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

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Abstract

We analyze the electoral consequences of India's 2016 'demonetization': a unique policy that unexpectedly made 86% of the currency-in-circulation redundant overnight, and led to severe cash shortages in the subsequent months. We leverage a discontinuity in the number of bank branches arising from a nationwide district-level bank expansion policy, instituted by the previous government in 2005. We first document that the impacts of the branch-expansion policy around the cut-off were meaningful: areas just above the cut-off had fewer bank branches, less outstanding credit, and households were less likely to report having bank accounts. Importantly, these effects strongly persisted in 2016, when the demonetization policy was instituted. As districts with fewer banks had greater cash shortages, we identify the impacts of demonetization at the bank-expansion cut-off. Regression discontinuity estimates show that following demonetization, places with fewer banks had lower economic activity, as measured by nighttime lights, and voters reported having less favorable views on demonetization. Using electoral data and a difference-in-discontinuity design, we find that in elections following demonetization, the ruling party did relatively worse in regions with discontinuously fewer banks, receiving a 4.7 percentage point lower fraction of votes. We finally show that voters that were historically strongly aligned with the ruling party, are nearly unresponsive in voting behavior, despite having a less favorable view of the policy itself.

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1 Introduction

How, and to what extent, do voters respond to policies that are associated with negative economic shocks? Such electoral responses are likely to depend on who is directly affected (Fujiwara et al., 2019), the salience and information available to voters (Ferraz and Finan, 2011), who is assigned credit or blame (Guiteras and Mobarak, 2014), and the extent of financial safety nets available (Cole et al., 2012). We analyze how voter behavior and electoral outcomes responded to an unexpected, salient policy, directly attributable to the ruling party, and differentially affecting voters based on their access to banking-sector safety nets. With the goal of tackling the black economy and counterfeit cash, India's 2016 demonetization made 86 percent of the cash held in the economy redundant overnight. While recent work (Chodorow-Reich et al., 2019) credibly shows the substantial detrimental effects of this policy on the country's economy, little is known about whether and how such government policies affect voter behavior and electoral outcomes.

Even though demonetization has been implemented in several countries over the last few decades, we examine a unique case of demonetization. The uniqueness stems from the fact that the policy was implemented suddenly, while impacting almost all transactions in the country.[1] On November 8, 2016, the Prime Minister unexpectedly announced over a televised address that, starting at midnight, two commonly used denominations of currency notes would cease to be legal tender, to stem the flow of counterfeit notes and hinder the black economy. New currency notes were to be provided in exchange for the old notes at bank branches, but with significant limits placed on how much one could withdraw. These constraints, coupled with the slow printing of new notes, led to a widespread shortage of cash across the country. There were reports of unrest and economic hardship in the weeks and months following the announcement (The Times of India, 2018; Lahiri, 2019).

In this paper, we analyze the effects of the demonetization policy on electoral outcomes and voter behavior, by exploiting a discontinuity in the number of bank branches in districts across the country that arose due to a bank-expansion policy instituted in 2005. We isolate the effect of access to cash (or a the lack thereof), by leveraging variation in the bank expansion policy implemented by the previous government (later the opposition party). The policy was targeted to all districts that had banks per capita below a certain cutoff. To isolate the effects of demonetization severity on political outcomes, we use a regression discontinuity (RD) design, with the banks per person as the running variable. We examine the discontinuity over time, and compare electoral outcomes before and after demonetization. This also allows us to account for differences in outcomes before demonetization, which may exist due to the direct effect on voter behavior of the bank branch expansion policy. We therefore use a difference-in-discontinuities design, and exploit the panel dimension of our data along with the RD.

We first document that the bank branch policy was well enforced, that there was high compliance for that policy around the cutoff, and that there was indeed a sharp discontinuity in the density of bank branches, in the number of bank accounts, and the amount of outstanding credit, as a result of the 2005 bank branch expansion policy, which persisted through to 2016. We first verify

[1] According to a survey conducted by MasterCard and Tufts University, 87% of all transactions were conducted in cash as of 2015, while around 86% of the currency that was in circulation was suddenly and unexpectedly deemed illegal overnight.

that we can replicate the result in (Chodorow-Reich et al., 2019) on the fall in economic activity due to demonetization, using our identification strategy. We find that in months that followed demonetization, areas that had discontinuously fewer banks indeed had a sharper drop in nighttime light intensity – a measure of economic activity commonly used in the literature. For districts that had 10% fewer bank branches, GDP was lower by 0.5% in the months following demonetization.

Before looking at the electoral impacts of demonetization, we first check whether the bank branch policy itself affected elections in the pre-demonetization period. We may expect voters to reward the party that rolled out the banking expansion, but find no such effects in the pre-demonetization period. Such bank expansions may be gradual, and voters may find it difficult to attribute the policy to the specific party, highlighting the importance of salience and information in voter responses.

Yet, with the sudden demonetization policy, with its widespread media coverage and salience in daily economic life, voters had a better sense of who was responsible. Using voter-level surveys conducted in the months following the demonetization, we explore whether opinions on the policy differed by access to a bank branch, and thereby easier access to cash. We find that regions with more banks were indeed more likely to have survey respondents that thought the policy was the right move, and were less likely to say that the policy was poorly implemented.

Next, we explore the electoral impacts for the ruling party and its allies, as a result of the demonetization. We find that greater access to banks (and therefore, access to cash), the ruling party did relatively better in regions with more bank branches: the ruling coalition received a 4.7 percentage point lower fraction of the votes in regions that had discontinuously fewer bank branches, and where demonetization was more severe. Our results suggest that a 10% decrease in the number of new bank branches translates into about a 0.9 percentage point decrease in the vote share for the ruling party after demonetization. This suggests that more bank branches mitigated the severity of the effects of demonetization, and somewhat protected the population that now had access to banking safety nets when faced with monetary-driven economic shocks. What is quite striking is that the previous ruling party's banking expansion program *helped* the current ruling party not lose its vote share.

While there is a growing literature on the electoral consequences of targeted fiscal transfers, not much is known about the electoral consequences of large-scale changes to monetary policy. For instance, Manacorda et al. (2011) show that large transfer programs in Uruguay shift beneficiary household political preferences to favor the current government, just as conditional cash-transfers in Mexico (under Progresa) increase pro-incumbent voting (De La O, 2013). Importantly, the extent of safety nets (like access to banks) may also affect voters directly exposed to economic shocks (Cole et al., 2012). The findings in these papers are consistent with theories of rational voters, who, if well informed reward incumbents, which could be in anticipation of continued transfers in the future, or due to implicit reciprocity arrangements (Finan and Schechter, 2012).[2]

Yet, information and salience may matter: we find that voters in India did not reward the implementers of the gradual, less-salient banking expansion, but did respond to the salient demonetization

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

policy, clearly attributable to the current ruling party. Consistent with our findings, in Bangladesh, Guiteras and Mobarak (2014) show that while voters initially reward local politicians for development projects conducted by NGOs, they rationally update when given accurate information about the implementation of the project. Indeed, knowing the importance of salience, politicians may respond to how much information is available to voters (Ferraz and Finan, 2011), and implement transfer policies specifically around elections (Cole, 2009).

A key feature of many democracies, include that of India, is that citizens typically get one vote. So during elections, it is very likely that voters make decisions over a bundle of issues. Even if an issue is highly salient and has a negative economic impact, voters that are more strongly aligned with the party, may not be as responsive electorally with respect to it (Besley and Coate, 2008, 2003; Iversen and Goplerud, 2018).[3] To investigate this, we look to see if there are heterogeneous effects with respect to those regions that have been strongholds for the ruling party or allies. We find evidence to suggest that in ruling party strongholds, the electoral impacts of the demonetization were absent, *despite* the fact that in the voter survey data, individuals in these areas do not have more favorable views of the policy itself. Taken together, our results suggest that in democracies where voters get one chance to vote, and where the policy space is multi-dimensional, voters who align more closely with a particular politician (or party), are likely to be less responsive to any specific policy, however salient it may be.

The remainder of the paper is organized 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 used in the analysis. Section 4 explains our empirical strategy, Section 5 discusses the results, and Section 6 concludes with a brief discussion.

2 Background

2.1 India's Demonetization

Demonetization is the act of rendering one or more units of a currency illegal as tender. It is usually accompanied by the replacement of the outdated currency units with new notes or coins. This type of policy was implemented from time to time in several around the world.[4] The primary reasons cited in most cases included fighting the black economy, reducing corruption and counterfeit currency. India has seen demonetization being implemented four times, including the one in 2016.[5]

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

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

[5]The first occurred towards the end of Colonial rule in 1946, when less that 10 percent of the cash in circulation in the country at that time was declared illegal as tender, presumable affecting a small fraction of the population and economy. The second was in the princely state of Hyderabad in 1953, while attempting to integrate the state into the rest of the country's economy. While the cash in Hyderabad was impacted, the policy did not take effect

On November 8, 2016, the Prime Minister of India, declared that the 500 and 1000 rupees,[6] would cease to be legal tender, and that new 500 and 2000 rupee notes would be issued over time. Individuals could deposit the old notes in banks, but not conduct any transactions using these, and all old notes were required to be deposited by December 31 in exchange for the new notes.[7] In addition, only 4000 rupees per person could be drawn per day. The stated objectives of this policy was to curtail the shadow economy, fight terrorism, and tackle counterfeit currency. The stock market crashed the day after the policy was announced, and in the following weeks and months, there was a sharp decline in the availability of cash in the economy (Lahiri, 2019).

2.1.1 Impact on the Economy

That demonetization had real detrimental effects on the economy has been credibly shown by Chodorow-Reich et al. (2019), who use data on the spatial distribution of demonetized and new notes, and find that districts subject to more demonetization had less economic activity (as measured by nightlights), and lower bank credit growth. The decline in cash lowered nightlights-based economic activity and employment by at least 3 percentage points in November and December of 2016, and the authors suggest that this is a lower bound of the aggregate consequences.[8]

In other work, in the context of one city in India, Banerjee and Kala (2017) surveyed 400 wholesale and retail (self-reported) traders in Bangalore in the two months following the demonetization announcement. They find that 20% of respondents reported a fall in sales of greater than 40%, and that on average, sales were 20% lower. Similar contemporaneous work finds that demonetization led to a sharp increase in unemployment (Subrmaniam, 2019) and a reduction in household consumption expenditures (Wadhwa, 2019). Aggarwal and Narayanan (2019) study how agricultural markets were impacted, and find a 13% reduction in trade value in the short run, and a 10% reduction even eight months after demonetization[9].

2.1.2 Reactions to the Policy

The policy implemented in 2016 differed starkly from the previous episodes of demonetization in a significant manner: the two denominations declared illegal as tender accounted for 86 percent of the currency in circulation. As a result, the proportion of the population potentially impacted was high. The restrictions placed on the amount of new cash that could be withdrawn each week, combined with the number of individuals impacted that led to significant disruption in the country.

immediately, and individuals were given a period of two years to change the old notes for new tender. The third was in 1978 when high denomination notes (less than one percent of currency in circulation) were declared illegal tender with presumably little impact.

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

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

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

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

Media reports were rife with news about lines outside bank branches, and how impacts were more severe for vulnerable populations like the elderly.[10]

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

Given the widespread negative media reports and negative economic impacts, it was not clear that there were any political repercussions for the ruling party, the Bharatiya Janata Party (BJP), or its coalition, the National Democratic Alliance (NDA). In the months following the demonetization policy, the BJP and its allies won the majority of state and local elections held around the country, suggesting that voters perhaps did not seek to electorally punish the BJP for the policy.[11] Yet, the BJP may have just been on an upward trajectory of votes, and as such these aggregate trends may hide the true causal reaction of voters to the policy. Our aim is to isolate the causal effect from these aggregate trends.

In concurrent work, Bhavnani and Copelovich (2018) analyze the electoral consequences and find that regions with fewer banks actually saw an increase in the vote share for the ruling BJP in subsequent elections. They leverage variation in a state-level banking scheme from the 1980s, and use about 75 of the 600 districts in the country to show an increase in vote shares for the ruling party in less banked areas. One way to reconcile our findings is to consider the possibility that areas with fewer banks after the end of the 1980s scheme are the areas that received the 2005 banking expansion policy, and as such, have more banks today. Indeed, the 2005 policy was targeted to 'underbanked' districts. Additionally, the banking sector in the 1980s largely consisted of state banks, but deregulation in the 1990s expanded the private banking sector and made them major players. The incentives we study in our bank-branch expansion policy rapidly increased the number of private banks in underbanked districts.

2.2 Bank Branch Expansions

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

We utilize a policy reform in India that took place in September 2005, wherein additional commercial bank branches were encouraged to open in "underbanked" districts. Free entry of bank

[10] For example, see The Times of India (2018). Yet, for political reasons, the government denied the negative effects on the Indian economy (The New York Times, 2019).

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

branches is typically not permitted in India, and any new bank licenses are granted infrequently by the Reserve Bank of India (RBI). The 2005 bank expansion reform allowed easier entry of bank branches into districts that were given "under banked status" by the RBI, based on the district average persons per branch, relative to the national population per branch. This provides us with a discontinuity in the number of branches in districts around the national average cutoff.

In a recent paper, Young (2017) uses the this variation to evaluate the economic effects of the bank branch expansion policy, and finds improvements in agriculture, manufacturing, and local GDP (measured by nightlights). Essentially, the 2005 policy was a branch licensing reform that incentivized new commercial banks to enter in regions with fewer banks. As Young (2017) shows, the new rules required banks to expand in underbanked regions to be eligible for licensed entry into richer lucrative markets. Additionally, banks were required to make accounts accessible to low-income customers by having accounts with limited fees and low minimal balances.

3 Data and Measurement

3.1 Electoral, Banking, and Economic Data

Banking Data. We use district banking data from the Reserve Bank of India (RBI) from 2005 to 2016. This data includes information on the number of bank branches, the number of accounts, as well outstanding credit. We also use data at the bank branch level by district from the RBI's Master Office File (MOF), which includes information on bank branch locations, and the year that they were established.

Elections. To look at electoral outcomes, we restrict attention to the sample of states where elections were held in 2017 and 2018 (following the implementation of the demonetization policy). In India, parliamentary elections are held at the national level every five years (the last year being 2014), and elections for state legislatures are also help every five years, but in a staggered manner.

Given our sample restriction, our data set contains electoral information for 1979 constituencies across 21 states, for the years 2009-2018. This data is obtained from the Electoral Commission of India, and contains information at the constituency level on candidates that stood for election, their party affiliations, number of votes obtained, and demographic information such as age, gender, and reservation category. We extend this electoral data set by creating a variable that contains information on whether the candidate belongs to the ruling party's coalition (the NDA), or the opposing coalition (the UPA), at the time when the policy was implemented.

Nightlights. We follow past research by Henderson et al. (2012); Chodorow-Reich et al. (2019); Michalopoulos and Papaioannou (2013), and Pinkovskiy and Sala-i Martin (2016) in using night-time lights as a proxy for economic activity. These data, in particular the stable lights product have been shown to correlate extremely well with measures of economic development, incomes, electrification rates, and urbanization. Luminosity data are taken from the Defense Meteorological Satellite Program's Operational Linescan System. Major advantages of these data include their arbitrary divisibility, their consistency across multiple political jurisdictions, their high spatial resolution, and their availability given the weaknesses of official high-frequency, high-spatial resolution data India. These data are constructed as a monthly average of satellite images of the earth taken daily between

20:30 and 22:00 local time. The raw data are at a 30 second resolution, which implies that each pixel in the raw data is roughly one square kilometer. The raw luminosity data for each pixel is reported as a six-bit integer ranging from 0 to 63. We average over pixels within a district.

3.2 Data on Voter Preferences

The final part of our data set consist of responses on a nation-wide survey conducted by the Centre for the Study of Developing Societies (CSDS). Between May 1-15, 2017, 11,373 respondents, across 19 States of India were surveyed by CSDS, where sample of respondents were randomly chosen from 584 locations and 146 Assembly Constituencies (ACs).[12] The survey collected detailed demographic and socioeconomic data, and asked questions about respondents' political preferences and attitudes about the government, political parties, and importantly, about several policies in the country, including demonetization.

We use the CSDS household level surveys to study whether the banking policy changed household access to banks as well. The CSDS asks respondents whether they have a bank or post-office account,[13] and whether they have a credit or debit card.

4 Empirical Strategy

We isolate the effect of access to cash by leveraging variation in the bank expansion policy of 2005. Specifically, to isolate the effects of demonetization severity on political outcomes, we use a regression discontinuity design (RD), with the banks per person as the running variable:

$$Y_d = \beta RD_d + f(Banks\ per\ cap_d) + \epsilon_d \ \ for\ d \in \{-D, D\}\ , \tag{1}$$

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

When looking at electoral outcomes, we perform not just this cross-sectional regression, but also leverage variation over time t. We compare electoral outcomes before and after demonetization. Any differences in vote shares before demonetization would be evident in the pre-period electoral outcomes. Such differences may exist as voters may reward the earlier ruling party (the Congress party) for their banking expansion scheme. As such, we use a difference-in-discontinuities design and exploit the panel dimension of the data in the following specification:

$$Y_{dt} = \delta(Post_t \times RD_d) + \gamma_d + \mu_t + \epsilon_{dt} \ \ for\ d \in \{-D, D\} \tag{2}$$

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

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

Here $Post_t$ is an indicator for whether $t > November\ 8, 2016$ (which is the date when the demonetization policy was unexpectedly announced). $f(.)$ is a continuous function in the running variable, that is, the number of banks per capita in district d. The parameter of interest is δ, which provides us with an estimate of the difference-in-discontinuities, at the cutoff (that is, having *less* severe demonetization), within a bandwidth around the cutoff. As there are no optimal bandwidth procedures for a difference-in-discontinuities analysis, we vary the bandwidth manually to test for robustness for all of the results presented using this strategy.

4.1 Compliance with the 2005 Branch-Expansion Policy

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

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

Consistent with Young (2017), we find that the bank expansion policy did indeed lead to differential bank branch growth only *after* the bank expansion policy was instituted in 2005, and not before. Table 2 shows that the number of new branches (built between 2006-10) were substantially higher (Panel a), even as there was no discernible discontinuity in the number of branches built between 2001-5 (Panel c). Table 3 shows how the policy substantially increased the number of accounts, and outstanding credit in the districts that received the banking policy.

4.2 Persistence in Impacts of the Branch-Expansion Policy

Our identification strategy relies on there being differential access to bank branches (and therefore new currency notes following the demonetization) around the month of November in 2016. For this, we verify that differences in bank branches and outstanding credit do indeed persist in the years and months leading up to the 2016 demonetization. We illustrate this using data on bank branches in Table A.1 in the Appendix. Table A.3 further adds to this evidence: this table uses data from the CSDS voter surveys, where respondents were asked (i) whether they had a bank or post office

account, and (ii) whether they had a bank or credit card. Both of these are significantly higher around the RD cut-off.[14]

5 Results

5.1 Economic Consequences of Demonetization

While recent papers have provided evidence for worse economic outcomes in the months following demonetization (for example, Chodorow-Reich et al. (2019) among others), the sample of districts and identification strategy are different from ours. We therefore start our analysis by first verifying that there had indeed been a more adverse impact on local economic activity in areas with more severe demonetization. In particular, we look differential impacts on nightlights for districts with more versus less severe exposure to demonetization. Consistent with the findings on nightlights in Chodorow-Reich et al. (2019), but using our differences-in-discontinuity design, we find that this is indeed the case, and the results are shown in Table 5. Places that received fewer banks were likely to see a 9 percent fall in nighttime lights in the months following demonetization. Together with the results in Table 2, we estimate that a 10% decrease in the number of bank branches was associated with a 1.7% decrease in nighttime lights following demonetization. Henderson et al. (2012) and Chodorow-Reich et al. (2019) suggest using an elasticity of 0.3 to translate nighttime lights to GDP, which suggest a 0.5% fall in GDP for a 10% reduction in the number of bank branches.

5.2 Correlates of Support for Demonetization

We start our analysis of how voters reacted to demonetization by looking at data from the CSDS voter surveys. Respondents were asked whether demonetization was (a) the right move, (b) the right move but poorly implemented, or (c) was the wrong move. Just under half of all respondents reported saying that demonetization was indeed the right move. Yet, if we look at Table 6, we see that regions with more banks were more likely to have respondents that thought it was the right move, and were less likely to have respondents who thought it was badly implemented.

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

It may be possible that demonetization was perceived as 'moral' (in the sense that it would address issues of corruption and counterfeit currency), which would be consistent our finding that being religious is correlated with support for the policy (Norenzayan et al., 2016; Scheve and Stasavage,

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

2006; McCleary and Barro, 2006).[15] In the same specification, we also control for the respondent's views on anti-minority legislation: in particular, their support for a beef ban, and whether they think the prime minister should take action for the recent rise in cow lynchings.[16] It is reasonable to think that the latter more saliently captures the respondent's anti or pro-minority sentiment, if any. It is interesting to note that those with anti-minority sentiment are more likely to support the prime minister, but not more likely to support demonetization.

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

5.3 Impact of Demonetization on Citizen's Views

These patterns are also reflected at the RD in figure 5. In the first panel we use the CSDS data to show that respondents were discontinuously more likely to have access to debit or credit card in regions with more banks. In the remaining panels, we show that these very regions had respondents with somewhat more favorable views toward demonetization. Individuals in regions with more banks were more likely to think demonetization was the right move, and less likely to think it was badly implemented.

5.4 Effects on Elections

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

Table 7 provides the estimate for the difference at the cutoff in the vote shares, and the probability of winning in elections that were held *before* the demonetization policy of 2016.[17] We find no detectable effects of the banking policy in the pre-period. One may expect that voters reward the UPA for the banking policy, but we fail to document such evidence. One possible reason is that the benefits of the banking policy were likely to be gradual, and as a result, less salient, unlike the sudden demonetization change. Indeed, the importance of salience, and who gets credit for different policies is found to be of paramount importance in other parts of the literature (Guiteras and Mobarak, 2014; Ferraz and Finan, 2011). Similarly, we find it interesting that a slow, less salient policy did not reap electoral rewards for the party.

[15] We capture the idea of "being religious" with a "religiosity" index: an index (the first component using principle components analysis) based on the number of religious activities that the respondent reports doing.

[16] There has been a rise in India, in the number of individuals (mostly Muslims or low caste Hindus) being lynched when being suspected of carrying or consuming beef.

[17] These are elections held in 2015-2016, but before November 8, 2016.

Table 8 shows the vote shares after demonetization, and presents the results for regression specification 1. The vote shares for the ruling coalition are higher in regions that have more banks. Table 9 shows the Difference-in-Discontinuities results by leveraging the panel dimension of the data, and estimating equation 2. We interpret these coefficients as the gain in vote shares over the previous election in the same constituency, around the cutoff. Once again, it is evident that in regions that had a discontinuously higher number of banks, bank accounts, and credit, the vote shares and likelihood of winning for the ruling party were higher post demonetization. Vote shares for the ruling party are higher by at least 4.76 percentage points following demonetization, in areas where demonetization was less severe.[18] Together with the results in Table 2, this suggests that a 10% decrease in the number of bank branches was associated with a 0.9 percentage point decrease in vote shares for the ruling party. In Table 10 we estimate the likelihood of winning the constituency by party-affiliation. Consistent with the results on vote shares, we see a substantial increase in likelihood of winning.

Interestingly, Fisman et al. (2018) find that soon after demonetization, opposition party candidates were less likely to run for elections, and argue that this may have been the result of having less access to cash during political campaigns, relative to politicians affiliated with the ruling party. As such, these results suggest that who runs for the election in a constituency may also be affected by access to cash. Given their evidence, it is likely that our estimates are a lower bound for the electoral effects: if the opposition was made worse off as a result of demonetization, our estimates may be even larger.

We finally explore whether voters punish incumbents in areas that faced a more severe demonetization, and we find no support for this. Table A.5 provides the difference in discontinuity results for vote shares of candidates from incumbent parties. The coefficients are both statistically and economically insignificant.

5.5 Robustness Checks

5.5.1 Differential Trends Around the Cut-Off

A crucial assumption in our difference-in-discontinuities identification strategy is that, there were no differential trends around the branch-expansion cut-off prior to demonetization. In order to test for this, we do a pre-trends analysis, and check whether there was a differentially changing *trend* for BJP or allies' votes around the RD cutoff.

In Table A.6, we run the Difference-in-Discontinuities specification (equation 2), for different *placebo* years as cutoffs (2012, 2013, 2014, 2015), all the while excluding data post 2016 (after when the demonetization effects will materialize). We see no detectable discontinuities in vote shares for any of the placebo year cutoffs.

[18] In Table A.4 we explore the effects on the national (federal) elections in 2019. While the magnitudes in some specifications are economically meaningful, we cannot statistically rule out a zero effect at the RD cutoff. If indeed there was no consequence at the federal elections, it may be because between 2016 and 2019 other issues became more salient.

5.5.2 Sensitivity to Bandwidth Selection

Since a method for optimal bandwidth selection does not currently exist for the difference-in-discontinuities approach, we redo our analysis for a range of bandwidths. In particular, we vary the bandwidth between the values of 2 banks per 100000 people to 15 banks per 100000 people around the cutoff (where the maximum value of the running variable is 19.8 banks per 100000 people). The regression coefficients from this exercise are shown in Figure 7, and we see that our estimates are robust for a range of bandwidths.

5.5.3 Robustness to Variable Definitions

Recall that in defining our outcomes of interest, whenever a party did not field a candidate, we coded such cases as missing values. However, one could argue that whether a party fields a candidate or not is endogeneous. To address this concern, in Appendix Table A.7 we re-do our main difference in discontinuity analysis including constituencies where the parties did not field any candidate at all. These are now coded as zeros instead of missing, and our results remain similar.

5.6 Electoral Effects Driven By Non-Stronghold Areas

Demonetization was arguably very salient in the immediate weeks and months following the announcement of the policy. This, coupled with the results that there were negative economic consequences as shown by us and others, makes it reasonable to expect impacts on voter behavior, and we have just seen this to be true. At the same time, however, citizens only had one vote in the elections, where there was no referendum or direct democracy on the demonetization issue. It was therefore not entirely obvious that we would detect a corresponding negative impact on the ruling party's electoral outcomes.[19]

Indeed, the fact that Indian voters are evaluating many different issues simultaneously motivates one to find a suitable identification strategy (such as the bank expansion) to isolate the effect of a specific issue. This is why one may see aggregate vote shares for the BJP go up across the country, even as places with fewer bank branches see limited growth in BJP support.

Issue bundling may also suggest that what we *should* expect to see is a muted (or no) effect on voting behavior for those voters whose preferences are strongly aligned with the ruling party. To investigate this, we explore heterogeneous effects for constituencies that are BJP or it coalition (NDA) strongholds.[20] Tables 11 and 12 provide the results for the BJP's and NDA's electoral outcomes, respectively. Consistent with the literature on issue bundling (Besley and Coate, 2008; Iversen and Goplerud, 2018) these results suggest that there was indeed no (or very minimal) impacts on vote shares for the ruling party and its allies in their political strongholds, as a result of the demonetization policy.

[19] We already saw in A.2 that it was not the case that there is perfect alignment between support for the demonetization policy, and support for the policy maker (the prime minister) for different types of people

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

Table 13 illustrates this point further. The results show that it was *not* the case that voters in the ruling party stronghold areas were more likely to feel that demonetization was a good policy.

A natural question to then ask, is whether ruling party or ally stronghold areas were less impacted by the demonetization, which might explain the results. We rule out this possibility in Table A.8, where the results illustrate that there are *no* differential impacts around the RD cut-off for BJP or ally stronghold regions.

Taken together, our evidence is consistent with the idea that in democracies where voters get one chance to vote, and where the policy space is multi-dimensional, issues get bundled during elections. Thus voters who align more closely with a particular politician (or party), despite being negatively impacted by a single policy implemented by that politician (or party), may still vote for them in the next election.

6 Discussion

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

We find that the bank licensing policy expanded the number of new branches, accounts, and credit limits. Interestingly, voters did not seem to reward the party that implemented the banking expansion, perhaps as the benefits were gradual and were less publicized. In contrast, voters seem to have responded to the salient demonetization policy. In regions that had discontinuously fewer banks, the ruling party had a discontinuously lower vote share and likelihood of winning the constituency. Our analysis, thereby examines the effects of two different policies which differ on the salience and immediateness of the effects, and whether they hurt or helped affected populations. The difference in voter response between these policies is thereby interesting and of policy relevance in and of itself too.

While recent work has investigated the effects of targeted fiscal transfers on electoral gains, there is far less work on the effects of monetary-driven shocks on voter perceptions and electoral outcomes. We find that in areas where the demonetization was more severe due to poorer access to new currency notes, the ruling party and coalition had relatively lower vote shares, and a lower likelihood of winning elections.

The magnitudes of our effects are meaningful. At the RD cutoff, there was about a 52 percent increase in new bank branches (Table 2), and almost a 62 percent increase in the number of bank accounts (Table 3). This corresponded to about a 9 percent higher nighttime luminosity following demonetization (Table 5), and a 19 percent increase in the fraction of voters who thought demone-

tization was the right move (Table 6). Together, these translated into a meaningful 4.7 percentage point increase in the BJP vote share (Table 9). In sum, a 10% increase in the number of new bank branches translates into about a 0.9 percentage point increase in the vote share of the population that has access to banking safety nets when faced with such economic shocks.

Without a causal analysis, we may be misled to think that voters did not respond to demonetization. In fact, the ruling party won several state elections in 2017, the year after demonetization. In the absence of well-identified variation in banking safety nets, the media concluded that demonetization was not punished by the voter base (The Indian Express, 2017). We dispel this flawed view with stronger identification. While the ruling party made substantial gains in the years following demonetization, their gains would have been far larger in the absence of such a policy. We conclude that Indian voters do indeed respond to policy consequences in a rational manner.

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Tables

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

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

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

Table 2: New and Old branches at the RD cutoff

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

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

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

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

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

Table 4: Household Survey Data: Bank Accounts and Credit Access

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

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

Table 5: Economic Impact of Demonetization

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

Notes: Dependent variable is the logarithm of luminosity. All specifications restrict the sample around the RD cutoff, and control for the running variable (banks per person) *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Views on Demonetization

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

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

Table 7: 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 8: 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 9: Difference-in-Discontinuities: Vote Shares

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

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

Table 10: Probability of Winning: Difference-in-Discontinuities

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

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

Table 11: Ruling Party Vote Shares, by Political Strongholds

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

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

Table 12: Ruling Coalition Vote Shares, by Political Strongholds

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

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

Table 13: Views on Demonetization, by Political Strongholds

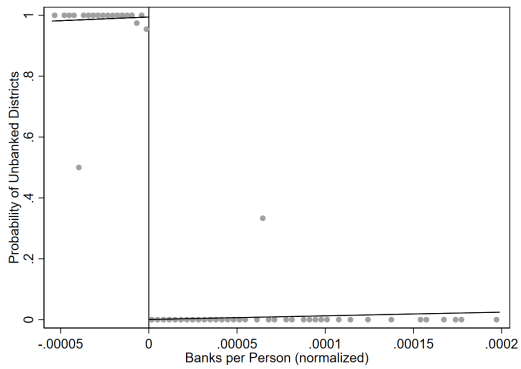
	Demonetization was the Right Move			
Banks	0.121*	0.174***	0.117*	0.164**
	(0.0703)	(0.0648)	(0.0693)	(0.0654)
Banks*NDA-Stronghold	0.0266	0.0141		
	(0.0473)	(0.0456)		
Banks*BJP-Stronghold			0.0507	0.0443
			(0.0616)	(0.0585)
Observations	3,911	4,071	3,911	4,071
R-squared	0.322	0.307	0.324	0.309
BW	[-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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

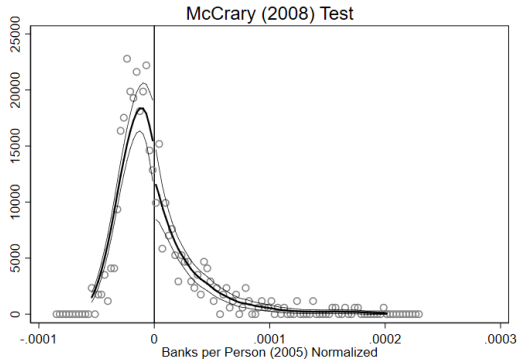
Figures

Figure 1: First Stage and McCrary (2008) Test

Panel A. Compliance to Policy Rule



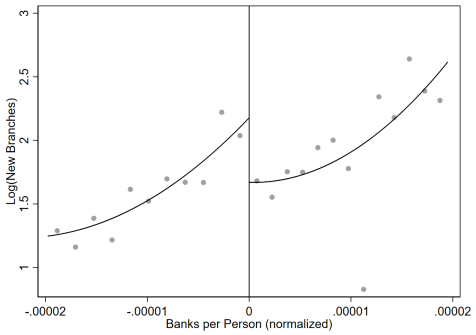
Panel B. District Density at RD cutoff



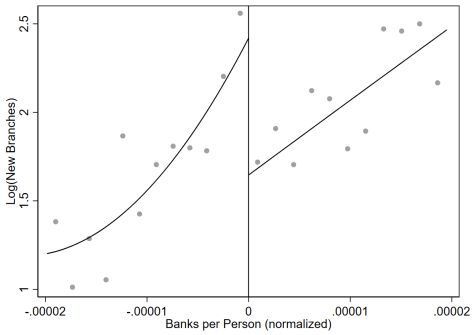
Notes: Graphs show the first stage (Panel A), and density of districts (Panel B) at the cutoff.

Figure 2: Private Bank Branches at the RD Cutoff

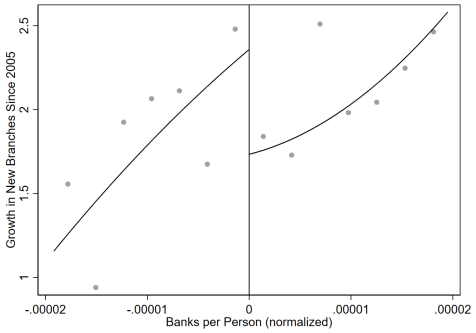
(a) New branches (2006-10): Aggregate



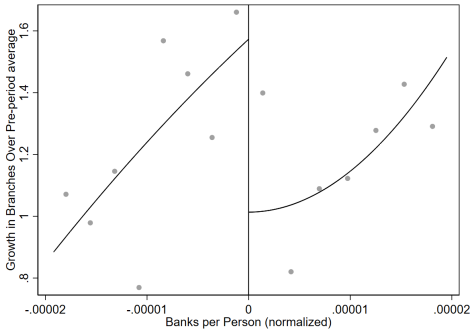
(b) New branches (2006-10): MOF Data



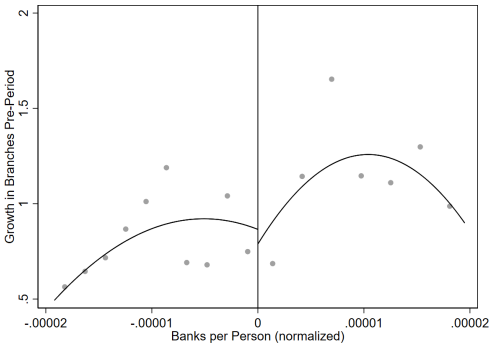
(c) Growth in branches since 2005



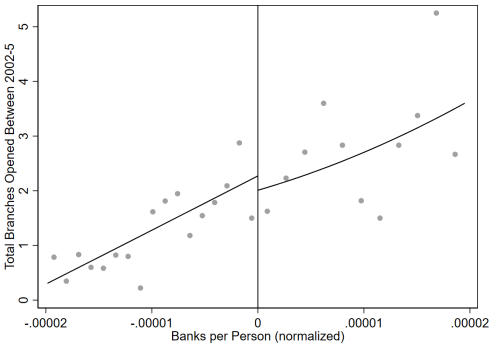
(d) Growth in branches since (2000-2005) average



(e) Log(Branches Opened Between 2002-5)



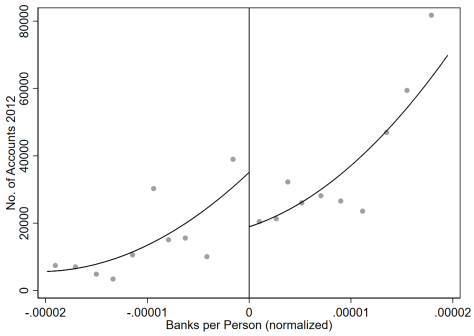
(f) Number of Branches Opened 2002-5



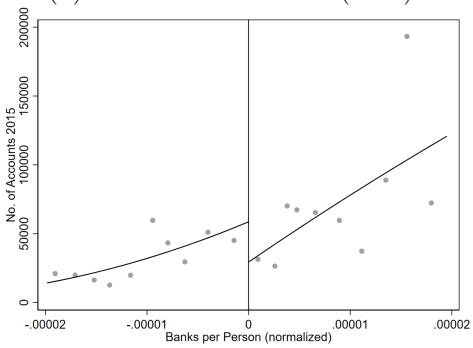
Graphs show the effect of unbanked status on private-sector bank branches and growth. Panel (a) uses the aggregate district level data on number of newly opened branches between 2006 and 2010. Panel (b) uses the Reserve Bank of India's (RBI) 2016 Master Office File (MOF) at the bank-branch level, and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

Figure 3: Number of Accounts and Credit

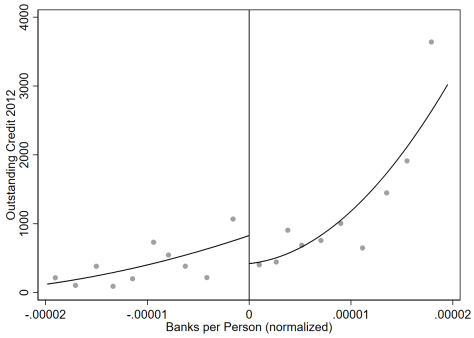
(a) Number of Accounts (2012)



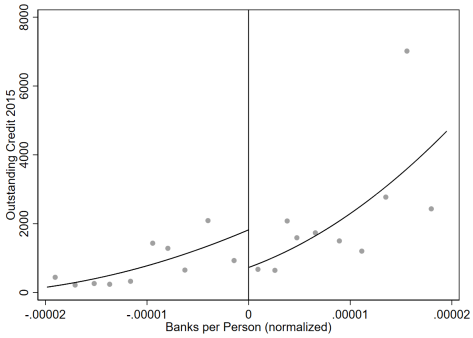
(b) Number of Accounts (2015)



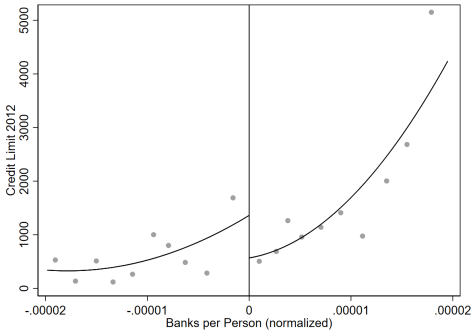
(c) Outstanding Credit (2012)



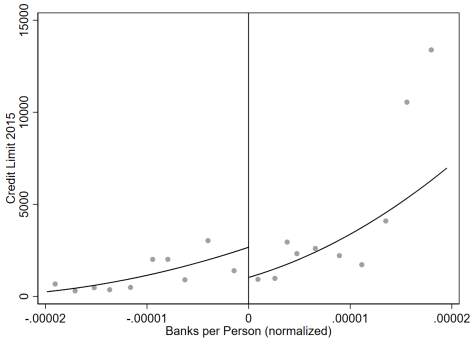
(d) Outstanding Credit (2015)



(e) Credit Limit (2012)

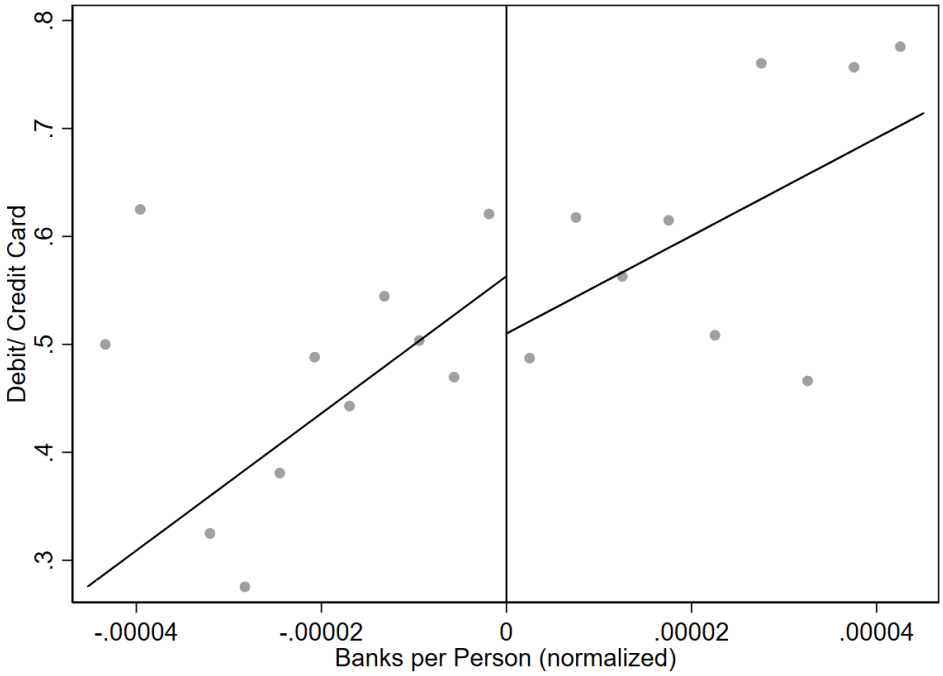


(f) Credit Limit (2015)



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

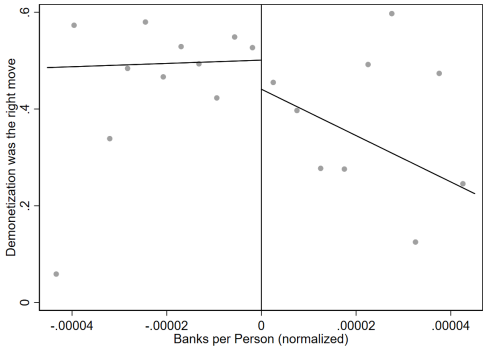
Figure 4: Bank Access (Voter Survey Data)



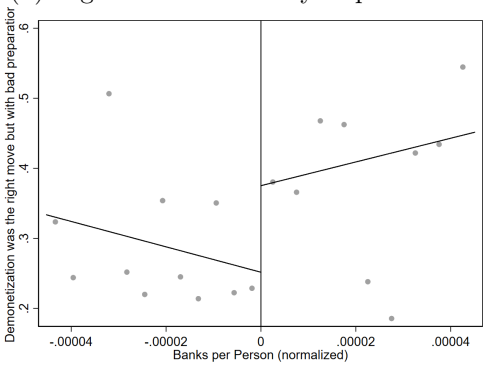
This figure uses household level CSDS survey data to show the effect of unbanked status. In particular, this shows the access to debit or credit cards.

Figure 5: Views on Demonetization (Voter Survey Data)

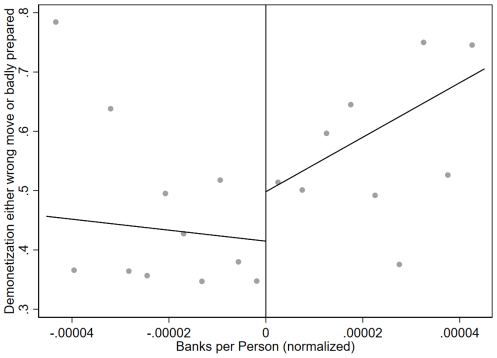
(a) Demonetization was the Right Move



(b) Right Move but Badly Implemented



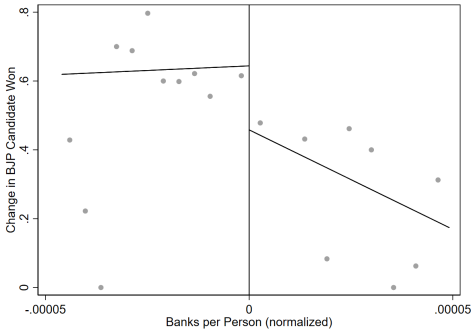
(c) Wrong Move or Badly Implemented



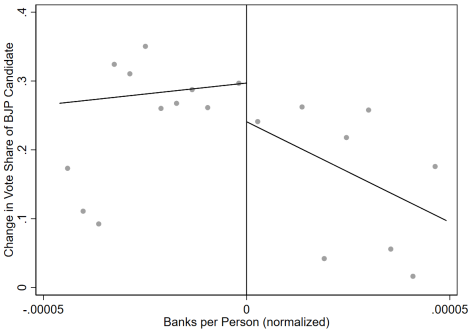
Graphs use household level CSDS show the effect of unbanked status. Figure (a) is an indicator for whether or not the respondents thought demonetization was the right move. Figure (b) is an indicator for whether or not the respondent thought demonetization was the right move, but badly implemented, and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

Figure 6: Probability of Winning and Number of Votes

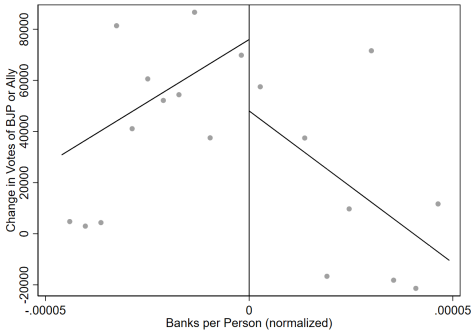
(a) Change in P(Ruling Party Victory)



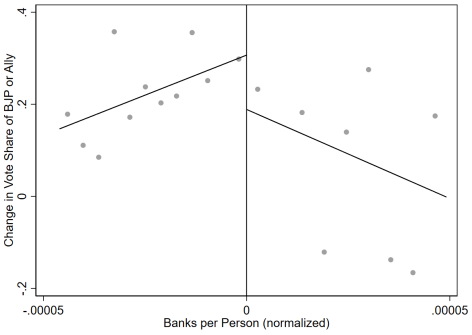
(b) Change in Vote Shares for Ruling Party



(c) Changes in Votes for Ruling Coalition

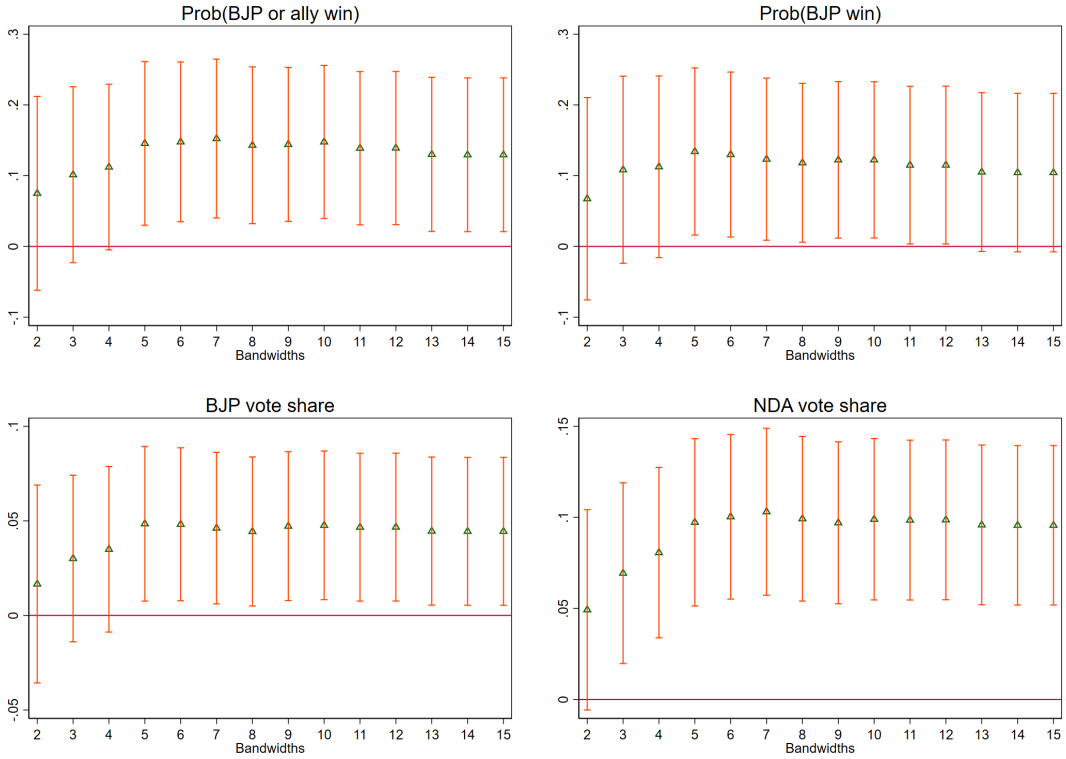


(d) Change in Vote Shares for Ruling Coalition



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

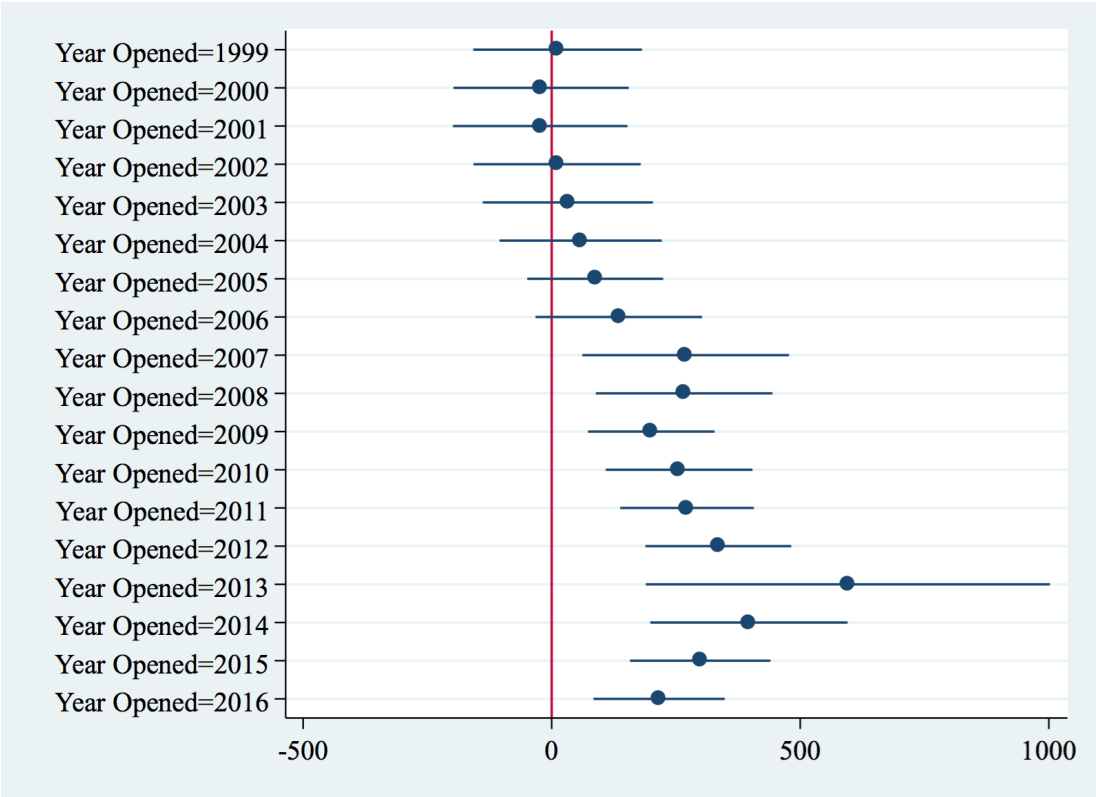
Figure 7: Sensitivity to Bandwidths



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.

ONLINE APPENDIX

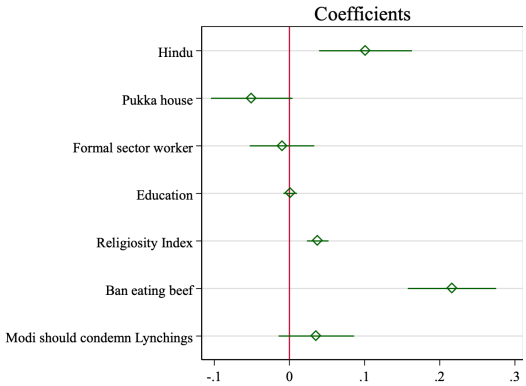
Figure A.1: New Branches over Time



This figure shows the RD coefficient on the the number of new branches opened by year. We restrict the sample around the optimal bandwidth and flexibly control for the running variable (baseline banks per capita). The policy was implemented in 2005.

Figure A.2: Correlates of Support For:

(a) Demonetization



(b) Prime Minister

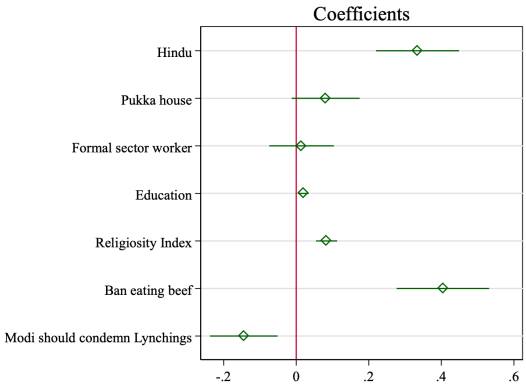
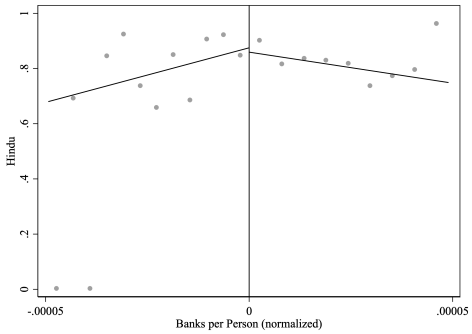
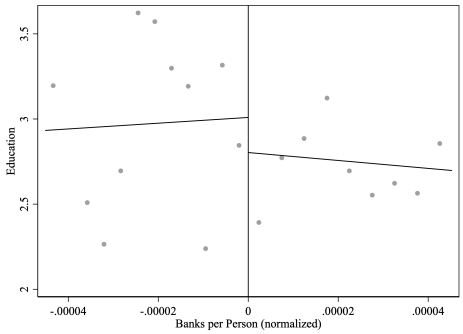


Figure A.3: Continuity in Demographics at the RD Cutoff (Voter Survey Data)

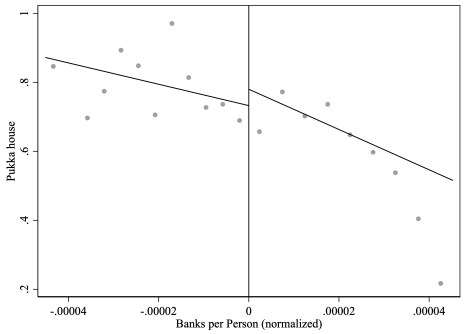
(a) Hindu



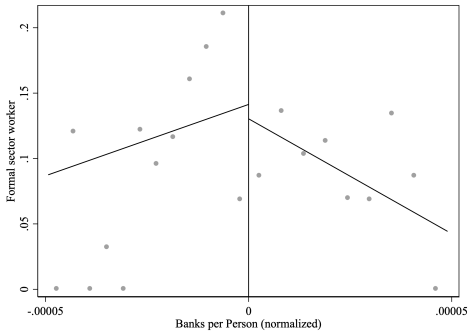
(b) Education



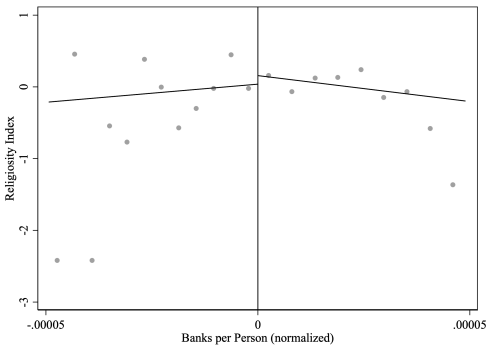
(c) Pukka House



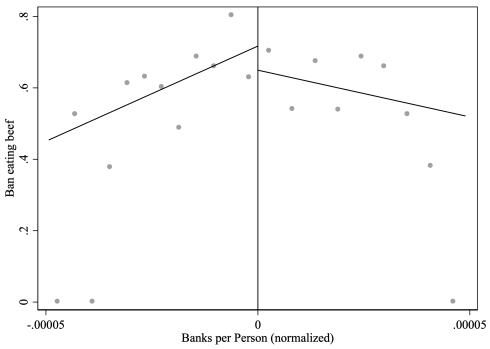
(d) Formal Sector Worker



(e) Religiosity



(f) Supports Beef Ban



Graphs show the relationship between unbanked status and individual characteristics.

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

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

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

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

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

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

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

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

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

Table A.4: National Election Results in 2019

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

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

Table A.5:
Difference-in-Discontinuities:
Incumbent Vote Shares

Vote shares	Incumbents	
Post*Banks	0.00962	0.0124
	(0.00875)	(0.0081)
Observations	6,515	6,923
R-squared	0.404	0.396
BW	[-5; 5]	[-10; 10]
Mean D.V.	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.6: 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.7: 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.8: 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.9: 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.