Political Punishment and Financial Safety Nets: Evidence from India's Demonetization*

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Abstract

How do voters respond to popular policies that appeal to moral sentiments, but that have negative economic consequences? We examine voter dynamics following one such policy – India's 2016 demonetization that made 86% of currency-in-circulation redundant overnight. Leveraging discontinuities in banking access, we identify the impacts of demonetization's severity. Places with more severe demonetization had lower economic activity, and voters had unfavorable views on demonetization. Difference-in-discontinuity estimates show that the ruling party subsequently received a 4.7pp lower vote share and were less likely to win in regions with fewer banks. Yet, ruling-party strongholds remained unresponsive in voting behavior.

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1 Introduction

Recent years have seen a rise in elections won by strong, charismatic personalities, who often make moral appeals to voters to implement drastic policies (Ravanilla et al., 2020; Enke, 2019). These policies are meant to signal the integrity of the single leader who represents the 'morality' of the people, without need for parliaments or independent agencies (Guriev and Papaioannou, 2020; Acemoglu et al., 2013). Yet, policies that bypass democratic checks and balances may be poorly implemented, and may adversely affect constituents. Whether this recent trend in extreme policies is here to stay depends on how voters respond to such actions. So we ask: What are the electoral consequences of economically costly policies with strong moral appeal?

Recent work on voter behavior documents the electoral effects of targeted fiscal transfers, or random events, such as weather shocks (Manacorda et al., 2011; Zimmermann, 2020; Cole et al., 2012). Yet, we know little about voting behavior following large national-level policies which adversely affect citizens' economic well-being, but are implemented by popular leaders with strong messages appealing to prevailing moral sentiments. We examine such electoral dynamics in the context of a unique and unexpected policy in India. Specifically, we study how voters responded to a national-level demonetization, that led to salient economic adversities, but at the same time, derived broad appeal from its stated objectives of targeting corruption and terrorism.

On November 8, 2016, the Prime Minister unexpectedly announced over a televised address that, to stem the flow of counterfeit notes and hinder the black economy, starting at midnight, two commonly used denominations of currency notes would cease to be legal tender. This meant that 86% of the currency that was in circulation was suddenly and unexpectedly deemed illegal overnight.[1] New currency notes were to be provided in exchange for old notes at bank branches, but with significant limits on how much one could withdraw. Government agencies, parliament and banks were not forewarned of this unexpected policy, leading to confusion and difficulties in implementation.[2] These constraints, coupled with the slow printing of new notes, led to a widespread shortage of cash across the country. There were reports of unrest and economic hardship in the months following the announcement (The Times of India, 2018). Notably, the policy was highly salient, directly attributable to the ruling party and Prime Minister, and differentially affected voters based on their access to banking-sector safety nets.[3]

Despite these negative consequences, the policy was not unpopular: using data from a nationwide survey conducted by the Center for the Study of Developing Societies (CSDS), we find that only 16 percent of respondents felt that demonetization was a "bad policy." As much as 45 percent of respondents felt it was a "good policy," while 32 percent felt that it was a "good policy that could have been implemented better." This broad appeal may reflect a combination of the policy's valence,

[1]Like most low and middle income countries, transactions in India are cash dependent. In 2014, 87% of all transactions in India were conducted in cash (IBGC, 2014).

[2]Since none were aware of the policy, there were therefore no debates in legislatures or discussion in the media. Writ petitions were filed on behalf of individuals and organizations in eight High Courts and the Supreme Court of India, questioning the constitutionality of the notifications. However, by March, 2017 legislation was finally passed by the Parliament of India to give legal effect to demonetization.

[3]Recent work shows detrimental effects of the policy on the country's economy (Chodorow-Reich et al., 2019; Lahiri, 2019)

the strong messaging, and/or the popularity of the Prime Minister.[4] The messaging by the ruling party framed the policy as one that would combat corruption and black money, which are popular concerns of the electorate.[5] Messaging that appeals to such voter morality may produce electoral gains despite economic costs (Sandel, 2005; Enke, 2019). Indeed, in subsequent state elections, the implementing party was victorious, and most commentators and politicians claimed the policy was an electoral success (The Indian Express, 2017).

The central bank's own reports, however, showed that the anti-corruption objectives were never met (Quartz, 2018). As such, the policy had broad appeal among a majority of voters, but at the same time, led to severe shortages in access to cash and consequent negative economic effects, especially in regions with less access to banks. The subsequent victories in state-level elections may simply reflect aggregate pre-determined trends in support for the ruling party, and so the electoral consequences of such policies are ambiguous and worthy of rigorous empirical investigation. We conduct such an investigation, and further explore whether such adverse economic impacts were sufficient to even sway voters that were likely to vote for the ruling party.

We isolate the effect of access to cash (or the lack thereof) during demonetization, by leveraging a district-level discontinuity in the number of bank branches that arose due to a banking expansion program instituted in 2005 by the previous government. Using the same variation and a regression discontinuity design, Young (2017) showed that the 2005 bank branch expansion improved banking outcomes in districts just below the cutoff. Cramer (2020) uses the empirical strategy to describe how access to banks created a financial safety net, and led to resilience during times of poor harvest or adverse health shocks. Similarly, we expect that access to banks creates a safety net from such policy-induced shocks, especially as the banks improve access to the new currency notes.

The nationwide policy was targeted to all districts that had banks per capita below a certain cutoff, which was determined by the autonomous central bank, the Reserve Bank of India. To isolate the effects of demonetization severity, we use a regression discontinuity (RD) design, with banks per person as the running variable. We examine electoral outcomes across the cutoff and over time, before and after demonetization. This allows us to account for differences in outcomes before demonetization that may exist due to direct effects on voter behavior of the bank expansion policy. As such, our difference-in-discontinuities design leverages the panel dimension along with the RD.

We first document that the bank expansion policy was well enforced, that there was high policy-compliance around the cutoff, and that there was indeed a sharp discontinuity in the density of bank branches, bank accounts, and amount of outstanding credit, which persisted through to 2016. We posit that more access to banks mitigated the negative consequences of demonetization, as individuals had to wait in shorter lines to exchange cash, and did not have to travel far to access banks. Less banked areas, in contrast, would have a shortage of cash, hurting economic activity

[4] In the CSDS survey, a majority of respondents reported feeling satisfied with the Prime Minister. Between 2013 and 2017, around four-fifths of the electorate consistently had favorable views of the Prime Minister (Pew, 2017) https://www.pewresearch.org/global/2017/11/15/india-modi-remains-very-popular-three-years-in.

[5] In a New Year's Eve speech (fifty days after demonetization), the Prime Minister urged that the "*fight against black money and corruption should not be stopped.*" He recognized that there would be inconveniences, but in many ways the costs were indicative of the important long-term gains: "*Citizens have proved that for them, truth and honesty are important, despite inconveniences post demonetization.*" The speech can be watched here: https://www.youtube.com/watch?v=ivAIsJz$_t$ − $gab_channel = NDTV$

more broadly (The Indian Express, 2017).

Consistent with the findings in Chodorow-Reich et al. (2019), we use our research design to verify that there was indeed a reduction in economic activity due to demonetization. We find that in months that followed demonetization, areas that had discontinuously fewer banks had a sharper drop in nighttime light intensity – a commonly used measure of economic activity. For 10% fewer bank branches, GDP was lower by 0.5% in the months following demonetization. Using voter level surveys, we also find citizens in worse hit districts had a less favorable view of demonetization. We interpret these results to suggest that more bank branches mitigated the severity of the effects of demonetization, and somewhat protected the population that now had access to banking safety nets.

Turning to our main outcome of interest – the electoral impacts as a result of demonetization – we first document that on aggregate there was the appearance that the ruling party continued to win more votes after demonetization. Alone, this may suggest to many, as the popular press proposed, that demonetization did not hurt the ruling party electorally. Yet, the ruling party may have just been on an increasing trajectory of votes, confounding any detectable impacts of demonetization. Our empirical strategy allows us to isolate the impacts of demonetization from any possible confounding aggregate trends.

We find that in areas with lower access to banks (and therefore, less access to cash), the ruling party did relatively worse. The ruling coalition received a 4.7 percentage point lower fraction of votes in regions that had discontinuously fewer bank branches (i.e., where demonetization was more severe). Our results suggest that a 10% decrease in the number of new bank branches was associated with a 0.9 percentage point lower vote share for the ruling party after demonetization. Importantly, the ruling party was less likely to win seats in places with fewer banks. What is striking is that the previous ruling party's banking expansion program helped the current ruling party not lose votes.

Importantly, we find that while ruling party stronghold areas suffered similar adverse economic effects, they saw no detectable changes in voting behavior. This suggests that there are a meaningful fraction of voters that do not update their voting behavior based on one particular policy, even if their own livelihoods are impacted. These may reflect either strong alignment with the ruling party on ideological grounds, or that other issues on the party platform are relatively more important (Besley and Coate, 2008). Indeed, in survey data we find that the policy was more popular with individuals aligned with the ruling party on ideological grounds: voters that are Hindu, more religious, or think that beef should be banned. Yet, the moral appeal of the policy, or ideological alignment may not explain the entirety of the non-responsiveness of voters. Indeed, even in stronghold areas, the policy was relatively unpopular in underbanked regions, where the impact of demonetization was more severe; but this unpopularity did not translate into fewer votes. This suggests that voter non-responsiveness, despite adverse economic consequences, may reflect voter beliefs that the party in power would address other issues they value.

Our paper is broadly related to a small, but growing literature showing how voters respond to negative economic downturns. For instance, Autor et al. (2020) find that voters in the US with greater exposure to a negative trade shock were more likely to vote for conservative candidates perceived to be less in favor of free trade. Cole et al. (2012) find that when there are negative rainfall shocks, voters in India are more likely to punish incumbents that did not spend enough on

disaster relief. In contrast, we study consequences of economic adversities that were more directly induced by economic policy, rather than external shocks.

While there is a growing literature on the electoral effects of targeted government transfers (De La O, 2013; Imai et al., 2020; Manacorda et al., 2011), we know far less about voter responsiveness to policies that have broad support, but negative economic consequences. The findings in the papers on targeted transfers are consistent with theories of rational voters, who, if well informed, reward incumbents. This could be in anticipation of continued transfers in the future, or implicit reciprocity arrangements (Finan and Schechter, 2012).[6] Evidence from India suggests that voters may hold local leaders accountable when implementation quality of programs deteriorate (Zimmermann, 2020).

Salience and information are also important factors when considering voter responses (Ferraz and Finan, 2011; George et al., 2020). Especially information on who deserves credit or blame (Guiteras and Mobarak, 2014). While testing for this would require variation in salience, our findings are consistent with the idea that the more salient the policy, the more responsive voters are likely to be. We find that voters did *not* reward the implementers of the bank-expansion policy itself in the decade following the expansions. This may be because bank expansions are typically gradual (as was the case here), and voters find it difficult to attribute the policy to the specific party. In contrast, for the sudden demonetization with its widespread media coverage and salience in daily economic life, voters had a better sense of who was responsible. Using voter-level surveys conducted in the months following the demonetization, we find that respondents in regions with more banks were more likely to say the policy was the right move, and were less likely to say the policy was poorly implemented.

Our finding that voters in areas that were strongholds of the Prime Minister's party were electorally unresponsive to the demonetization is consistent with work on the prevalence of issue-bundling during elections (Besley and Coate, 2008, 2003; Iversen and Goplerud, 2018). Even when an issue is highly salient and has an immediate negative economic impact (as in the case of demonetization), voters more strongly aligned with the party, may not be as responsive electorally with respect to it. We find that in ruling party strongholds, the electoral impacts of the demonetization were absent, *despite* the fact that in voter surveys, individuals in these areas did not have more favorable views of the policy itself. Taken together, our results suggest that in democracies where voters get one chance to vote, but where the policy space is multi-dimensional, voters who align more closely with a particular politician or party, are less responsive to any specific policy, however salient it may be.[7]

The remainder of the paper is as follows: Section 2 provides a description of the institutional background, and in particular, the demonetization and the bank branch policies. Section 3 describes the data. Section 4 explains our empirical strategy, Section 5 discusses the results and robustness check, Section 6 discussed mechanisms, and Section 7 concludes with a brief discussion.

[6] Blattman et al. (2017) show that rather than rewarding the incumbent government, beneficiaries favor the opposition, in response to receiving grants for new enterprises. They hypothesize that the financial independence that resulted from the receipt of the grants, makes voters less reliant on patronage and transfers.

[7] The literature discusses the issues surrounding the multi-dimensional policy space in democracies. Funk and Gathmann (2011) and Feld et al. (2010) explore how direct democracy (that is, voting on a specific policy) affects voter behavior and therefore policy-making. DeLaO and Rodden (2008) explores whether religious preferences distract the attention of poorer households from redistributive policies. Fernández and Levy (2008) provides a formal framework for thinking about how income and preference heterogeneity may affect redistributive policies in a democracy.

2 Background

2.1 India's 2016 Demonetization

Demonetization is the act of rendering one or more units of a currency illegal as tender. It is usually accompanied by the replacement of the outdated currency with new notes or coins, and has been implemented from time to time around the world.[8] The primary reasons cited in most cases include fighting the black economy while reducing corruption and counterfeit currency. India has seen demonetization being implemented four times, including the one in 2016.[9] During the 2016 demonetization, 500 and 1000 rupee currency notes were suddenly no longer considered to be legal tender.[10] The announcement claimed that new 500 and 2000 rupee notes would be issued over time. Individuals could deposit old notes in banks, but not conduct any transactions using these, and all old notes were required to be deposited by December 31 in exchange for the new notes.[11] In addition, only 4000 rupees per person could be drawn per day. The stated objectives of this policy was to curtail the shadow economy, fight terrorism, and tackle counterfeit currency.[12] The stock market crashed the day after the policy was announced, and in the following weeks and months, there was a sharp decline in the availability of cash in the economy (Lahiri, 2019).

2.1.1 Impact on the Economy

Recent work has looked into the impact of the 2016 demonetization on the economy, finding largely negative effects in the short and medium run. Chodorow-Reich et al. (2019) use data on the spatial distribution of demonetized and new notes, and find that districts subject to more severe demonetization had less economic activity (as measured by nightlights), and lower bank credit growth. The decline in cash lowered nightlights-based economic activity and employment by at least 3 percentage points in November and December of 2016, and the authors suggest that this is a lower bound of the aggregate consequences.[13]

In other work, Banerjee and Kala (2017) find that 20% of 400 surveyed wholesale and retail traders reported a fall in sales of greater than 40%, and that on average, their sales were 20% lower. Similar contemporaneous work finds that demonetization led to a sharp increase in unemployment (Subramaniam, 2019), a reduction in household consumption expenditures (Wadhwa, 2019), and

[8] For instance, in 1871, gold was standardized as the legal tender in the United States and silver was removed from circulation. Again, in 1969, currency notes above USD 100, were deemed illegal by President Nixon in order to tackle the black economy. Similarly, Ghana (in 1982), Nigeria (in 1984), Myanmar (in 1987), and the USSR (in 1991) were among some of the countries that implemented a demonetization policy.

[9] The first occurred towards the end of Colonial rule in 1946, when less that 10 percent of the cash in circulation was declared illegal as tender, presumable affecting a small fraction of the population and economy. The second was in the princely state of Hyderabad in 1953, while attempting to integrate the state into the rest of the country's economy. Individuals were given a period of two years to change the old notes for new tender. The third was in 1978 when high denomination notes (less than one percent of currency in circulation) were declared illegal tender with presumably little impact.

[10] Approximately corresponding to USD $7.5 and $15 in 2016.

[11] The fraction of transactions that are conducted in India using cash is high. According to a survey conducted by MasterCard and the Institute for Business in the Global Context (IBGC) at Tufts University, 87% of all transactions were conducted in cash, as of 2015.

[12] There were some changes introduced in the rules for depositing and withdrawing cash in the weeks following the announcement. See Banerjee et al. (2018) for a careful summary.

[13] Since we focus on districts around a discontinuity, we verify whether we see the same economic effects in our Local Average Treatment Effect (LATE). These results are shown in Section 5.

sharp and persistent reductions in agricultural trade values even eight months after demonetization (Aggarwal and Narayanan, 2019).[14]

2.1.2 Reactions to the Policy

The restrictions placed on the amount of new cash that could be withdrawn each week, combined with the number of individuals impacted led to significant disruption in the country. Several legal challenges were introduced in courts across the country and there was significant debate about the legality of the policy itself (Kumar, 2016). The opposition parties and civil bodies decried the lack of deliberation preceding the unexpected, unilateral decision. Government agencies were not prepared for smoothly implementing the policy. Media reports were rife with news about lines outside bank branches, and how impacts were more severe for vulnerable populations like the elderly.[15]

Yet, a nation-wide survey conducted by the Center for the Study of Developing Societies (CSDS) in May 2017, found that 45 percent of respondents believed that demonetization was "the right move," compared to only 16 percent who thought it was not needed. 32 percent felt the policy was a good one, but implemented in a hurry without the necessary groundwork.

Despite widespread negative media reports and adverse economic impacts, it was unclear that there were any political repercussions for the ruling Bharatiya Janata Party (BJP), or its coalition, the National Democratic Alliance (NDA). In the months following demonetization, the BJP and its allies won many state and local elections held around the country, giving the impression that perhaps voters did not seek to electorally punish the BJP for the policy.[16] The victories may reflect strong messaging by the popular Prime Minister, appealing to voters' anti-corruption sentiments.

It is also plausible however, that the BJP may have just been on an upward trajectory of votes, and as such these aggregate trends may hide the true causal reaction of voters to the policy. Our aim is to isolate the causal effect from these aggregate trends.

In concurrent work, using state-level bank expansions from the seventies and eighties as instruments for banks that existed in the year 2009, Bhavnani and Copelovich (2020) explore whether there were electoral consequences in 75 districts across seven states in India.[17] Their findings suggest that regions with fewer banks in 2009 saw increases in votes for the ruling party (BJP) in elections after demonetization. One way to reconcile our findings is to consider the possibility that areas with fewer banks after the end of the 1980s scheme are areas that actually received the 2005 bank expansion policy, and as such, have significantly greater bank growth in the years preceding 2016's demonetization.[18] Indeed, the 2005 bank expansion policy was targeted to 'underbanked' districts.

[14] In addition, Aggarwal et al. (2019) show that areas with high informality rates saw a greater switch to digital payments when the digital infrastructure was available. Chanda and Cook (2019) find a positive correlation between bank deposit growth after demonetization and subsequent economic activity.

[15] For example, see The Times of India (2018). The government largely denied the negative effects on the Indian economy (The New York Times, 2019).

[16] This was exemplified in a quote by the BJP Chief Minister of Gujarat: *"Elections for gram panchayats were held immediately after demonetisation... 80 per cent of the gram panchayats were won by the BJP. Thereafter elections were held in Maharashtra where BJP won. Then state assembly polls were held in five states and BJP emerged victorious with thumping majority in Uttar Pradesh and Uttarakhand. Congress was swept away. This clearly shows that Congress does not enjoy people's support on the issue of demonetisation."* (The Indian Express, 2017)

[17] They leverage variation similar to Burgess and Pande (2005), but only for 7 states.

[18] It is likely that in the small subset of districts used, the old bank expansion policy does predict banks till 2009, but is not generalizable to the rest of the country, and it may not extend to 2016, the year of demonetization.

Additionally, the banking sector in the 1980s largely consisted of state banks, but deregulation in the 1990s expanded the private banking sector and made them major players. The incentives we study in our bank-branch expansion policy led to a significant increase the number of banks (Figure A.1), including *private banks* in underbanked districts, which grew rapidly in the last two decades with effects persisting through to 2016.

2.2 Bank Branch Expansions

Our primary source of variation in how severe demonetization was across the country is a measure of access to bank branches. Since cash could be deposited and withdrawn primarily from bank branches, the severity of demonetization would be greater in places with few or no bank branches. More banks per capita would mean less time waiting in long queues, less travelling to branches further away to exchange money, and a greater ease of exchanging old currency for the new.

We utilize a policy reform in India that took place in September 2005, wherein additional commercial bank branches were encouraged to open in 'underbanked' districts. Free entry of bank branches is not permitted in India, and any new bank licenses are granted infrequently by the Reserve Bank of India (RBI). The 2005 bank expansion reform allowed easier entry of bank branches into districts that were given 'under banked status' by the RBI, based on the district average persons per branch. The cutoff chosen was the national average of persons per branch in a district. This provides us with a discontinuity in banking programs around the national average cutoff.[19]

In recent work, Young (2017) uses this variation to evaluate the economic effects of the bank branch expansion policy, and finds improvements in agriculture, manufacturing, and local GDP (measured by nightlights). More recently, Cramer (2020) shows that access to such banks protected households from negative health and agricultural shocks. Essentially, the 2005 policy was a branch licensing reform that incentivized new commercial banks to enter in regions with fewer banks. As Young (2017) and Cramer (2020) show, the new rules required banks to expand in underbanked regions to be eligible for licensed entry into richer lucrative markets. Additionally, banks were required to make accounts accessible to low-income customers by having accounts with limited fees and low minimal balances. We first replicate the banking expansion shown by other work.

3 Data and Measurements

Banking Data. We use district-level banking data from the Reserve Bank of India (RBI) from 2002 to 2016, which includes information on the number of bank branches, the number of accounts, and outstanding credit. We also use data at the bank branch level by district from the RBI's Master Office File (MOF), which includes bank branch locations, and the year that they were established.

Elections. To look at electoral outcomes, we restrict attention to the sample of states where elections were held in 2017 and 2018 (following the implementation of the demonetization policy).[20]

[19]Districts on either side of the bank expansion cutoff were spatially distributed throughout the country, and there was no indication of geographical clustering.

[20]The states with recent elections (or by-elections) include Assam, Chhattisgarh, Delhi, Goa, Gujarat, Himachal Pradesh, Jharkhand, Karnataka, Madhya Pradesh, Manipur, Meghalaya, Nagaland, Punjab, Rajasthan, Sikkim, Telengana, Tamil Nadu, Tripura, Uttar Pradesh and Uttaranchal.

In India, parliamentary elections are held at the national level every five years (the last year being 2019), while elections for state legislatures are also held every five years, but in a staggered manner.

Our data contains electoral information for all states between 1999 and 2018. This data is obtained from the Electoral Commission of India, and contains information at the constituency level on candidates that stood for elections, their party affiliations, number of votes obtained, and demographic information such as age, gender, and reservation category. We extend this electoral data set by creating a variable that contains information on whether the candidate belongs to the ruling party's coalition (the NDA), or the opposing coalition (the UPA), at the time when the policy was implemented.

Nightlights. We follow past research by Henderson et al. (2012); Michalopoulos and Papaioannou (2013), and Pinkovskiy and Sala-i Martin (2016) in using night-time lights as a proxy for economic activity. More recently, this has been used in the Indian context (Chodorow-Reich et al., 2019; Prakash et al., 2019; Mahadevan, 2020), where high-frequency, high-spatial resolution data on economic activity is rare. These data, in particular the stable lights product have been shown to correlate extremely well with measures of economic development, incomes, electrification rates, and urbanization. Luminosity data are taken from the Defense Meteorological Satellite Program's Operational Linescan System. Major advantages of these data include their spatial divisibility, their consistency across multiple political jurisdictions, their high spatial resolution, and their monthly availability. These data are constructed as a monthly average of satellite images of the earth taken daily between 20:30 and 22:00 local time. The raw data are at a 30 second resolution, which implies that each pixel in the raw data is roughly one square kilometer. The raw luminosity data for each pixel is reported as a six-bit integer ranging from 0 to 63. We average over pixels within a district.

Voter Preferences. The final part of our data set consist of responses on a nation-wide survey conducted by the Centre for the Study of Developing Societies (CSDS). Between May 1-15, 2017, 11,373 respondents, across 19 states were surveyed by CSDS, where sample of respondents were randomly chosen from 584 locations and 146 Assembly Constituencies (ACs).[21] The survey collected detailed demographic and socioeconomic data, and asked questions about respondents' political preferences and attitudes about the government, political parties, and importantly, about several policies in the country, including demonetization.

We use the CSDS household level surveys to study whether the banking policy changed household access to banks as well. The CSDS asks respondents whether they have a bank or post-office account, and whether they have a credit or debit card.[22]

4 Empirical Strategy

We isolate the effect of access to cash during demonetization by leveraging variation in the bank expansion policy of 2005. Specifically, to isolate the effects of demonetization severity on political outcomes, we use two main specifications. The first, which is our preferred specification, leverages

[21] CSDS also collects polling surveys around elections. But we use the data from the Mood of the Nation Survey as it was simultaneously conducted across most of the country in May, 2017.

[22] Post office accounts are often used in rural areas in lieu of bank accounts.

both the variation around the bank expansion cutoff as well as the variation across time. We restrict our sample to a bandwidth around the cutoff, and compare electoral outcomes before and after demonetization. The cutoff chosen by the central bank was the national average, and as such is unlikely to be manipulated, as we show with the help of validation tests. Choosing the national average also ensures that there are a large number of districts in a bandwidth near the cutoff.

While the RD bandwidth ensures we are comparing across similar districts, the panel dimension accounts for any dynamics leading up to the policy change. Any differences in vote shares before demonetization would be evident in the pre-period electoral outcomes. Such differences may exist as voters may reward the earlier ruling party (the Congress party) for their banking expansion scheme.[23] Using a difference-in-discontinuities design and exploiting the panel dimension of the data, we estimate the following specification:

$$Y_{dt} = \delta(Post_t \times RD_d) + \gamma_d + \mu_t + \epsilon_{dt} \quad for\ d \in \{-D, D\} \tag{1}$$

Here $Post_t$ is an indicator for whether $t > November\ 8, 2016$ (the date when the demonetization policy was unexpectedly announced). The restricted bandwidth ensures we are only comparing similar districts, while the γ_d district fixed effects control for district characteristics (including polynomials of the running variable). The parameter of interest is δ, which provides us with an estimate of the difference-in-discontinuities, at the cutoff (that is, having *less* severe demonetization), within a bandwidth around the cutoff. As there are no optimal bandwidth procedures for a difference-in-discontinuities analysis, we vary the bandwidth manually to test for robustness for all results presented using this strategy.

For outcomes for which we have data only for a cross section, we use a regression discontinuity design (RD), with the banks per person as the running variable:[24]

$$Y_d = \beta RD_d + f(Banks\ per\ cap_d) + \epsilon_d \quad for\ d \in \{-D, D\}\ , \tag{2}$$

where, Y_{dt} is an outcome of interest in district d, and $RD_d = 1$ for underbanked districts: those that were above the cutoff and received the bank expansion scheme. We would expect these districts to be less severely affected by demonetization. $f(Banks\ per\ cap_d)$ is a flexible polynomial on either side of the cutoff. In practice, we identify the optimal bandwidths $\{-D, D\}$ as suggested by the procedures in Calonico et al. (2014), and show robustness across a wide range of bandwidths.

4.1 Compliance with the 2005 Bank Branch Expansion Policy

We first confirm the results established by earlier work (Young, 2017; Cramer, 2020), and show that the 2005 bank branch expansion policy was properly enforced and that it had high rates of compliance. We also verify that the difference around the cutoff clearly persisted through to 2016, when demonetization was implemented.

[23] We explicitly explore electoral effects of the bank branch expansion policy later in this paper.

[24] We primarily rely on this specification when using data from the voter surveys. In this case, using data on covariates from the same surveys, we also verify that there is indeed continuity around the RD cutoff.

Panel (a) of Figure 1 shows a strong first stage. It demonstrates that the bank expansion policy was well enforced: the probability of being classified by the RBI as an 'underbanked district' jumps discontinuously at the announced cutoff, namely, the national average number of persons per bank branch. Panel (b) shows the McCrary (2008) density test, wherein we do not find any evidence of manipulation around the cutoff, and a large density of districts around the cutoff to aid estimation. Table 1 provides the corresponding point estimates around the cutoff, which are both statistically and economically significant. The probability of being classified as an underbanked district jumps by about 97 percentage points at the cutoff, and this led to a growth in bank branches.

The graphs in Figure 2 show the effect of having been given underbanked status (and therefore more banks), on private-sector bank branches and growth, in the pre bank-expansion policy period (2002-2005), as well as in the post bank-expansion period (2006-2010). Panel (a) shows a clear increase in the aggregate district level data on the number of newly opened branches between 2006 and 2010. Panel (b) uses the Reserve Bank of India's (RBI) 2016 Master Office File (MOF) at the bank-branch level, where we code the year of establishment for each branch, and finds a similar pattern. Panel (c) shows a substantial growth in branches at the RD cutoff between 2006-10 and the year before the policy started (2005). Panel (d) shows a similar growth relative to the 2000-2005 average. Panels (e) and (f) show a lack of a discontinuity in the pre-treatment (2002-5) period.

Consistent with Young (2017), we find that the bank expansion policy did indeed lead to differential bank branch growth only *after* the bank expansion policy was instituted in 2005, and not before. Table 2 shows that the number of new branches (built between 2006-10) were higher (Panel a), even as there was no discernible discontinuity in the number of branches built between 2001-05 (Panel c). Table 3 shows how the policy significantly increased the number of accounts, and outstanding credit in the districts that received the banking policy.

We also verify that differences in bank branches and outstanding credit do indeed persist in the years and months leading up to the 2016 demonetization. First, we can see in Figure A.1 that there was a significant increase in new branches across the country after 2005. We verify that this increase around the 2005 policy cutoff, was strongly visible in 2015, as illustrated in Table A.1. Table A.3 further adds to this evidence using data from the CSDS voter surveys, where respondents were asked (i) whether they had a bank or post office account, and (ii) whether they had a bank or credit card. Both of these are significantly higher around the RD cut-off.[25]

5 Results

5.1 Economic Consequences of Demonetization

While recent papers have already provided evidence for worse economic outcomes in the months following demonetization (for instance, Chodorow-Reich et al. (2019) among others, as discussed in Section 2), the sample of districts and identification strategy in that work are different from ours. We therefore begin our analysis by first verifying that there had indeed been a more adverse

[25]While it is certainly plausible that the CSDS indicators of banking access increased differentially around the cut-off *after* demonetization, the fact that the increase was asymmetric around the RD cutoff illustrates the lasting impact of the 2005 bank expansion policy on financial access.

impact on local economic activity in areas with more severe demonetization, as defined by the bank branch expansion cutoff. In particular, we look at differential impacts on nightlights for districts with more/less severe exposure to demonetization. Consistent with the findings on nightlights in Chodorow-Reich et al. (2019), but using our differences-in-discontinuity design, we find that this is indeed the case, as shown in Table 4. Places that received fewer banks were likely to see a 9 percent fall in nighttime lights in the months following demonetization. Combining these results with the estimates in Table 2 on the difference in bank branches on either side of the bank expansion cutoff, we calculate that a 10% decrease in the number of bank branches was associated with a 1.7% decrease in nighttime lights following demonetization. Furthermore, Henderson et al. (2012) and Chodorow-Reich et al. (2019) suggest using an elasticity of 0.3 to translate nighttime lights to GDP, which implies a 0.5% fall in GDP for a 10% reduction in the number of bank branches.

5.2 Impact of Demonetization on Citizens' Views

We start our analysis of voters' responses to demonetization by looking at data from the CSDS voter surveys. Respondents were asked whether demonetization was (a) the right move, (b) the right move but poorly implemented, or (c) was the wrong move. Just under half of all respondents reported saying that demonetization was indeed the right move. Yet, we find that regions with more banks were more likely to have respondents that thought it was the right move, and were less likely to have respondents who thought it was badly implemented. These results are reported in Table 5, and uses the RD specification.[26]

Figure A.2 provides a summary of the strongest correlates of the support for demonetization and the Prime Minister, using the CSDS survey data. Some clear patterns emerge. First, those who are Hindu are more likely to support the pro-Hindu prime minister ("satisfied with Modi") and his policy ("demonetization was the right move"). Indeed, those who think eating beef should be banned (a policy championed by the BJP and seen to cater to the Hindus), are again more like to think demonetization was the right move, and are more satisfied with the BJP Prime Minister.

Furthermore, we find that "being religious" is predictive of support for demonetization. The Prime Minister often promoted demonetization on 'moral' grounds, as the primary stated goal was to address issues of corruption and counterfeit currency. We capture this notion with a "religiosity" index: an index (the first principal component) of the religious activities that the respondent reports doing.[27] In the same specification, we also control for the respondent's views on anti-minority legislation: in particular, their support for a beef ban, and whether they thought the Prime Minister should have taken action for recent increases in cow lynchings.

We also find that poorer and less educated individuals were less likely to say demonetization was the right policy, and that being in the formal sector was not related to support for the policy.

[26] We do not use a difference-in-discontinuity specification when using data from the voter survey since this data was only collected at one point in time, in May 2017. Given that, especially in the context of India, various other factors, such as caste, religion, or other forms of identity of politicians and voters may impact voting patterns (Neggers, 2018), we test whether these identities vary discontinuously at the bank policy cutoff. Figure A.3 demonstrates continuity in these characteristics around the cutoff, giving us confidence that these covariates are not driving our results.

[27] To the extent that "being religious" and morality are correlated, it is interesting but unsurprising that being religious is a strong correlate of support for demonetization (Norenzayan et al., 2016; Scheve and Stasavage, 2006; McCleary and Barro, 2006).

We next turn to our regression discontinuity estimates, to investigate whether respondents who faced less severe demonetization were more likely to be in favor of it. In Figure 4, we show that regions with less severe demonetization had respondents did indeed have a more favorable view toward demonetization. Individuals in regions with more banks were more likely to think demonetization was the right move, and less likely to think it had been badly implemented.

5.3 Effects on Elections

We now investigate whether citizens' views on demonetization translated to effects on actual voting patterns. We do this by studying the electoral effects of the policy for the two main political entities: (i) the ruling party, namely, the BJP, (ii) and the ruling coalition, also known as the NDA. We look at whether or not either of these two groups won the constituency, their vote share as a fraction of all votes tallied, and their vote share as a fraction of the votes between the ruling coalition (NDA) and the main opposition (UPA). As such, the gain in the vote share for this last measure also (approximately) reflects the loss in vote share for the UPA. Indeed, we verify this by looking at the vote shares of parties in the opposition.

Table A.4 provides the estimate for the difference at the cutoff in the vote shares, and the probability of winning in elections that were held *before* the demonetization policy of 2016.[28] We find no detectable effects of the banking policy in the pre-period. One may expect that voters reward the Congress, or their allies (the UPA) for the banking policy, but we fail to document such evidence. One possible reason is that the benefits of the banking policy were likely to be gradual, and as a result, less salient, unlike the sudden demonetization change. Indeed, the importance of salience, and who gets credit for different policies is found to be of paramount importance in other parts of the literature (Guiteras and Mobarak, 2014; Ferraz and Finan, 2011). Similarly, we find it interesting that a slow, less salient policy did not reap electoral rewards for the party. We explore effects on the vote shares after demonetization in Table A.5, estimating regression specification 2, the simple RD. The vote shares for the ruling coalition are higher in regions that have more banks.

In Table 6, we present the results from our preferred specification, that is, the difference-in-discontinuities estimation by leveraging the panel dimension of the data, and estimating equation 1. We interpret these coefficients as the gain in vote shares over the previous election in the same constituency, around the cutoff. Once again, it is evident that in regions that had a discontinuously higher number of banks, bank accounts, and credit, the vote shares and likelihood of winning for the ruling party were higher post demonetization. Vote shares for the ruling party are higher by at least 4.76 percentage points following demonetization, in areas where demonetization was less severe.[29] Together with the results in Table 2, this suggests that a 10% decrease in the number of bank branches was associated with a 0.9 percentage point decrease in vote shares for the ruling party. In Table 7, we estimate the likelihood of winning the constituency by party-affiliation, and consistent with the results on vote shares, we see a meaningful increase in likelihood of winning.

[28] These are elections held in 2005-2016, but before November 8, 2016.

[29] In Table A.7 we explore the effects on the national (federal) elections in 2019. While the magnitudes are economically meaningful, we cannot statistically rule out a zero effect at the RD cutoff. If indeed there was no consequence at the federal elections, it may be because between 2016 and 2019 other issues became more salient.

In Figure 6, we perform an RD-event study analysis to explore trends leading up to the demonetization event, starting in 1999. We restrict our sample to bandwidths around the cutoff, and then show the year-by-year effects leading up to, and following demonetization. The banking expansion started in 2005, but there were no detectable changes at the discontinuity, either in vote shares or the likelihood of winning the constituency. Following demonetization, however, the regression discontinuity coefficient rises substantially, showing that the ruling party enjoyed relatively higher vote shares in areas with more banks (or relatively lower vote shares in areas with fewer banks).

5.4 Robustness Checks

We conduct a number of robustness checks to address potential concerns with our empirical framework. First, a crucial assumption in our difference-in-discontinuities identification strategy is that, there were no differential trends around the branch-expansion cut-off prior to demonetization. Figure 6 shows the lack of pre-trends leading up the policy. In Table A.9, we run the difference-in-discontinuities specification (equation 1), for different *placebo* years as cutoffs (2012, 2013, 2014, 2015), all the while excluding data post the 2016 demonetization. We see no detectable discontinuities in vote shares for any of the placebo year cutoffs.

Another concern about the difference-in-discontinuity approach might be the choice of bandwidths. Since a method for optimal bandwidth selection does not currently exist for the difference-in-discontinuities approach, we redo our analysis for a range of bandwidths. In particular, we vary the bandwidth between the values of 0.2 banks per million people to 1.5 banks per million people around the cutoff (where the maximum value of the running variable is 2 banks per million people). The regression coefficients from this exercise are shown in Figure A.4, and we see that our estimates are robust for a range of bandwidths.

Next, we address the potential consequences of how winning probabilities are defined. Recall that in defining our outcomes of interest, whenever a party did not field a candidate, we coded such cases as missing values. However, one could argue that whether a party fields a candidate or not is endogeneous.[30] To address this, in Appendix Table A.10 we re-do our main difference in discontinuity analysis including constituencies where the parties did not field any candidate at all. These are now coded as zeros instead of missing, and our results remain similar.

Finally, the national average is likely to be an unmanipulated cutoff, and we are unaware of other policies that discontinuously vary at the cutoff. For instance, policies like the introduction of the Goods and Services Tax (GST) were implemented across the whole country in a uniform fashion.[31]

6 Drivers of the Electoral Effects

We show that demonetization led to relatively lower vote shares for the ruling party in areas with fewer banks. However, these average effects may hide substantial heterogeneity in electoral

[30] Indeed, Fisman et al. (2018) show that soon after demonetization, opposition party candidates were less likely to run for elections, and argue that this may have been the result of having less access to cash during political campaigns, relative to politicians affiliated with the ruling party. As such, their results suggest that who runs for the election in a constituency may also be affected by access to cash.

[31] The GST, implemented in 2017, was a tax reform that made disparate indirect tax schemes uniform across states.

responses that may shed light on potential mechanisms and drivers of our results. We now try to ascertain whether and why certain voters may have responded to the policy, and which types of voters are unlikely to react despite the economic hardships created by the policy.

6.1 Political Strongholds

There may be a number of reasons for why voters in political strongholds (of the ruling party) may be less responsive electorally. We discuss these below, but as a first step, we investigate whether there were indeed any heterogeneous effects for constituencies that are BJP or its coalition (NDA) strongholds.[32] Tables 8 and 9 provide the results for the BJP's and NDA's electoral outcomes, respectively. We find that voters in BJP or NDA strongholds were unlikely to change their voting behavior when faced with more severe demonetization.

Why were voters in stronghold areas unlikely to change their voting behavior? We explore a few possible reasons that shed light on the underlying electoral dynamics in ways that may resonate with other contexts as well.

Targeted Economic Relief: The ruling party may have targeted economic relief to less-banked areas, mitigating the adverse economic consequences of demonetization. This would reflect the work on patronage politics in India (Asher and Novosad, 2017; Mahadevan, 2020). Indeed, it is plausible that there exists some form of favoritism, wherein local politicians received additional resources to address economic hardships from the party in power.

We are unable to find evidence in support of this. Indeed, Table A.12 shows that the economic consequences of demonetization severity were similar in stronghold and non-stronghold areas. We further find that people's opinion of demonetization fell in a similar manner at the RD cutoff, across both stronghold and non-stronghold areas (Table A.11). As such, regardless of whether or not they resided in strongholds, individuals in districts with fewer banks were adversely affected and had a less favorable view of demonetization. Yet, stronghold areas were not electorally responsive to the policy at the RD cutoff.

Campaigning and Vote buying: A somewhat similar reason for these patterns could perhaps be explained by ease of campaigning or vote buying in areas that are strongholds. Perhaps stronghold areas have established incumbent candidates that can oversee a strong campaign and vote buying that mitigates the adverse consequences of demonetization. For instance, parties may find it easier to provide resources to constituents in their strongholds (Keefer and Khemani, 2009). To drive these patterns, this strong campaign would need to be more heavily targeted to less banked districts. We continue to find that opinions on demonetization are less favorable in under-banked stronghold districts (Table A.11). As such, these campaigns or vote buying behavior are unlikely to have targeted voters' views on the topic. While we cannot conclusively rule out this channel, the evidence of no differential economic consequences in ruling party strongholds suggest that this type of favoritism is unlikely to be prominent in our context.

Voter Knowledge and Awareness: Strongholds may have better targeted messaging to the

[32] We define a "stronghold" constituency by first counting the number of times the BJP (or NDA) won an election in the four elections that took place before demonetization. We then assign a constituency "stronghold" status for the BJP (or NDA) if it lies above the median constituency with respect to electoral wins for the BJP (or NDA).

voter base on the merits and demerits of the policy, and who was to be responsible for the consequences. Yet, the policy was highly salient, and the Prime Minister, in his TV addresses, regularly made it clear that he was the main architect of policy.[33] Importantly, the evidence in Table A.11 that at the RD cutoff, voters were less likely to view demonetization in a favorable light, even in stronghold areas, suggests that messaging was unlikely to be the underlying reason.

Issue Bundling and Multi-policy Platforms: The negative economic consequences of demonetization as shown by us and others (Chodorow-Reich et al., 2019), makes it reasonable to expect impacts on voter behavior, and we saw evidence for this in Section 5. Yet, theoretically, it is unclear ex ante whether this policy-induced shock to the economy would lead to any electoral effects in stronghold areas, where voters are much more strongly aligned with the party (the BJP) on a number of other issues. The intuition for this is simple: in the absence of direct democracy for specific issues, as is the case in many democracies, including India, citizens only have one vote, and consider a *bundle* of issues when casting their vote. It is therefore not entirely obvious that we would detect a corresponding negative impact on the ruling party's electoral outcomes.[34]

Theoretically, issue bundling in voting behavior suggests that what we *should* expect to see is a muted effect (if any) on voting behavior for voters whose preferences are strongly aligned with the ruling party (Besley and Coate, 2008; Iversen and Goplerud, 2018). We posit that this phenomenon may be one of the contributors that drives the lack of voter response in stronghold areas. This is consistent with the idea that in democracies, where voters typically get one opportunity to vote, and where the policy space is multi-dimensional, issues get bundled during elections. Voters that align more closely with a particular politician (or party), despite being negatively impacted by a single policy implemented by that politician (or party), may still vote for them.

6.2 Incumbency Effects and Turnout

Even in cases such as demonetization, where local incumbent politicians had very little to do with the introduction or implementation of the policy, voters may have attributed credit or blame of the policy to a local incumbent for a number reasons. These may include a lack of awareness of who was responsible for policy implementation (Guiteras and Mobarak, 2014).[35] In contrast to democracies such as the United States, incumbents in India are typically at a significant disadvantage in subsequent elections (Uppal, 2009). Table A.8 provides the difference-in-discontinuity results for vote shares of candidates from incumbent parties, where the coefficients are both statistically and economically insignificant.

A final driver of our electoral results may be differential turnout in areas with more severe demonetization.[36] We explore this by using data on voter turnout and employing the difference-in-

[33] If voters had been unaware of this, one possibility might be that they thought their incumbent leader was responsible, we may have seen effects differentially on incumbents; which we do not (Table A.8).

[34] We already saw in A.2 that it was not the case that there is perfect alignment between support for demonetization, and support for the policy maker (the Prime Minister).

[35] There is also a literature that argues that voters (or other actors) may irrationally attribute a random shock or event to an incumbent. For instance, Bertrand and Mullainathan (2001) show that shareholders at corporations in the United States reward CEO's for aggregate economic booms that were not related to the company's performance.

[36] In particular, at least some part of the electoral effects may be driven by those more severely affected, turning out more to vote. Furthermore, these effects may be exacerbated by social pressures (Dellavigna et al., 2016).

discontinuities specification. The results in Table A.6 show no evidence to support the hypothesis that the policy made voters who were more severely impacted, any more likely to vote.

7 Discussion

We analyze the electoral consequences of India's 2016 demonetization policy. As the policy was implemented on the same day throughout the country, it is challenging to isolate the effects of demonetization from other secular political trends. We overcome these challenges by leveraging a discontinuity in the number of bank branches in districts across the country, that arose as a consequence of a bank licensing policy instituted in 2005 by the previous government in power. As the policy necessitated the exchange of old currency notes for new ones, a lack of access to bank branches led to greater difficulty in acquiring new notes. In this way, we derive variation in *severity* of the consequences of demonetization from the change in bank density around the cutoff.

We find that the bank licensing policy expanded the number of new branches, accounts, and credit limits. Interestingly, voters did not seem to reward the party that implemented the banking expansion, perhaps as the benefits were gradual and were less publicized. In contrast, voters seem to have responded to the salient demonetization policy. In regions that had discontinuously fewer banks, the ruling party had a discontinuously lower vote share and likelihood of winning the constituency. The two different policies differ on the salience and immediateness of the effects, and as such the difference in electoral responses between them is indicative of voter behavior.

The magnitudes of our effects are meaningful. In sum, a 10% increase in the number of new bank branches corresponded to a 0.9 percentage point increase in the vote share of the population that had access to banking safety nets when faced with such policy-induced economic adversities.

Yet, despite the negative economic consequences, ruling party stronghold areas were not responsive in voting behavior. We consider whether such stronghold areas were relatively protected, or were subject to stronger messaging in support of the policy. Interestingly, we find that stronghold areas faced similar economic adversities, and the policy was similarly unpopular in underbanked regions that had faced more severe demonetization. The fact that this lack of popularity did not lead to a reduction in votes, suggests that strong supporters of a particular party are unlikely to be swayed by one particular policy. Such voters may instead vote on either ideological grounds, or on a broader set of issues that align them with the ruling party.

The ruling party won several state elections in 2017, the year after demonetization. Without a causal analysis, we may be misled to think that voters did not respond to demonetization. Indeed, the policy was implemented by a popular Prime Minister, that appealed to voter morality to bear such costs in helping the nation fight corruption and terrorism, which were the stated goals of demonetization. In the absence of well-identified variation in banking safety nets, the media concluded that demonetization was not punished by the voter base (The Indian Express, 2017). We dispel this flawed view with stronger identification.

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Tables and Figures

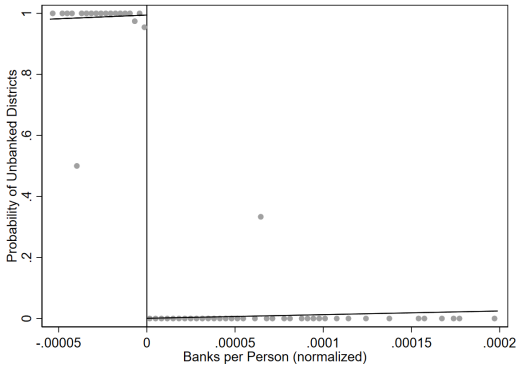
Table 1: Unbanked Status, and growth in branches at the RD cutoff

	P(Unbanked Status)			ΔLog(New Branches)
RD Cutoff	0.971***	0.968***	0.960***	1.757***
	(0.0179)	(0.0182)	(0.0369)	(0.553)
Bandwidth	[-2; 2]	[-2; 2]	[-1.3 ;1.3]	[-.6 ;.6]
Specification	Linear	Quadratic	MSE	MSE

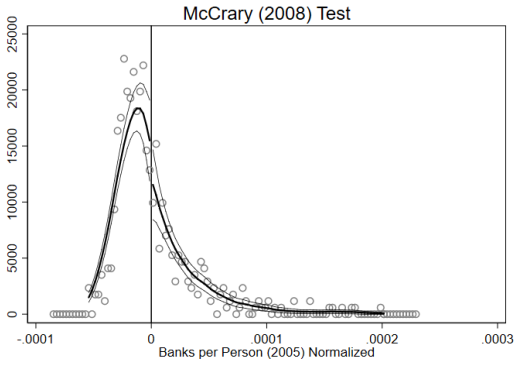
Notes: District level regressions in the cross section. The first three columns show the first stage where P(Unbanked Status) is the likelihood of receiving unbanked status when being above the cutoff. 'ΔLog(New Branches)' is the growth in branches – the difference between the total number of newly opened branches in the five years after receiving unbanked status and the five years before. Bandwidth in units of banks per hundred thousand people. 'Linear' and 'Quadratic' indicate functional form controls of the running variable. 'MSERD' uses the Calonico et al. (2014) optimal bandwidth selection and bias correction method that has one common mean square error-optimal bandwidth selector for the treatment effect estimator. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 1: First Stage and McCrary (2008) Test

Panel A. Compliance With Policy Rule



Panel B. District Density at RD cutoff



Notes: Graphs show the first stage (Panel A), and density of districts (Panel B) at the cutoff as in the McCrary (2008) Test.

Table 2: New and Old branches at the RD cutoff

Panel A		Log(New Branches)		
RD Estimate	0.584*	0.553	0.521**	0.541**
	(0.299)	(0.351)	(0.216)	(0.219)
Bandwidth	[-2 ;1.1]	[-1.5 ;.8]	[-2; 2]	[-2; 2]
Mean	1.807	1.778	1.667	1.667
Specification	MSE2	CER2	Linear	Quadratic
Panel B		ΔLog(Branches)		
RD Estimate	0.960**	1.275**	0.737**	0.729**
	(0.425)	(0.523)	(0.314)	(0.316)
Bandwidth	[-2.3 ;.7]	[-1.8 ;.6]	[-2; 2]	[-2; 2]
Mean	1.751	1.755	1.761	1.761
Specification	MSE2	CER2	Linear	Quadratic
Panel C		Log(Old Branches)		
RD Estimate	-0.141	-0.189	0.0403	0.0447
	(0.225)	(0.250)	(0.198)	(0.198)
Bandwidth	[-2.4 ;1.3]	[-1.9 ;1]	[-2; 2]	[-2; 2]
Mean	0.391	0.396	0.363	0.363
Specification	MSE2	CER2	Linear	Quadratic

Notes: District level regressions in the cross section. Log(New Branches) is the number of newly opened branches in the first five years after the policy (2006-2010). 'ΔLog(New Branches)' is the growth in branches – the difference between the total number of newly opened branches in the five years after receiving unbanked status and the five years before. Log(Old Branches) are the number of branches opened in the five years leading up to the policy (2001-2005). Bandwidth in units of banks per hundred thousand people. 'Linear' and 'Quadratic' indicate functional form controls of the running variable. 'MSE2' uses the Calonico et al. (2014) optimal bandwidth selection and bias correction method that allows for different mean square error-optimal bandwidths on either side of the cutoff, and 'CER2' allows for different coverage error rate-optimal bandwidths on either side of the cutoff. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Accounts and Credit at the RD cutoff in 2012

Panel A		Number of Accounts		
RD Estimate	12,299	15,005	16,545*	18,522**
	(8,986)	(11,172)	(9,035)	(9,197)
Bandwidth	[-1.3 ;1.6]	[-.9 ;1.1]	[-2; 2]	[-2; 2]
Mean	19844	22578	21589	21589
Specification	MSE2	CER2	Linear	Quadratic
Panel B		Total Credit Limit		
RD Estimate	886.9**	937.3**	1,002*	1,105*
	(377.4)	(432.6)	(580.4)	(591.2)
Bandwidth	[-.7 ;1.8]	[-.5 ;1.3]	[-2; 2]	[-2; 2]
Mean	658.03	766.321	938.6	938.6
Specification	MSE2	CER2	Linear	Quadratic
Panel C		Total Credit Outstanding		
RD Estimate	523.4**	519.3*	710.1*	761.4*
	(255.0)	(282.7)	(381.0)	(388.3)
Bandwidth	[-.8 ;1.5]	[-.5 ;1.1]	[-2; 2]	[-2; 2]
Mean	521.537	586.171	650.2	650.2
Specification	MSE2	CER2	Linear	Quadratic

Notes: District level regressions in the cross section. 'Number of Accounts' is the number of open bank accounts. 'Total Credit Limit' and 'Total Outstanding Credit' in 10 million Indian rupees (year 2012). 'Linear' and 'Quadratic' indicate functional form controls of the running variable. 'MSE2' uses the Calonico et al. (2014) optimal bandwidth selection and bias correction method that allows for different mean square error-optimal bandwidths on either side of the cutoff, and 'CER2' allows for different coverage error rate-optimal bandwidths on either side of the cutoff. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Economic Impact of Demonetization

	Log Nightlights	
Post×Banks	0.0977***	0.0895***
	(0.0202)	(0.0199)
Observations	4,591	4,839
R-squared	0.894	0.900
Mean DV	.0.537	0.576
BW	[-5; 5]	[-10; 10]

Notes: Difference-in-Discontinuities specification. Dependent variable is the logarithm of luminosity at the district-by-month level. Standard errors clustered at the district level. All specifications restrict the sample around the RD cutoff, and include district and year fixed effects *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Views on Demonetization

	Was right move		Right move but w/ bad prep.	
RD Estimate	0.0867	0.168**	-0.123*	-0.156**
	(0.0748)	(0.0728)	(0.0655)	(0.0606)
Observations	10,318	10,882	10,318	10,882
R-squared	0.018	0.011	0.015	0.013
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.452	0.458	0.318	0.317

Notes: Individual level regressions in 2017. Dependent variable is views on demonetization using household-level CSDS data. Standard errors are clustered at the assembly level. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person) *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Share of Votes: Difference-in-Discontinuities

	Vote Shares			
	BJP or ally		BJP	
Post×Banks	0.0973***	0.0990***	0.0485**	0.0476**
	(0.0234)	(0.0226)	(0.0209)	(0.0200)
Observations	10,633	11,220	9,021	9,465
R squared	0.662	0.660	0.520	0.515
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.319	0.319	0.267	0.267

Notes: Dependent variable is vote shares. Standard errors are clustered at the district level. Panel A and B are district level panel-based difference-in-discontinuities specifications, that include district and year fixed effects. Here, the post-period is 2017-18, and the pre periods include years 2009 to 2016. All specifications restrict the sample around the RD cutoff. The ruling party is BJP, ruling coalition is the NDA. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Probability of Winning: Difference-in-Discontinuities

	Prob(Winning)			
	BJP		BJP or Ally	
Post×Banks	0.134**	0.122**	0.146**	0.148***
	(0.0602)	(0.0564)	(0.0590)	(0.0552)
Observations	9,021	9,465	10,633	11,220
R squared	0.342	0.339	0.269	0.262
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.332	0.332	0.376	0.374

Notes: Dependent variable is the probability of winning the constituency. The sample includes all constituencies, even if the party did not field a candidate. Panel-based difference-in-discontinuities specifications, that include district and year fixed effects. Here, the post-period is 2017, and the pre periods include years 2009 to 2016. Standard errors are clustered at the district level. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person). The ruling party is BJP, ruling coalition is the NDA. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Ruling Party Vote Shares, by Political Strongholds

	BJP Vote Shares			
Post×Banks	0.137***	0.146***	0.123***	0.134***
	(0.0460)	(0.0447)	(0.0360)	(0.0356)
Post×Banks×NDA-Stronghold	-0.106**	-0.121***		
	(0.0436)	(0.0422)		
Post×Banks×BJP-Stronghold			-0.120***	-0.139***
			(0.0378)	(0.0372)
Observations	8,705	9,130	8,705	9,130
R-squared	0.667	0.665	0.669	0.667
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.265	0.266	0.265	0.266

Notes: Dependent variable is vote shares for the ruling party (BJP). The NDA is the ruling party alliance. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person). Standard errors are clustered at the district level. District level panel-based difference-in-discontinuities specifications, that include district and year fixed effects. $Post = 1$ only for the years after 2016. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

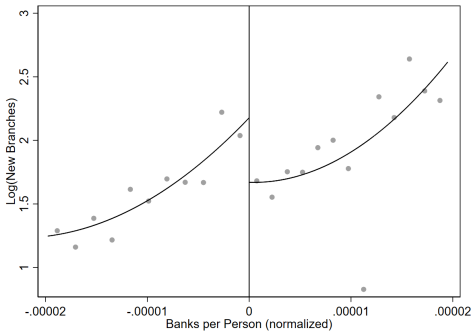
Table 9: Ruling Coalition Vote Shares, by Political Strongholds

	NDA Vote Shares			
Post×Banks	0.171***	0.176***	0.193***	0.205***
	(0.0420)	(0.0395)	(0.0324)	(0.0305)
Post×Banks×NDA-Stronghold	-0.0953**	-0.101**		
	(0.0418)	(0.0400)		
Post×Banks×BJP-Stronghold			-0.177***	-0.198***
			(0.0342)	(0.0326)
Observations	10,289	10,853	10,289	10,853
R-squared	0.519	0.515	0.524	0.521
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.318	0.318	0.318	0.318

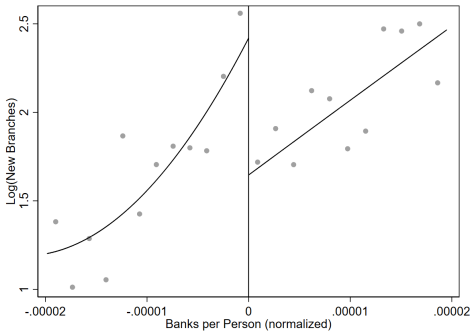
Notes: Dependent variable is ruling alliance (NDA) vote shares. The BJP is the ruling party. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person). Standard errors are clustered at the district level. District level panel-based difference-in-discontinuities specifications, that include district and year fixed effects. $Post = 1$ only for the years after 2016. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 2: Private Bank Branches at the RD Cutoff

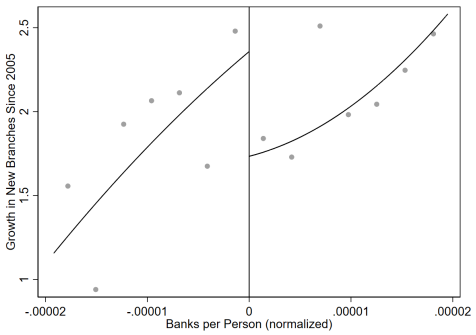
(a) New branches (2006-10): Aggregate



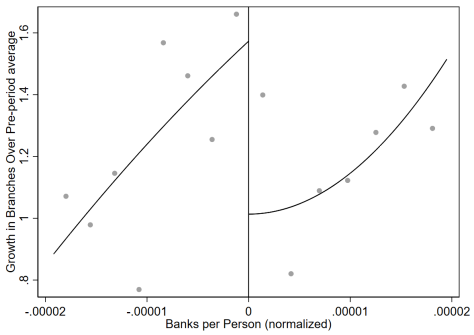
(b) New branches (2006-10): MOF Data



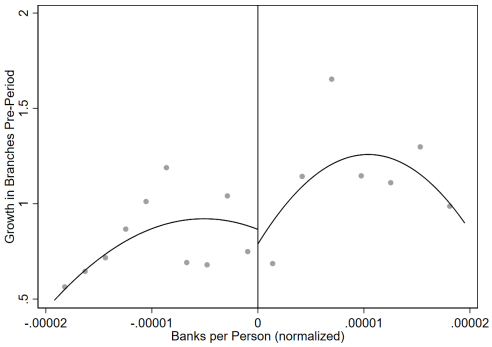
(c) Growth in branches since 2005



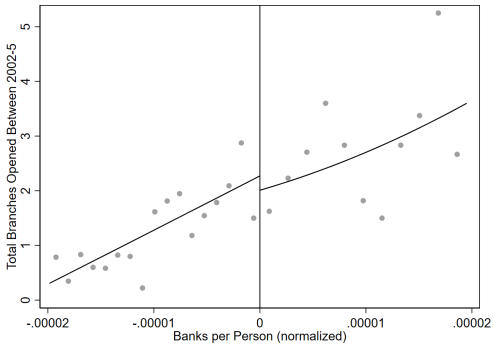
(d) Growth in branches since (2000-2005) average



(e) Old Branches Opened Between 2002-5



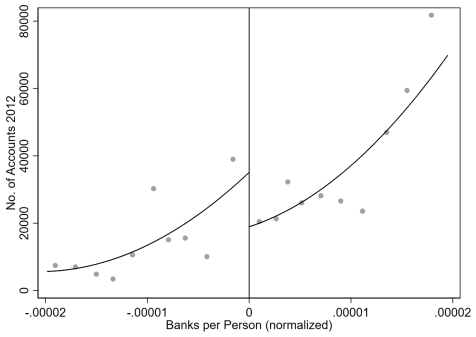
(f) Number of Branches Opened 2002-5



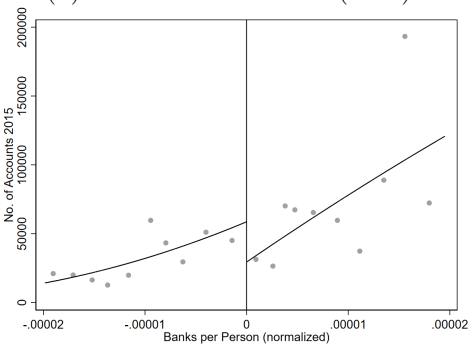
Graphs show the effect of unbanked status on private-sector bank branches and growth. Panel (a) uses the aggregate district level data on number of newly opened branches between 2006 and 2010. Panel (b) uses the Reserve Bank of India's (RBI) 2016 Master Office File (MOF) at the bank-branch level, and codes up the year of establishment for each branch. Panel (c) looks at the growth at the RD cutoff between the 2006-10 and the year before the policy started (2005). Panel (D) compares the new number of branches in years 2006-10 with newly opened branches in 2000-2005. Panel (e) and (f) show pre-treatment (2002-5) baseline tests using the RBI MOF.

Figure 3: Number of Accounts and Credit

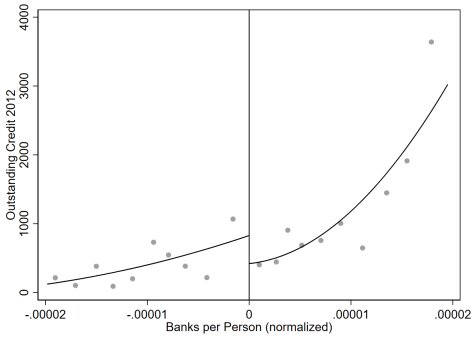
(a) Number of Accounts (2012)



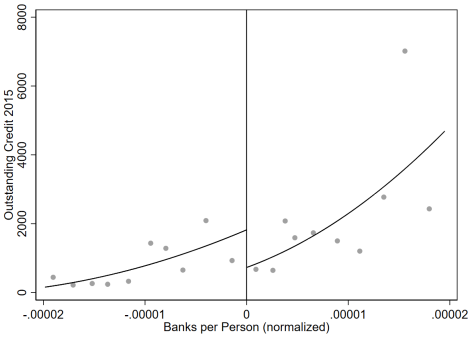
(b) Number of Accounts (2015)



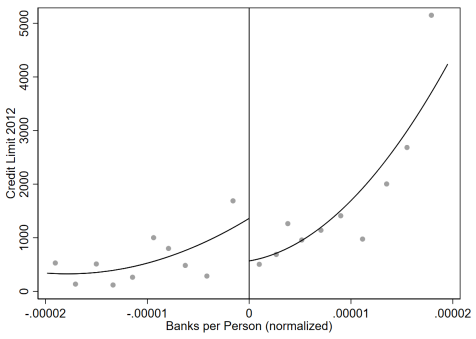
(c) Outstanding Credit (2012)



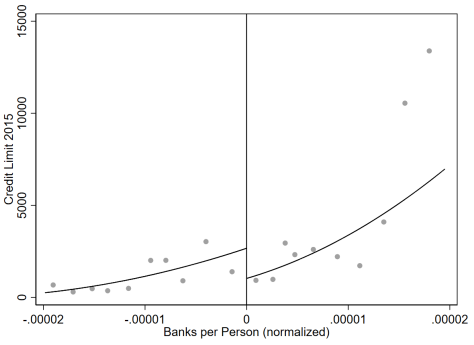
(d) Outstanding Credit (2015)



(e) Credit Limit (2012)



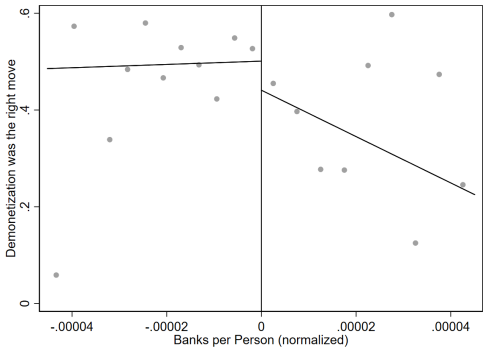
(f) Credit Limit (2015)



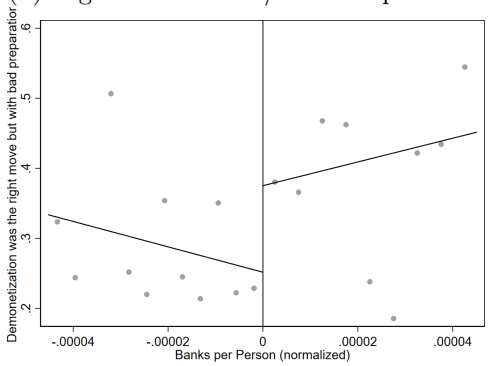
Graphs show the effect of unbanked status on number of accounts, credit limits and outstanding credit. Figure and Figure show the number of bank accounts in 2012 and 2015. Figure and Figure show the amount of outstanding credit (ten million rupees) in 2012 and 2015. Figure and show the total credit limit in districts (ten million rupees) in 2012 and 2015.

Figure 4: Bank Access and Views on Demonetization (Voter Survey Data)

(a) Demonetization was the Right Move



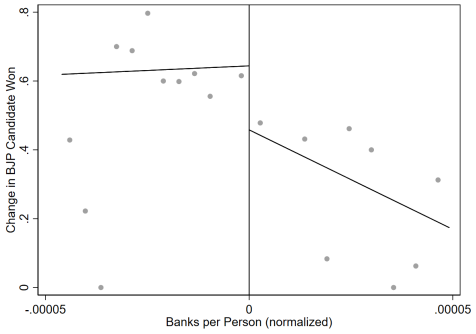
(b) Right Move but w/ Bad Preparation



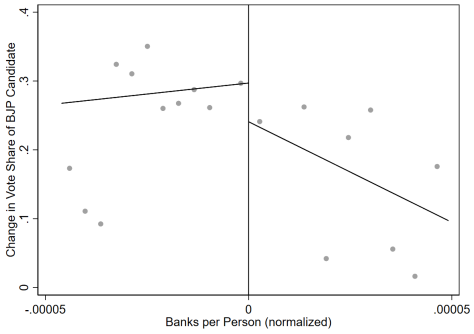
Graphs use household level CSDS and show the effect of unbanked status around the RD cutoff. Figure (a) is an indicator for whether or not the respondents thought demonetization was the right move. Figure (b) is an indicator for whether or not the respondent thought demonetization was the right move, but badly implemented.

Figure 5: Probability of Winning and Number of Votes

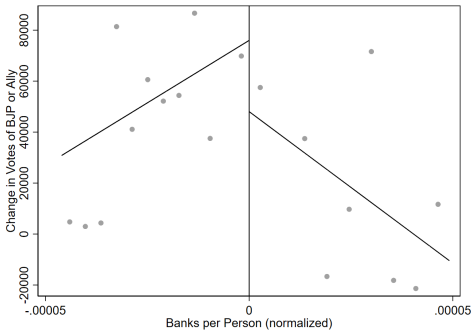
(a) Change in P(Ruling Party Victory)



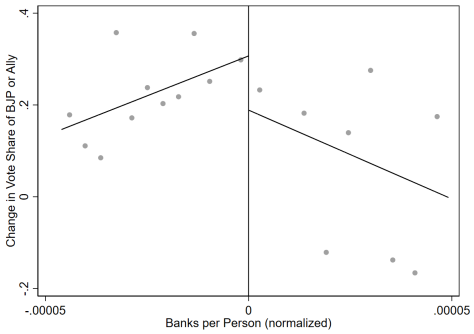
(b) Change in Vote Shares for Ruling Party



(c) Changes in Votes for Ruling Coalition



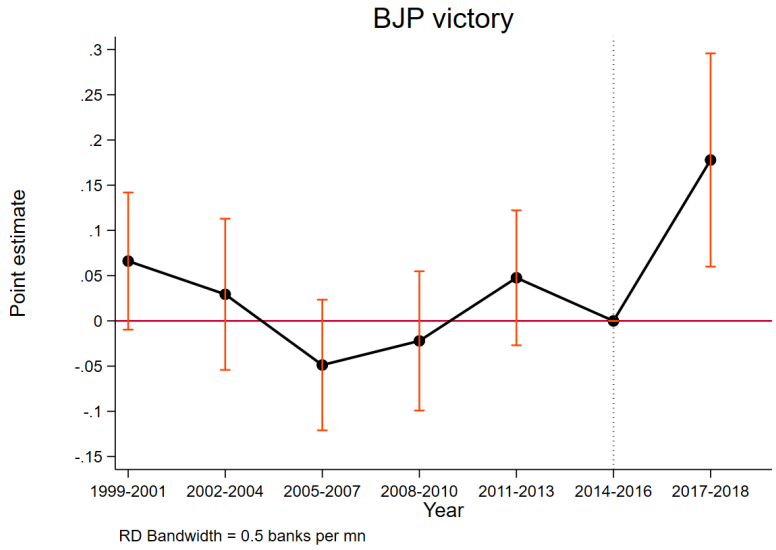
(d) Change in Vote Shares for Ruling Coalition



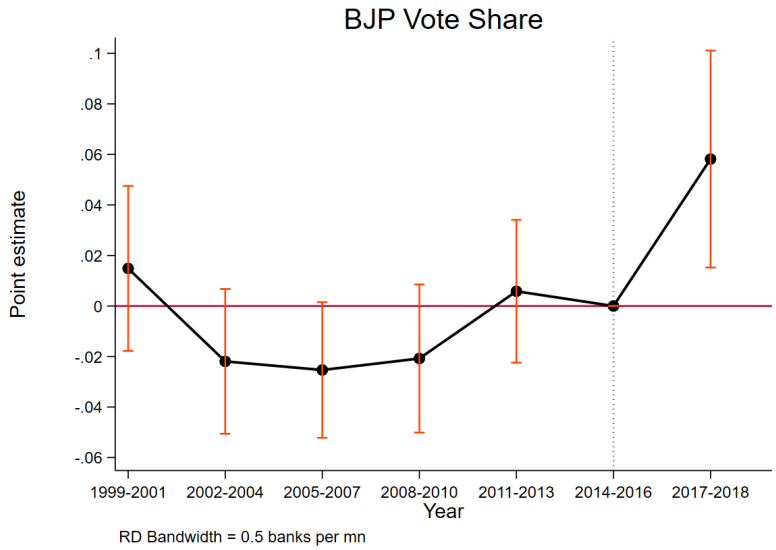
Graphs show the effect of unbanked status on change in vote shares, and pre-treatment baseline estimates. Figure (a) and Figure (b) show the change in vote shares for the ruling party and coalition respectively. Figure (c) shows the probability of winning, and Figure (d) shows the change in total votes.

Figure 6: Event Study Analysis of Elections at RD Cutoff

(a) P(Ruling Party Victory)



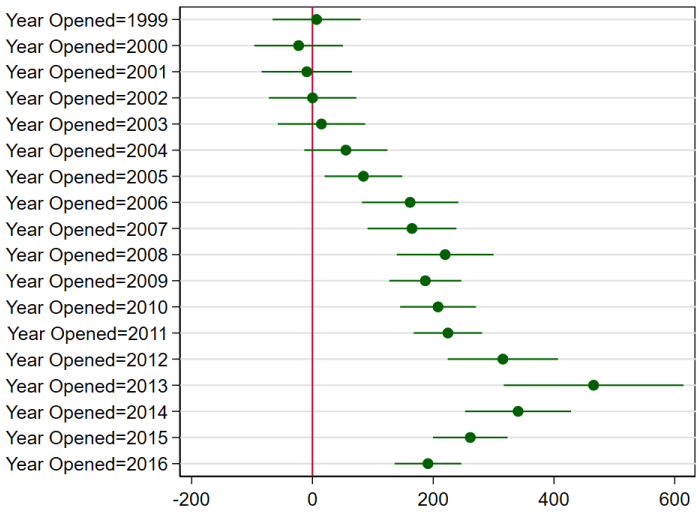
(b) Vote Shares for Ruling Party



Graphs show the effect of unbanked status (being above the RD cutoff) on vote shares, and probability of victory for the BJP relative to the period of demonetization. We club periods into three-year bins (inclusive) given the infrequency of state elections.

ONLINE APPENDIX

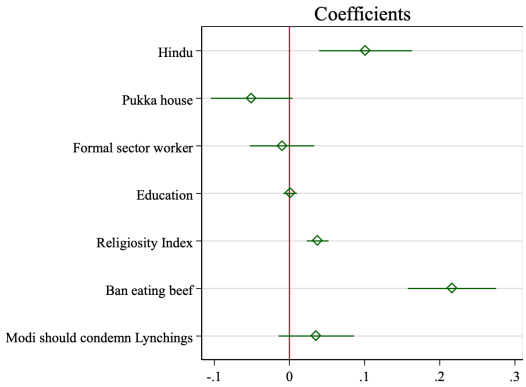
Figure A.1: New Branches over Time



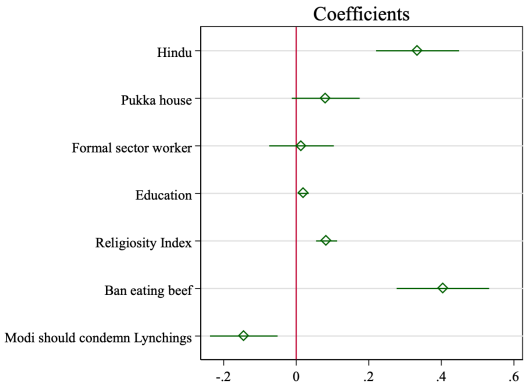
This figure shows the coefficient on the the number of new branches opened each year relative to 1998. The policy was implemented in 2005.

Figure A.2: Correlates of Support For:

(a) Demonetization



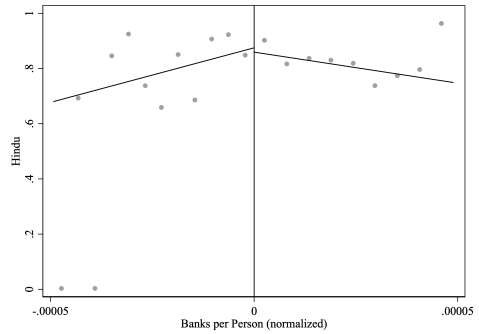
(b) Prime Minister



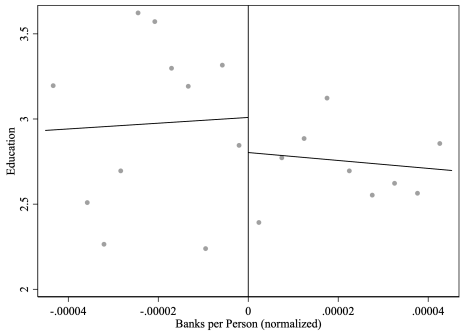
Panel (a) plots the coefficients for the correlates of support for the demonetization policy, while Panel (b) does the same for support for the Prime Minister, Mr Modi. The data comes from the CSDS voter surveys.

Figure A.3: Continuity in Demographics at the RD Cutoff (Voter Survey Data)

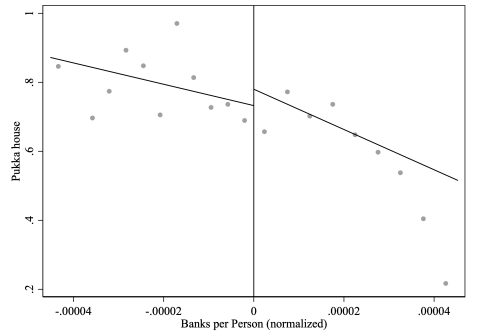
(a) Hindu



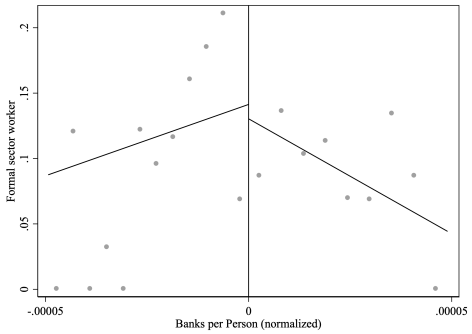
(b) Education



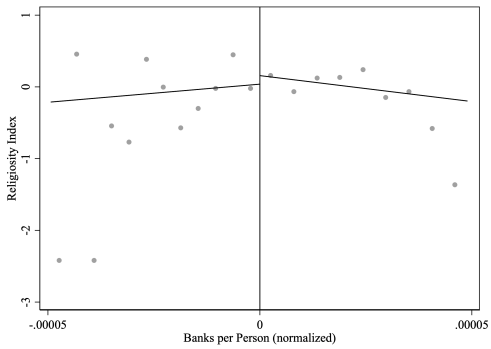
(c) Pukka House



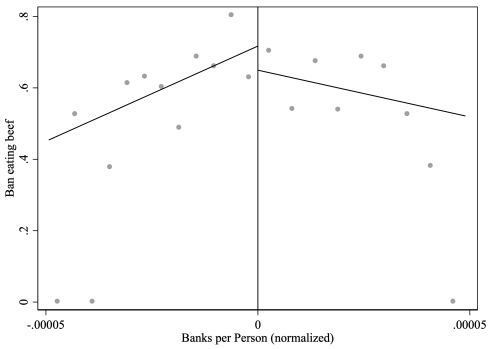
(d) Formal Sector Worker



(e) Religiosity

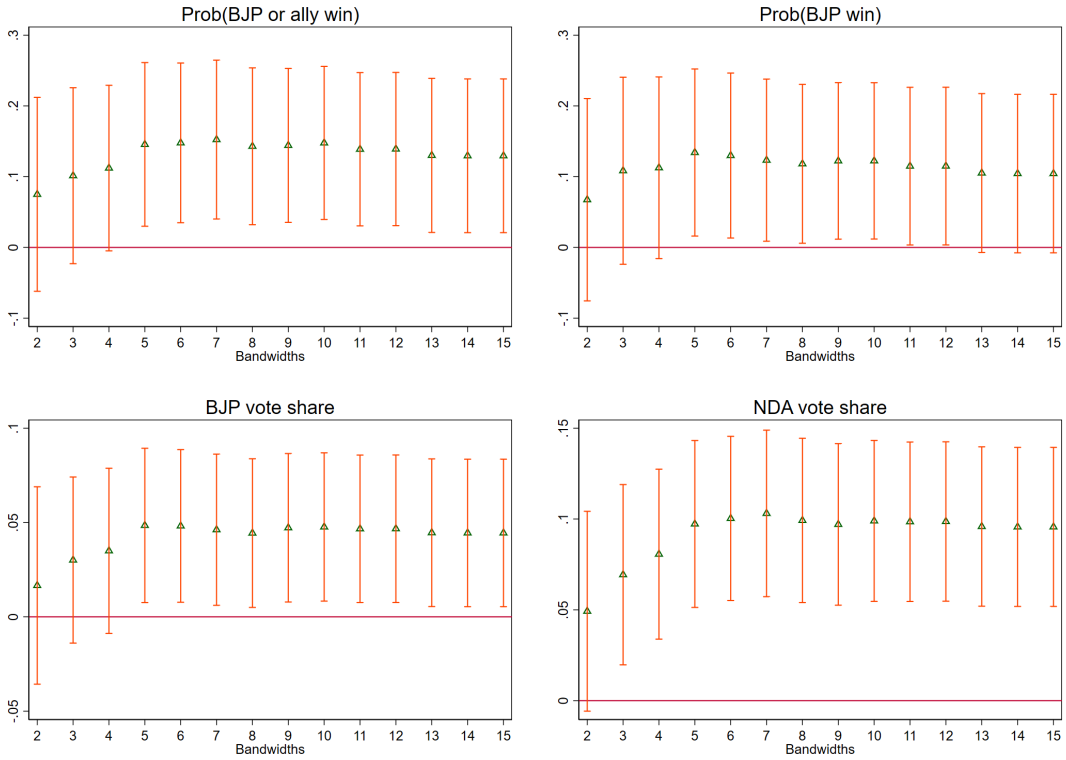


(f) Supports Beef Ban



Graphs show the relationship between unbanked status and individual characteristics using the CSDS Mood of the Nation survey.

Figure A.4: Sensitivity to Bandwidths



Graphs show the sensitivity of our main results to alternative bandwidths around the RD cutoff. We vary the bandwidth between the values of 2 banks per 100000 people to 15 banks per 100000 people around the cutoff. The maximum value of the running variable is 19.8 banks per 100000 people.

Table A.1: Accounts and Credit at the RD cutoff in 2015

Panel A		Number of Accounts		
RD Estimate	22,090	31,886*	25,629	26,485
	(16,627)	(19,311)	(19,946)	(20,353)
Bandwidth	[-1.2 ;1.3]	[-.9 ;1]	[-2; 2]	[-2; 2]
Mean	44469.285	48547.612	44139	44139
Specification	MSE2	CER2	Linear	Quadratic
Panel B		Total Credit Limit		
RD Estimate	1,894**	1,879*	3,860**	4,607***
	(907.6)	(983.8)	(1,736)	(1,757)
Bandwidth	[-.6 ;1.3]	[-.4 ;1]	[-2; 2]	[-2; 2]
Mean	1628.501	1876.49	2111	2111
Specification	MSE2	CER2	Linear	Quadratic
Panel C		Total Credit Outstanding		
RD Estimate	1,022*	1,117*	1,209*	1,275*
	(597.4)	(616.9)	(715.5)	(729.9)
Bandwidth	[-1 ;1.3]	[-.7 ;1]	[-2; 2]	[-2; 2]
Mean	1164	1344.347	1216	1216
Specification	MSE2	CER2	Linear	Quadratic

Notes: District level regressions in the cross section. 'Number of Accounts' is the number of open bank accounts. 'Total Credit Limit' and 'Total Outstanding Credit' in 10 million Indian rupees (year 2012). 'Linear' and 'Quadratic' indicate functional form controls of the running variable. 'MSE2' uses the Calonico et al. (2014) optimal bandwidth selection and bias correction method that allows for different mean square error-optimal bandwidths on either side of the cutoff, and 'CER2' allows for different coverage error rate-optimal bandwidths on either side of the cutoff. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.2: MOF Data: Branches at the RD cutoff

Panel A		Log(New Branches)		
RD Estimate	0.712**	0.762**	0.574***	0.746**
	(0.279)	(0.311)	(0.214)	(0.352)
Bandwidth	[-1.9 ;1.1]	[-1.4 ;.8]	[-2; 2]	[-2; 2]
Mean	1.941	1.932	1.725	1.725
Specification	MSE2	CER2	Linear	Quadratic
Panel B		ΔLog(New Branches)		
RD Estimate	0.576	0.932**	0.666**	0.761
	(0.382)	(0.442)	(0.331)	(0.537)
Bandwidth	[-3.4 ;1.1]	[-2.6 ;.9]	[-2; 2]	[-2; 2]
Mean	2.021	2.042	1.912	1.912
Specification	MSE2	CER2	Linear	Quadratic
Panel C		Log(Old Branches)		
RD Estimate	0.0560	-0.0546	0.109	-0.170
	(0.201)	(0.207)	(0.193)	(0.312)
Bandwidth	[-4.3 ;1]	[-3.4 ;.8]	[-2; 2]	[-2; 2]
Mean	.436	.392	0.349	0.349
Specification	MSE2	CER2	Linear	Quadratic
Panel D		ΔLog(Old Branches)		
RD Estimate	-0.0405	0.0933	-0.0348	0.0540
	(0.377)	(0.459)	(0.235)	(0.393)
Bandwidth	[-3.3 ;1]	[-2.5 ;.8]	[-2; 2]	[-2; 2]
Mean	1.03	.999	0.967	0.967
Specification	MSE2	CER2	Linear	Quadratic

Notes: District level regressions in the cross section using Master Office File database. Log(New Branches) is number of opened branches in the first five years after the policy. 'ΔLog(New Branches)' is the growth in branches before / after policy. Log(Old Branches) are the number of branches opened in the five years before policy. ΔLog(Old Branches) is growth in branches pre-policy. Bandwidth in units of banks per hundred thousand people. 'Linear' and 'Quadratic' indicate functional form controls of the running variable. 'MSE2' and 'CER2' use the Calonico et al. (2014) optimal bandwidth selection and bias correction methods. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.3: Household Survey: Bank Accounts and Credit Access

	Bank or Post Office Account		Debit or Credit Card	
RD Estimate	0.0853***	0.00271	0.212***	0.304***
	(0.0208)	(0.0274)	(0.0472)	(0.0655)
Observations	1970	1385	2504	1715
BW Type	MSE2	CER2	MSE2	CER2
Mean DV	.883	.862	0.553	.545
BW	[-1 ;.5]	[-.6 ;.3]	[-1.6 ;.5]	[-1 ;.3]
Robust p-value	0.002	0.909	0.000	0.000

Notes: Household level regressions in the cross section using CSDS data. Log(New Branches) is number of opened branches in the first five years after the policy. Respondents are asked whether or not they have a bank or post-office account, and whether or not they have a debit or credit card. Bandwidth in units of banks per hundred thousand people. 'MSE2' and 'CER2' use the Calonico et al. (2014) optimal bandwidth selection and bias correction methods. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.4: Electoral Effects of Bank Policy Pre-Demonetization

	Prob(Winning)			
	Congress		UPA	
Received Banks	-0.0880	0.00739	-0.0372	-0.0155
	(0.0645)	(0.0416)	(0.0449)	(0.0380)
BW Type	MSE1	MSE2	MSE1	MSE2
Robust p-value	0.230	0.891	0.297	0.559
BW	[-.7 ;.7]	[-2.6 ;1]	[-1.2 ;1.2]	[-2.2 ;1.4]
	Vote Shares			
	Congress		UPA	
Received Banks	-0.0315	0.0215	0.00726	0.0158
	(0.0214)	(0.0171)	(0.0219)	(0.0153)
BW Type	MSE1	MSE2	MSE1	MSE2
Robust p-value	0.120	0.320	0.993	0.581
BW	[-.6 ;.6]	[-1.5 ;.6]	[-.7 ;.7]	[-1.3 ;1]

Notes: Dependent variable in Panel A is vote shares, and in Panel B is probability of winning. Sample restricted to the years 2005 to 2016. Standard errors are clustered at the district level. All specifications (Panels A through B) restrict the sample around the RD cutoff, and control for the running variable (banks per person) with a flexible quadratic slope around the cutoff. The ruling coalition is the NDA. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.5: Vote shares post 2016

	Regression Discontinuity			
Vote shares	BJP or ally		BJP	
Received Banks	0.113***	0.103***	0.101***	0.0930**
	(0.0334)	(0.0285)	(0.0307)	(0.0397)
BW Type	MSE1	MSE2	MSE1	MSE2
Robust p-value	0.003	0.002	0.001	0.010
BW	[-.7 ;.7]	[-2 ;.5]	[-.6 ;.6]	[-1.7 ;.4]

Notes: Dependent variable is vote shares. Standard errors are clustered at the district level. Panel A and B are district level regressions in the cross section for the year 2017-18. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person) with a flexible quadratic slope around the cutoff. The ruling party is BJP, ruling coalition is the NDA. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.6: Impact on Turnout: Difference-in-Discontinuity Results

	Log(Turnout)	
Post×Banks	0.0160	0.0232
	(0.0192)	(0.0216)
Bandwidth	[-5; 5]	[-10; 10]
Mean	11.64	11.61

Notes: Dependent variable is the logarithm of voter turnout in a constituency. This is a panel-based difference-in-discontinuities specifications, that include district and year fixed effects. Here, the post-period is 2017 and 2018, and the pre periods include years 2009 to 2016. Standard errors are clustered at the district level. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person). The ruling party is BJP, ruling coalition is the NDA. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.7: National Election Results in 2019

Vote shares	BJP or ally		BJP	
Post×Banks	0.0891	0.0661	0.0161	0.00582
	(0.0848)	(0.0824)	(0.0622)	(0.0606)
Observations	547	564	465	477
R squared	0.651	0.650	0.729	0.727
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]

Notes: Dependent variable is vote share in 2019. We assign national parliamentary constituencies to districts. The BJP is the ruling party. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person). Standard errors are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.8: Difference-in-Discontinuities: Incumbent Vote Shares

	Vote Share for Incumbents	
Post×Banks	0.00962	0.0124
	(0.00875)	(0.0081)
Observations	6,515	6,923
R-squared	0.404	0.396
BW	[-5; 5]	[-10; 10]
Mean DV	0.460	0.462

Notes: Dependent variable is vote shares of incumbents in a constituency. Standard errors are clustered at the district level. These are panel-based difference-in-discontinuities specifications, that include district and year fixed effects. Here, the post-period is 2017-18, and the pre periods include years 2009 to 2016. All specifications restrict the sample around the RD cutoff. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.9: Falsification and Pre-trends with Placebo Cutoff Years

	Vote Shares: Difference in Discontinuities			
Panel A: 2015 Cutoff Year	BJP		BJP or ally	
Post×Banks	0.0345	0.0259	0.0302	0.0234
	(0.0221)	(0.0215)	(0.0197)	(0.0192)
Observations	7,506	7,877	9,029	9,510
R-squared	0.694	0.692	0.525	0.525
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
	Vote Shares: Difference in Discontinuities			
Panel B: 2014 Cutoff Year	BJP		BJP or ally	
Post×Banks	0.0345	0.0259	0.0302	0.0234
	(0.0221)	(0.0215)	(0.0197)	(0.0192)
Observations	7,506	7,877	9,029	9,510
R-squared	0.694	0.692	0.525	0.525
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
	Vote Shares: Difference in Discontinuities			
Panel C: 2013 Cutoff Year	BJP		BJP or ally	
Post×Banks	-0.0270	-0.0336**	-0.0228	-0.0232
	(0.0176)	(0.0168)	(0.0167)	(0.0158)
Observations	7,506	7,877	9,029	9,510
R-squared	0.694	0.692	0.525	0.526
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
	Vote Shares: Difference in Discontinuities			
Panel D: 2012 Cutoff Year	BJP		BJP or ally	
Post×Banks	0.0184	0.0152	0.0149	0.0146
	(0.0149)	(0.0141)	(0.0148)	(0.0138)
Observations	7,506	7,877	9,029	9,510
R-squared	0.694	0.692	0.525	0.525
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]

Notes: Dependent variable is vote shares. Standard errors are clustered at the district level. District level panel-based difference-in-discontinuities specifications, that include district and year fixed effects. All years post 2016 are excluded. When using 2015 as the cutoff year (Panel A), $Post = 1$ only for the year 2016. When using 2012 as the cutoff year, $Post = 1$ for all years post 2012. The pre-period starts in 2009. All specifications restrict the sample around the RD cutoff. The ruling party is BJP, ruling coalition is the NDA. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.10: Prob(Winning) Incl. Constituencies not Competed in

	Diff-in-Disc Prob(Winning Election)			
	Ruling Party		Ruling Coalition	
Post×Banks	0.158***	0.147***	0.156**	0.153***
	(0.0535)	(0.0491)	(0.0562)	(0.0525)
Bandwidth	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean	0.248	0.246	0.331	0.329

Notes: Dependent variable is the probability of winning the constituency. The sample includes all constituencies, even if the party did not field a candidate. This is a panel-based difference-in-discontinuities specifications, that include district and year fixed effects. Here, the post-period is 2017 and 2018, and the pre periods include years 2009 to 2016. Standard errors are clustered at the district level. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person). The ruling party is BJP, ruling coalition is the NDA. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.11: Views on Demonetization, by Political Strongholds

	Demonetization was the Right Move			
Banks	0.121*	0.174***	0.117*	0.164**
	(0.0703)	(0.0648)	(0.0693)	(0.0654)
Banks*NDA-Stronghold	0.0266	0.0141		
	(0.0473)	(0.0456)		
Banks*BJP-Stronghold			0.0507	0.0443
			(0.0616)	(0.0585)
Observations	3,911	4,071	3,911	4,071
R-squared	0.322	0.307	0.324	0.309
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.421	0.424	0.421	0.424

Notes: Dependent variable is whether the respondent says that demonetization was a good policy. The BJP is the ruling party, and the NDA is the ruling alliance. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person). Standard errors are clustered at the district level. District level panel-based difference-in-discontinuities specifications, that include district and year fixed effects. $Post = 1$ only for the years after 2016. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.12: Economic Impact of Demonetization: Heterogeneity by Political Strongholds

	Log (Nightlights)			
Post*Banks	0.0791**	0.0405	0.106***	0.0863***
	(0.0401)	(0.0418)	(0.0301)	(0.0312)
Post*Banks*NDA-Stronghold	0.0443	0.0994		
	(0.0590)	(0.0604)		
Post*Banks*BJP-Stronghold			-0.0104	0.0203
			(0.0490)	(0.0500)
Observations	4,543	4,771	4,543	4,771
R-squared	0.860	0.872	0.860	0.872
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	-0.351	-0.346	-0.351	-0.346

Notes: Dependent variable is the logarithm of luminosity. The BJP is the ruling party, and the NDA is the ruling alliance. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person). Standard errors are clustered at the district level. District level panel-based difference-in-discontinuities specifications, that include district and year fixed effects. $Post = 1$ only for the years after 2016. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.13: Impact on Views on Satisfaction with Prime Minister: Heterogeneity by Political Strongholds

	Satisfaction with Prime Minister			
Received Banks	-0.0747	-0.0424	-0.0282	0.0118
	(0.161)	(0.156)	(0.161)	(0.160)
Banks*NDA-Stronghold	-0.0918	0.0945		
	(0.0839)	(0.0838)		
Banks*BJP-Stronghold			0.0362	0.0152
			(0.123)	(0.118)
Observations	3,911	4,071	3,911	4,071
R-squared	0.429	0.406	0.430	0.408
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.526	0.522	0.526	0.522

Notes: Dependent variable is whether the respondent's satisfaction with Modi is above the national mean. The BJP is the ruling party, and the NDA is the ruling alliance. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person). Standard errors are clustered at the district level. District level panel-based difference-in-discontinuities specifications, that include district and year fixed effects. $Post = 1$ only for the years after 2016. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.