Demonetization and Child Health: Evidence from India

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Abstract

This paper analyzes the impact of the 2016 Indian demonetization episode on infant mortality. The demonetization policy rendered approximately 86% of the circulating Indian currency invalid as legal tender, resulting in substantial shortages of cash in the following months. Old notes could be exchanged for new ones from banks. Thus, the effects of demonetization are expected to be more pronounced for districts with lower bank access. In order to identify the health effects of demonetization, we use a previous bank branch expansion policy that resulted in a discontinuity in access to banks at a predetermined policy cutoff. We employ a difference-in-discontinuity estimation strategy, wherein we compare districts with greater bank accessibility to those with lower bank access around the cutoff defined by the bank branch expansion policy. Our findings reveal that among districts located around the policy cutoff, those with higher levels of bank accessibility experienced lower infant mortality during the post-demonetization period compared to districts with comparatively lower access to banks.

Keywords: Demonetization, Infant Mortality, India
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1 Introduction

Although the effect of macro-economic conditions on health outcomes has been widely studied in the economics literature[1], the effect of monetary policy on health is relatively under-explored both in the context of developed and developing countries. The improvement of health indicators can yield substantial economic benefits (Acemoglu et al., 2001; Case et al., 2005; Well, 2007; Black et al., 2007; Markussen, 2012; Lundborg et al., 2014). This is particularly significant in low-income countries where life expectancy is comparatively shorter and mortality risks are more pronounced in contrast to their high-income counterparts (Cutler et al., 2006). Child health assumes a particularly important role in these nations, given that child mortality[2] constitutes a significantly larger proportion of overall deaths when compared with developed countries (Cutler et al., 2006). Infant mortality is a significant constituent of child mortality (World Bank, 2018) both globally and in India. The global share of infant mortality as a fraction of child mortality increased from 70% in 1990 to 74% in 2019. In India, the share of infant mortality in relation to child mortality has risen from 70% in 1990 to 82% in 2019 (World Bank, 2018). India ranks lower than other developing countries within the South Asian region, such as Bangladesh, Nepal, Bhutan, and Sri Lanka, in

[1] In developed countries, there is a large literature on the effects of macroeconomic shocks on health outcomes (Currie et al., 2015; Miller et al., 2009; Deaton et al., 2004; Ruhm, 2003; Ruhm et al., 2002; Dee, 2001; Ruhm, 2000), and in particular, child health outcomes(Dehejia et al., 2004; Angelini et al., 2014; Asali, 2015). Many of these papers show a positive relationship between health outcomes and recessions (Angelini et al., 2014; Miller et al., 2009; Deaton et al., 2004; Dehejia et al., 2004; Ruhm, 2003; Ruhm et al., 2002; Ruhm, 2000). In developing countries, however, most papers find a negative relationship between economic downturns and health outcomes (Chevalier et al., 2017; Bhalotra, 2010; Paxson et al., 2005; Baird et al., 2011; Cutler et al., 2002).

[2] Child mortality refers to mortality in the first five years of life. Infant mortality is the mortality in the first year of life.

terms of infant mortality.[3] Thus, an analysis of the effect of government policy on early life mortality outcomes is extremely relevant in the Indian context.In this paper, we analyze the health effects of a large-scale unexpected demonetization in India, which involved the discontinuation of the two largest currency denominations, the INR 500 and INR 1000 bills, as legal tender.

On November 8, 2016, Indian Prime Minister Narendra Modi announced the government policy of demonetizing INR 500 and INR 1000 notes through an unscheduled national televised address. The policy affected about 86% of India's existing cash in circulation. A challenge in assessing the impact of demonetization arises from its nationwide implementation, which affected all regions simultaneously. Since exchanging old notes and withdrawing cash primarily occurred through banks, districts with better access to banking services are expected to be less affected by demonetization. However, bank access may itself be an endogenous variable, potentially biasing estimates. Therefore, we leverage a government policy that led to changes in bank accessibility to identify the effect of demonetization.

In 2005, a new policy was launched by the Indian central bank, the Reserve Bank of India (RBI), under which banks were incentivized to open branches in underbanked districts. A district was classified as underbanked if its population per branch was greater than the national average. Consequently, there was a significant growth in private bank branches in these districts (Young, 2017). As a result of this policy, underbanked districts slightly below the bank branch expansion cutoff in 2005 had a discontinuously higher growth in the number of new branches compared to districts just above the cutoff. Khanna et al. (2023) show that in 2015, the year before demonetization, bank access

[3]Infant mortality is 6 per thousand in Sri Lanka, 23 per thousand in Bangladesh, Bhutan and Nepal, compared to 26 per 1000 in India.

was significantly higher for underbanked districts below the cutoff, compared to districts immediately above the cutoff. Following Khanna et al. (2023), we leverage this policy initiated by RBI and employ a difference-in-discontinuity design by exploiting variations over time and around the bank branch expansion cutoff. Using this design we are able to compare child health outcomes before and after demonetization across similar districts.

We use the birth level data from the fifth round of the National Family Health Survey, conducted in 2019-2021. Since the year of birth is reported for all children and the age of death is reported for every deceased child, we can clearly distinguish between children born in the pre and post-demonetization period based on their month and year of birth[4]. We begin our analysis by showing that the 2005 bank branch expansion policy was well implemented. At the announced cutoff, there was a discontinuous jump in the probability of a district being classified as an underbanked district by RBI. We also perform the McCrary (2008) density test to ensure that there was no manipulation around the cutoff. We then show that underbanked districts just below the cutoff experienced comparatively lower infant mortality in the post-demonetization period compared to the non-underbanked districts just above the cutoff. Our results are more pronounced for the rural sector, which is in line with the existing literature. Bhalotra (2007) found significant impact of state health expenditure in bring down rural infant mortality as opposed to insignificant effects in urban areas. Given that our results are more pronounced in rural areas, all subsequent analyses focus on rural areas.

[4]Due to data limitations, we solely focus on infant mortality instead of child mortality. Child mortality is the mortality during the first five years of life. To consider the effect on child mortality, we have to consider children born more than 5 years before the survey to ensure complete exposure to the risk of child mortality. Since we use the 2019-2021 round of NFHS, which is the latest available NFHS round, complete exposure implies no post-period data.

To investigate the mechanisms underlying our findings, we examine the differential impact of demonetization on access to healthcare services across districts. Specifically, we compare districts with greater banking access (previously classified as underbanked) to those with slightly limited access to banks. We particularly focus on the effect of demonetization on access to antenatal care, delivery services and postnatal care. Our analysis reveals that districts with higher banking access experienced significantly increased use of skilled birth attendants, higher rates of cesarean and hospital deliveries as well as higher access to timely postnatal care following demonetization. We do not find any significant effect of demonetization on access to antenatal care. This is likely because antenatal care typically occurs prior to childbirth. For children born after demonetization, this care would have taken place before its implementation, in a period unaffected by the liquidity crunch. Consequently, we do not expect significant differences in access to antenatal care for children born before or after demonetization, which is in line with our finding of an insignificant effect on antenatal care. Our findings underscore the critical role of financial institutions in maintaining health outcomes during economic disruptions.

We further validate our findings by performing a wide range of robustness tests. First, we check the sensitivity of our results to variations in bandwidths, following the approach of Khanna et al. (2023). Our findings remain robust across a range of bandwidths, including 0.5–0.75 banks per 100,000 people. Second, we perform a sensitivity analysis for the selection of time periods, altering both the pre- and post-demonetization periods to confirm that our results are not sensitive to the chosen time frames. Third, we examine the sensitivity of our findings to the use of NFHS survey weights by re-estimating our results with unweighted data(Hu et al., 2015). Fourth, we include addi-

tional controls for maternal characteristics, such as height and education, in our regressions, with no significant change to the results. Finally, we show that our findings are not driven by maternal or birth-level characteristics, such as the mother's age at birth, fertility, sex composition at birth, birth order, or the sex composition of previous births.

We next show that there exists significant variation in the impacts of demonetization. Female infants in underbanked districts (districts that received more banks and, hence, were less affected by demonetization) witnessed a significantly greater reduction in mortality rates compared to their male counterparts, indicating higher sensitivity of girl child health (Bhalotra, 2010; Baird et al., 2011). We don't find any significant differences in infant mortality across religious and caste groups. Children from wealthier families witnessed significantly greater reductions in infant mortality in previously underbanked districts. This supports the evidence of policy having a disproportionate effect on poorer people. (Lahiri, 2020; Mohindra et al., 2018).

Our paper contributes to several strands of literature. Firstly, our paper contributes to the literature examining the impact of 2016 demonetization in India, which serves as a unique case to explore the health impacts of a large-scale, unexpected monetary policy shock. While the effects of macroeconomic conditions on health outcomes have been extensively studied, the link between monetary policy and health remains underexplored, particularly in developing countries. Demonetization, involving the abrupt withdrawal of the two largest currency denominations (INR 500 and INR 1000), disrupted economic activity and affected households' ability to access essential goods and services. Prior studies documents declines in economic activity, employment, and domestic agricultural trade (Chodorow-Reich et al., 2020; Subramaniam, 2019; Aggarwal et al., 2023). Political consequences were also noted, with some studies

highlighting reduced economic activity and negative voter perceptions in areas with lower bank access (Khanna, 2023), while others found minimal electoral punishment for the BJP in underbanked areas (Bhavnani, 2018). Our paper contributes to this body of literature by analyzing the health consequences of Indian demonetization in 2016.[5] Specifically, we investigate the impact of demonetization on child health outcomes, with a particular focus on infant mortality.

We also contribute to the literature examining the effects of macro-level policy changes on micro-level outcomes, particularly focusing on household and individual well-being. Previous studies have highlighted the negative impact of economic shocks, such as financial crises and policy interventions, on household expenditures and consumption patterns, including reductions in education and health spending (Thomas et al., 1999; Fallon et al., 2002). Research in both developed and developing countries has also explored the relationship between recessions and health outcomes. While studies in developed countries often show a positive relationship between recessions and health outcomes (Angelini et al., 2014; Miller et al., 2009; Ruhm, 2003), research in the context of developing countries, suggest a negative association between recessions and health outcomes (Chevalier et al., 2017; Bhalotra, 2010; Paxson et al., 2005). Building on this work, our paper contributes to this body of literature by analyzing the health consequences of this unprecedented macroeconomic shock, which eliminated the possibility of any ex-ante preparation by households to mitigate its impact.

Finally, we contribute to the literature examining the relationship between bank presence and health. Previous studies have generally reported no sig-

[5]Ahsan et al. (2024) examined the gendered health impact of 2016 demonetization in India, and documents a 4% decline in women's hemoglobin levels, widening the gender gap by 21%, driven by reduced consumption of iron-rich foods.

nificant effect on health for savings (Schaner, 2018; Dupas et al., 2018), microcredit (Banerjee et al., 2019; Tarozzi et al., 2015), bank credit (Karlan et al., 2010), and health insurance (Malani et al., 2021; Haushofer et al., 2020). Exceptions to this include work by Lin et al. (2021) and Gruber et al. (2014) in the context of China and Thailand, respectively. These studies exploit a natural experiment and found a positive impact of health insurance and investment in public health care expenditure on health outcomes. In the context of India, Cramer (2021) documents a positive relationship between bank presence and health, highlighting the role of banks in providing credit to hospitals and health insurance to households. Similarly, our findings reveal a positive impact of bank presence on infant health, attributable to improved access to healthcare services in districts with greater banking penetration.

The remainder of the paper is divided into 9 sections. Section 2 provides details on the institutional background for our study - particularly demonetization and bank branch expansion policy. We provide a description of the datasets used in our analysis in Section 3. Section 4 explains our empirical strategy. Section 5 discusses the results. Mechanism for our results are discussed in Section 6. We provide robustness to our results in Section 7 and perform the heterogeneity analyses in Section 8. Section 9 concludes.

2 Background

2.1 Demonetization

On November 8, 2016, Indian Prime Minister Narendra Modi announced the demonetization of Rs. 500 and Rs. 1000 notes during an unscheduled live televised address. The policy affected about 86% of India's existing cash in

circulation. The INR 500 and INR 1000 notes ceased to be recognized as legal tender beyond midnight on the same day, with new INR 500 and INR 2000 notes subsequently issued. Individuals could deposit their old notes into banks by December 31, 2016. The demonetized banknotes could also be exchanged for legal tender at banks. However, strict limits on cash withdrawals and exchanges exacerbated public difficulties (Lahiri, 2020). The policy aimed to combat black money, corruption, and tax evasion. The sudden move significantly affected a large segment of the population due to the heavy reliance on cash transactions in the Indian economy(Aggarwal et al., 2023).

The demonetization had an immediate and significant impact on economic activity. Given the size of the informal sector in India, coupled with low digital payment adoption, the sudden withdrawal of a large portion of currency led to substantial disruption. The policy's immediate effects were felt across various sectors, with a decline in overall cash circulation by approximately 50% in the short term. It took about a year for currency circulation to return to pre-demonetization levels (Lahiri, 2020).

This cash shortage had a notable impact on economic activity and growth. Studies showed that the decline in cash circulation resulted in at least a 2% reduction in economic activity, as measured by nightlights data, and a 3% drop in employment (Chodorow-Reich et al., 2020). In line with these findings, the immediate aftermath saw a decline in business sales, with sales figures from 400 traders in Bangalore showing a 20% reduction in retail and wholesale transactions (Banerjee et al., 2017). In rural areas, where households were predominantly cash-dependent and had limited access to banking services, the effects were even more pronounced. Research by Zhu et al. (2018) indicated that household income in rural regions dropped by approximately 15.5% within two months following the demonetization.

There have been several episodes of demonetization around the world with India itself experiencing two such episodes before 2016. However, the 2016 demonetization episode in India entailed several unique features. First, it was an unexpected move since India was not experiencing any economic shock or financial crisis - typically associated with episodes of demonetization. Secondly, this was a sudden move as there were no prolonged public discussions or debates regarding the policy before the announcement. Third, it affected a large section of the population due to the widespread use of these notes and the huge dependence on cash.

2.2 Bank Expansion Policy (2005)

India's central bank, the Reserve Bank of India (RBI) introduced a liberalized branch authorization policy in 2005 (Reserve Bank of India, 2005a). This policy simplified the branch authorization process and incentivized the opening of new bank branches in underbanked districts. As a result, there was a substantial increase in the number of private bank branches in these areas (Young, 2017).

Districts were classified as underbanked based on their population per branch. For each district, population-per-branch was computed using district population from the 2001 census and the number of operational commercial bank branches as of 31st March 2005. This was then compared to the national average population per branch. A district was classified as underbanked if its population per branch exceeded the national population per branch. Hence, *Underbanked* is defined as:

$$Underbanked_d = 1((Population\ per\ branch)_d > (\overline{Population\ per\ branch}))$$

Here, $(\overline{Population\ per\ branch})$ is the national average population per bank branch and $(Population\ per\ branch)_d$ is the population per bank branch corresponding to district d. RBI published a list of 386 underbanked districts (Reserve Bank of India, 2005b) based on this rule.

3 Data

3.1 Banking Data

The Reserve Bank of India (RBI) publishes annual district banking data, which includes a comprehensive list of all operational bank branches in each district as of March 31st, as well as information on the number of branches, accounts, and outstanding credit. We use this data for the year 2005 and combine it with the 2001 population census to create a list of underbanked districts.[6] We also use RBI's Master Office File (MOF) data, which provides information on all bank branch levels and the year of their establishment.

3.2 Birth Level Data

We use birth-level data from the fifth round of the National Family Health Survey (NFHS-V), conducted in 2019-2021, for our analysis. The NFHS is a nationally representative household survey and contains detailed information on the birth histories of the surveyed women, including the child's date of

[6] While the rule for classifying districts as underbanked was followed for the vast majority of districts, we found a mismatch of 1.9% from the announced list.

birth, current survival status, sex, and age at death for deceased children.[7] For our analysis, we restrict our sample to children born from 2014-2019.[8]

4 Estimation Strategy

In order to evaluate the impact of demonetization, we rely on the fact that this policy did not affect all the districts equally. Districts that were less affected offered a reasonable counterfactual for those that were strongly affected by this policy as long as these districts are similar in other terms. Since the policy affected 86% of the country's cash in circulation and, hence, affected access to cash, we leverage the 2005 bank expansion policy[9] to isolate the effect of access to cash. This can be achieved by using a regression discontinuity design (RDD) using banks per person as the running variable.

$$Y_d = \beta_0 + \beta_1 UB_d + f(Banksperperson)_d + \epsilon_d \; for \; d \in [-h, h] \qquad (1)$$

Here, Y_d is the outcome of interest, and $UB_d = 1$ for underbanked districts: those that received more bank branches and hence, are expected to be less severely affected by demonetization. $f(Banksperperson)_d$ is a flexible polynomial on either side of the cutoff.

However, using this empirical strategy, our coefficient of interest, β_1 would not just capture the effect of demonetization, but it would capture a compound

[7]The NFHS data also reports the district of residence for these children at the time of the survey. Although the district of current residence may differ from the district of birth, such instances are rare in the Indian context due to low interdistrict migration rates (Munshi et al., 2009).

[8]We also show the robustness of our results by including additional years for both the pre and post demonetization period. These results are discussed in Section 7.2.

[9]This policy classified some districts as underbanked based on population per branch. This was accompanied by a discontinuous rise in new bank branches in these districts around the cutoff.

treatment effect of discontinuous changes in other variables that are related to being born in the underbanked districts.

Thus, in order to isolate the effect of demonetization on infant mortality, we exploit the temporal variation along with this cross section RD. To ensure comparability of mortality outcomes in similar districts, we restrict our sample to a bandwidth around the cutoff.[10] This approach allows us to compare child-level mortality outcomes before and after demonetization across similar districts. As there is no optimal bandwidth procedure for a difference-in-discontinuities design, we conduct robustness checks by manually varying the bandwidth around the cutoff (Khanna et al., 2023).

Following Grembi et al. (2016), we employ a difference-in-discontinuity design, and estimate the following model:

$$\begin{aligned} Y_{idt} = \delta_0 + \delta_1 Pop^*_{dt} + Post_t(\alpha_0 + \alpha_1 Pop^*_{dt} + UB_d(\beta_0 + \beta_1 Pop^*_{dt})) \\ + UB_d(\gamma_0 + \gamma_1 Pop^*_{dt}) + X_{idt} + \epsilon_{idt} \; for \; Pop^*_{dt} \in [-h, h] \end{aligned} \tag{2}$$

Here, Y_{idt} is infant mortality for child i, born in district d and time t. The dummy variable, $Post_t$ takes the value of 1 for children born post-demonetization (years 2017-2019) and 0 for children born before demonetization (Jan 2014 - Oct 2016). The dummy variable, UB_d, takes the value of 1 for underbanked districts i.e. the districts that were eligible for the 2005 bank expansion policy and thus, less severely affected by demonetization. The variable, $Pop^*_{dt} = Pop_{dt} - Pop_c$ is the normalized bank population ratio. This term measures the deviation of each district's population from the mean bank population ratio for entire country measured in the year 2005.[11] δ_1 indicates

[10] This cutoff is the national average banks per person in 2005 i.e. 6.8 bank branches per 100,000 people

[11] This is the year in which the bank branch expansion policy was introduced. The mean

the changes in infant mortality as the population deviates from the threshold value in the absence of any treatment. We also include a vector of individual level controls, X_{idt}, which include dummies for urban areas, religion, caste, gender of the child, and variables measuring the mother's age at first birth, and dummies for child's birth order. Standard errors are clustered at the district level.

γ_0 and γ_1 captures the pre-treatment discontinuities i.e. γ_0 measures the change in outcome variable between the districts that are just below and just above the population threshold before demonetization, while γ_1 allows this effect to vary with population size of the district. α_0 and α_1 captures the treatment effect of demonetization on infant mortality, where α_1 allows this effect to vary with population size of the district. β_0 and β_1 captures the post-treatment discontinuities. Our main coefficient of interest, β_0 captures the difference in infant mortality for districts below and above population threshold lying in the bandwidth of 0.5 banks per 100,000 people in the post demonetization period. Since, we want to compare district just above and just below the population threshold, we estimate our results in the bandwidth of 0.5 banks per 100,000 people. β_1 allows this effect to vary with population size of the district. We test the robustness of our results to multiple bandwidths by manually varying the bandwidth.

We additionally estimate the following regression model while restricting our sample to districts in the interval $Pop_d^* \in [-h, h]$:-

$$Y_{idt} = \theta_0 + Post_t(\eta_1 Pop_{dt}^* + UB_d(\mu_0 + \mu_1 Pop_{dt}^*)) + \lambda_d + \tau_t + \psi_{ds \times t} + X_{idt} + \epsilon_{idt} \tag{3}$$

bank population ratio in this year was 6.8 bank branches per 100,000 people.

In this regression specification, we additionally include district fixed effects (λ_d), child's birth month and birth year fixed effects (τ_t), and district-specific time trends ($\psi_{ds\times t}$). The coefficient μ_0 is the diff-in-disc estimator that identifies the effect of demonetization on infant mortality.

5 Results

5.1 Improved Financial Access by the 2005 Bank Branch Expansion Policy

We begin our analysis by showing the policy compliance of the 2005 bank branch expansion policy. We find that the policy was well enforced. Figure 1a shows that at the announced cutoff i.e. national average number of banks per person (6.8 branches per 100,000 persons in 2005), there is a discontinuous jump in the probability of a district being classified as an underbanked district by RBI. To check for any manipulation around the cutoff we perform the McCrary (2008) density test. The result from the test is reported in Figure 1b. We do not find any evidence of manipulation around the cutoff and a large density of districts in the neighborhood of the cutoff.

We further examine the growth in bank branches at the RD cutoff in the years 2006-10 relative to the years 2002-05. Figure 1c shows a substantial growth in bank branches at the RD cutoff between the years 2006-10 and 2002-05. We also show this result using various optimal bandwidths procedures in Panel A of Table 2. Column 1 summarizes the result for MSERD bandwidth, while columns 2 and 3 summarizes the result for CER2 and MSE2 bandwidth respectively. Results for a bandwidth of 0.5 banks per 100,000 people with linear and quadratic functional form controls of running variable are presented

in columns 4 and 5 respectively.

In line with Young (2017), we confirm that bank expansion policy led to differential growth in bank branches only after this policy was implemented in 2005 and not before that. Panel B of Table 2 shows that the number of new branches built between 2006-10 were substantially higher across a wide range of bandwidths. In contrast, we did not find any discontinuity in bank branch opening for the years 2002-05. These results are presented in Panel C of Table 2.

Our identification strategy relies on the differential access to bank branches (and consequently new currency notes) in the period following demonetization. Thus, we need to verify that this differential access to bank branches persisted in the years leading up to demonetization. Figure 1d shows differential access to bank branches around the cutoff in 2016 (the year of demonetization).

We further verify that differences in number of bank branches, deposits, and outstanding credit between underbanked districts and the non underbanked districts around the 2005 policy cutoff persisted in the years leading up to demonetization (2013-16). These results are presented in Table 3. We present the results for number of bank branches, deposits and outstanding credits in Panel A, B and C, respectively. In columns 1-3, we show these differences for various optimal bandwidths - namely MSERD, CER2 and MSE2, respectively. Results for a bandwidth of 0.5 banks per 100,000 people with linear and quadratic functional form controls of running variable are presented in columns 4 and 5 respectively. Our results suggest that around the cutoff, underbanked districts have a higher number of bank branches, deposits, and outstanding credit in the years leading up to demonetization.

To summarize, our analysis revealed that the 2005 bank branch expansion policy was well enforced. We did not find any manipulation around the cutoff.

Our findings revealed that districts classified as underbanked witnessed a differential growth in bank branches once this policy was instituted in the year 2005. We further show that this differential growth persisted with the underbanked districts maintaining a higher number of bank branches, deposits and outstanding credits in the years leading up to demonetization.

5.2 Effect on Infant Mortality

We estimate equations 2 and 3 to evaluate the impact of demonetization on infant mortality. These results are presented in Table 4. Panel A corresponds to the overall sample, and Panels B and C correspond to urban and rural samples, respectively. We present the results for equations 2 and 3 in columns 1-2 and 3-4 respectively. Columns 1 and 3 present the results in the absence of any controls. We add individual level controls in columns 2 and 4. The sample of our analysis is restricted to districts lying in a bandwidth of 0.5 banks per 100,000 people around the cutoff.

In Panel A of Table 4, our main parameter of interest ($Post \times Underbanked$) is negative and significant for both of specifications once we include controls in columns 2 and 4. This means that in a bandwidth of 0.5 banks per 100,000 people around the cutoff, there was a *41.1 percentage points* decrease in infant mortality for the underbanked districts in the post-demonetization period compared to districts not classified as underbanked. *Together with results in Table 2, it suggests that a 1% increase in the number of bank branches led to a lesser rise in infant mortality by 9.1%*

This is because underbanked districts received more banks under the 2005 branch expansion policy and, thus, were less severely affected by demonetization since they had higher access to banks during the time of demonetization. This is in contrast to the districts immediately to the right of the cutoff, which

did not get the underbanked status and, as a result, had a comparatively lower access to banks during the time of demonetization.

To check if there is any differential impact of demonetization across rural and urban areas, we estimate these results separately for rural and urban samples. The results for urban and rural areas are presented in Panels B and C of Table 4. From Panel B, we did not find any statistically significant effect of demonetization on infant mortality in the urban areas.

Panel C presents the results for the rural sector. Our main parameter of interest ($Post \times Underbanked$) is negative and significant for both of our specifications. This means that in a bandwidth of 0.5 banks per 100,000 people around the cutoff, the underbanked districts witnessed a *57.3 percentage points* decrease in infant mortality in the post-demonetization period than those not classified as underbanked. *Together with results in Table 2, it suggests that a 1% increase in the number of bank branches led to a lesser rise in infant mortality by 11.7% in the rural areas.*

This differential impact on infant mortality in urban and rural areas is in line with the existing literature. Bhalotra (2007) documents a significant effect of state health expenditure on infant mortality for rural areas, while it was insignificant for urban areas. This is because, despite the health services being relatively sparse and variable in rural areas, there is a greater scope of bringing down mortality in these areas. Based on our results, we conclude that the effect of demonetization on infant mortality is primarily driven by it's impact in the rural areas. Thus, all of our subsequent analysis is based on rural sample.

6 Mechanisms

In this section, we explore the mechanism behind our results. Our results from subsection 5.2 suggest that rural areas of districts that had more bank branches (those that received more bank branches under the 2005 bank branch expansion scheme) reported a 11.7% lesser rise in infant mortality, thus our subsequent analysis primarily focuses on the rural sample.

Interventions aimed at reducing infant mortality often include enhancements in postnatal care, place and type of delivery and an increased access to skilled birth attendants (Bhalotra, 2010; Kiross et al., 2021; Das et al., 2021; Titaley et al., 2009; Singh et al., 2012). We explore all these mechanisms using the child-level data from rounds IV and V of the National Family Health Survey (NFHS), which provides detailed health information on children under the age of five surveyed in 2015-16 (NFHS-IV) and 2019-21 (NFHS-V). This includes information on antenatal care, post natal care and information regarding place and kind of delivery. By appending NFHS-IV and NFHS-V, we construct a datset that contains health information on all children under 5 born since 2010. We specifically focus on children between the years 2014 and 2019 to align with the time frame used in our main analysis.

Based on our main results, we anticipate that children born in rural areas of underbanked districts—which received additional bank branches under the 2005 bank branch expansion scheme and thus had greater access to banking services around the cutoff—would receive improved care during delivery as well as enhanced postnatal care.

Table 5 reports the result for the presence of skilled birth attendants and an indicator for Cesarean delivery. Panel A presents the results for deliveries by a doctor, nurse, or other health personnel. Additionally, in Panel B, we

include deliveries by traditional birth attendants along with those by doctors, nurses, or health personnel. Panel C reports the results specifically for cesarean deliveries. Results in columns 1-2 and 3-4 correspond to the regression specifications provided in equations 2 and 3, respectively. Columns 1 and 3 present results without any controls, while columns 2 and 4 include individual level controls.

The coefficient for our main parameter of interest (Post $\times$ Underbanked) is positive and significant across all specifications, for all these variables. This suggests that, within a bandwidth of 0.5 banks per 100,000 people around the cutoff, children born in rural areas of districts that had more banks (previously classified as underbanked) were more likely to have received skilled birth attendance and cesarean delivery.

In Table 6, we present results for place of delivery and postnatal care in Panels A and B, respectively. Hospital delivery is an indicator variable taking the value of one if the child was delivered in a hospital. Postnatal care is an indicator variable taking the value of one if the child received postnatal care within 2 months. We present results for specifications corresponding to equations 2 and 3 in columns 1-2 and 3-4, respectively. Columns 1 and 3 report results without controls, while columns 2 and 4 include individual level controls.

The coefficient for our main parameter of interest (Post $\times$ Underbanked) is positive and significant across all specifications, for all these variables. This suggests that in a bandwidth of 0.5 banks per 100,000 people around the cutoff, the children born in rural areas of districts that had more banks (previously classified as underbanked) were more likely to be born in hospital and to receive a postnatal check-up within 2 months.

7 Robustness Checks

7.1 Sensitivity to Bandwidth Selection

In our baseline regressions, we estimated the impact of demonetization on infant mortality using a bandwidth of 0.5 banks per 100,000 people. Given the absence of a universally accepted optimal bandwidth for the difference-in-discontinuities design, we manually vary the bandwidth as a robustness check (Khanna et al., 2023). We vary the bandwidth between 0.5 banks per 100,000 people to 0.75 banks per 100,000 people around the cutoff (the maximum value of our running variable is 21.8 banks per 100,000 people). These results are presented in Table 7.

Table 7 displays the results for bandwidths of 0.55, 0.6, 0.65, 0.7, and 0.75 banks per 100,000 people in Panels A, B, C, D, and E, respectively. Results for specifications corresponding to equations 2 and 3 are presented in columns 1–2 and 3–4, respectively. Columns 1 and 3 report the results in the absence of any controls. Individual level controls are included in columns 2 and 4. The coefficient for our main parameter of interest (Post $\times$ Underbanked) is negative and mostly significant across specifications for all these bandwidths. This suggests that our estimates are robust across a range of bandwidths.

7.2 Sensitivity to Time Period Selection

In our baseline regressions, we restricted the sample to children born in years 2014 to 2019,[12] to maintain an equal numbers of year before and after demonetization. To ensure the robustness of our findings and to mitigate potential biases arising from the choice of the time period, we conduct a sensitivity analysis. This analysis involved the alteration of both the pre- and post-

[12]We exclude the months of November and December 2016 from our entire analysis.

demonetization years. The results of this sensitivity analysis are presented in Tables 8 and 9.

In Table 8, we repeat our baseline analysis, varying the length of the pre-demonetization period. Panels A, B and C of this table show results for children born born between 2013-19, 2012-19 and 2011-19 respectively. Results in columns 1-2 and 3-4 corresponds the regression specifications given in equation 2 and 3 respectively. Columns 1 and 3 reports the results the absence of any controls. We additionally include individual level controls in columns 2 and 4 respectively.

Similarly, in Table 9, we vary the duration of the post-demonetization period. Panels A, B and C of this table corresponds to the children born in the years 2014-17, 2014-18, and 2014-20 respectively. In both Tables 8 and 9, our main coefficient of interest (Post $\times$ Underbanked) is negative and statistically significant across all specifications. This suggests that our mortality results are robust and are not sensitive to time period selection for both pre and post demonetization.

7.3 Sensitivity to the Inclusion of Weights

In our baseline regressions, we use the NFHS survey weights to estimate our results. In this section, we examine the sensitivity of our results to the inclusion of NFHS survey weights (Hu et al., 2015). For this, we re-estimate our results excluding the survey weights. These results are presented in Table 10. We present the results for specifications corresponding to equations 2 and 3 in columns 1-2 and 3-4, respectively. Columns 1 and 3 report the results in the absence of any control. Individual level controls are included in columns 2 and 4.

The coefficient for our main parameter of interest (Post $\times$ Underbanked)

is negative and significant across all specifications, which suggests that our results are not sensitive to the inclusion of survey weights.

7.4 Controlling for Mother's Height and Education

In our baseline regressions, we estimated the impact of demonetization on infant mortality including various controls like caste, religion, gender, child's birth order and mother's age at first birth. Literature suggests that mother's education and height can also impact child mortality. Martorell et al. (1981) reports higher mortality rate for children of shorter mothers. Studies have also shown some effects of maternal education on child mortality (Caldwell, 1979; Caldwell et al., 1985).

Thus, in this section, we additionally control for two mother-level characteristics i.e. mother's height and education. These results are presented in Panels A and B of Table 11. Results in columns 1-2 and 3-4 corresponds the regression specifications given in equation 2 and 3 respectively. Columns 1 and 3 reports the results the absence of any controls. We additionally include individual level controls in columns 2 and 4. The coefficient for our main parameter of interest (Post $\times$ Underbanked) is negative and significant across all specifications, which suggests that our results are not sensitive to these mother-level characteristics.

7.5 Mother and Birth-Level Characteristics

In this section, we explore the possibility of our result being driven by some other maternal or birth related factors. Literature suggests that maternal characteristics like mother's age at birth and fertility can influence infant mortality (Knodel et al., 1984; Trussell et al., 1984; Knodel et al., 1984).

Thus, in other to ensure that our results are not driven by these maternal characteristics, we estimate equations 2 and 3 with mother's age at birth and fertility as the dependent variable. Information on mother's age at birth is available from the NFHS survey. However, to estimate the impact on fertility, we create woman month-year panel such that each woman is observed for every month year combination from the time women turns 15 till the date of survey. The dependent variable is an indicator variable that takes value 1 if woman gave birth in that month year combination.

We present these results in Table 12. Panel A reports the result for mother's age at birth, while Panel B reports the result for fertility (Birth dummy). Results in columns 1-2 and 3-4 correspond to the regression specification given in equations 2 and 3 respectively. Columns 1 and 3 reports the result in the absence of any control, while individual level controls are included in columns 2 and 4. In our most robust specification (column 4), the coefficient for our main variable of interest (Post $\times$ Underbanked) is statistically insignificant for both -mother's age at birth and birth dummy.

Studies suggest that in addition to these maternal characteristics, some birth level characteristics are also likely to influence our results. These include sex composition at birth, birth order, birth interval and sex composition of previous births (Bhalotra, 2007; Raj et al., 2019; Trussell et al., 1984; Knodel et al., 1984). We next verify that our results are not influenced by these birth level characteristics. For this, we estimate equations 2 and 3 with these birth-level characteristics as the dependent variable.

To analyze the effect of demonetization on sex composition at birth we use female child dummy as the dependent variable. For the order of birth, we create a dummy variable that takes value 1 if the birth order is less than mean birth order in the sample. Birth interval is defined as the difference in months

between the last birth and the current birth. We define sex composition of the previous child as the proportion of females among the children born to mother previously.

We present our results for these birth-level characteristics in Table 13. Panels A, B, C and D reports the result for sex composition at birth, birth order, birth interval and previous sex composition respectively. We present the results for specifications corresponding to equations 2 and 3 in columns 1-2 and 3-4 respectively. Columns 1 and 3 report the results in the absence of any control. Individual level controls are included in columns 2 and 4. The coefficient for our main parameter of interest (Post $\times$ Underbanked) is statistically insignificant for all of these variables.

To summarize, our results suggest that the observed effects of demonetization on infant mortality are not likely driven by maternal characteristics such as mother's age at birth and fertility, and birth level characteristics such as sex composition of birth, birth order, birth interval and sex composition of previous births. The coefficient for our main variable of interest (Post $\times$ Underbanked) is statistically insignificant for all these variables providing robustness to our results.

8 Heterogeneity

In Section 5.2, we observed significant heterogeneity in our infant mortality results for rural and urban samples. In this section, we further explore heterogeneity by gender, religion, caste and wealth group for the sample of rural households.

8.1 Heterogeneity by Gender

Demonetization triggered a period of economic hardship for the general population by creating significant challenges in accessing new currency notes and managing routine daily activities (Lahiri, 2020). Previous studies suggest that, during periods of economic difficulty, household investment in girl child is particularly sensitive to changes in household income (Bhalotra, 2010; Rose, 1999; Behrman et al., 1986; Baird et al., 2011). We therefore, evaluate our main results separately for female and male children born in rural areas in Table 14.

We estimate equations 2 and 3 separately for female and male children in Panels A and B respectively. Results in columns 1-2 and 3-4 corresponds the regression specifications given in equations 2 and 3 respectively. Columns 1 and 3 reports the results the absence of any controls. We additionally include individual level controls in columns 2 and 4.

In our most robust specification (column 4), the coefficient for our main variable of interest (Post $\times$ Underbanked) is negative and statistically significant for female children (Panel A), while it is insignificant for male children (Panel B). We further perform a test evaluating the equality of the coefficient of our main parameter of interest reported in Panels A and B. The *p-values* for the same are reported at the bottom of the table. This test indicates a slightly higher effect of demonetization on infant mortality for the girl child in rural areas. Specifically, within a bandwidth of 0.05 banks per 100,000 people around the cutoff, post-demonetization, female children born in rural areas of district that had more banks (previously classified as underbanked) were less likely to die before the age of one.

8.2 Heterogeneity by Religion

Despite generally having lower levels of education and income, Indian Muslims show a notable advantage in child survival compared to high-caste Hindus (Bhalotra et al., 2010). Accordingly, we present our main results separately for Muslim children and children from other religious groups in rural areas in Table 15. Panels A and B correspond to the rural sample of Muslim children and children from other religious groups, respectively. We present results for equation 2 and 3 in columns 1-2 and 3-4 respectively. Columns 1 and 3 present the results in the absence of any controls. We add individual level controls in columns 2 and 4.

The coefficient of our main parameter of interest (Post $\times$ Underbanked) is negative and statistically significant for children belonging to other religious groups (Panel B), while it is insignificant for Muslim children (Panel B). We then perform a test evaluating the equality of the coefficient of our main parameter of interest reported in Panels A and B. The *p-values* for the same are reported at the bottom of the table. This test indicates no statistical difference in the coefficient of our main parameter of interest between Muslim and Non-Muslim children.

8.3 Heterogeneity by Caste Group

Scheduled Castes (SCs) and Scheduled Tribes (STs) are among the most marginalized and vulnerable ethnic groups in Indian society (Das et al., 2000). Literature suggests that these marginalized groups account for higher mortality rate as compared to women belonging to other caste groups in India (Vishwakarma et al., 2020; Das et al., 2000). Their rural residence, further amplify the likelihood of infant and neonatal mortality (Dwivedi et al., 2013). We

therefore estimate our main result separately for children belonging to SC/ST families and those born in families belonging to other caste groups in Table 16. Panels A and B correspond to the rural sample of children belonging to SC/ST caste group and those belonging to other caste groups, respectively. Results in columns 1-2 and 3-4 correspond to the regression specifications provided in equations 2 and 3, respectively. Columns 1 and 3 present results without any controls, while columns 2 and 4 include individual level controls.

In our most robust specification (column 4), the coefficient for our main variable of interest (Post $\times$ Underbanked) is negative and statistically significant for SC/ST children (Panel A), while it is insignificant for children belonging to other caste groups (Panel B). We further perform a test evaluating the equality of the coefficient of our main parameter of interest reported in Panels A and B. The *p-values* for the same are reported at the bottom of the table. This test indicates no statistical difference in the coefficient of our main parameter of interest between SC/ST children and children belonging to other caste groups.

8.4 Heterogeneity by Wealth

Demonetization was primarily designed to address black money, reduce corruption, and remove counterfeit currency. It was presented as a measure targeting relatively wealthy individuals who had accumulated undeclared wealth (Chodorow-Reich et al., 2020; Lahiri, 2020). However, evidence suggests that the policy may have had a disproportionate impact on the poorer sections of society (Lahiri, 2020; Mohindra et al., 2018). We thus, estimate our main results separately for children belonging to rich and poor families in Table 17.

Since the NFHS survey does not have income data, we utilize the wealth index variable reported in these surveys. Wealth index is a composite measure

of household's living standards derived from various factors such as access to sanitation facilities and ownership of items like bicycle and television. Based on these factors, households are categorized into five wealth categories - poorest, poorer, middle, richer and richest. We categorize the households belonging to richer and richest wealth group as rich and those belonging to other three wealth groups as poor.

Panels A and B of Table 17 correspond to the rural sample of children born in rich and poor families, respectively. Results in columns 1-2 and 3-4 correspond to the regression specifications provided in equations 2 and 3, respectively. Columns 1 and 3 present results without any controls, while columns 2 and 4 include individual level controls.

The coefficient for our main variable of interest (Post $\times$ Underbanked) is negative and statistically significant for children born in rich families (Panel A), while it is insignificant for children born in poor families (Panel B). We further perform a test evaluating the equality of the coefficient of our main parameter of interest reported in Panels A and B. The *p-values* for the same are reported at the bottom of the table. This test indicates a slightly higher effect of demonetization on infant mortality for the children born in rich families in rural areas. Specifically, within a bandwidth of 0.05 banks per 100,000 people around the cutoff, post-demonetization, children born in rich families of rural areas of districts that had more banks (previously classified as underbanked) were less likely to die before the age of one.

9 Conclusion

This study examines the impact of the 2016 Indian demonetization on infant mortality, leveraging the 2005 bank branch expansion policy as a natural

experiment to differentiate the effect of access to banks. Our findings revealed that districts with higher access to bank witnessed a significantly lesser rise in infant mortality as compared to their counterparts in the post-demonetization period. Our results were mainly driven by the rural areas i.e., areas with higher reliance on cash transactions, which further amplifies the effects of demonetization.

Our results were robust to a wide range of tests across bandwidths, time frames, and sensitivity to inclusion of survey weights. We further tested the validity of our results by ensuring that these are not driven by maternal or birth-level characteristics of these children. Enhanced healthcare access as measured by skilled birth attendants, cesarean deliveries, hospital deliveries and timely postnatal care were found to be instrumental in reducing infant mortality for districts with higher banking access in the post-demonetization period.

This study underscore the critical role of financial infrastructure in mitigating the adverse impacts of sudden economic disruptions on health outcomes. Our results highlights how access to banking services can help reduce the negative health effects of economic shocks, especially for rural and under-served communities. More research is needed to understand the long-term effects and see if similar results apply to other economic policies.

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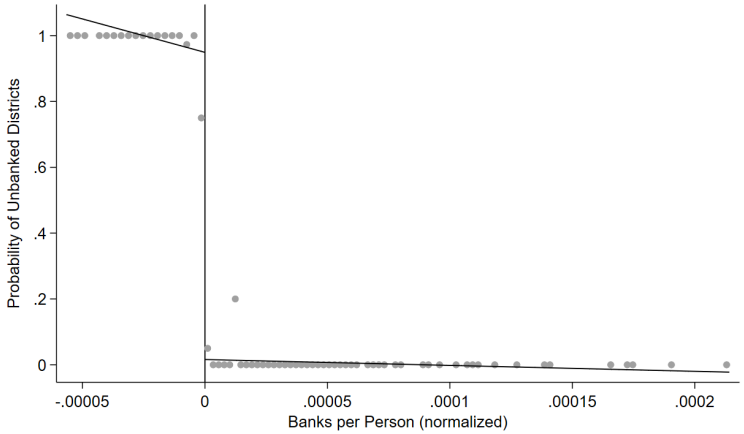
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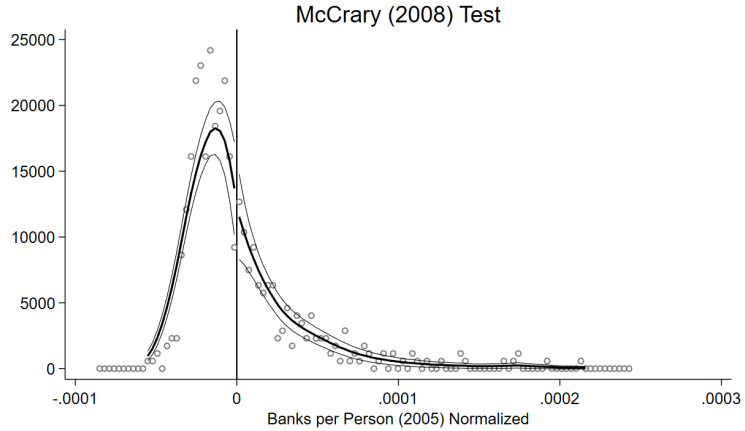
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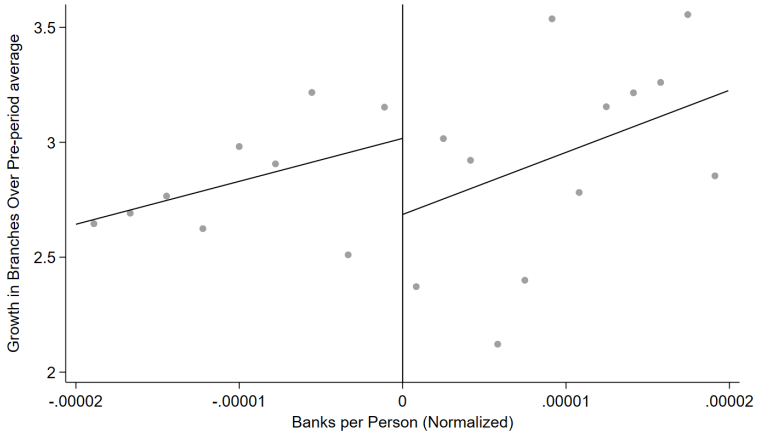
Figure 1: First Stage, McCrary (2008) Test and Banking Growth



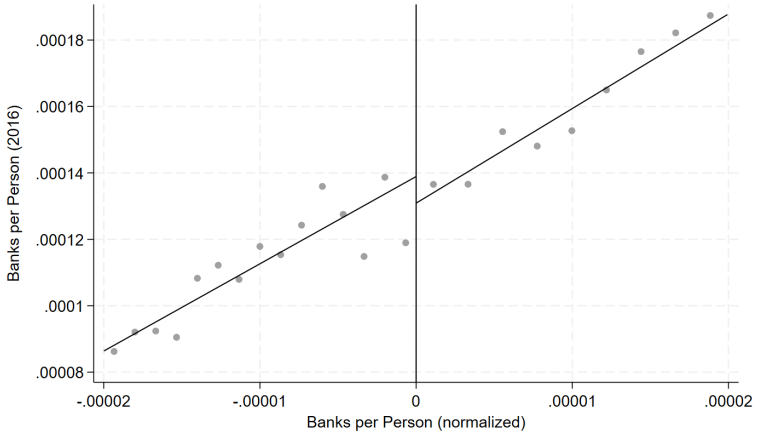
a Compliance with Policy Rule



b District Density at RD Cutoff



c Growth in Branches



d No. of Branches per Capita (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. Fig. 1c compares the new number of branches in 2006-10 to the newly opened branches in 2002-05. Fig. 1d shows the number of branches per capita in 2016 (the year of demonetization).

Table 1: Summary Statistics

	Mean	Standard Deviation	Observations
Panel A: Overall Sample			
Infant Mortality	0.039	0.195	226206
Post	0.486	0.5	243844
Underbanked	0.73	0.444	250761
Panel B: Rural Sample			
Infant Mortality	0.042	0.201	178856
Post	0.487	0.5	193202
Underbanked	0.779	0.415	198601
Panel C: Urban Sample			
Infant Mortality	0.028	0.166	47350
Post	0.479	0.5	50642
Underbanked	0.545	0.498	52160

Notes: The table reports the means and standard deviations of our key dependent and independent variables. The sample consists of children born from 2014 - 2019 excluding those born in November and December 2016 from all states of India excluding the north eastern states.

Table 2: Improved Financial Access Due to 2005 Bank Branch Expansion Policy

	(1)	(2)	(3)	(4)	(5)
Panel A: Δ Log(New Branches)					
Underbanked	0.793**	0.767*	0.508	1.134**	2.182***
	(0.331)	(0.394)	(0.375)	(0.544)	(0.703)
Bandwidth	[-1.41;1.41]	[-1.7;.8]	[-2.31;1.1]	[-.5;.5]	[-.5;.5]
Specification	MSERD	CER2	MSE2	Linear	Quadratic
Panel B: Log(New Branches)					
Underbanked	0.948***	0.953**	0.778**	1.169**	2.090***
	(0.320)	(0.391)	(0.350)	(0.507)	(0.617)
Bandwidth	[-1.36;1.36]	[-1.34;.83]	[-1.82;1.14]	[-.5;.5]	[-.5;.5]
Specification	MSERD	CER2	MSE2	Linear	Quadratic
Panel B: Log(Old Branches)					
Underbanked	-0.139	0.027	-0.260	-0.192	0.549
	(0.425)	(0.376)	(0.393)	(0.690)	(1.275)
Bandwidth	[-1.52;1.52]	[-2.4;.93]	[-3.2;1.25]	[-.5;.5]	[-.5;.5]
Specification	MSERD	CER2	MSE2	Linear	Quadratic

Notes: Each cell in each panel represents a separate regression. RD Bandwidth is in units of banks per 100,000 people. Panel A's dependent variable is Δ Log(New Branches), which captures growth in branches - the difference between newly built bank branches five years after receiving underbanked status (2006-10) compared to the period before that (2002-05). Panel B's dependent variables is Log(New Branches), the number of newly opened bank branches opened in the first five years after the policy (2006-10). Panel C's dependent variable is Log(Old Branches), the number of newly opened bank branches opened during the years 2002-05. The main independent variable is the underbanked dummy, which equals one for districts that were eligible for the 2005 bank expansion policy. These districts received more bank branches and hence, were less affected by demonetization. 'Linear' and 'Quadratic' indicate the functional form of the running variable. 'MSERD' follows the Calonico et al. (2014) optimal bandwidth selection and bias correction method with a common bandwidth, while 'MSE2' and 'CER2' allow for different bandwidths on each side of the cutoff, using MSE-optimal and CER-optimal bandwidths, respectively. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Branches, Deposits and Credits at the RD Cutoff (2013-16)

	(1)	(2)	(3)	(4)	(5)
Panel A: Number of Branches					
Underbanked	106.751	413.387***	267.829***	140.826***	313.977***
	(67.994)	(33.845)	(41.053)	(38.259)	(57.771)
Bandwidth	[-.27;.27]	[-.41;.2]	[-.6;.29]	[-.5;.5]	[-.5;.5]
Specification	MSERD	CER2	MSE2	Linear	Quadratic
Panel B: Amount in Deposits					
Underbanked	829.625***	2231.908***	924.438***	681.711***	1782.842***
	(267.371)	(305.797)	(279.070)	(210.434)	(359.912)
Bandwidth	[-.38;.38]	[-.28;.25]	[-.41;.36]	[-.5;.5]	[-.5;.5]
Specification	MSERD	CER2	MSE2	Linear	Quadratic
Panel C: Outstanding Credits					
Underbanked	613.111*	645.337***	480.389***	598.578***	1191.945***
	(313.389)	(159.608)	(151.684)	(145.001)	(250.279)
Bandwidth	[-.32;.32]	[-.42;.42]	[-.62;.62]	[-.5;.5]	[-.5;.5]
Specification	MSERD	CER2	MSE2	Linear	Quadratic

Notes: Each cell in each panel represents a separate regression. RD Bandwidth is in units of banks per 100,000 people. The dependent variables are mentioned at the top of the panels. Amount in deposits and outstanding credits are reported in 100 millions. The main independent variable is the underbanked dummy, which equals one for districts that were eligible for the 2005 bank expansion policy. These districts received more bank branches and hence, were less affected by demonetization. 'Linear' and 'Quadratic' indicate the functional form of the running variable. 'MSERD' follows the Calonico et al. (2014) optimal bandwidth selection and bias correction method with a common bandwidth, while 'MSE2' and 'CER2' allow for different bandwidths on each side of the cutoff, using MSE-optimal and CER-optimal bandwidths, respectively. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Demonetization and Infant Mortality

	(1)	(2)	(3)	(4)
	Infant mortality			
Panel A: Overall Sample				
Post × Underbanked	-0.282	-0.320*	-0.258	-0.411**
	(0.181)	(0.170)	(0.202)	(0.164)
Observations	25333	24365	25333	24365
Panel B: Urban Sample				
Post × Underbanked	0.062	-0.133	0.617	-0.027
	(0.252)	(0.222)	(0.426)	(0.353)
Observations	5424	5197	5424	5197
Panel C: Rural Sample				
Post × Underbanked	-0.430**	-0.424**	-0.573***	-0.573***
	(0.167)	(0.162)	(0.099)	(0.102)
Observations	19909	19164	19909	19164
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell in each panel represents a separate regression. The dependent variable is infant mortality. The main independent variable is the interaction of the post dummy with the underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Columns 3 and 4 include birth month and year fixed effects and district-specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. We present the results for the overall sample in Panel A. Results for urban and rural samples are presented in Panels B and C respectively. The sample consists of children born from 2014 - 2019 excluding those born in November and December 2016 in districts lying in the bandwidth of 0.5 banks per 100,000 people around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Infant Mortality Mechanisms: Skilled Birth Attendance

	(1)	(2)	(3)	(4)
Panel A: Delivery by Doctor, Nurse or Health Personnel				
Post × Underbanked	0.019***	0.035**	0.077***	0.078***
	(0.007)	(0.015)	(0.029)	(0.029)
Observations	20540	14939	20540	14939
Mean of DV	0.913	0.913	0.913	0.913
Panel B: Delivery by Doctor, Nurse Health Personnel or TBA				
Post × Underbanked	0.035***	0.050***	0.076***	0.080***
	(0.005)	(0.010)	(0.019)	(0.020)
Observations	20540	14939	20540	14939
Mean of DV	0.957	0.957	0.957	0.957
Panel C: Child Delivered by Cesarean				
Post × Underbanked	0.037***	0.057**	0.258***	0.275***
	(0.013)	(0.024)	(0.035)	(0.038)
Observations	20540	14939	20540	14939
Mean of DV	0.186	0.184	0.186	0.184
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell in each panel represents a separate regression. The dependent variables are mentioned at the top of each panel. The main independent variable is the interaction of the post dummy with the underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Columns 3 and 4 include birth month and year fixed effects and district-specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. The sample consists of children born from 2014 - 2019 excluding those born in November and December 2016 in rural areas of districts lying in the bandwidth of 0.5 banks per 100,000 people around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Infant Mortality Mechanisms: Hospital Births and Postnatal Care

	(1)	(2)	(3)	(4)
Panel A: Child Delivered in Hospital				
Post × Underbanked	0.040***	0.033	0.132***	0.101**
	(0.013)	(0.027)	(0.039)	(0.042)
Observations	20515	14916	20515	14916
Mean of DV	0.753	0.751	0.753	0.751
Panel B: Child's Post Natal Care Within 2 Months				
Post × Underbanked	0.115***	0.088**	0.126**	0.150**
	(0.017)	(0.042)	(0.059)	(0.067)
Observations	15928	10903	15928	10903
Mean of DV	0.494	0.493	0.494	0.493
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell in each panel represents a separate regression. The dependent variables are mentioned at the top of each panel. The main independent variable is the interaction of the post dummy with the underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Columns 3 and 4 include birth month and year fixed effects and district-specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. The sample consists of children born from 2014 - 2019 excluding those born in November and December 2016 in rural areas of districts lying in the bandwidth of 0.5 banks per 100,000 people around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Infant Mortality and Demonetization: Robustness Across Bandwidths

	(1)	(2)	(3)	(4)
	Infant mortality			
Panel A: Bandwidth = [-.55;.55]				
Post × Underbanked	-0.386***	-0.380***	-0.424***	-0.426***
	(0.140)	(0.137)	(0.141)	(0.142)
Observations	21889	21024	21889	21024
Panel B: Bandwidth = [-.6;.6]				
Post × Underbanked	-0.111*	-0.112*	-0.151	-0.145
	(0.063)	(0.063)	(0.094)	(0.097)
Observations	25079	24077	25079	24077
Panel C: Bandwidth = [-.65;.65]				
Post × Underbanked	-0.099*	-0.100*	-0.164**	-0.163**
	(0.054)	(0.054)	(0.074)	(0.076)
Observations	26707	25664	26707	25664
Panel D: Bandwidth = [-.7;.7]				
Post × Underbanked	-0.099*	-0.100*	-0.162**	-0.162**
	(0.054)	(0.054)	(0.074)	(0.076)
Observations	28936	27817	28936	27817
Panel E: Bandwidth = [-.75;.75]				
Post × Underbanked	-0.065*	-0.065*	-0.144***	-0.142***
	(0.034)	(0.034)	(0.045)	(0.046)
Observations	32275	31067	32275	31067
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes

Notes: Each cell in each panel represents a separate regression. The dependent variable is infant mortality. The main independent variable is the interaction of Post dummy with underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Columns 3 and 4 include birth month and year fixed effects and district specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. The sample consists of children born from 2014 - 2019 excluding those born in November and December 2016. We present the results for children born in the rural areas of districts lying in different bandwidths around the cutoff. These bandwidths (in units of banks per 100,000 people) are mentioned at the top of panels. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Sensitivity Analysis: Varying Pre-Demonetization Period

	(1)	(2)	(3)	(4)
	Infant mortality			
Panel A: 2013-19				
Post × Underbanked	-0.453***	-0.457***	-0.447***	-0.439***
	(0.164)	(0.160)	(0.126)	(0.127)
Observations	24169	23261	24169	23261
Panel B: 2012-19				
Post × Underbanked	-0.444***	-0.447***	-0.476**	-0.480**
	(0.135)	(0.131)	(0.215)	(0.220)
Observations	28486	27397	28486	27397
Panel C: 2011-19				
Post × Underbanked	-0.392***	-0.391***	-0.589***	-0.600***
	(0.146)	(0.144)	(0.159)	(0.152)
Observations	32706	31428	32706	31428
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell in each panel represents a separate regression. The dependent variable is infant mortality. The main independent variable is the interaction of Post dummy with underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Columns 3 and 4 include birth month and year fixed effects and district specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. We considered different configurations of the pre-demonetization period in all the three panels, mentioned at the top of the panels. We present the results for children born in the rural areas of districts lying in 0.5 banks per 100,000 people bandwidth around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Sensitivity Analysis: Varying Post-Demonetization Period

	(1)	(2)	(3)	(4)
	Infant mortality			
Panel A: 2014-17				
Post × Underbanked	-0.496***	-0.498***	-0.522***	-0.529***
	(0.185)	(0.185)	(0.092)	(0.100)
Observations	15074	14473	15074	14473
Panel B: 2014-18				
Post × Underbanked	-0.457**	-0.454**	-0.546***	-0.550***
	(0.185)	(0.180)	(0.082)	(0.087)
Observations	18559	17834	18559	17834
Panel C: 2014-20				
Post × Underbanked	-0.429**	-0.423**	-0.561***	-0.560***
	(0.166)	(0.161)	(0.091)	(0.093)
Observations	20066	19321	20066	19321
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell in each panel represents a separate regression. The dependent variable is infant mortality. The main independent variable is the interaction of Post dummy with underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Columns 3 and 4 include birth month and year fixed effects and district specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. We considered different configurations of the post-demonetization period in all the three panels, mentioned at the top of the panels. We present the results for children born in the rural areas of districts lying in 0.5 banks per 100,000 people bandwidth around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Robustness to Inclusion of Survey Weights

	(1)	(2)	(3)	(4)
	Infant mortality			
Post × Underbanked	-0.364***	-0.357***	-0.492***	-0.494***
	(0.101)	(0.100)	(0.104)	(0.112)
Observations	19909	19164	19909	19164
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell represents a separate regression. The dependent variable is infant mortality. All regressions exclude sample weights. The main independent variable is the interaction of the post dummy with the underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Columns 3 and 4 include birth month and year fixed effects and district-specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. The sample consists of children born from 2014 - 2019, excluding those born in November and December 2016 in rural areas of districts lying in the bandwidth of 0.5 banks per 100,000 people around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 11: Controlling for Mother's Height and Education

	(1)	(2)	(3)	(4)
	Infant mortality			
Panel A: Controlling for mother's height				
Post × Underbanked	-0.430**	-0.377*	-0.573***	-0.530***
	(0.167)	(0.191)	(0.099)	(0.110)
Observations	19909	18766	19909	18766
Panel B: Controlling for mother's education				
Post × Underbanked	-0.430**	-0.428**	-0.573***	-0.582***
	(0.167)	(0.165)	(0.099)	(0.102)
Observations	19909	19164	19909	19164
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell in each panel represents a separate regression. The dependent variable is infant mortality. The main independent variable is the interaction of the post dummy with the underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Columns 3 and 4 include birth month and year fixed effects and district-specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. We additionally control for the mother's height and education in Panels A and B, respectively. The sample consists of children born from 2014 - 2019, excluding those born in November and December 2016 in rural areas of districts lying in the bandwidth of 0.5 banks per 100,000 people around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 12: Robustness Check: Maternal Characteristics

	(1)	(2)	(3)	(4)
Panel A: Mother's age at birth				
Post × Underbanked	-0.272	-3.398*	-1.425	-1.889
	(5.901)	(1.727)	(5.220)	(1.386)
Observations	19909	19164	19909	19164
Mean of DV	24.332	24.299	24.332	24.299
Panel B: Birth Dummy				
Post × Underbanked	0.007	0.005	-0.010**	-0.002
	(0.007)	(0.003)	(0.005)	(0.002)
Observations	2263345	2163378	2263345	2163378
Mean of DV	.012	.012	.012	.012
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell in each panel represents a separate regression. The dependent variables are mentioned at the top of the panels. The main independent variable is the interaction of the post dummy with the underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Columns 3 and 4 include birth month and year fixed effects and district-specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. The sample consists of children born from 2014 - 2019, excluding those born in November and December 2016 in rural areas of districts lying in the bandwidth of 0.5 banks per 100,000 people around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 13: Robustness Check: Birth-Level Characteristics

	(1)	(2)	(3)	(4)
Panel A: Female Dummy				
Post × Underbanked	0.051	0.041	0.526	0.575
	(0.183)	(0.183)	(0.810)	(0.869)
Observations	19909	19164	19909	19164
Mean of DV	.471	.471	.471	.471
Panel B: Birth Order				
Post × Underbanked	0.121	0.049	0.375	0.523
	(0.246)	(0.170)	(0.548)	(0.526)
Observations	19909	19168	19909	19168
Mean of DV	.718	.719	.718	.719
Panel C: Birth Interval				
Post × Underbanked	-21.652	-22.284	-29.394	-24.497
	(29.694)	(27.390)	(30.097)	(27.479)
Observations	13120	12637	13120	12637
Mean of DV	38.042	37.921	38.042	37.921
Panel D: Previous sex composition				
Post × Underbanked	-0.059	0.017	0.295	0.307
	(0.267)	(0.282)	(0.342)	(0.347)
Observations	13177	12692	13177	12692
Mean of DV	.537	.538	.537	.538
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell in each panel represents a separate regression. The dependent variables are mentioned at the top of each panel. The main independent variable is the interaction of the post dummy with the underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Columns 3 and 4 include birth month and year fixed effects and district-specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. The sample consists of children born from 2014 - 2019, excluding those born in November and December 2016 in rural areas of districts lying in the bandwidth of 0.5 banks per 100,000 people around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 14: Heterogeneity by Gender

	(1)	(2)	(3)	(4)
Panel A: Female				
Post × Underbanked (β_1)	-0.279	-0.266	-1.069***	-1.119***
	(0.186)	(0.197)	(0.299)	(0.318)
Observations	9416	9058	9416	9058
R-squared	0.002	0.004	0.029	0.030
Panel B: Male				
Post × Underbanked (β_2)	-0.542*	-0.535*	-0.230	-0.215
	(0.313)	(0.302)	(0.197)	(0.195)
Observations	10493	10104	10493	10104
R-squared	0.002	0.005	0.023	0.025
p-value: $\beta_1 = \beta_2$	0.522	0.515	0.074	0.061
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell ian each panel represents a separate regression. The dependent variable is infant mortality. The main independent variable is the interaction of the post dummy with the underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Panels A and B present results for female and male children, respectively. Columns 3 and 4 include birth month and year fixed effects and district-specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, mother's age at first birth, and dummies for child's birth order. At the bottom of the table, we report the *p-value* for the hypothesis test evaluating the equality of the coefficient for our main parameter of interest (Post × Underbanked) between Panels A and B. The sample consists of children born from 2014 - 2019, excluding those born in November and December 2016 in rural areas of districts lying in the bandwidth of 0.5 banks per 100,000 people around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 15: Heterogeneity by Religion

	(1)	(2)	(3)	(4)
Panel A: Muslim				
Post × Underbanked (β_1)	-0.946	-0.879	-0.428	-0.362
	(1.420)	(1.397)	(1.149)	(1.227)
Observations	2199	1797	2193	1789
R-squared	0.027	0.058	0.204	0.231
Panel B: Other Religions				
Post × Underbanked (β_2)	-0.371***	-0.366***	-0.503**	-0.518**
	(0.090)	(0.089)	(0.202)	(0.222)
Observations	17710	17366	17710	17366
R-squared	0.001	0.003	0.014	0.016
p-value: $\beta_1 = \beta_2$	0.674	0.703	0.954	0.911
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell in each panel represents a separate regression. The dependent variable is infant mortality. The main independent variable is the interaction of the post dummy with the underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Panels A and B present results for muslim and children from other religions, respectively. Columns 3 and 4 include birth month and year fixed effects and district-specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. At the bottom of the table, we report the *p-value* for the hypothesis test evaluating the equality of the coefficient for our main parameter of interest (Post × Underbanked) between Panels A and B. The sample consists of children born from 2014 - 2019, excluding those born in November and December 2016 in rural areas of districts lying in the bandwidth of 0.5 banks per 100,000 people around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 16: Heterogeneity by Caste Group

	(1)	(2)	(3)	(4)
Panel A: SC & ST				
Post × Underbanked (β_1)	-1.215***	-1.173***	-1.668**	-1.590**
	(0.379)	(0.352)	(0.661)	(0.625)
Observations	8482	8480	8482	8480
R-squared	0.005	0.008	0.033	0.035
Panel B: Other Caste Groups				
Post × Underbanked (β_2)	-0.091	-0.093	-0.260	-0.244
	(0.084)	(0.083)	(0.269)	(0.268)
Observations	10686	10683	10686	10683
R-squared	0.001	0.004	0.021	0.024
p-value: $\beta_1 = \beta_2$	0.006	0.004	0.116	0.115
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell in each panel represents a separate regression. The dependent variable is infant mortality. The main independent variable is the interaction of the post dummy with the underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Panels A and B present results for SC & ST children, and children from other caste groups, respectively. Columns 3 and 4 include birth month and year fixed effects and district-specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. At the bottom of the table, we report the *p-value* for the hypothesis test evaluating the equality of the coefficient for our main parameter of interest (Post × Underbanked) between Panels A and B. The sample consists of children born from 2014 - 2019, excluding those born in November and December 2016 in rural areas of districts lying in the bandwidth of 0.5 banks per 100,000 people around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 17: Heterogeneity by Wealth

	(1)	(2)	(3)	(4)
Panel A: Rich				
Post × Underbanked (β_1)	-0.729***	-0.745***	-1.015***	-1.031***
	(0.203)	(0.205)	(0.252)	(0.243)
Observations	8118	7771	8118	7771
R-squared	0.004	0.010	0.028	0.034
Panel B: Poor				
Post × Underbanked (β_2)	-0.152	-0.145	-0.178	-0.180
	(0.246)	(0.245)	(0.248)	(0.244)
Observations	11791	11391	11791	11391
R-squared	0.002	0.004	0.024	0.025
p-value: $\beta_1 = \beta_2$	0.093	0.089	0.081	0.066
Individual Level Controls	No	Yes	No	Yes
District Fixed Effects	No	No	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes
District Specific Time Trends	No	No	Yes	Yes
Bandwidth	[-.5;.5]	[-.5;.5]	[-.5;.5]	[-.5;.5]

Notes: Each cell in each panel represents a separate regression. The dependent variable is infant mortality. The main independent variable is the interaction of the post dummy with the underbanked dummy (Post × Underbanked). Post is a dummy variable that takes value 1 for children born post-demonetization (2017-19) and 0 for children born before demonetization (2014-Oct2016). Underbanked is an indicator variable for the districts that were eligible for the 2005 bank expansion policy and thus received more banks and, hence, were less severely affected by demonetization. Panels A and B present results for SC & ST children, and children from other caste groups, respectively. Columns 3 and 4 include birth month and year fixed effects and district-specific time trends. Regressions in columns 2 and 4 include individual level controls. These controls include SC dummy, ST dummy, OBC dummy, muslim dummy, male child dummy, mother's age at first birth, and dummies for child's birth order. At the bottom of the table, we report the *p-value* for the hypothesis test evaluating the equality of the coefficient for our main parameter of interest (Post × Underbanked) between Panels A and B. The sample consists of children born from 2014 - 2019, excluding those born in November and December 2016 in rural areas of districts lying in the bandwidth of 0.5 banks per 100,000 people around the cutoff. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1