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 or stir up popular sentiment among voters (Guriev and Papaioannou, 2020). By certain estimates, a quarter of the nations across the world currently have populist governments, often implementing drastic policies that may materially and otherwise hurt the population, with substantial economic cost (Funke et al., 2020). Not infrequently, this may be done with questionable constitutional validity (Ravanilla et al., 2020), packaged in populist rhetoric, bypassing democratic safeguards (Acemoglu et al., 2013), and with autocratic tendencies (Freedom House, 2017). Recent developments in Brazil, India, Turkey, the Philippines and the US reflect such democratic backsliding seen across the world (V-DEM, 2022). Whether leaders continue to implement similar policies in the future partly depends on how voters exercising democratic privileges respond to such actions. Yet, we know little about the electoral effects of such large national-level policies that may adversely affect voter well-being, but are implemented by popular leaders with strong moral messages.

We examine whether voters in the world's largest democracy hold populist politicians accountable for policies that may hurt citizens economically. We derive plausibly exogenous variation from India's nationwide demonetization. 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, appealing 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. This, coupled with the slow printing of new notes, led to a widespread shortage of cash, with reports of unrest and economic hardship in the following months (The Times of India, 2018). 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.[2]

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 the wrong move. 45% of respondents felt it was the right move, while 32% felt that it was the right move, but 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 (Pew, 2017). His messaging framed the policy as combating corruption and black money, which are popular concerns of the electorate.[3] 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,

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

[3] 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 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.*"

and many 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). 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 the national average. We use a regression discontinuity (RD) design, with banks per person as the running variable (Young, 2017; Cramer, 2020). We also use a difference-in-discontinuity design that leverages the panel dimension along with the RD, allowing 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 was indeed a sharp discontinuity in bank branches, accounts, and credit, which persisted through 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 verify using nighttime light intensity data.[4]

Using voter surveys conducted by the Center for the Study of Developing Societies (CSDS) in May 2017, 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.

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.

While ruling party strongholds suffered similar adverse economic effects, they saw no detectable changes in electoral outcomes. This suggests that a meaningful fraction of voters did not change their voting behavior based on this policy, even if their livelihoods were 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), caused by trade shocks (Autor et al., 2020), rainfall shocks (Cole et al., 2012), or austerity measures (Fetzer, 2019).[5] We study the consequences of economic adversities that were directly induced by economic policy, rather than external shocks.

[4]Using nightlights data we see that for 10% fewer bank branches, GDP was lower by 0.4% following demonetization.
[5]Similarly, positive shocks may lead to electoral gains for incumbents (Bagues and Esteve-Volart, 2016).

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; Zimmermann, 2020), but we know less about voter responsiveness to policies that have broad support, but negative economic consequences. Rewarding incumbents for transfers could be in anticipation of implicit reciprocity arrangements (Finan and Schechter, 2012; Shenoy and Zimmermann, 2021). [6]

Our results are consistent with the idea that the more salient the policy, the more responsive voters may be.[7] We find that voters did *not* reward the implementers of the 2005 bank-expansion policy itself in the decade following the expansions.[8] 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.[9]

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.

2 Background

2.1 The Sudden Demonetization and its Subsequent Economic Fallout

The 2016 demonetization was distinct from other episodes:[10] First, a huge fraction (86%) of the currency in circulation was made illegal.[11] Second, it was implemented overnight, with no forewarning to government agencies, legislatures, banks or citizens. The sudden announcement and

[6] 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).

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

[8] 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.

[9] 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).

[10] 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.

[11] 500 and 1000 rupee currency notes (approximately USD $7.5 and $15) were suddenly no longer legal tender.

lack of preparation was costly, given the overwhelming reliance on cash for a majority of economic transactions.[12] New 500 and 2000 rupee notes were to 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.[13] 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; Banerjee and Kala, 2017; Subramaniam, 2019; Wadhwa, 2019; Aggarwal and Narayanan, 2019). [14] 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.

Reactions to the Policy

Several legal challenges were introduced in courts across the country questioning its legality (Kumar, 2016). 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 (CSDS) in May 2017, when asked their opinion on the demonetization policy, 45% of respondents believed that demonetization was "The right move," compared to only 16% who thought it was a "Wrong move". 32% felt that it was the "Right move but with bad preparation". In the following months, the ruling party (BJP) and its allies won many state and local elections around the country.[15] Thus, despite widespread negative media reports and 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.[16] 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 received our 2005 bank-expansion policy, and have significantly greater bank growth in the years preceding 2016's demonetization. This is because the policy from the 1980s is predictive of more banks in 2009, implying that the policy from the 1980s had expanded banking access by 2009. Hence, places that received the 1980s policy were less likely to be the places that received the 2006 policy that we utilize in our paper.

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

[13] Banerjee et al. (2018) summarize these changes.

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

[15] 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).

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

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 bank branches were encouraged to open in 'underbanked' districts. Responses to this policy were concentrated among private banks (Young, 2017). 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. 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).[17]

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.[18] 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 the time of the policy. In India, state elections are held every five years, but in a staggered manner.[19]

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, the 'Mood of the Nation Survey', which was conducted by Lokniti, Centre for the Study of Developing Societies (CSDS), Delhi for ABP News between May 1 and May 15, 2017 among 11,373 respondents spread across 19 States of India.[20] The survey was conducted at 584 locations in 146 Assembly Constituencies (ACs), spread across Parliamentary Constituencies (PCs).

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

[18] 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.

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

[20] Andhra Pradesh, Assam, Bihar, Chhattisgarh, Delhi, Gujarat, Haryana, Jharkhand, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Odisha, Punjab, Rajasthan, Tamil Nadu, Telangana, Uttar Pradesh, and West Bengal.

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.

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. Using a difference-in-discontinuities design and exploiting the panel dimension, we estimate:

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

Here $Post_t$ is an indicator for $t > November\ 8, 2016$ (when demonetization was unexpectedly announced). $ReceivedBanks_d = 1$ for underbanked districts: those eligible for the policy due to the discontinuity produced by the bank expansion policy, and so less severely affected by demonetization. The small bandwidth around the cutoff ensures we compare similar districts, while γ_d district fixed effects control for district characteristics. The parameter of interest, δ, 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:

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

where, Y_{dt} is an outcome in district d, and $f(Banks\ per\ cap_d)$ 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.

Given that various factors, such as caste, religion, or identity may impact voting patterns, 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,

accompanied by growth in bank branches.

Table 1 and Figure 1 show effects on private-sector bank branches and growth, and 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 by 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). These results are consistent with Young (2017) and Cramer (2020). Figures A.1, 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

We verify that the effects on economic activity using our differences-in-discontinuity design are consistent with Chodorow-Reich et al. (2019). Panel A of Table 2 shows that there was no discontinuity in lights just before demonetization, but places that received banks were likely to have a 9 percent higher nighttime light intensity than places that did not, in the months following demonetization (also, Figure A.5e). Combining these results with Table 1 on bank branches, we calculate that a 10% decrease in branches was associated with a 1.33% decrease in luminosity.[21] Using an elasticity of 0.3 to translate luminosity to GDP (Henderson et al., 2012; Chodorow-Reich et al., 2019), implies a 0.4% 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. Figure A.2 shows the correlates of support for demonetization and the Prime Minister, respectively. 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 the 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).

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. These are confirmed by RD graphs in Figure 2a and Figure A.5d.

[21] From Panel B, Table 1, the differential growth in bank branches was 0.737 log points. The differential growth in luminosity (Panel of A, Table 2 was 0.0977 log points. This implies a 10% differential growth in bank branches was associated with a $(0.0977/0.737) \times 10\% = 1.33\%$ differential growth in luminosity.

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.

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.[22] 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 Figure A.5f, we show the corresponding RD figures for vote shares, and probabilities of victory for the ruling party and coalition. In Figures 2e and 2f, 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. There were no detectable electoral effects before demonetization. Following demonetization, the RD coefficient rises substantially, showing the ruling party experienced relatively higher (lower) vote shares in areas with more (fewer) banks.

Alternative Specifications and Robustness

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 2e and 2f 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), 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 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. The results are also robust to including other fixed effects (Table A.8), in particular, district and

[22] 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.

state-by-year fixed effects, as well as constituency and year fixed effects.

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.10 we re-do our main analysis including constituencies where the parties did not field candidates, now coded as zeros, and our results remain similar.

We also examine whether incumbents were less likely to contest elections in more severely affected areas. In Figure A.8 we fail to detect any change in the incumbent turnover rate following demonetization.

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 were implemented across the country in a manner unrelated to our bank expansion cutoff.

6 Drivers of the Electoral Effects & Political Strongholds

The average effects shown above may hide substantial heterogeneity. We explore why certain voters may have responded to the policy, and why others did not, despite 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.[23] Table 3 shows that voters in BJP or NDA strongholds were unlikely to change their voting behavior or affect the winning probabilities, when faced with more severe demonetization.[24]

The estimates in Panel A of Table A.20, using data from the 2009, 2014 and 2019 national elections, while noisy, have the expected sign. Panels B and C illustrate that heterogeneous effects of national elections 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, Vote Buying, and Voter Awareness

The ruling party may have exhibited favoritism or targeted economic relief to less-banked areas, mitigating economic adversities (Asher and Novosad, 2017; Mahadevan, 2020). We find little evidence in support of this. Table A.14 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.13). Regardless of whether they resided in strongholds, districts with fewer banks were adversely affected and had a less favorable view of demonetization.

We also directly examine whether targeted transfers were made to worse hit areas. The largest transfer program that can be targeted is MNREGS. In 2018 it was 6% of discretionary expenditure.

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

[24] The coefficient on the interaction term in Table 3 should be interpreted cautiously as 'stronghold' status is not randomly assigned. Table A.12 shows that religiosity may be the only meaningful correlate of stronghold status. We show in Table A.18 that our RD balance tests are unrelated to stronghold areas, implying that stronghold voters on either side of the RD cutoff are similar.

The only programs that were larger are the food and fertilizer subsidy schemes, both of which are difficult to target to specific constituencies. We obtain monthly data for persons worked and households worked under the MNREGS program. We compute the level and inverse hyperbolic sine of days worked. In Table A.15, we are unable to detect any differential impact of demonetization on MNREGS take up in unbanked regions. In subsequent panels below that, we look at strongholds and non-strongholds separately and are still unable to detect any such impact.

Along similar lines, we may expect that perhaps lending or banking access was improved in worse hit areas. In Table A.16 we find no detectable impact on credit or number of accounts. Nonetheless, we cannot rule out the possibility that there may have been other types of targeted transfers as well.

Another reason for the patterns in stronghold areas might be explained by the ease of campaigning, vote buying or clientelism, 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 views on demonetization also makes this unlikely to have occurred.

Strongholds may also have better targeted messaging to the voter base on the merits of the policy, and who was responsible for the consequences. Yet, the policy was highly salient, and the Prime Minister, in his national TV addresses, regularly made it clear that he was the main architect. Importantly, the evidence in Table A.13 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 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

Voters may also misattribute credit/blame to incumbents, given a lack of awareness on who was responsible for their economic circumstances (Guiteras and Mobarak, 2014; Bertrand and Mullainathan, 2001). 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 and Table A.19).

7 Conclusion

We analyze the electoral consequences of a popular policy, which, as we and others show, had negative economic effects. We find that in this context, voters on average do hold politicians accountable for policies that hurt citizens. Yet, there were no electoral consequences in parts of the country that were the politician's ideological strongholds.

As the policy was implemented on the same day nation-wide, it is challenging to isolate the effects of demonetization from other secular trends. We overcome these challenges, leveraging a a district-level discontinuity in banking access. 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. In the absence of well-identified variation in banking, the media concluded that demonetization was not punished by any part of the voter base (The Indian Express, 2017). Our causal analysis shows that the electoral consequences were more nuanced. While, on average, voters that faced more severe demonetization were less likely to vote for the implementing party, voters were unresponsive in stronghold areas.

There has been a rising trend in both populism and democratic backsliding in major nations across the world, like the US, Brazil, Turkey, India and the Philippines (V-DEM, 2022). The results of our current analysis have wider implications for contexts where populist leaders garner support while hurting the population economically.[25] Indeed, recent political developments in the US and Brazil suggest that voters may punish populist leaders for policy mismanagement, but such leaders still continue to maintain strong electoral support in ideological strongholds. Such electoral responses may, in turn, have implications for how enduring this trend in populism is.

[25] Examples may include support among parts of the population in Russia for the war in Ukraine despite significant negative economic consequences, and COVID-19 policy mismanagement in some countries.

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Table 1: The 2005 Banking Policy Improved Banking Access

Panel A	P(Unbanked Status)			ΔLog(Branches)
Received Banks	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)			
Received Banks	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 DV	1.751	1.755	1.761	1.761
Specification	MSE2	CER2	Linear	Quadratic
Panel C	Log(Old Branches) – Falsification Test			
Received Banks	-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 DV	0.391	0.396	0.363	0.363
Specification	MSE2	CER2	Linear	Quadratic
Panel D	District-Level Total Credit (2015)			
Received Banks	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 DV	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	
Received Banks	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 DV	.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	
Received Banks	-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
Bandwidth	[-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	
Received Banks	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
Bandwidth	[-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	10,633	11,220	9,021	9,465
R squared	0.662	0.660	0.520	0.515
Bandwidth	[-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	
PostxBanks	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
Bandwidth	[-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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

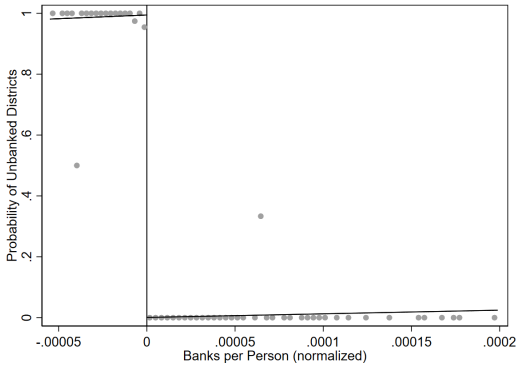
Table 3: Political Strongholds are Electorally Less Responsive

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
Bandwidth	[-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
Bandwidth	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.318	0.318	0.318	0.318
Panel C	Prob (winning) for Ruling Party			
PostxBanks	0.302***	0.318***	0.297***	0.323***
	(0.098)	(0.0889)	(0.0788)	(0.0713)
PostxBanksxParty-Stronghold	-0.294***	-0.353***		
	(0.0963)	(0.0908)		
PostxBanksxCoalition-Stronghold			-0.203*	-0.245**
			(0.107)	(0.100)
Observations	8,705	9,130	8,705	9,130
R squared	0.352	0.349	0.346	0.343
Bandwidth	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.332	0.331	0.332	0.331
Panel D	Prob (winning) for Ruling Coalition			
PostxBanks	0.301***	0.320***	0.335***	0.371***
	(0.098)	(0.0889)	(0.0788)	(0.0713)
PostxBanksxParty-Stronghold	-0.388***	-0.449***		
	(0.0886)	(0.0822)		
PostxBanksxCoalition-Stronghold			-0.236**	-0.258***
			(0.1000)	(0.0919)
Observations	10,289	10,853	10,289	10,853
R squared	0.274	0.268	0.271	0.265
Bandwidth	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.377	0.375	0.377	0.375

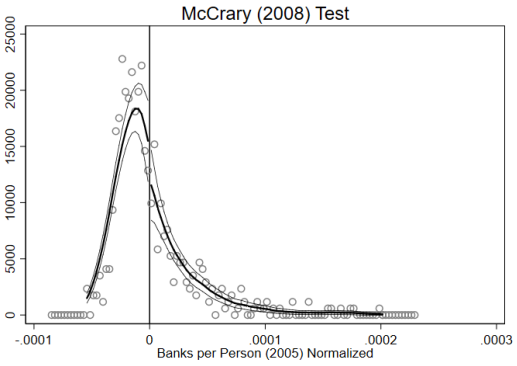
Notes: RD bandwidth in units of banks per hundred thousand people. District level panel-based difference-in-discontinuities specifications, that include district and year fixed effects. $Post = 1$ only for the years after 2016. All specifications restrict the sample around the RD cutoff. Panel A: Dependent variable is vote shares for the ruling party (BJP). Panel B: Dependent variable is the vote share for the ruling coalition (NDA). Panel C: Dependent variable is probability of victory for the ruling party. Panel D: Dependent variable is probability of victory for the ruling coalition. 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

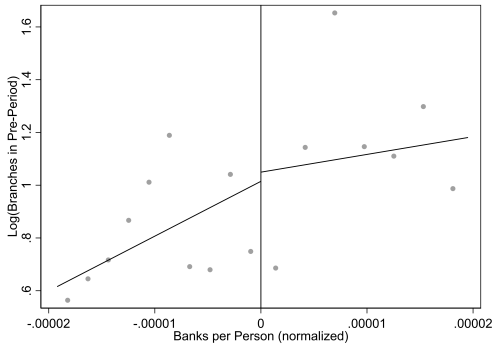
(a) Compliance With Policy Rule



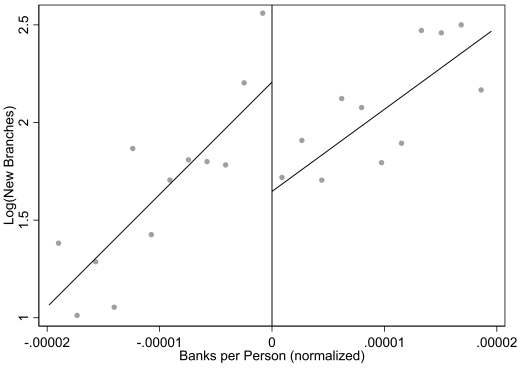
(b) District Density at RD Cutoff



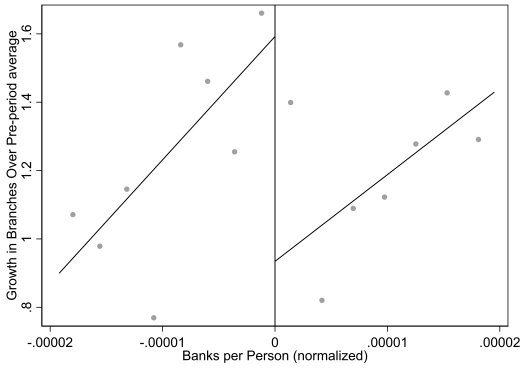
(c) Number of Old Branches (Placebo)



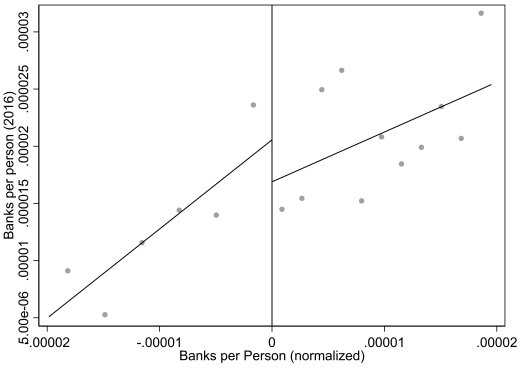
(d) New branches



(e) Growth in Branches



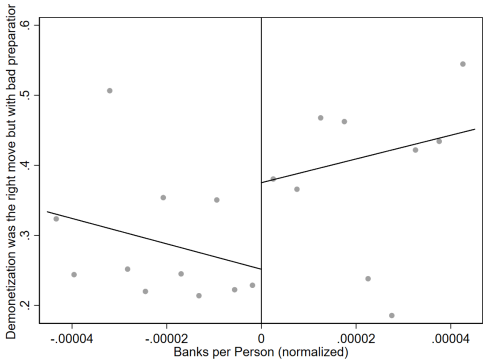
(f) Number of Branches per cap (2016)



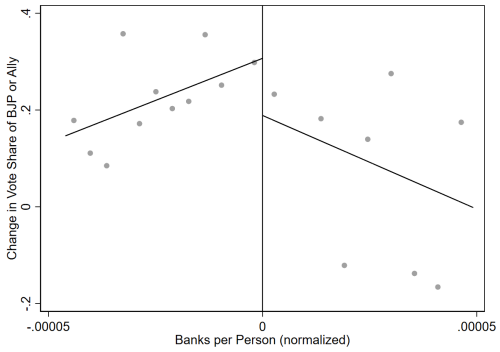
Notes: Top panel graphs show the first stage (Fig 1a), and density of districts (Fig 1b) at the cutoff as in the McCrary (2008) Test. Subsequent graphs show the effect of unbanked status on private-sector bank branches and growth. Figure 1c plots the old branches in the four years preceding the policy. 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

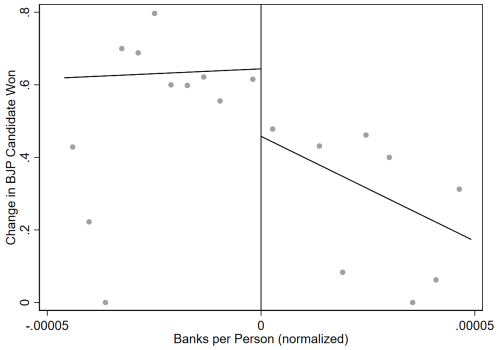
(a) Demonetization had Bad Preparation



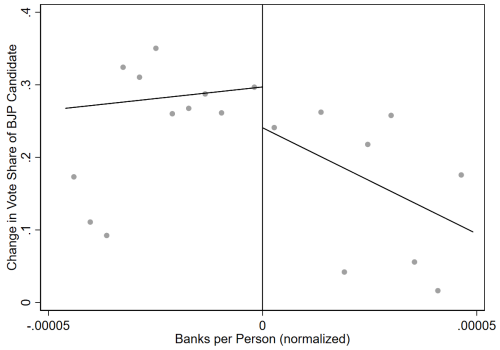
(b) ΔVote Shares (Ruling Coalition)



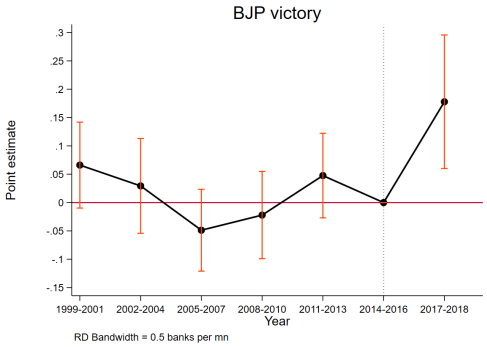
(c) Change in P(Ruling Party Victory)



(d) ΔVote Shares (Ruling Party)



(e) P(Ruling Party Victory) Over Time



(f) Vote Share by Year (Ruling Party)

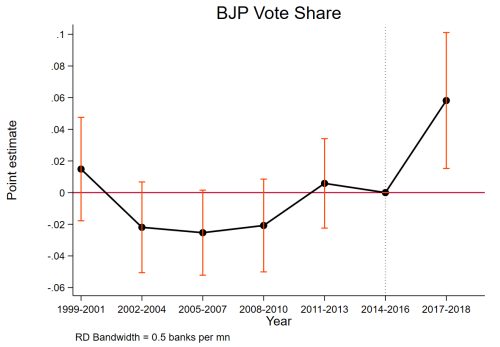
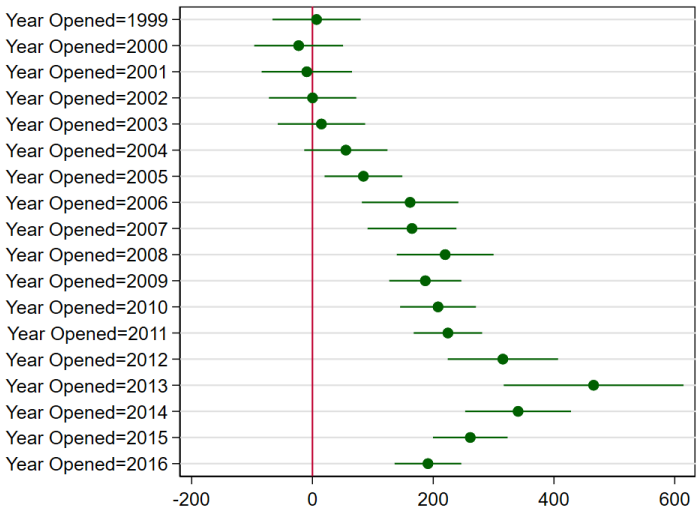


Figure 2a use household level CSDS and show the effect of unbanked status around the RD cutoff on views on demonetization, where the outcome is an indicator for whether or not the respondent thought demonetization was the right move, but badly implemented. Figure 2b shows the change in the vote shares 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, and plot 95% confidence intervals. See Appendix Figure A.5 for more robustness checks.

ONLINE APPENDIX

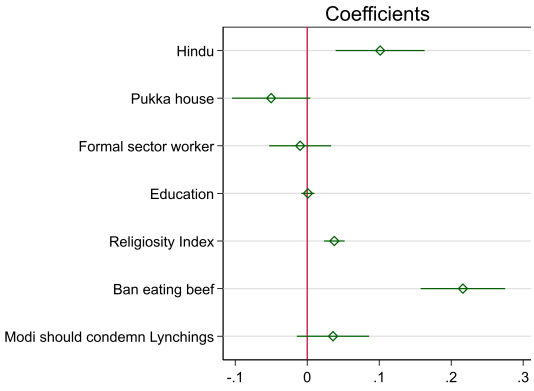
Figure A.1: New Branches over Time



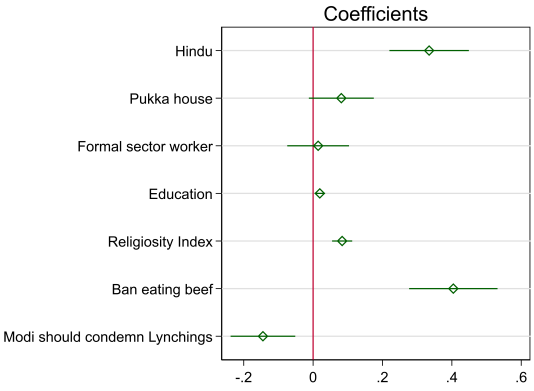
This figure shows the coefficient on the the number of new branches opened each year relative to 1998. The policy was implemented in 2005.

Figure A.2: Correlates of Support For:

(a) Demonetization



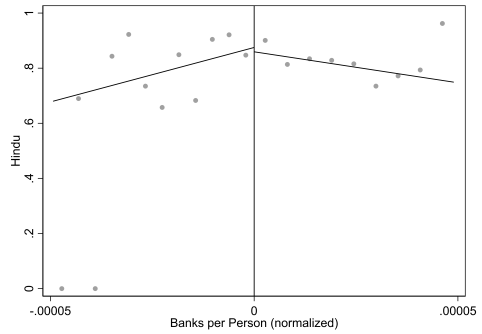
(b) Prime Minister



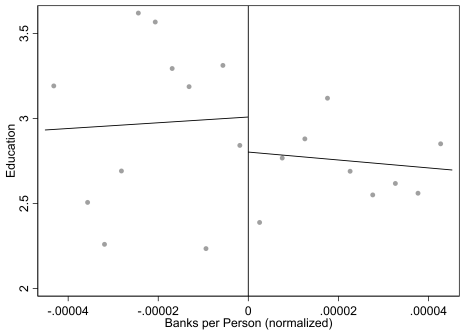
Panel (a) plots the coefficients for the correlates of support for the demonetization policy, while Panel (b) plots the coeffcients for the correlates of for support for the Prime Minister, Mr Modi. The data comes from the CSDS voter surveys. 95% Confidence intervals are shown.

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

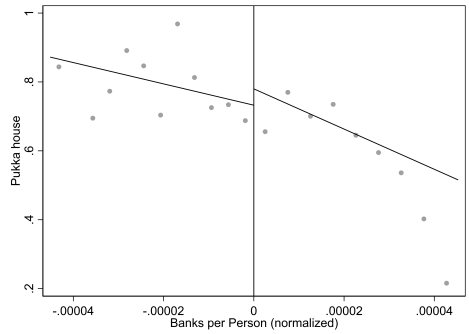
(a) Hindu



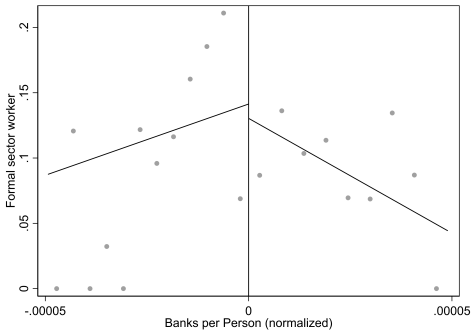
(b) Education



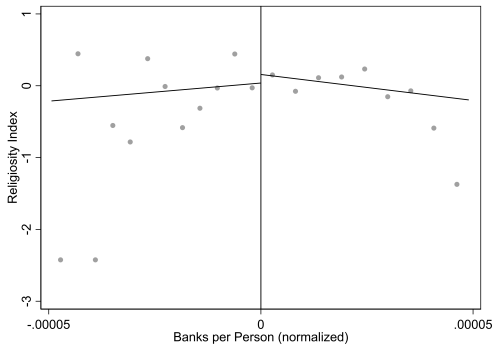
(c) Pukka House



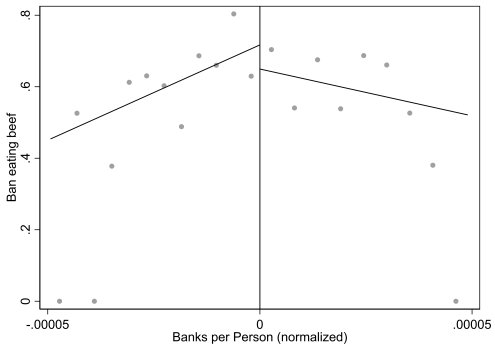
(d) Formal Sector Worker



(e) Religiosity



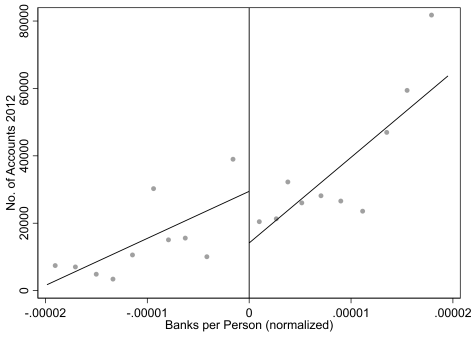
(f) Supports Beef Ban



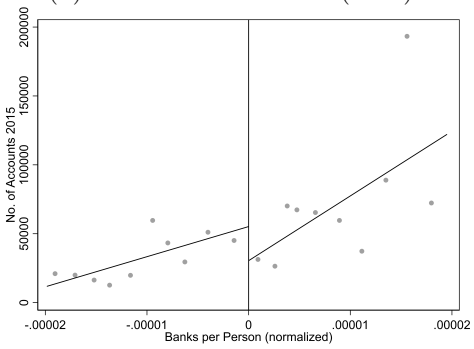
Graphs show the relationship between unbanked status and individual characteristics using the CSDS Mood of the Nation survey.

Figure A.4: Number of Accounts and Credit

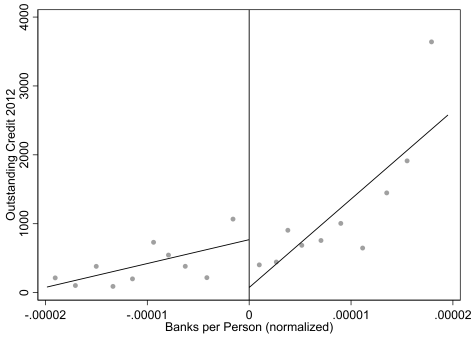
(a) Number of Accounts (2012)



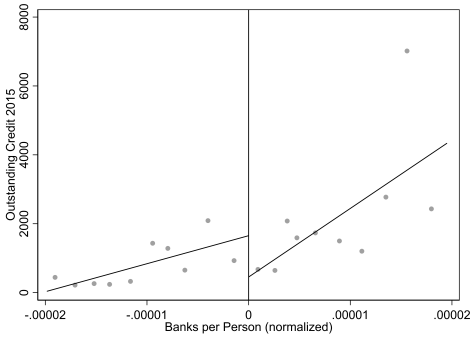
(b) Number of Accounts (2015)



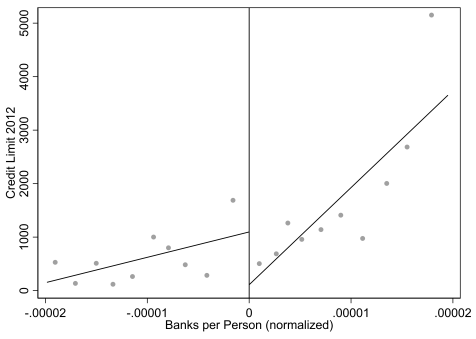
(c) Outstanding Credit (2012)



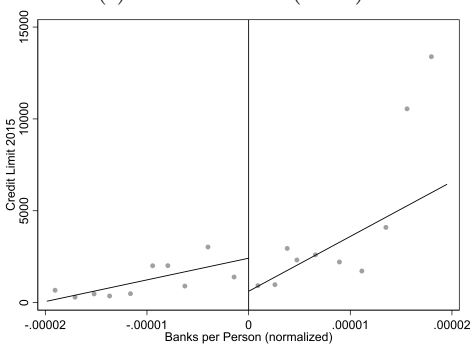
(d) Outstanding Credit (2015)



(e) Credit Limit (2012)



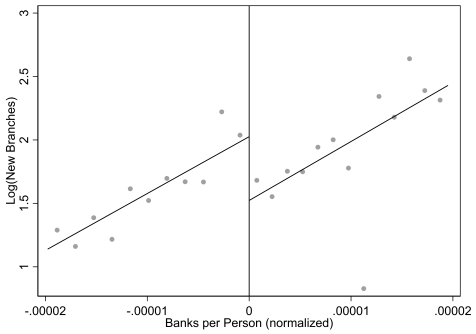
(f) Credit Limit (2015)



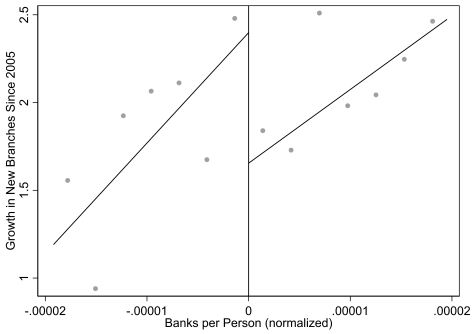
Graphs show the effect of unbanked status on number of accounts, credit limits and outstanding credit. Figure and Figure show the number of bank accounts in 2012 and 2015. Figure and Figure show the amount of outstanding credit (ten million rupees) in 2012 and 2015. Figure and show the total credit limit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

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

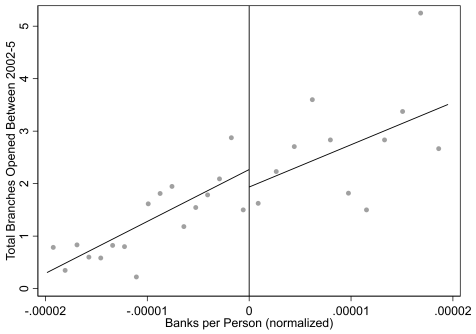
(a) New branches (2006-10): Aggregate



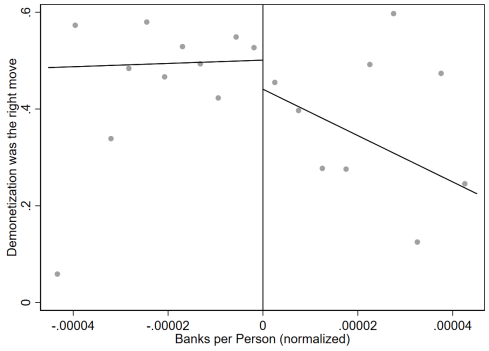
(b) Growth in branches since 2005



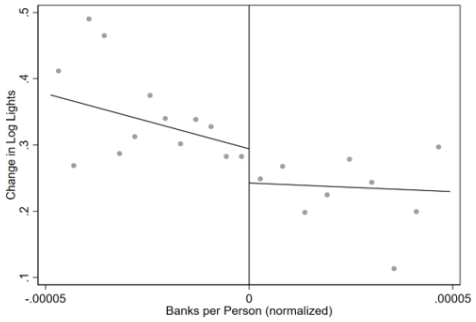
(c) Old Branches 2001-05



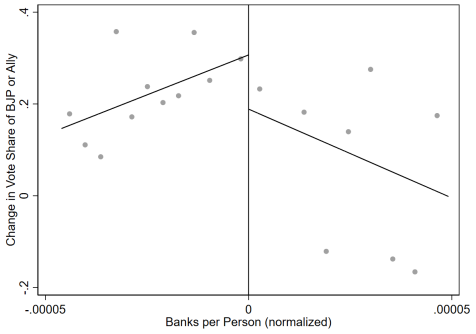
(d) Demonetization was the Right Move



(e) Changes in Nightlights

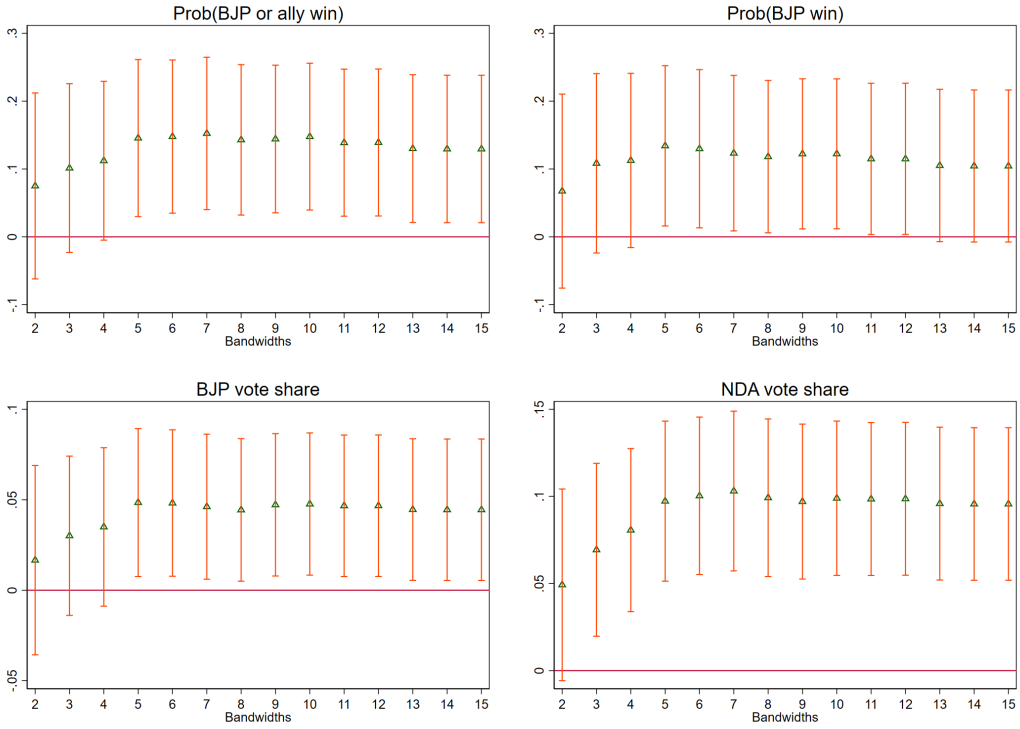


(f) Changes in Vote Share for Ruling Coalition



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.

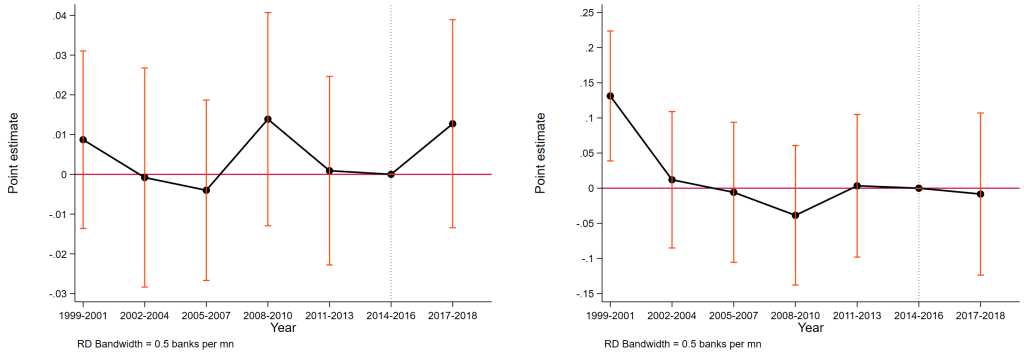
Figure A.6: Sensitivity to Bandwidths



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.

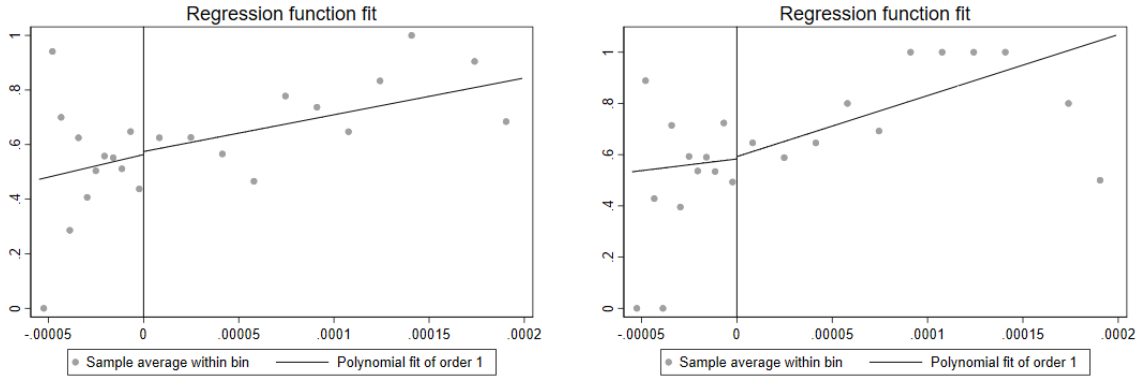
Figure A.7: Dynamic Effects on Incumbent Votes

(a) Incumbent Vote Shares (b) Incumbent Prob(Winning)



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.

Figure A.8: Fraction of Incumbents who Recontested Elections



(a) Fraction Recontested (All Parties) (b) Fraction Recontested (BJP)

Notes: Figures plot the fraction of incumbents that recontested elections after demonetization. That is, the outcome is the probability that an incumbent recontests an election in the years 2017 and 2018.

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

Panel A		Number of Accounts		
Received Banks	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 DV	44469.285	48547.612	44139	44139
Specification	MSE2	CER2	Linear	Quadratic
Panel B		Total Credit Limit		
Received Banks	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 DV	1628.501	1876.49	2111	2111
Specification	MSE2	CER2	Linear	Quadratic
Panel C		Total Credit Outstanding		
Received Banks	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 DV	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)			
Received Banks	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 DV	1.807	1.778	1.667	1.667
Specification	MSE2	CER2	Linear	Quadratic
Panel B	Number of Accounts			
Received Banks	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 DV	19844.712	22578.383	21589	21589
Specification	MSE2	CER2	Linear	Quadratic
Panel C	Total Credit Limit			
Received Banks	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 DV	658.03	766.321	938.6	938.6
Specification	MSE2	CER2	Linear	Quadratic
Panel D	Total Credit Outstanding			
Received Banks	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 DV	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)		
Received Banks	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 DV	1.941	1.932	1.725	1.725
Specification	MSE2	CER2	Linear	Quadratic
Panel B		ΔLog(New Branches)		
Received Banks	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 DV	2.021	2.042	1.912	1.912
Specification	MSE2	CER2	Linear	Quadratic
Panel C		Log(Old Branches)		
Received Banks	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 DV	.436	.392	0.349	0.349
Specification	MSE2	CER2	Linear	Quadratic
Panel D		ΔLog(Old Branches)		
Received Banks	-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 DV	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)			
	Ruling Party		Ruling Coalition	
Received Banks	-0.0880	0.00739	-0.0372	-0.0155
	(0.0645)	(0.0416)	(0.0449)	(0.0380)
Bandwidth Type	MSE1	MSE2	MSE1	MSE2
Robust p-value	0.230	0.891	0.297	0.559
Bandwidth	[-.7 ;.7]	[-2.6 ;1]	[-1.2 ;1.2]	[-2.2 ;1.4]
	Vote Shares			
	Ruling Party		Ruling Coalition	
Received Banks	-0.0315	0.0215	0.00726	0.0158
	(0.0214)	(0.0171)	(0.0219)	(0.0153)
Bandwidth Type	MSE1	MSE2	MSE1	MSE2
Robust p-value	0.120	0.320	0.993	0.581
Bandwidth	[-.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	Ruling Coalition		Ruling Party	
Received Banks	0.113***	0.103***	0.101***	0.0930**
	(0.0334)	(0.0285)	(0.0307)	(0.0397)
Bandwidth Type	MSE1	MSE2	MSE1	MSE2
Robust p-value	0.003	0.002	0.001	0.010
Bandwidth	[-.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
Bandwidth	[-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: Electoral Effects: Robustness to State-by-Year Fixed Effects and Constituency Fixed Effects

	Vote Shares: Difference in Discontinuities			
Panel A: Including State-by-Year Fixed Effects	Ruling Party		Ruling Coalition	
PostXBanks	0.0197**	0.0186**	0.0246**	0.0195*
	(0.00880)	(0.00825)	(0.0104)	(0.0103)
Observations	9,013	9,455	10,625	11,211
R-squared	0.753	0.750	0.605	0.605
Bandwidth	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.267	0.267	0.319	0.319

	Vote Shares: Difference in Discontinuities			
Panel B: Including Constituency Fixed Effects	Ruling Party		Ruling Coalition	
PostXBanks	0.0611**	0.0553**	0.0918***	0.0927***
	(0.0274)	(0.0265)	(0.0273)	(0.0262)
Observations	7,492	7,834	9,251	9,736
R-squared	0.779	0.781	0.689	0.688
Bandwidth	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	0.278	0.278	0.324	0.325

Notes: The Ruling Party here refers to the ruling party in the central (federal) government - that is, the prime minister's party, which is the BJP. The Ruling Coalition here refers to the set of parties allied with the ruling party in the central (federal) government. Panel A includes district and state by year fixed effects. Panel B includes constituency and year fixed effects. Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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

	Vote Shares: Difference in Discontinuities			
Panel A: 2015 Cutoff Year	Ruling Party		Ruling Coalition	
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
Bandwidth	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
	Vote Shares: Difference in Discontinuities			
Panel B: 2014 Cutoff Year	Ruling Party		Ruling Coalition	
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. In 2015 only Delhi had by-elections, and none of their constituencies were in the RD bandwidth. As a result, the 2015 elections are redundant for this exercise, and so the 2014 and 2015 placebo cutoff have the same results. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.10: Prob(Winning) Including 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)
Observations	12,078	12,756	12,078	12,756
R-squared	0.351	0.349	0.289	0.284
Bandwidth	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	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: Change in Incumbent Turnover Rate

	Fraction of Incumbents Recontested					
Received Banks	-0.0249	-0.301	0.342	-0.139	-0.138	-0.153
	(0.159)	(0.224)	(0.289)	(0.137)	(0.206)	(0.173)
Observations	234	123	75	664	285	223
RD	Parametric			CCT		
Bandwidth	[-.5; .5]	[-.5; .5]	[-.5; .5]	[-1.3 ;1.3]	[-1.3 ;1.3]	[-1.3 ;1.3]
Party	All	BJP	INC	All	BJP	INC
Mean DV	0.513	0.513	0.513	0.513	0.513	0.513

Notes: Dependent variable is the probability that an incumbent recontests an election in the years 2017 and 2018. 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). First three columns use a parametric RD, and the last three columns use the CCT method. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.12: 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.13: 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
Bandwidth	[-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.14: 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
Bandwidth	[-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.15: Impacts on MNREGS Days Worked

Full Sample	Households worked		Persons worked		Households worked (IHS)		Persons worked (IHS)	
Post*Banked	-131.8	-145.9	-6,281	-9,100	0.0814	0.124	0.083	0.132
	(1,148)	(1,071)	(17,283)	(16,158)	(0.100)	(0.102)	(0.121)	(0.123)
Observations	6,526	6,864	6,526	6,864	6,526	6,864	6,526	6,864
R-squared	0.789	0.8	0.617	0.62	0.672	0.656	0.649	0.629
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	25194	25144	327586	322382	9.567	9.546	12.11	12.08
Strongholds	Households worked		Persons worked		Households worked (IHS)		Persons worked (IHS)	
Post*Banked	543.5	535.3	-3,634	-4,944	0.126	0.103	0.123	0.0984
	(971.6)	(926.9)	(16,786)	(15,922)	(0.0905)	(0.0862)	(0.113)	(0.106)
Observations	3,939	4,095	3,939	4,095	3,939	4,095	3,939	4,095
R-squared	0.697	0.750	0.594	0.598	0.724	0.722	0.695	0.691
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	15293	15562	242719	241056	9.513	9.509	12.19	12.18
Non Strongholds	Households worked		Persons worked		Households worked (IHS)		Persons worked (IHS)	
Post*Banked	-2,086	-2,159	-21,832	-28,156	-0.110	0.0178	-0.115	0.0275
	(2,380)	(2,186)	(34,333)	(31,650)	(0.202)	(0.206)	(0.242)	(0.249)
Observations	2,522	2,704	2,522	2,704	2,522	2,704	2,522	2,704
R-squared	0.794	0.800	0.615	0.618	0.597	0.578	0.561	0.539
Bandwidth	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	41235	40192	467744	452515	9.817	9.756	12.19	12.11

Notes: Results show the impact of demonetization severity on MNREGS households and persons worked. Last two columns show inverse hyperbolic sine (IHS) transformations. Middle panel is for the subset of BJP strongholds and bottom panel for non-strongholds. Data were accessed from the official MNREGS data portal on June 26, 2022: https://mMNREGSweb4.nic.in/. Standard errors clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.16: Impacts on Credit and Accounts After Demonetization

	No. of Accounts		Credit Limit (Crores)	
Post*Banked	-2,210	-3,148	-1,308	-1,173
	(2,953)	(3,435)	(1,058)	(963.9)
Observations	1,476	1,547	1,476	1,547
R-squared	0.973	0.984	0.830	0.902
Bandwidth	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	49327	56156	2281	2898

Notes: Results show the impact of demonetization severity on credit and number of bank accounts from the banking data. Credit Limit is in rupee crores. Unit of observations is district-year. Standard errors clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.17: 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.18: 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			
Received Banks	-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			
Received Banks	-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
Bandwidth	[-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			
Received Banks	-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			
Received Banks	-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
Bandwidth	[-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.19: Impact on Turnout by Stronghold Status

	Log(Turnout)			
PostxBanks	0.0810	0.0730	0.0531	0.0487
	(0.0601)	(0.0626)	(0.0393)	(0.0421)
PostxBanksxCoalition-Stronghold	-0.0802	-0.0727		
	(0.0614)	(0.0617)		
PostxBanksxParty-Stronghold			-0.0608	-0.0534
			(0.0465)	(0.0464)
BW	[-5; 5]	[-10; 10]	[-5; 5]	[-10; 10]
Mean DV	11.60	11.57	11.60	11.57

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. Standard errors clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.20: 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
Bandwidth	[-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
Bandwidth	[-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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.