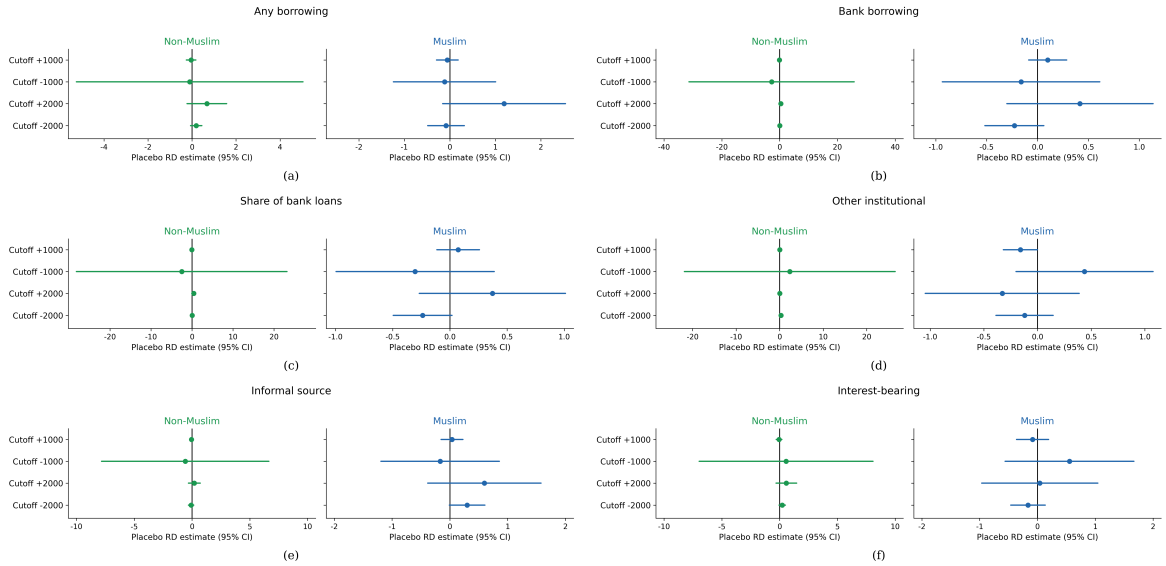
The figure plots placebo regression discontinuity estimates for the five banking-sector outcomes in Table A12: (a) the log amount deposited, (b) the log credit outstanding, (c) the log number of accounts, (d) the number of all bank branches, and (e) the number of private bank branches. Within each panel, the four rows report estimates at artificial cutoffs displaced by +1000, −1000, +2000 and −2000 population per branch from the true underbanked threshold. Markers are the point estimates and horizontal bars the 95 per cent confidence intervals; the vertical line marks zero. Data source: Reserve Bank of India, Basic Statistical Returns.

Figure A21: Placebo Tests: Household Estimates



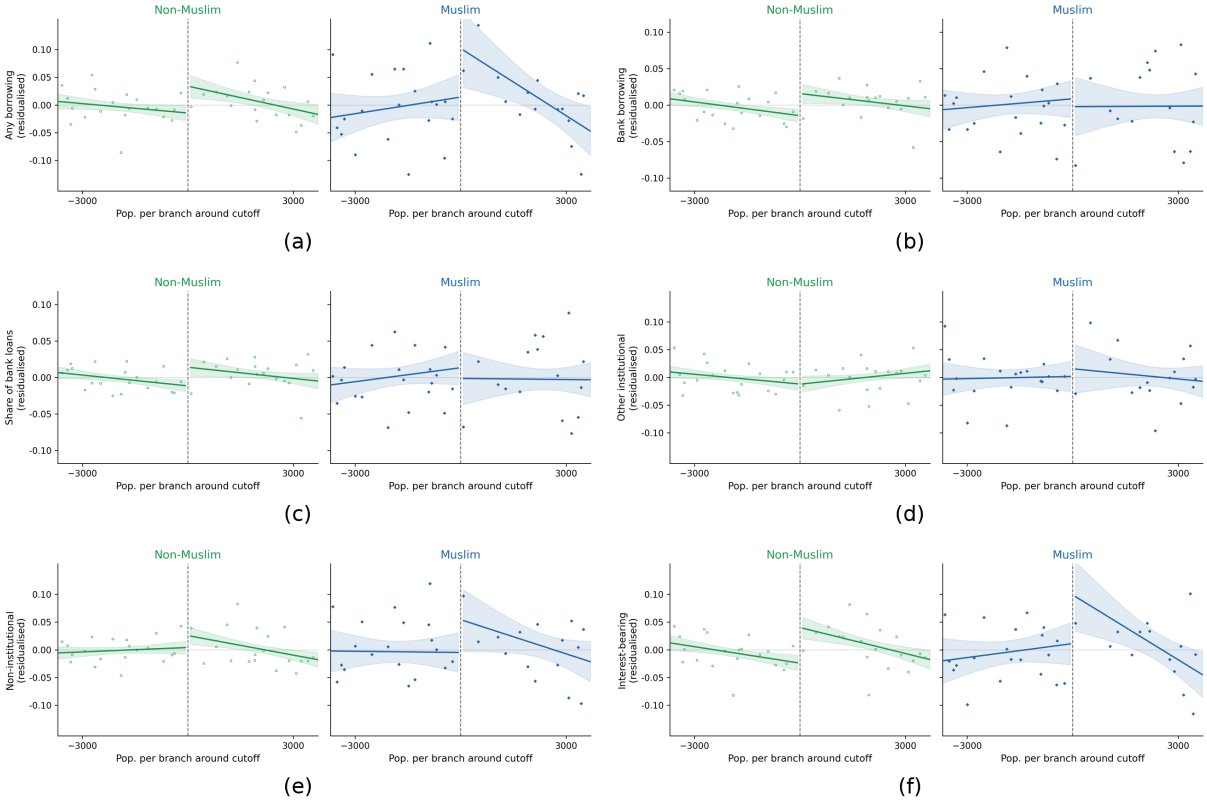
The figure plots placebo regression discontinuity estimates for the six household borrowing outcomes in Table A13 for the full sample of households, separately for non-Muslim (green, open circles) and Muslim (blue, filled diamonds) households: (a) any borrowing, (b) bank borrowing, (c) share of bank loans, (d) borrowing from other institutional sources, (e) borrowing from informal sources, and (f) interest-bearing borrowing. Within each sub-panel, the four rows report estimates at artificial cutoffs displaced by +1000, −1000, +2000 and −2000 population per branch from the true underbanked threshold. Markers are the point estimates and horizontal bars the 95 per cent confidence intervals; the vertical line marks zero. Data source: NSS AIDIS 70th round.

Figure A22: Placebo Tests: Household Estimates (Rural Sector)



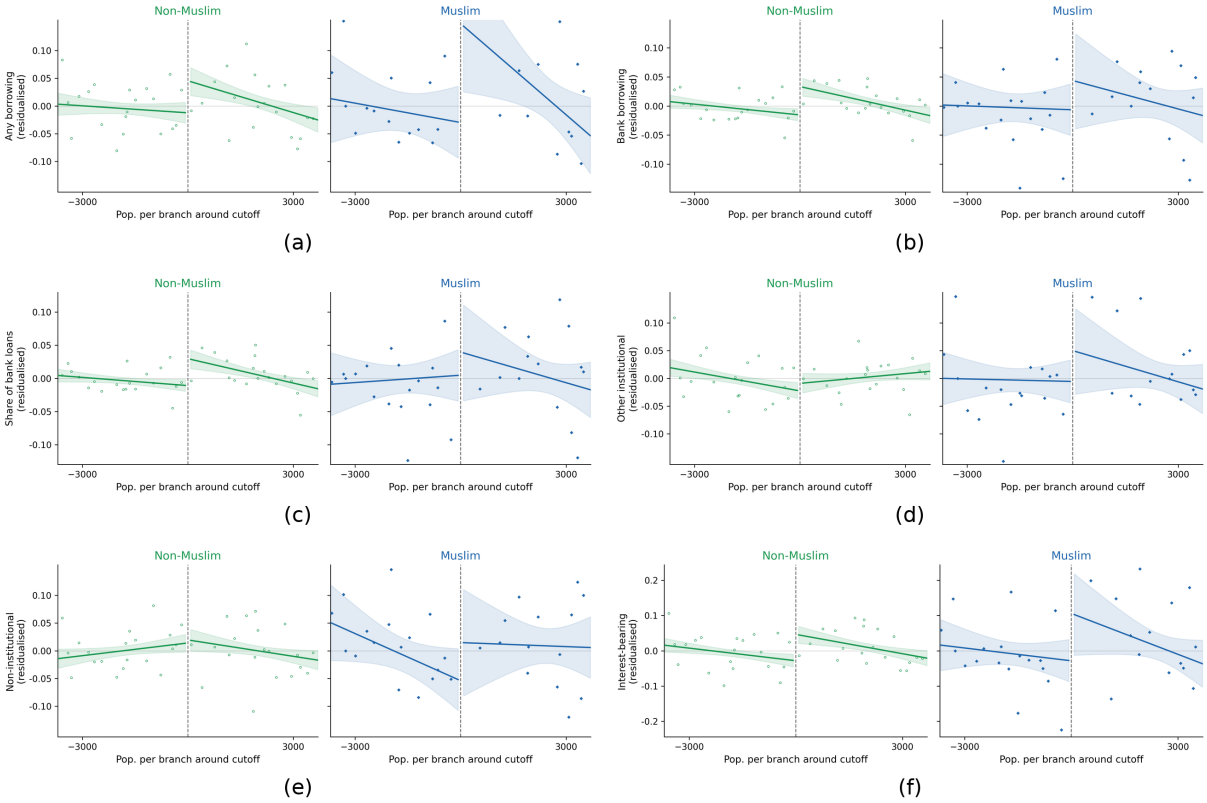
The figure plots placebo regression discontinuity estimates for the six household borrowing outcomes in Table A14 for households in the rural sector, separately for non-Muslim (green, open circles) and Muslim (blue, filled diamonds) households: (a) any borrowing, (b) bank borrowing, (c) share of bank loans, (d) borrowing from other institutional sources, (e) borrowing from informal sources, and (f) interest-bearing borrowing. Within each sub-panel, the four rows report estimates at artificial cutoffs displaced by +1000, −1000, +2000 and −2000 population per branch from the true underbanked threshold. Markers are the point estimates and horizontal bars the 95 per cent confidence intervals; the vertical line marks zero. Data source: NSS AIDIS 70th round.

Figure A23: Covariate-Adjusted Household Estimates



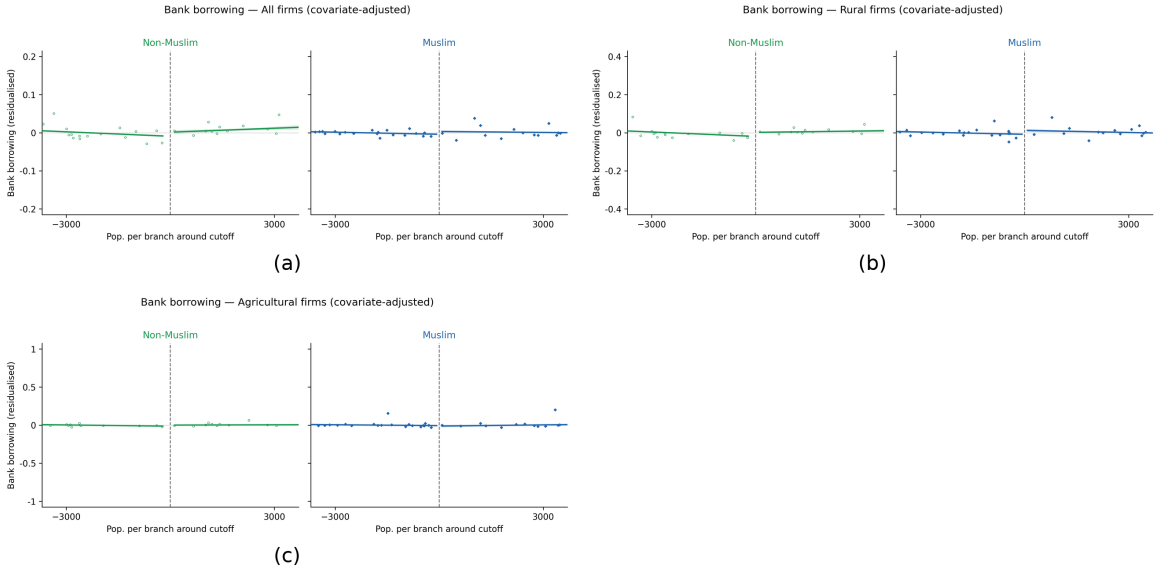
The figure plots the covariate-adjusted regression discontinuities in the six household borrowing outcomes from Table 3, separately for non-Muslim (green open circles, left) and Muslim (blue filled diamonds, right) households: (a) any borrowing, (b) bank borrowing, (c) share of bank loans, (d) borrowing from other institutional sources, (e) borrowing from non-institutional sources, and (f) interest-bearing borrowing. Each outcome is residualised on state fixed effects and household-level covariates: the age, sex, educational attainment and primary occupation of the household head, family size, and land ownership. Data source: NSS AIDIS 70th round.

Figure A24: Covariate-Adjusted Household Estimates (Rural Sector)



The figure plots the covariate-adjusted regression discontinuities in the six household borrowing outcomes from Table A2 for households in the rural sector, separately for non-Muslim (green open circles, left) and Muslim (blue filled diamonds, right) households: (a) any borrowing, (b) bank borrowing, (c) share of bank loans, (d) borrowing from other institutional sources, (e) borrowing from non-institutional sources, and (f) interest-bearing borrowing. Each outcome is residualised on state fixed effects and household-level covariates: the age, sex, educational attainment and primary occupation of the household head, family size, and land ownership. Data source: NSS AIDIS 70th round.

Figure A25: Covariate-Adjusted Firm Estimates



The figure plots the covariate-adjusted regression discontinuities in firm bank borrowing from Table 5, separately for non-Muslim (green open circles, left) and Muslim (blue filled diamonds, right) establishments: (a) all establishments, (b) establishments in the rural sector, and (c) farm-based establishments. The outcome is a dummy equal to one if the establishment reports a commercial bank as a major source of finance, residualised on state fixed effects and establishment-level covariates. Data source: Economic Census 6th round.