# Political Accountability for Populist Policies: Lessons from the World's Largest Democracy*

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**Abstract**

We know little about the electoral effects of policies with broad appeal that are implemented by popular leaders, but which have adverse economics effects. We analyze voter behavior following one such policy implemented in the world's largest democracy — India's 2016 'Demonetization,' which unexpectedly made 86% of the currency-in-circulation redundant overnight, and led to severe cash shortages and economic hardship in subsequent months. Yet, the policy appealed to a majority of voters, and was framed as one that would combat corruption. We leverage a discontinuity in the number of bank branches arising from a nationwide, district-level bank expansion policy. Using the fact that districts with fewer banks had greater cash shortages, we identify the impacts of demonetization's economic severity at the bank-expansion cutoff. Regression discontinuity estimates show that following demonetization, voters in places with more severe demonetization had less favorable views of the policy. Using a difference-in-discontinuity design, we find that the ruling party performed relatively worse in regions with more severe demonetization, receiving a 4.7 percentage point lower fraction of votes, and were relatively less likely to win seats in state legislatures. Areas that were historically strongly aligned with the ruling party were nearly unresponsive in voting behavior, despite having a less favorable view of the policy itself.

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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 when implementing drastic policies with questionable constitutional validity (Ravanilla et al., 2020) These policies, often packaged in populist rhetoric, may bypass democratic checks and balances (Acemoglu et al., 2013). Whether this recent trend is here to stay depends on how voters respond to such actions. We know little about the electoral effects of such large national-level policies that adversely affect economic well-being, but are implemented by popular leaders with strong moral messages.

We have the unique opportunity to study the electoral consequences of one such policy implemented in the world's largest democracy. We derive plausibly exogenous variation from India's 2016 nationwide demonetization policy that was enforced by the Prime Minister, without deliberation with the legislature or governing bodies. On November 8, 2016, the Prime Minister unexpectedly announced over a televised address that, starting at midnight, two commonly used currency denominations would cease to be legal tender. This meant that 86% of the currency in circulation was deemed illegal overnight.[1] New currency notes were to be provided in exchange for old notes at bank branches, but with significant withdrawal limits. The policy was implemented by a popular Prime Minister, who appealed to voter morality to bear the economic costs to help fight corruption and terrorism, by stemming the flow of counterfeit notes and hindering the black economy.

Government agencies, the legislature, and banks were not forewarned, 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. There were reports of unrest and economic hardship in the following months (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 institutions.[3]

Despite these negative consequences, the policy was not unpopular: data from a nation-wide survey conducted by the Center for the Study of Developing Societies (CSDS), indicates that only 16% of respondents felt that the sudden demonetization was a "bad policy." As much as 45% of respondents felt it was a "good policy," while 32% 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, the strong messaging, and/or the popularity of the Prime Minister.[4] His messaging framed the policy as combating 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; Cruz et al., 2018). Indeed, in some subsequent state elections, the implementing party was victorious, and most commentators and politicians claimed the policy was an electoral

[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] Writ petitions were filed in eight High Courts, and Supreme Court, questioning the policy's constitutionality.

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

[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).

[5] In a New Year's Eve speech, the Prime Minister urged that the "*fight against black money and corruption should not be stopped.*" He recognized inconveniences, but in that the costs were indicative of the long-term gains: "*Citizens have proved that for them, truth and honesty are important, despite inconveniences post demonetization.*"

success (The Indian Express, 2017). The central bank's own reports, however, showed that the anti-corruption objectives were never met (Quartz, 2018). The subsequent electoral victories may simply reflect aggregate pre-trends in support for the ruling party. Thus, the electoral consequences of such drastic policies are ambiguous and worthy of rigorous empirical investigation.

Our measure of the severity of demonetization is the lack of access to cash. We isolate the effect of cash access (or 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. That nationwide policy was targeted to all districts with banks per capita below a certain cutoff, 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, as in (Young, 2017; Cramer, 2020). We examine electoral outcomes across the cutoff and over time, before and after demonetization. This difference-in-discontinuities design that leverages the panel dimension along with the RD, 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.

We first document that there was was high policy-compliance around the cutoff for bank expansion policy, and that there there was indeed a sharp discontinuity in bank branches, accounts, and 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 cash shortages, hurting economic activity (The Indian Express, 2017); a claim we ground using nighttime light intensity data.[6]

Using voter surveys, we find citizens in worse hit districts had a less favorable view of demonetization. As such, more bank branches mitigated the severity of the effects of demonetization, and protected the population that now had access to banks.

Turning to our main outcome – the electoral impacts of demonetization – we find that in areas with fewer banks, the ruling party did relatively worse, and the ruling coalition received a 4.7 percentage point lower vote share in regions with discontinuously fewer bank branches (where demonetization was more severe). 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. The ruling party was also less likely to win seats.

Furthermore, we find that while ruling party strongholds suffered similar adverse economic effects, they saw no detectable changes in electoral outcomes. This suggests that there is a meaningful fraction of voters that did not change their voting behavior based on this policy, even if their livelihoods are impacted. This may reflect either strong alignment with the policy on ideology, or that other issues on the party platform are relatively more important (Besley and Coate, 2008).

We speak to prior work on voter responses to economic downturns (Margalit, 2019), for instance those caused by trade shocks (Autor et al., 2020), rainfall shocks (Cole et al., 2012), or austerity measures (Fetzer, 2019).[7] In contrast, we study the consequences of economic adversities that were

[6] Using night lights data we see that for 10% fewer bank branches, GDP was lower by 0.5% in the months following demonetization.

[7] Similarly, positive shocks may lead to electoral gains for incumbents (Bagues and Esteve-Volart, 2016).

more directly induced by economic policy, rather than external shocks.

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; Shenoy and Mahadevan, 2021), but we know less about voter responsiveness to policies that have broad support, but negative economic consequences. The findings in these papers are consistent with theories of rational voters, who reward incumbents. This could be in anticipation of continued future transfers or implicit reciprocity arrangements (Finan and Schechter, 2012; Blattman et al., 2017; Shenoy and Zimmermann, 2021). Evidence from India suggests that voters may hold local leaders accountable when implementation quality of programs deteriorate (Zimmermann, 2020).[8]

Our findings are consistent with the idea that the more salient the policy, the more responsive voters may be.[9] We find that voters did *not* reward the implementers of the 2005 bank-expansion policy itself in the decade following the expansions.[10] However, we find that survey-respondents in regions with more banks were less likely to say the demonetization policy was poorly implemented.

Our finding that voters in areas that were strongholds of the Prime Minister's party were electorally unresponsive to demonetization is consistent with work on issue-bundling during elections (Besley and Coate, 2008, 2003; Iversen and Goplerud, 2018). Even when a policy has an immediate negative economic impact, voters more strongly aligned with the party, may not be as responsive electorally. 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 substantially more favorable views of the policy. Our results are consistent with the observation that in democracies where voters get one chance to vote, a policy multi-dimensional policy space implies that voters who align more closely with a particular politician or party, are less responsive to any specific policy, however salient.[11]

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, and establishes that the bank branch expansion policy indeed affected access to cash in 2016. Section 5 discusses the results, Section 6 discussed mechanisms, and Section 7 concludes with a brief discussion.

[8] There is a vast and growing literature on Indian democratic politics highlighting features commonly found in democracies across the world, such as, the importance of patronage (Asher and Novosad, 2017; Mahadevan, 2020), leader religious identity (Bhalotra et al., 2022), ethnic violence (Nellis et al., 2016), and criminally implicated politicians (George et al., 2020; Prakash et al., 2019).

[9] Salience and information are also important factors when considering voter responses (Ferraz and Finan, 2011; George et al., 2020; Guiteras and Mobarak, 2014).

[10] 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 consistent messaging from the Prime Minister, and salience in daily life, voters had a better sense of who was responsible.

[11] A literature discusses issues surrounding the multi-dimensional policy space in democracies (Funk and Gathmann, 2011; Feld et al., 2010; DeLaO and Rodden, 2008; Fernández and Levy, 2008).

# 2 Background

## 2.1 The Sudden Demonetization and its Subsequent Economic Fallout

The 2016 demonetization was distinct from other episodes:[12] First, a huge fraction (86%) of the currency in circulation was made illegal.[13] Second, it was implemented overnight, with no forewarning to government agencies, legislatures, banks or citizens. The sudden announcement and lack of preparation led to considerable costs, given the overwhelming reliance on cash for a majority of economic transactions.[14] The announcement claimed that new 500 and 2000 rupee notes would be issued over time, and individuals could deposit old notes till December 31 in exchange. Yet, only 4000 rupees per person could be drawn per day.[15] The stock market crashed the next day, and in the following months, there was a sharp decline in cash availability (Lahiri, 2020).

Recent work finds negative economic impacts in the short and medium run. Chodorow-Reich et al. (2019) use proprietary data on currency chests, and find that districts with less cash saw lower nightlights-based economic activity and employment by at least 3 percentage points.[16] Banerjee and Kala (2017) find that 20% of surveyed traders reported a large fall in sales, with average sales being 20% lower. Contemporaneous work finds that demonetization led to spikes in unemployment (Subramaniam, 2019), reductions in household consumption (Wadhwa, 2019), and persistent reductions in agricultural trade values even after eight months (Aggarwal and Narayanan, 2019).[17]

### Reactions to the Policy

Several legal challenges were introduced in courts across the country questioning its legality (Kumar, 2016). Opposition parties and civil bodies decried the lack of deliberation preceding the unexpected, unilateral decision. Government agencies were unprepared, and media reports rife with news about lines outside banks and severe impacts on vulnerable populations (The Times of India, 2018). Yet, a nation-wide survey conducted by the Center for the Study of Developing Societies in May 2017, found that 45% of respondents believed that demonetization was "the right move," compared to only 16% who thought it was not needed. 32% felt the policy was good, but implemented in a hurry without the necessary groundwork. In the following months, the ruling party (BJP) and its allies won many state and local elections around the country, perhaps suggesting that voters did not seek to electorally punish the BJP.[18] Thus, despite widespread negative media reports and

[12] Demonetization is the act of rendering currency units illegal as tender, and replacing them. Implemented from time-to-time across the world, India has seen three other demonetization events at much smaller scales.

[13] 500 and 1000 rupee currency notes (approximately USD $7.5 and $15) were suddenly no longer legal tender.

[14] In a 2015 survey by MasterCard and Tufts University, 87% of all transactions were conducted in cash.

[15] Banerjee et al. (2018) summarize all changes.

[16] Since we focus on districts around a discontinuity, we verify the same economic effects in Section 5.

[17] Aggarwal et al. (2019) show that areas with high informality eventually saw greater switches to digital payments. Chanda and Cook (2019) show that regions with more bank deposit growth after demonetization, also saw relatively more subsequent economic activity, pointing to the importance of banking access for citizens trying to cope with the sudden demonetization.

[18] The BJP Chief Minister of Gujarat declared: *"Elections...were held immediately after demonetisation... 80% 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).

economic adversities, it was unclear whether there were political repercussions for the ruling party.

It is also plausible however, that the BJP may have just been on an upward trajectory electorally, and as such, aggregate trends may hide the true causal electoral effects to the policy. Our aim is to isolate the causal effect.

In concurrent work, using state-level bank expansions from the seventies and eighties to predict banking in 2009, Bhavnani and Copelovich (2020) study elections in 75 districts across seven states.[19] Their findings suggest that regions with fewer banks in 2009 saw increases in BJP votes. One way to reconcile our findings is to consider the possibility that areas with fewer banks after the 1980s scheme were areas that actually received our 2005 bank-expansion policy, and as such, have significantly greater bank growth in the years preceding 2016's demonetization.[20]

### 2.2 Bank Branch Expansions and Access to Cash

Our main source of variation in demonetization severity is a measure of access to bank branches and cash. Since cash could be deposited and withdrawn primarily from bank branches, the severity of demonetization would be greater in places with fewer branches. More banks per capita would mean less time waiting in long queues, less travelling to far-off branches to exchange money, and greater ease of exchanging old currency.

We leverage a policy reform implemented in 2005, wherein additional commercial bank branches were encouraged to open in 'underbanked' districts. New bank licenses are granted infrequently by the Reserve Bank of India (RBI), but the reform allowed easier entry of branches in districts with 'underbanked status,' based on the district average persons-per-branch. Additionally, banks were required to make accounts accessible to low-income customers, allowing accounts with low fees and minimal balances. The cutoff chosen was the national average of persons-per-branch in a district, producing a discontinuity in banking around the national average (Young, 2017; Cramer, 2020).[21]

## 3 Data and Measurement

Our district-level banking data from the Reserve Bank of India (RBI) documents the number of bank branches, accounts, and credit between 2002-2016. We also use bank-branch level information from the RBI's Master Office File, which includes branch locations and establishment year.

We examine election outcomes in all states that held elections in 2017-18, covering at least two election cycles *before*, and one election *after* demonetization.[22] We also study the 2019 national elections. These data are from the Election Commission of India contains information on candidates, and we identify whether they belong to the ruling coalition (NDA), or opposing coalition (UPA), at

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

[20] It is likely that in the small subset of districts used, the old bank expansion policy predicts banks till 2009, but is not generalizable to the rest of the country or in 2016.

[21] Districts on either side of the bank expansion cutoff were spatially distributed throughout the country, with no indication of geographical clustering.

[22] 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.

the time of the policy. In India, national parliamentary elections are held every five years (the last being 2019), while state elections are also held every five years, but in a staggered manner.[23]

We follow past research in using nighttime lights from the Defense Meteorological Satellite Program's Operational Linescan System, as a proxy for economic activity (Henderson et al., 2012). More recently, this has been used in India (Chodorow-Reich et al., 2019; Prakash et al., 2019; Mahadevan, 2020), where high-frequency, high-spatial resolution economic data is rare.

We use a nation-wide survey conducted by the Centre for the Study of Developing Societies (CSDS). In May 2017, 11,373 respondents across 19 states and 146 assemblies were surveyed on socioeconomic information, and views on the government, politics, and (importantly) demonetization.

# 4 Empirical Strategy

We isolate the effect of access to cash during demonetization, leveraging variation in the 2005 bank expansion policy, and use two main specifications. The first (our preferred) specification leverages both variation around the bank expansion cutoff and variation over time. We restrict our sample to a bandwidth around the cutoff, and compare electoral outcomes before and after demonetization. The cutoff chosen by the RBI was the national average (6.6 branches per 100,000 persons), and as such, is unlikely to be manipulated, as we show with the help of validation tests.[24]

While the RD bandwidth ensures we are comparing across similar districts, the panel dimension accounts for dynamics leading up to the policy change. Any differences in vote shares before demonetization would be evident in pre-period electoral outcomes.[25] Using a difference-in-discontinuities design and exploiting the panel dimension, we estimate:

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

Here $Post_t$ is an indicator for $t > November\ 8, 2016$ (when demonetization was unexpectedly announced). The small bandwidth around the cutoff ensures we compare similar districts, while $\gamma_d$ district fixed effects control for district characteristics. The parameter of interest, $\delta$, provides us with a difference-in-discontinuities estimate (having *less* severe demonetization), within a bandwidth around the cutoff. As there are no optimal bandwidth procedures for difference-in-discontinuities, we conduct robustness checks by varying the bandwidth manually.

For outcomes with cross-section data, we use an RD, with banks per person as the running variable:

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

where, $Y_{dt}$ is an outcome in district $d$, and $RD_d = 1$ for underbanked districts: those eligible for the policy, and so 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 $\{-D, D\}$ using Calonico et al. (2014), and show robustness across a wide range of bandwidths.

[23] Only state election constituencies are fully contained within districts, allowing for a 100 percent match.

[24] Other concurrent changes like the Jan Dhan Yojana (Agarwal et al., 2017) are therefore also unrelated to our bank branch expansion cutoff

[25] We explicitly explore electoral effects of the bank-branch expansion policy itself.

Given that various factors, such as caste, religion, or identity may impact voting patterns (Neggers, 2018), we test whether they vary discontinuously at the cutoff. Figure A.3 on our RD balance tests, demonstrates continuity in these characteristics around the cutoff. Furthermore, any other nationwide changes, such as the Jan Dhan Yojana scheme, or changes to financial asset disclosures (Fisman et al., 2018) are unlikely to be correlated with the RD cutoff.

## The 2005 Bank Policy Improved Financial Access

Panel (a) of Figure 1 shows a strong first stage as the bank expansion policy was well enforced: the probability of being classified by the RBI as an 'underbanked district' jumps discontinuously at the cutoff. Panel (b) shows the McCrary (2008) test, with no evidence of manipulation, and a large density of districts around the cutoff. Panel A of Table 1 provides the corresponding first-stage point estimates: The probability of being classified as underbanked jumps by 97 percentage points, and this led to a growth in bank branches.

The rest of Table 1 and Figure 1 show effects on private-sector bank branches and growth. We consistently find sharp discontinuities in the number of new branches, bank accounts, total credit, and a higher growth rate of branches in years leading up to demonetization. We verify that this increase around the 2005 policy cutoff, was strongly visible in 2015 – the year preceding demonetization. Higher bank-branch growth, combined with a lack of a discontinuity in the pre-period, led to higher levels of banking access in 2016, in terms of credit access (Table 1 Panel D, Figure A.4), bank accounts (Table 1 Panel D, Table A.1 and Figure A.4), bank branches per capita in 2016 (Figure 1f), bank branch growth and flow of branches (Figures 1d, 1e and A.5) and debit cards (Table 1 Panel E). The discontinuity is confirmed using multiple sources: the district level bank-branch data, the RBI's Master Office File, and CSDS survey data. Importantly, there was no discontinuity in older branches in years leading up to the bank-expansion policy.

Consistent with Young (2017) and Cramer (2020), we find that the bank expansion policy led to differential bank branch growth *after* the policy was instituted in 2005, and not before. Figures A.4 and A.5, and Tables A.1, A.2 and A.3 show using multiple specifications and sources, that in 2015, there were more branches and bank accounts, and this increase persists in the months leading up to 2016's demonetization, illustrating the lasting impact of the policy on financial access.

## Less Financial Access Decreased Economic Activity After Demonetization

Since we estimate a Local Average Treatment Effect (LATE), we investigate whether the effects using our RD sample are consistent with the findings on economic activity in Chodorow-Reich et al. (2019). Using our differences-in-discontinuity design, we find that this is indeed the case in Panel A of Table 2: There was no discontinuity in lights just before demonetization, but places that received fewer banks were likely to see a 9 percent fall in nighttime lights in the months following demonetization (also, Panel (e) of Figure A.5). Combining these results with Table 1 on bank branches, we calculate that a 10% decrease in branches was associated with a 1.7% decrease in luminosity. Using an elasticity of 0.3 to translate luminosity to GDP (Henderson et al., 2012; Chodorow-Reich et al., 2019), implies a 0.5% fall in GDP for a 10% reduction in bank branches.

# 5 Results

## 5.1 Correlates of Demonetization Support & Effects on Citizens' Views

We begin our analysis of voter responses to demonetization using data from CSDS voter surveys. Respondents were asked whether demonetization was 'the right move', 'the right move but poorly implemented', or 'the wrong move'. Figure A.2 shows the correlates of support for demonetization and the Prime Minister. Hindus, those who say eating beef should be banned, or do not hold the Prime Minister responsible for cow-related lynchings, are more likely to support the demonetization as well as Prime Minister. "Being religious" is predictive of support for demonetization, perhaps as the Prime Minister often promoted demonetization on 'moral' grounds (Norenzayan et al., 2016; Scheve and Stasavage, 2006; McCleary and Barro, 2006). Poorer and less educated individuals were less likely to say demonetization was the right policy.

In Panel B of Table 2 we estimate Equation 2, and find that respondents in regions with less severe demonetization were more likely to think it was the right move, and less likely to think it was badly implemented.[26] These are confirmed by RD graphs in Figure 2a and Figure A.5, Panel (d).

## 5.2 Effects on Elections

To investigate whether the effects on views on demonetization translated to actual voting patterns, we look at vote shares and winning probabilities for the two main political entities: the ruling party (BJP), and ruling coalition (NDA). We first check whether the banking policy itself led to electoral effects before demonetization. Table A.4 provides estimates for the discontinuity at the cutoff in vote shares, and winning probabilities for elections held pre-demonetization, and finds no detectable effects of the banking policy.

It is plausible that the benefits of the banking policy were gradual and less salient, unlike the sudden demonetization. We find it interesting that a slow, less salient policy did not reap electoral rewards for the party. Table A.5 explores effects on vote shares after demonetization, estimating Equation 2. The vote shares for the ruling coalition are higher in regions with more banks.

In Panel C of Table 2, we present results from our preferred difference-in-discontinuities specification 1, leveraging the panel dimension of the data. The coefficients capture the gain in vote shares over the previous election in constituencies, around the cutoff. In Panel D, 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 the likelihood of winning. Again, it is evident that in regions that had discontinuously more 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 4.76 percentage points following demonetization, in areas with less severe demonetization. Together with Table 1, this suggests that a 10% decrease in bank branches was associated with a 0.9 percentage point decrease in vote shares for the ruling party.

In Figures 2b, 2c, 2d, and Panel (f) of Figure A.5, we show the corresponding RD figures for vote shares, and probabilities of victory for the ruling party and coalition. In Figures 2f and 2f,

[26] We do a cross-sectional RD as voter survey data was only collected in May 2017.

we perform an RD-event study 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 in vote shares or winning probabilities. Following demonetization, the RD coefficient rises substantially, showing the ruling party enjoyed relatively higher vote shares in areas with more banks (or relatively lower vote shares in areas with fewer banks).

### Alternative Specifications and Robustness

Bank expansions may be popular in their own right. Yet, as we discuss, we detect no effect on vote shares for the implementing party, and we use a difference-in-discontinuities to isolate the effect of changes that occurred at the cutoff only *after* 2016.

A crucial assumption in our difference-in-discontinuities identification strategy is that, there were no differential pre-trends around the branch-expansion cut-off. Figures 2f and 2f shows the lack of pre-trends leading up the policy. In Table A.8, we run the difference-in-discontinuities specification (equation 1), for different *placebo* years as cutoffs (2012, 2013, 2014, 2015), while excluding data post the 2016 demonetization, and detect no discontinuities in outcomes.

One may also consider that bank expansions affect votes for incumbents. In Figure A.7 we estimate dynamic effects on incumbent votes in state elections and fail to detect discontinuities either before or after demonetization.

Our results are also robust to a range of bandwidths. In Figure A.6, we show robustness to bandwidths between 0.2 banks per million people to 1.5 banks per million people around the cutoff.

Next, we address the potential consequences of how winning probabilities are defined. We had coded cases of parties not fielding a candidate as a missing value. However, fielding a candidate may be endogenous. In Appendix Table A.9 we re-do our main analysis including constituencies where the parties did not field candidates, now coded as zeros, 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), or the Jan Dhan Yojana (Agarwal et al., 2017) were implemented across the country in a manner unrelated to our bank expansion cutoff.

## 6 Drivers of the Electoral Effects & Political Strongholds

We have shown that demonetization led to relatively lower vote shares for the ruling party in areas where its severity was greater. However, these average effects may hide substantial heterogeneity. We explore why certain voters may have responded to the policy, and why others did not, despite the economic hardships.

Constituencies that are the BJP's or its coalition's strongholds may be less likely to punish the regime for hardships caused by demonetization, for a number of reasons.[27] Table 3 shows that voters

[27] A constituency is considered a 'stronghold' if it lies above the median constituency with respect to electoral wins for the BJP/NDA in the four elections that took place before demonetization.

in BJP or NDA strongholds were unlikely to change their voting behavior when faced with more severe demonetization.[28]

Electoral constituencies for national elections have an imperfect match to districts. The estimates in Panel A of Table A.15, using data from the 2009, 2014 and 2019 national elections, while noisy, have the expected sign. Panels B and C illustrate that heterogeneous effects by stronghold status are qualitatively similar to those in state elections.

Why were voters in strongholds unresponsive? We discuss possible reasons below.

### Targeted Economic Relief, Campaigning or Vote Buying

The ruling party may have targeted economic relief to less-banked areas, mitigating economic adversities (Asher and Novosad, 2017; Mahadevan, 2020). It is plausible that there exists some favoritism, wherein local politicians received additional resources to address hardships from the party-in-power. We find little evidence in support of this. Table A.12 shows that the economic consequences of demonetization severity were similar in strongholds and non-strongholds. Furthermore, people's views on demonetization were similar across strongholds and non-strongholds (Table A.11). As such, regardless of whether they resided in strongholds, districts with fewer banks were adversely affected and had a less favorable view of demonetization.

Another reason for the patterns in stronghold areas might be explained by the ease of campaigning, vote buying or clientelism in these areas, as parties find it easier to target resources or messaging in strongholds (Keefer and Khemani, 2009). Yet, politicians would need to target resources and messaging to less-banked districts around the cutoff, which seems implausible. The lack of heterogeneity in economic activity or favorability views also makes this unlikely to have occurred.

### Voter Knowledge and Awareness

Strongholds may also have better targeted messaging to the voter base on the merits and demerits of the policy, and who was 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.[29] Importantly, the evidence in Table A.11 that at the 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

It is theoretically unclear whether this policy would lead to electoral effects in strongholds, where voters are more strongly aligned with the party on a number of issues. In the absence of direct democracy for specific issues, citizens have one vote, but consider a *bundle* of issues when

[28]The coefficient on the interaction term in Table 3 should be interpreted cautiously as 'stronghold' status is not randomly assigned. In Table A.10 we see that other than religiosity we fail to detect meaningful correlates of stronghold status. We show in Table A.14 that our RD balance tests are unrelated to stronghold areas, implying that stronghold voters on either side of the RD cutoff are similar.

[29]If voters had been unaware, one possibility might be that they thought their incumbent leader was responsible, and we may have seen effects on incumbents; which we do not (Table A.7).

casting their vote. It is therefore not obvious that we would detect a corresponding negative impact on electoral outcomes.

Issue bundling suggests that we *should* expect to see a muted effect 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 what drives the lack of voter response in strongholds. Voters that align closely with a particular politician (or party), despite being negatively impacted by a specific policy, may still vote for them.

**Incumbency Effects and Turnout**

Despite local incumbents having little to do with the introduction or implementation of policies like demonetization, voters may attribute credit/blame to incumbents, given a lack of awareness on who was responsible (Guiteras and Mobarak, 2014).[30] Table A.7 and Figure A.7 show no detectable change in vote shares for incumbents. A final driver may be differential turnout in areas with more severe demonetization, but we find no evidence of this (Table A.6).

# 7 Conclusion

We analyze the electoral consequences of a popular policy, which, as we and others show, had negative economic effects. As the policy was implemented on the same day nation-wide, it is challenging to isolate the effects of demonetization from other secular political trends. We overcome these challenges, leveraging a discontinuity in the number of bank branches in districts across the country, as a consequence of a bank-licensing policy instituted in 2005 by the previous government. As demonetization necessitated the exchange of old currency notes for new ones, a lack of access to branches implied greater difficulty in acquiring new notes. We derive variation in the *severity* of demonetization from the change in bank-density around the cutoff.

In regions with fewer banks, the ruling party had discontinuously fewer votes and winning probabilities. Our magnitudes are meaningful: A 10% increase in the number of new branches corresponded to a 0.9pp increase in vote shares, when faced with such policy-induced economic adversities.

Despite facing similar negative economic consequences, ruling party strongholds were not responsive in voting behavior, suggesting that strong supporters of a 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 after demonetization. Without a causal analysis, we may misleadingly think that voters did not respond to demonetization. Indeed, the policy was implemented by a popular Prime Minister, who appealed to voter morality to bear such costs in helping the nation fight corruption. In the absence of well-identified variation in banking, the media concluded that demonetization was not punished by the voter base (The Indian Express, 2017). We dispel this flawed view with stronger identification.

[30] Voters may also attribute random shocks to incumbents (Bertrand and Mullainathan, 2001).

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Table 1: The 2005 Banking Policy Improved Banking Access

| *Panel A* | P(Unbanked Status) | | | ΔLog(Branches) |
|---|---|---|---|---|
| RD Estimate | 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 |
| *Panel B* | Δ Log(Newly Built 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) – Falsification Test | | | |
| 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 |
| *Panel D* | District-Level Total Credit (2015) | | | |
| 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 |
| | Household Banking Access (2017) | | | |
| *Panel E* | 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) |
| Bandwidth | [-1 ;.5] | [-.6 ;.3] | [-1.6 ;.5] | [-1 ;.3] |
| Mean | .883 | .862 | 0.553 | .545 |
| Specification | MSE2 | CER2 | MSE2 | CER2 |

Notes: RD 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. 'MSE2' 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. Panel A: 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 (also in Panel B). Panel C: Log(Old Branches) are the number of branches opened in the five years leading up to the policy. Panel D: District-level Total Credit Limit in 10 million Indian rupees in 2015. Panel E: Household level regressions in the cross section using CSDS data. 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. Standard errors clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: The Effects of Demonetization Severity

| *Panel A* | Log(Lights) | | | |
|---|---|---|---|---|
| | Pre-Period Placebo | | Difference-in-Discontinuities | |
| RD Estimate | -0.0464 | -0.0168 | | |
| | (0.140) | (0.130) | | |
| PostxBanks | | | 0.0977*** | 0.0895*** |
| | | | (0.0202) | (0.0199) |
| Observations | 1,836 | 1,936 | 4,591 | 4,839 |
| R-squared | 0.099 | 0.093 | 0.894 | 0.900 |
| BW | [-5; 5] | [-10; 10] | [-5; 5] | [-10; 10] |
| Mean DV | 0.544 | 0.590 | 0.537 | 0.576 |
| *Panel B* | Views on Demonetization | | | |
| | Demonetization was right move | | Right but with bad preparation | |
| 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 |
| *Panel C* | Electoral Outcomes: Vote Shares | | | |
| | Ruling Coalition | | Ruling Party | |
| PostxBanks | 0.0973*** | 0.0990*** | 0.0485** | 0.0476** |
| | (0.0234) | (0.0226) | (0.0209) | (0.0200) |
| Observations | 9,021 | 9,465 | 10,633 | 11,220 |
| 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 |
| *Panel D* | Electoral Outcomes: Prob(Winning) | | | |
| | Ruling Coalition | | Ruling Party | |
| Received Banks | 0.146** | 0.148*** | 0.134** | 0.122** |
| | (0.0590) | (0.0552) | (0.0602) | (0.0564) |
| Observations | 10,633 | 11,220 | 9,021 | 9,465 |
| R squared | 0.269 | 0.262 | 0.342 | 0.339 |
| BW | [-5; 5] | [-10; 10] | [-5; 5] | [-10; 10] |
| Mean DV | 0.376 | 0.374 | 0.332 | 0.332 |

Notes: RD Bandwidth in units of banks per hundred thousand people. 'Linear' and 'Quadratic' indicate functional form controls of the running variable. Panel A: The first two columns show a falsification test in the pre-period, whereas the last two show the difference-in-discontinuities effect of on lights. Dependent variable is the logarithm of luminosity at the district-by-month level. Pabel B: Individual level regressions in 2017. Dependent variable is views on demonetization using household-level CSDS data. Panel C: Dependent variable is vote shares. 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. Panel D: Dependent variable is the probability of winning the constituency. Standard errors clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

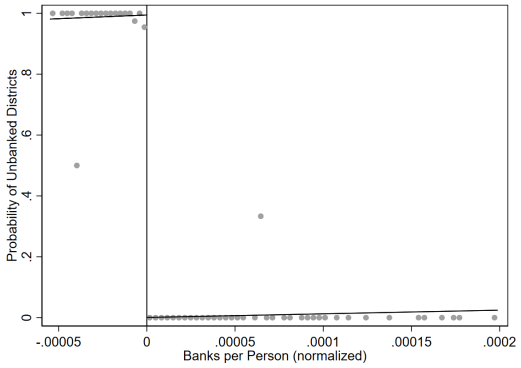
Table 3: Political Strongholds are Electorally Less Responsive

| Panel A | Vote Share for Ruling Party | | | |
|---|---|---|---|---|
| PostxBanks | 0.126*** | 0.137*** | 0.141*** | 0.150*** |
| | (0.0362) | (0.0359) | (0.0463) | (0.0450) |
| PostxBanksxParty-Stronghold | -0.124*** | -0.144*** | | |
| | (0.0383) | (0.0376) | | |
| PostxBanksxCoalition-Stronghold | | | -0.111** | -0.126*** |
| | | | (0.0441) | (0.0427) |
| Observations | 8,705 | 9,130 | 8,705 | 9,130 |
| R squared | 0.670 | 0.667 | 0.667 | 0.665 |
| BW | [-5; 5] | [-10; 10] | [-5; 5] | [-10; 10] |
| Mean DV | 0.265 | 0.266 | 0.265 | 0.266 |
| Panel B | Vote Share for Ruling Coalition | | | |
| PostxBanks | 0.196*** | 0.207*** | 0.175*** | 0.179*** |
| | (0.0325) | (0.0306) | (0.0423) | (0.0398) |
| PostxBanksxParty-Stronghold | -0.181*** | -0.202*** | | |
| | (0.0344) | (0.0328) | | |
| PostxBanksxCoalition-Stronghold | | | -0.100** | -0.106*** |
| | | | (0.0422) | (0.0404) |
| Observations | 10,289 | 10,853 | 10,289 | 10,853 |
| R squared | 0.525 | 0.522 | 0.519 | 0.516 |
| BW | [-5; 5] | [-10; 10] | [-5; 5] | [-10; 10] |
| Mean DV | 0.318 | 0.318 | 0.318 | 0.318 |

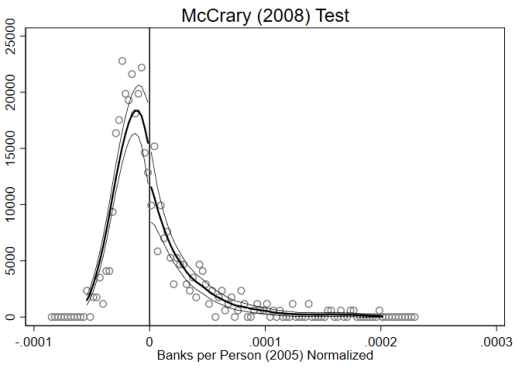
Notes: RD Bandwidth in units of banks per hundred thousand people. District level panel-based difference-in-discontinuities specifications, that include district and year fixed effects. $Post = 1$ only for the years after 2016. All specifications restrict the sample around the RD cutoff. Panel A: Dependent variable is vote shares for the ruling party (BJP). Panel B: Dependent variable is the vote share for the ruling coalition (NDA). Standard errors clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 1: First Stage, McCrary (2008) Tests and Banking Growth

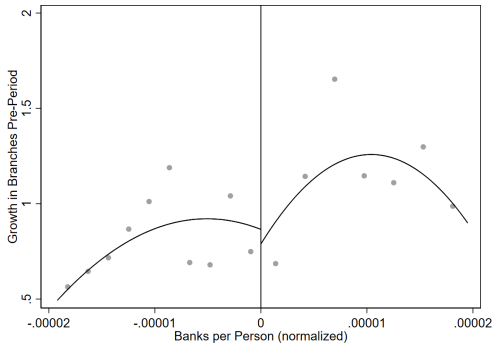
(a) Compliance With Policy Rule



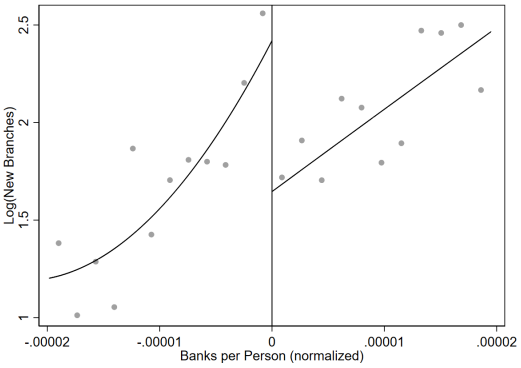

(b) District Density at RD Cutoff



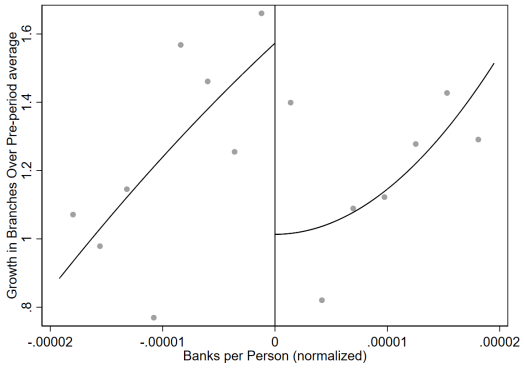

(c) Number of Old Branches (Placebo)



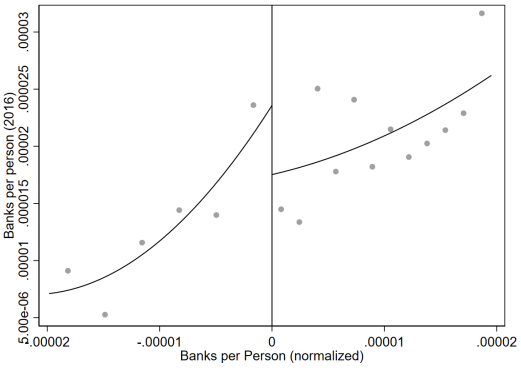

(d) New branches




(e) Growth in Branches



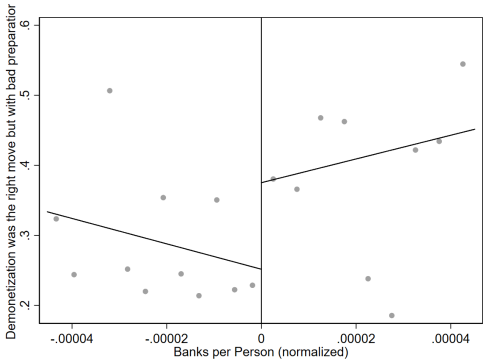

(f) Number of Branches per cap (2016)



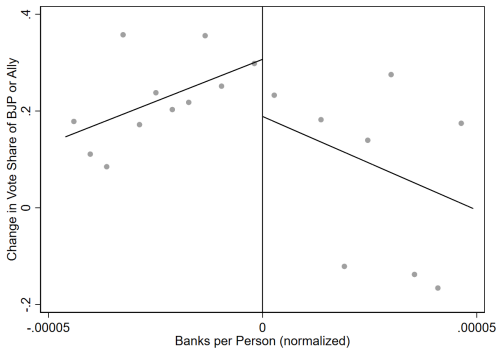

Notes: Top panel graphs show the first stage (Fig 1a), and density of districts (Fig 1b) at the cutoff as in the McCrary (2008) Test. Subsequent graphs show the effect of unbanked status on private-sector bank branches and growth. Figure 1c plots the old branches in the four years preceding the policy. Figure 1d 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, and plots the newly opened branches between in the subsequent five years. Figure 1e compares the new number of branches in the five years after the policy to the newly opened branches in five years preceding the policy. Figure 1f shows the number of branches per capita in 2016 (the year of demonetization). See Appendix Figures A.4 and A.5 for more robustness checks.

Figure 2: Views on Demonetization and Election Outcomes

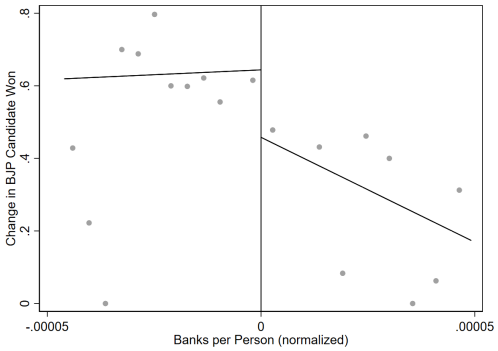
(a) Demonetization had Bad Preparation



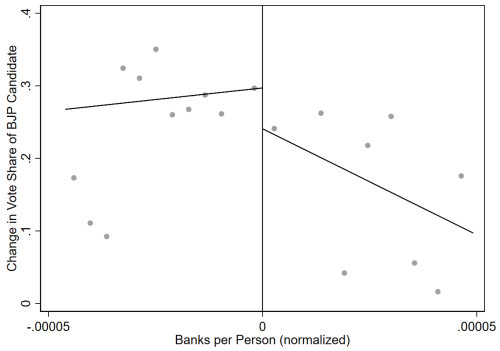

(b) ΔVote Shares (Ruling Coalition)



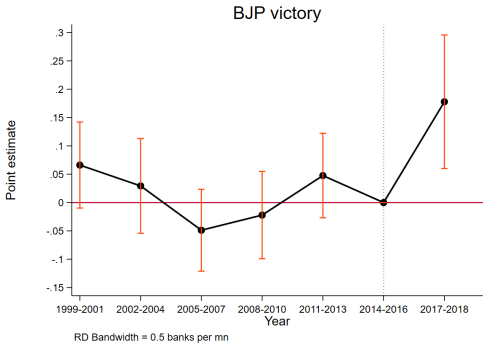

(c) Change in P(Ruling Party Victory)




(d) ΔVote Shares (Ruling Party)




(e) P(Ruling Party Victory) Over Time




(f) Vote Share by Year (Ruling Party)

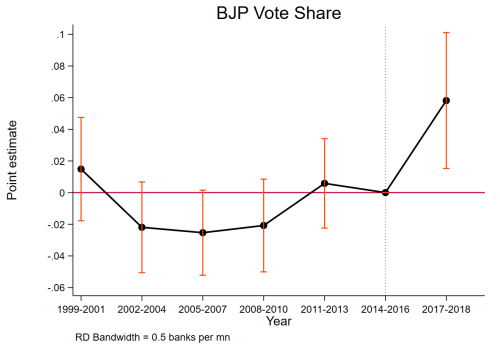
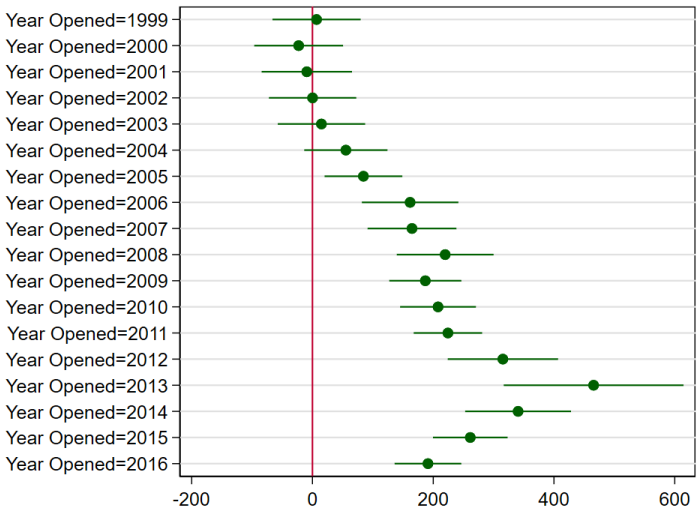



Figure 2a use household level CSDS and show the effect of unbanked status around the RD cutoff on views on demonetization, where the outcome is an indicator for whether or not the respondent thought demonetization was the right move, but badly implemented. Figure 2b shows the change in the vote shares for the ruling coalition. Figure 2c shows the change in the probability of victory for the ruling party. Figure 2d shows the change in the vote shares for the ruling party. The bottom panel 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. See Appendix Figure A.5 for more robustness checks.

# ONLINE APPENDIX

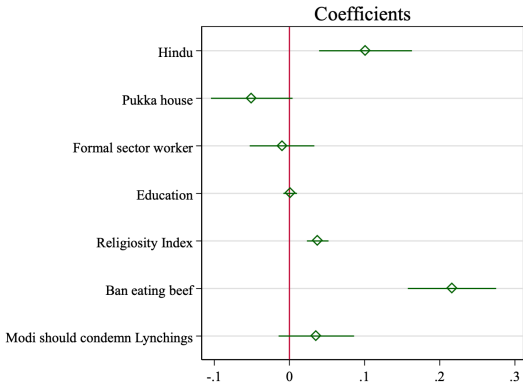
Figure A.1: New Branches over Time



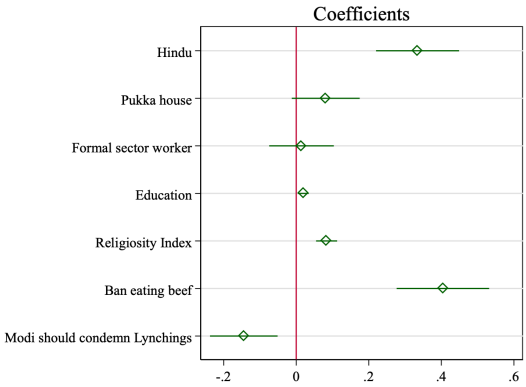

This figure shows the coefficient on the the number of new branches opened each year relative to 1998. The policy was implemented in 2005.

Figure A.2: Correlates of Support For:

(a) Demonetization



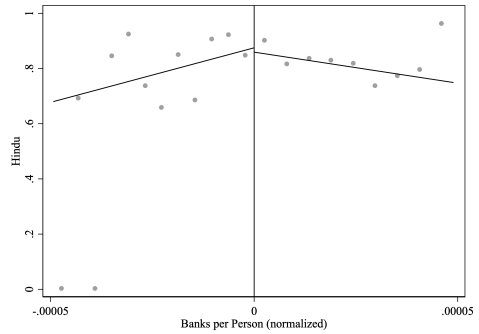

(b) Prime Minister



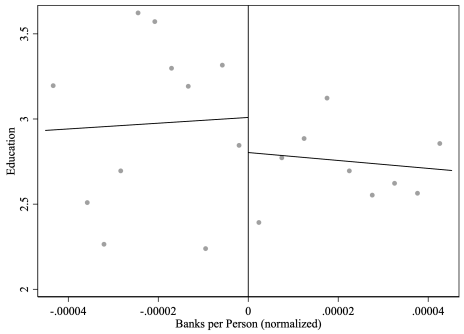

Panel (a) plots the coefficients for the correlates of support for the demonetization policy, while Panel (b) does the same for support for the Prime Minister, Mr Modi. The data comes from the CSDS voter surveys.

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

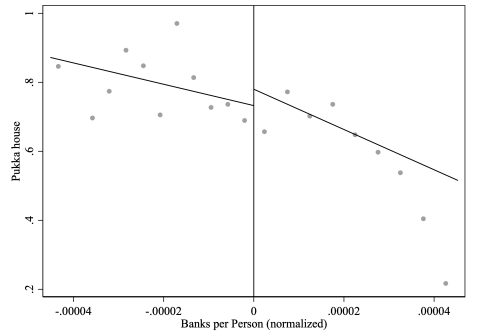
(a) Hindu



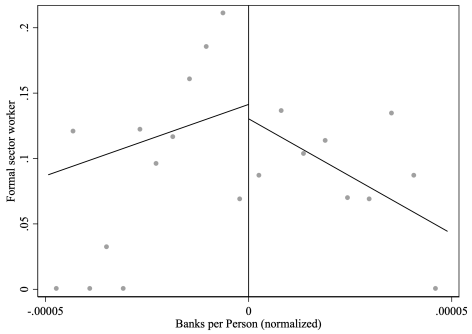

(b) Education



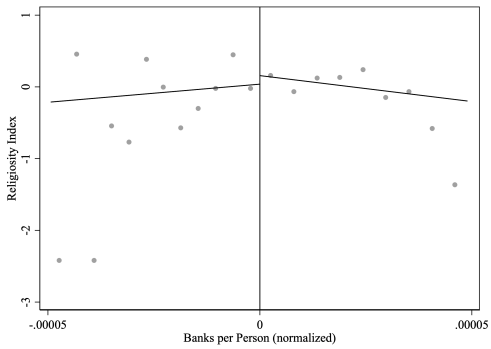

(c) Pukka House



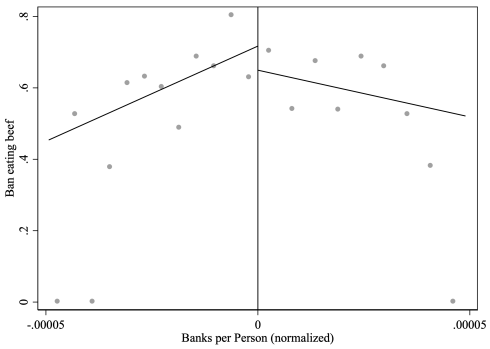

(d) Formal Sector Worker




(e) Religiosity



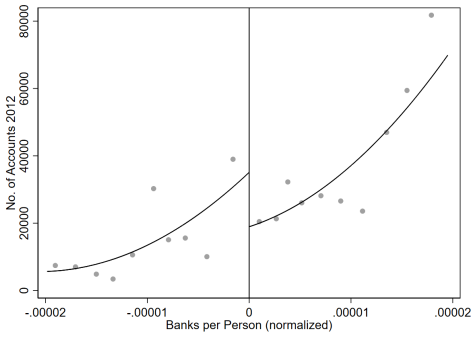

(f) Supports Beef Ban



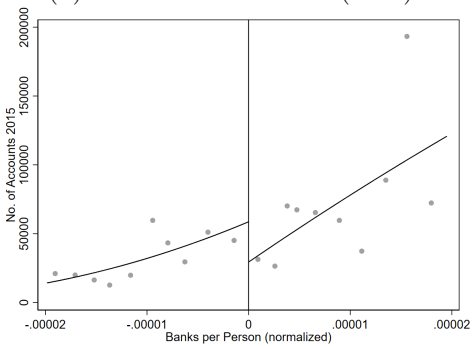

Graphs show the relationship between unbanked status and individual characteristics using the CSDS Mood of the Nation survey.

Figure A.4: Number of Accounts and Credit

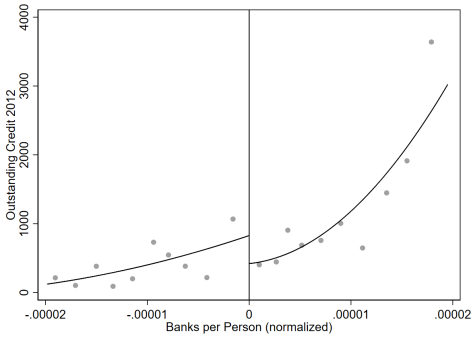
(a) Number of Accounts (2012)



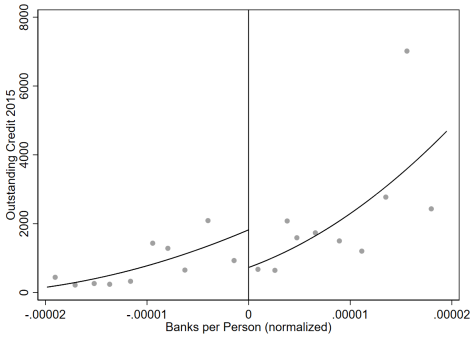

(b) Number of Accounts (2015)



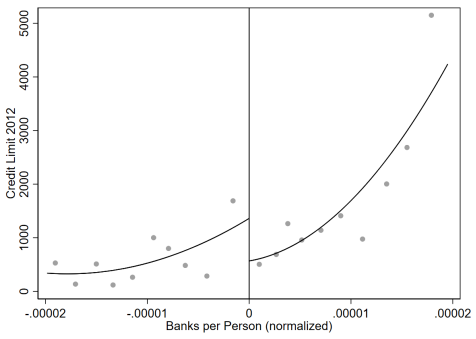

(c) Outstanding Credit (2012)



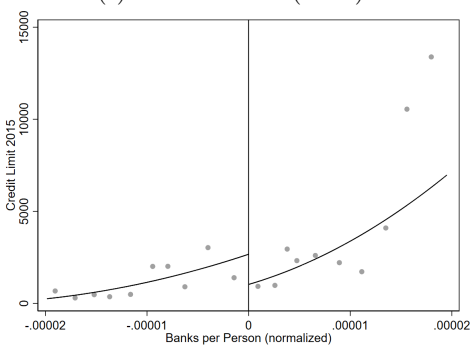

(d) Outstanding Credit (2015)




(e) Credit Limit (2012)



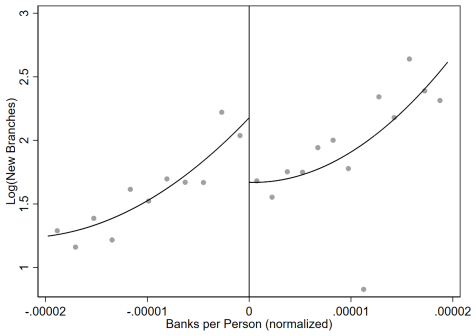

(f) Credit Limit (2015)



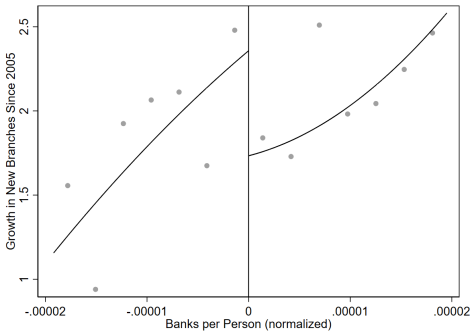

Graphs show the effect of unbanked status on number of accounts, credit limits and outstanding credit. Figure and Figure show the number of bank accounts in 2012 and 2015. Figure and Figure show the amount of outstanding credit (ten million rupees) in 2012 and 2015. Figure and show the total credit limit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

Figure A.5: Bank Branches and Views on Demonetization at the RD Cutoff

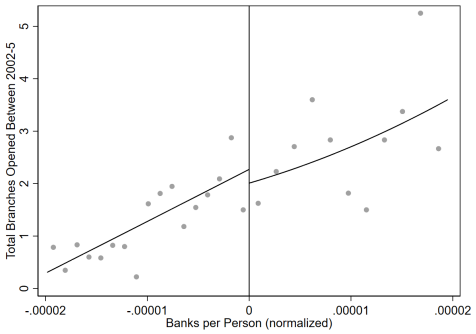
(a) New branches (2006-10): Aggregate



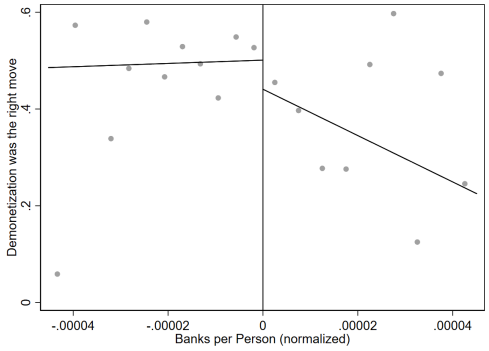

(b) Growth in branches since 2005



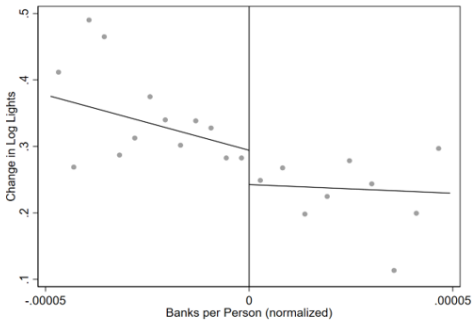

(c) Old Branches 2001-05



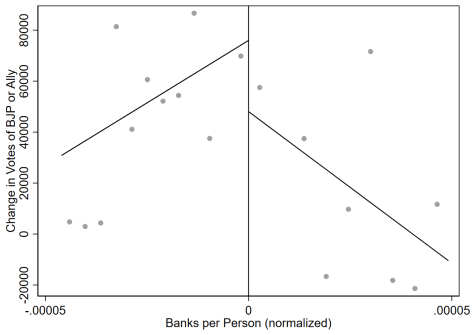

(d) Demonetization was the Right Move




(e) Changes in Nightlights

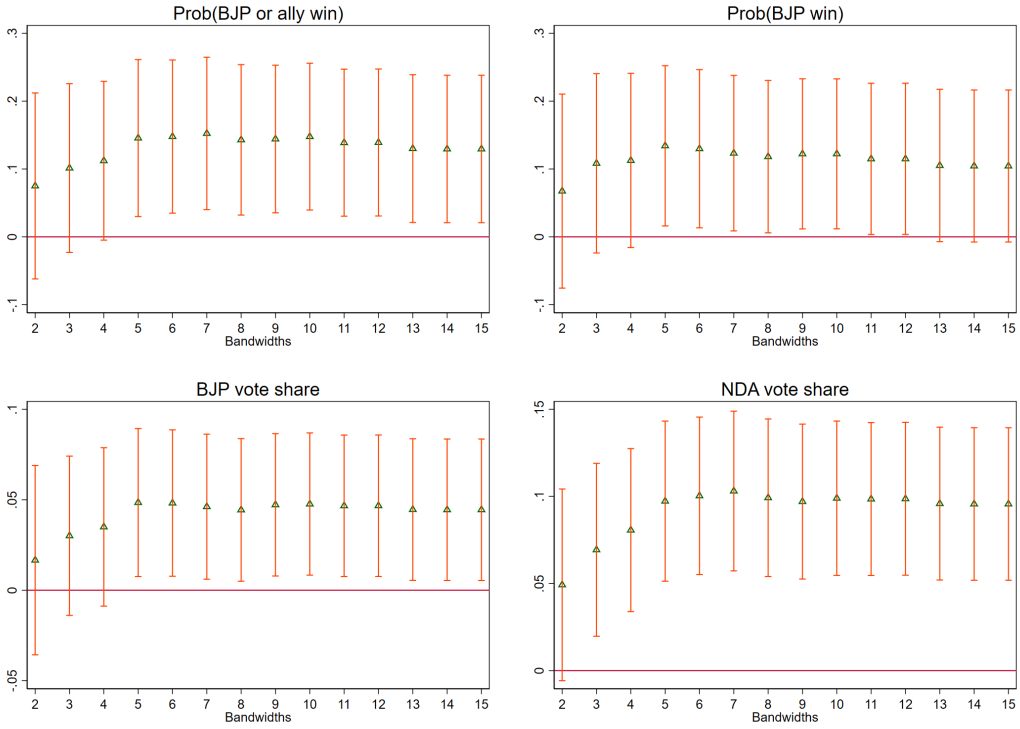



(f) Changes in Votes for Ruling Coalition




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 (Figure 1d uses the branch-level data). Panel (b) looks at the growth at the RD cutoff between the 2006-10 and the year before the policy started (2005), whereas Figure 1e compares it to the five year average preceding the expansion. Panel (c) shows pre-treatment (2001-5) baseline tests using the aggregate district-level data, whereas Figure 1c plots it with respect to RBI MOF data. Panel (d) plots the views on demonetization, where the respondent is asked if "demonetization was the right move", whereas Figure 2a is an indicator for whether or not the respondent thought demonetization was the right move, but badly implemented. Panel (e) shows the change in nightlights. Panel (f) shows the changes for the ruling coalition, whereas Figure 2 shows the other election outcomes.

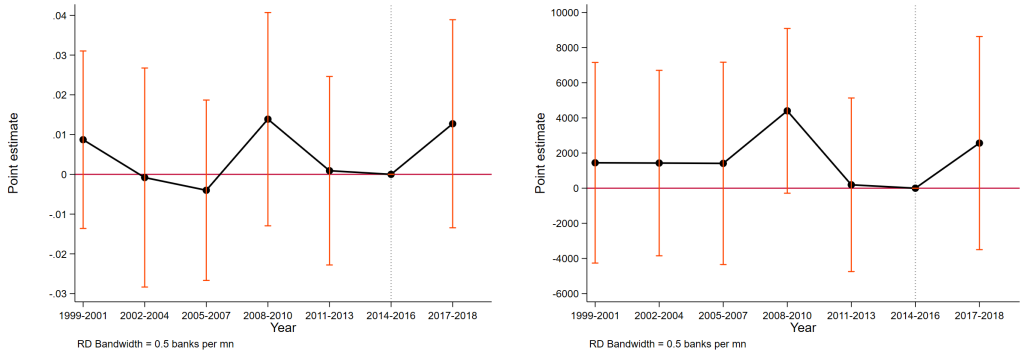
Figure A.6: Sensitivity to Bandwidths




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.

Figure A.7: Dynamic Effects on Incumbent Votes

(a) Incumbent Vote Shares (b) Incumbent Votes




Graphs show dynamic effects on incumbent votes in state-level elections around the banking RD cutoff. Sample restricted to bandwidth around the banking cutoff, and outcome is vote shares for incumbents in state elections.

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: Branches, Accounts and Credit at the RD cutoff in 2012

| *Panel A* | Log(Branches Built Post Policy) | | | |
|---|---|---|---|---|
| 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* | 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 | 19844.712 | 22578.383 | 21589 | 21589 |
| Specification | MSE2 | CER2 | Linear | Quadratic |
| *Panel C* | 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 D* | 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. 'Log(Branches Built Post Policy)' is the number of branches built after 2005. 'Number of Accounts' is the number of open bank accounts. 'Total Credit Limit' and 'Total Outstanding Credit' in 10 million Indian rupees. '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.3: 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.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: Difference-in-Discontinuities: Incumbent Vote Shares

| | Vote Share for Incumbents | |
|---|---|---|
| Post×Banks | 0.0053 | 0.0085 |
| | (0.0084) | (0.0078) |
| Observations | 6,515 | 6,923 |
| R-squared | 0.408 | 0.400 |
| 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.8: 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.9: 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.10: Correlates of Stronghold Status

| | Coalition Stronghold | Ruling Party Stronghold |
|---|---|---|
| Formal workers | -0.107 | -0.166 |
| | (0.136) | (0.189) |
| Income (per person) | 0.957 | 6.807 |
| | (3.659) | (4.465) |
| Religiosity index | 0.698*** | 0.670*** |
| | (0.117) | (0.113) |
| Hindu | 0.00261 | 0.0762 |
| | (0.105) | (0.104) |
| Pukka house | 0.0354 | -0.00828 |
| | (0.0725) | (0.108) |
| Should not condemn | -0.0279 | 0.0262 |
| | (0.121) | (0.145) |
| Constant | 0.0716 | -0.180 |
| | (0.158) | (0.160) |
| Observations | 1,569 | 1,569 |
| Mean DV | | |
| R-squared | 0.096 | 0.103 |

Notes: We examine the correlation between stronghold status and our primary correlates of interest. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.14: Balance Test by Stronghold Status

| NDA Stronghold | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Formal workers | | | | Income per cap | | | | Religiosity | | | |
| RD Estimate | -0.0179 | -0.0121 | -0.0145 | -0.0189 | -0.000861 | -0.00139 | -0.00103 | -0.00263* | -0.0919 | -0.0573 | -0.0394 | -0.0726 |
| | (0.0208) | (0.0155) | (0.0193) | (0.0162) | (0.00143) | (0.00111) | (0.00144) | (0.00132) | (0.0895) | (0.0557) | (0.0800) | (0.0562) |
| R-squared | 0.030 | 0.017 | 0.027 | 0.014 | 0.052 | 0.166 | 0.054 | 0.122 | 0.030 | 0.018 | 0.016 | 0.015 |
| | Hindu | | | | Should not condemn lynchings | | | | Pukka house | | | |
| RD Estimate | -0.145 | -0.0883 | -0.0826 | -0.111 | -0.0209 | 0.00256 | -0.0218 | -0.00467 | 0.106 | 0.191* | 0.0666 | 0.199** |
| | (0.106) | (0.0928) | (0.101) | (0.0920) | (0.0237) | (0.0111) | (0.0201) | (0.0120) | (0.114) | (0.101) | (0.0987) | (0.0916) |
| R-squared | 0.035 | 0.020 | 0.019 | 0.017 | 0.006 | 0.018 | 0.004 | 0.007 | 0.074 | 0.108 | 0.090 | 0.117 |
| Obs | 2,512 | 2,320 | 2,645 | 2,457 | 2,512 | 2,320 | 2,645 | 2,457 | 2,512 | 2,320 | 2,645 | 2,457 |
| BW | [-5; 5] | [-5; 5] | [-10; 10] | [-10; 10] | [-5; 5] | [-5; 5] | [-10; 10] | [-10; 10] | [-5; 5] | [-5; 5] | [-10; 10] | [-10; 10] |
| BJP Stronghold | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No |
| | Formal workers | | | | Income per cap | | | | Religiosity | | | |
| RD Estimate | -0.0144 | -0.0142 | -0.00976 | -0.0198 | -0.00195 | -0.000581 | -0.00169 | -0.00190 | -0.0590 | -0.0753 | -0.0288 | -0.0693 |
| | (0.0265) | (0.0169) | (0.0250) | (0.0168) | (0.00188) | (0.000991) | (0.00173) | (0.00129) | (0.0954) | (0.0663) | (0.0928) | (0.0586) |
| R-squared | 0.036 | 0.017 | 0.036 | 0.016 | 0.057 | 0.150 | 0.058 | 0.108 | 0.031 | 0.014 | 0.028 | 0.016 |
| | Hindu | | | | Should not condemn lynchings | | | | Pukka house | | | |
| RD Estimate | -0.115 | -0.102 | -0.0659 | -0.107 | -0.0165 | -0.00269 | -0.0270 | -0.00539 | 0.121 | 0.152 | 0.0650 | 0.168 |
| | (0.111) | (0.104) | (0.111) | (0.0950) | (0.0338) | (0.0119) | (0.0289) | (0.0125) | (0.145) | (0.113) | (0.130) | (0.104) |
| R-squared | 0.032 | 0.015 | 0.023 | 0.014 | 0.011 | 0.009 | 0.007 | 0.005 | 0.099 | 0.085 | 0.087 | 0.106 |
| Obs | 1,569 | 3,263 | 1,654 | 3,448 | 1,569 | 3,263 | 1,654 | 3,448 | 1,569 | 3,263 | 1,654 | 3,448 |
| BW | [-5; 5] | [-5; 5] | [-10; 10] | [-10; 10] | [-5; 5] | [-5; 5] | [-10; 10] | [-10; 10] | [-5; 5] | [-5; 5] | [-10; 10] | [-10; 10] |

Notes: In each column we vary the sample to be either stronghold constituencies or non-stronghold constituencies. The BJP is the ruling party, and the NDA is the ruling alliance. Dependent variable is outcomes in 2017. 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.15: 2019 National Elections by Stronghold Status

| Panel A | Vote shares | | | |
|---|---|---|---|---|
| | Ruling Coalition | | Ruling Party | |
| 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] |
| Mean DV | 0.588 | 0.587 | 0.576 | 0.577 |
| Panel B | Vote Share for Ruling Party | | | |
| PostxBanks | 0.0929 | 0.0826 | 0.184** | 0.174* |
| | (0.0728) | (0.0714) | (0.0916) | (0.0904) |
| PostxBanksxParty-Stronghold | -0.110 | -0.110 | | |
| | (0.0754) | (0.0752) | | |
| PostxBanksxCoalition-Stronghold | | | -0.198** | -0.198** |
| | | | (0.0881) | (0.0879) |
| Observations | 465 | 477 | 465 | 477 |
| R squared | 0.733 | 0.731 | 0.736 | 0.734 |
| BW | [-5; 5] | [-10; 10] | [-5; 5] | [-10; 10] |
| Mean DV | 0.576 | 0.577 | 0.576 | 0.577 |
| Panel C | Vote Share for Ruling Coalition | | | |
| PostxBanks | 0.110 | 0.0871 | 0.247** | 0.224** |
| | (0.101) | (0.0985) | (0.108) | (0.107) |
| PostxBanksxParty-Stronghold | -0.0326 | -0.0326 | | |
| | (0.0858) | (0.0858) | | |
| PostxBanksxCoalition-Stronghold | | | -0.192** | -0.192** |
| | | | (0.0915) | (0.0914)) |
| Observations | 547 | 564 | 547 | 564 |
| R squared | 0.651 | 0.651 | 0.658 | 0.657 |
| BW | [-5; 5] | [-10; 10] | [-5; 5] | [-10; 10] |
| Mean DV | 0.588 | 0.587 | 0.588 | 0.587 |

Notes: This analysis for this table makes use of the 2009 and 2014 national elections. RD Bandwidth in units of banks per hundred thousand people. District level panel-based difference-in-discontinuities specifications, that include district fixed effects and a dummy for the 2014 election year. $Post = 1$ only for the years after 2016. All specifications restrict the sample around the RD cutoff. Panel A: Dependent variable is vote shares for the ruling party (BJP). Panel B: Dependent variable is the vote share for the ruling coalition (NDA). Standard errors clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.