Digital Financial Inclusion and Infant Health: Evidence from Mobile Money Expansion in Kenya

Chinekwu Enwonwu*

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Abstract

Can expanding access to digital financial services improve infant health where households face severe liquidity constraints? I study this question using the rapid expansion of mobile money infrastructure in Kenya and examine its effects on infant mortality. Combining administrative data on mobile money agents with retrospective birth panels, I exploit variation in agent density across counties and over time, together with mother and province-by-year fixed effects, comparing siblings born to the same mother under different levels of local mobile money access. I further implement an instrumental-variables strategy based on proximity to early mobile-money adoption hubs interacted with national network expansion. The findings show that greater mobile money access substantially reduces infant mortality. An increase in agent density equal to the average positive within-county expansion during the 2001-2015 sample period (0.13 agents per 1,000 population) is associated with a 0.43 percentage point reduction in infant mortality, or 11.1 percent relative to the sample mean. The effects are largest where initial access is low and diminish as agent networks become denser. The dynamic estimates show no differential pre-trends, placebo tests using pre-mobile-money births yield no corresponding relationship, and the IV estimates produce similar results. Exploring potential mechanisms, I find that greater access to mobile money improves the quality of maternal healthcare received, as mothers shift from home delivery to institutional delivery and initiate antenatal care earlier. Improved mobile money access does not change free health inputs such as vaccinations, and it does not induce a change in maternal labor market outcomes. The findings demonstrate that digital financial inclusion can improve early-life health by relaxing short-run financial barriers to healthcare investment.

*Department of Economics, Northeastern University, Boston, MA 02115. Email: owoh.c@northeastern.edu.

1 Introduction

Access to financial services is an important determinant of health outcomes in low-income countries (Banerjee et al., 2023; Ahmed and Cowan, 2021). This is particularly the case where households face liquidity constraints, income volatility, and limited access to formal insurance. Mobile money systems create a unique access to financial services. Prior to the introduction of mobile money, financial access in Kenya was limited, with only about 26% of adults having access to a formal bank account (Ndung'u, 2017). Mobile money bridged this gap by allowing allowing users to store, send, and receive funds through their mobile phones without any need for a bank account. This technology has expanded rapidly across sub-Saharan Africa over the past two decades particularly in Kenya.[1] By lowering transaction costs and increasing the speed and reliability of transfers, mobile money improves households' ability to smooth consumption, respond to health shocks and finance health-related expenditures (Jack and Suri, 2014; Suri and Jack, 2016; Ahmed and Cowan, 2021).

Despite the rapid diffusion of mobile money and growing evidence on its welfare impacts, little is known about its effects on early-life health outcomes. Lacroze et al. (2021) conduct a randomized controlled trial in Madagascar that uses mobile money to deliver financial assistance for maternal health care and find increases in prenatal visits and facility deliveries. However, the intervention combines digital payments with conditional incentives, making it difficult to isolate the independent effect of mobile money access. The study also evaluates outcomes within a targeted program and focuses on short-run health care utilization. In addition, Ahmed and Cowan (2021) document positive associations between mobile money use and health care utilization, although the cross-sectional nature of their analysis limits the extent to which these associations can be interpreted as causal effects of financial access. As a result, whether the large-scale expansion of mobile money infrastructure improves early-life health outcomes at the population level remains an open question.

I motivate the analysis using the Grossman (1972) framework in which households

[1] Section 2 provides details on the mobile money technology and how it eases transaction.

invest in health subject to resource constraints. Facility delivery and preventive care may be underutilized when households face liquidity constraints because accessing care entails costs such as transportation and user fees. Mobile money can relax these constraints by lowering the cost of sending and receiving funds and improving households' ability to smooth consumption in response to shocks (Jack and Suri, 2014; Suri and Jack, 2016). Greater mobile money access may therefore increase maternal healthcare utilization and investments in infant health by making resources available when care is needed (Ahmed and Cowan, 2021; Banerjee et al., 2023).

A second potential channel operates through women's control over household resources. Mobile money provides accessible financial tools that may increase women's ability to manage resources directly (Aker and Mbiti, 2010; Suri and Jack, 2016). Since resources controlled by women may be more likely to be allocated toward child health and well-being (Duflo, 2003; Menon et al., 2014), greater mobile money access may also improve infant health by increasing women's control over health-related expenditures.

This paper investigates whether greater access to mobile money reduces infant mortality in Kenya. It exploits variation in the intensity of mobile money access across Kenyan counties and over time, measured by the number of mobile money agents per 1,000 population. The analysis uses administrative data on mobile money agent networks that record the number of active agents operating in each county and year. These data provide detailed information on the timing and geographic expansion of mobile money infrastructure across counties. Agent density captures the local availability of mobile money infrastructure and proxies households' ability to send and receive transfers, smooth consumption, and finance health-related expenditures.[2]

I combine these county-year measures of mobile money infrastructure with individual birth histories from the Demographic and Health Surveys (DHS) to estimate the effect of financial access on infant mortality. To the best of my knowledge, this paper is the first to link DHS birth histories with county-level variation in mobile money agent networks in Kenya. While DHS data are widely used to study maternal and child health outcomes, the agent network data provide novel information on the timing, intensity and geographic

[2]Agents serve as local cash-in and cash-out points that enable users to deposit and withdraw funds.

diffusion of mobile money infrastructure. The expansion of agent networks occurred unevenly across counties and over time, generating substantial spatial and temporal variation in financial access.

The main identification challenge is that the expansion of mobile money agents across counties may be correlated with changes in local economic conditions, healthcare access or other determinants of infant mortality. My empirical strategy exploits within-mother variation in exposure to mobile money access across births. Children born to the same mother are exposed to different levels of agent density depending on the timing and location of birth. Mother fixed effects absorb time-invariant maternal characteristics, while year fixed effects account for aggregate shocks and national trends. I additionally control for time-varying individual characteristics and differential local trends.

To address remaining concerns that the expansion of mobile money agents may be correlated with unobserved changes in local economic conditions or healthcare access, I complement the baseline estimates with an instrumental variables strategy. The instrument interacts counties' exposure to early mobile money adoption hubs, measured using geographic distance, with the national expansion of the mobile money network over time. This approach isolates variation in local agent density generated by counties' differential exposure to the national rollout rather than contemporaneous county-level conditions. The IV estimates produce the same negative and nonlinear relationship between mobile money access and infant mortality as the baseline estimates, although the estimated effects are larger.

The results indicate that greater mobile money access reduces infant mortality. The relationship is nonlinear, with the largest reductions occurring at low to moderate levels of agent density and diminishing marginal effects as agent density increases. Evaluated at the sample mean, an increase in agent density equal to the average positive within-county increase observed during the sample period reduces infant mortality by 0.43 percentage points, equivalent to an 11.1 percent decline relative to the sample mean.

I conduct several exercises to assess the robustness of these findings and address remaining identification concerns. First, the results remain similar when I allow for differential trends across counties and provinces by including county-specific linear trends and

province-specific linear time trends. Second, dynamic difference-in-differences estimates show no evidence of differential pre-trends and indicate a decline in infant mortality following increases in mobile money access. Third, placebo tests using births before the introduction of mobile money show no statistically significant relationship between subsequent agent expansion and infant mortality. Finally, I implement an instrumental variables strategy that exploits counties' differential exposure to early mobile money adoption hubs interacted with the national rollout of mobile money. The IV estimates recover the same negative and nonlinear relationship between mobile money access and infant mortality, although the estimated effects are larger.

I am able to identify mechanisms and the evidence points to improved maternal healthcare utilization. Mobile money expansion reduces the probability of giving birth at home and increases the probability of delivering in a health facility. The increase in facility delivery is concentrated in private facilities, with no significant change in government facility delivery. Greater mobile money access also increases the probability of initiating antenatal care early in pregnancy. In contrast, I find no significant effects on childhood vaccination. I also investigate whether the effects operate through women's economic activity or control over financial resources, but find no significant changes in labor market outcomes or control over their own earnings. Taken together, these results suggest that improved access to maternal healthcare is the primary channel linking mobile money expansion to lower infant mortality.

This paper contributes to three related strands of literature. First, it relates to the literature on mobile money, financial inclusion and household risk sharing. Mobile money reduces transaction costs, facilitates transfers and expands access to financial services in settings where formal banking is limited. In Kenya, access to M-Pesa improves households' ability to smooth consumption following adverse shocks by facilitating remittances across a broader network (Jack and Suri, 2014). Mobile money expansion has also been associated with changes in saving and transfer behavior and greater use of formal financial services (Mbiti and Weil, 2011), while its longer-run expansion generated substantial welfare gains and reduced extreme poverty (Suri and Jack, 2016). More recent evidence shows that improved access to mobile money strengthens households' resilience to severe

shocks (Yao et al., 2023). This literature establishes that mobile money can relax financial constraints and improve households' ability to respond to shocks. I extend this literature by examining whether these benefits extend to early-life health.

Second, this paper contributes to a growing literature on financial access, mobile money and healthcare utilization. Greater financial inclusion has been associated with improved population health in developing and transitional economies (Banerjee et al., 2023). In Kenya, access to informal insurance through mobile money is associated with greater consumption smoothing following uninsured health shocks, illustrating how mobile financial networks can protect households when health-related expenditures arise (Geng et al., 2018). More directly, mobile money increases the use of formal healthcare following negative health shocks in Kenya (Ahmed and Cowan, 2021). Related evidence indicates that mobile financial services can affect healthcare-seeking behavior and maternal healthcare utilization (Egami and Matsumoto, 2020). Experimental evidence from Madagascar further shows that a mobile money-based maternal health wallet can affect maternal healthcare utilization (Bogler et al., 2025). I extend this literature by examining whether greater mobile money access translates into improvements in infant survival and by investigating maternal healthcare utilization as a potential mechanism.

Third, the paper relates to the literature on the determinants of infant mortality and early-life health. Existing work shows that maternal and child healthcare, including antenatal care, facility-based delivery and preventive health services, can influence maternal and child health outcomes (Aaby et al., 2010; Carroli et al., 2001; Montgomery et al., 2014). A related empirical literature uses retrospective birth histories to study how changes in economic and institutional conditions affect infant survival (Kudamatsu, 2012; Bhalotra and Clots-Figueras, 2014). I contribute to this literature by examining financial infrastructure as a determinant of infant mortality. In doing so, I connect evidence on the financial and risk-sharing benefits of mobile money to the literature on maternal healthcare and early-life health.

The rest of the paper is organized as follows. Section 2 provides institutional background on mobile money in Kenya. Section 3 describes the data and variable construction. Section 4 outlines the empirical strategy. Section 5 presents the main results, while Section

6 reports robustness checks. Section 7 concludes.

2 Institutional Background

Mobile money was introduced in Kenya in 2007 with the launch of M-PESA by Safaricom.[3] Unlike traditional banking services, mobile money allows users to store, send, and receive funds using a basic mobile phone without requiring a formal bank account.[4] Transactions operate through a nationwide network of agents—typically small retailers—who convert cash into electronic balances and vice versa (Jack and Suri, 2014). This agent-based model substantially lowers barriers to financial access, particularly in areas with limited banking infrastructure.

Official reports from the Central Bank of Kenya document how mobile money grew to dominate the payments ecosystem, significantly increasing financial inclusion compared to traditional banking services (Central Bank of Kenya, 2023). Analysis of agent networks further shows how mobile money agents expanded geographically over time, improving accessibility in rural and urban areas alike (Muñoz Pérez et al., 2019). Broader industry trends reported by GSMA highlight Kenya's role at the forefront of mobile money adoption in sub-Saharan Africa (GSMA, 2025).

This system helps relax liquidity constraints faced by households during pregnancy and childbirth by facilitating rapid financial transfers. Infant mortality in low and middle income countries is closely linked to conditions surrounding pregnancy and childbirth. Prenatal care, skilled birth attendance, and delivery in health facilities are key determinants of neonatal survival. In Kenya, the share of facility-based births has increased over time but remains incomplete. According to the Kenya Demographic and Health Surveys, about 43 percent of births occurred in health facilities in the early 2000s, rising to over 60 percent by the mid-2010s (KNBS and ICF, 2015). A substantial share of births (particularly in rural

[3] The M in M-PESA stands for mobile while PESA is Swahili for money (Jack and Suri, 2011; Suri et al., 2021).

[4] The service does not require internet access or a smartphone; a basic mobile phone and national identification are sufficient to register and conduct transactions. Over time, mobile money expanded to include bill payments, savings products, salary disbursements, and microloans (Mas and Radcliffe, 2010; Central Bank of Kenya, 2023).

areas) still occur outside formal medical facilities, where delays in receiving skilled care and limited access to emergency treatment increase risks for both mothers and infants.

Financial barriers play an important role in shaping maternal health care utilization. Even when facilities are available, households often face out-of-pocket costs such as the cost of transportation, medicines and medical supplies. For households with volatile incomes and limited access to credit or insurance, these expenses can delay or discourage prenatal care and facility-based deliveries. As a result, short-term liquidity constraints can influence whether families seek timely maternal care or not.

Kenya is administratively divided into 47 geographically distinct counties. While mobile money services became available nationwide shortly after launch, the scale and intensity of agent deployment evolved unevenly across counties over time (Mas and Radcliffe, 2010; Muñoz Pérez et al., 2019). Mobile money agents play a central role in determining effective access to these services. A higher density of agents reduces travel time, waiting costs, and liquidity constraints faced by users (Jack and Suri, 2011). In practice, agent density governs how easily households can convert cash into electronic balances and vice versa, making it a meaningful measure of local mobile money infrastructure. Differences in population density, economic activity, and transportation networks led to substantial cross-county variation in the expansion of agent networks (Mas and Radcliffe, 2010). This variation persisted throughout the study period and forms the basis of the empirical strategy used in this paper.

The rollout of mobile money agents was not directly linked to health policy or investments in health infrastructure. Instead, agent expansion was primarily driven by commercial considerations related to demand for financial transactions and remittance services.[5] It occurred gradually and unevenly across counties. Counties with larger populations, higher levels of economic activity, and greater demand for financial services tended to experience earlier and denser agent deployment. As a result, mobile money access increased at different rates across counties, generating considerable spatial and temporal variation in the availability of agent infrastructure. This institutional setting suggests that the geographic expansion of agent networks was largely independent of changes in local health condi-

[5] See, for example, Jack and Suri (2011); Mas and Radcliffe (2010); Muñoz Pérez et al. (2019).

tions.This institutional feature supports the use of county-level variation in agent density as a proxy for changes in local access to mobile money services.

The country was an early adopter of mobile money, with nationwide availability beginning in 2007, and has since experienced rapid and sustained growth in mobile money usage.[6] Importantly, while mobile money became widely available shortly after launch, the intensity of access measured by the density of mobile money agents evolved heterogeneously across counties over time. This generates substantial variation in effective access to mobile money services.

Kenya is also well documented in the mobile money literature, with administrative and survey data available on both financial access and health outcomes. The Demographic and Health Surveys (DHS) provide detailed individual-level birth histories and maternal characteristics, while county-level data on mobile money agents allow for the construction of consistent measures of local access (KNBS and ICF, 2015, 2023). Together, these data enable a rich panel analysis linking changes in financial access to infant health outcomes within a single institutional context.

Therefore, Kenya provides a unique setting to examine the health implications of expanding digital financial infrastructure. The availability of micro-level health data and geographic expansion of mobile money agents makes this study possible. By linking individual birth histories to county-level variation in mobile money infrastructure this paper is able to identify the relationship between mobile money expansion and infant mortality.

3 Data

3.1 Health Data

The primary source of micro data is the Kenya Demographic and Health Surveys (DHS), which provide nationally representative household surveys containing detailed birth histories for women of reproductive age. I use the 2014 and 2022 DHS rounds (KNBS and ICF, 2015, 2023). Apart from being the most recent round of standard Kenya DHS, these

[6] See, for example, Central Bank of Kenya (2023); Jack and Suri (2014); Mas and Radcliffe (2010).

two rounds contain comparable information on infant mortality, maternal characteristics, household demographics, and health behaviors. The retrospective birth histories in these surveys allow the construction of a panel of births over time, which I link to county-level changes in mobile money access using geographic identifiers and year of birth. The analysis covers births from 2001 to 2015, corresponding to the period for which county-level mobile money agent data are available. The analysis therefore focuses on births to women aged 20–35 to reduce heterogeneity associated with high-risk maternal age groups. Figure A1 shows that approximately 73% of births occur among mothers aged 20–35, while births to teenage mothers account for about 21% of the sample.

I define infant mortality as an indicator equal to one if a child dies before reaching 12 months of age and zero otherwise. This measure is constructed from the DHS birth histories and assigned to the child's year of birth. As shown in the summary statistics, the average infant mortality rate in the sample is approximately 3.6%. See Table 2 for an overview of the variables. Infant mortality also varies across counties and over time.

To examine potential mechanisms, the analysis additionally uses information on maternal and child health care utilization contained in the DHS. These variables include antenatal care visits, place of delivery and childhood vaccination records. They provide measures of access to formal health services and preventive care through which improved financial access may affect infant mortality. This information is available in the DHS for the most recent births. These are explained further in the data appendix section and further discussed in the results section. The analysis also examines heterogeneity by household headship using information on whether the household is female-headed, which serves as a proxy for women's control over household financial resources and decision-making.

3.2 Measures of Mobile Money Access

I use data from the FinAccess Geospatial Mapping dataset available through the Harvard Dataverse (Bill & Melinda Gates Foundation et al., 2015). The data provide geocoded information on mobile money agents operating in Kenya between 2007 and 2015, which I aggregate to the county-year level to construct measures of agent density. I measure mobile money access using agent density, defined as the number of mobile money agents per 1,000

population in each county and year. Figure A2 shows the average number of mobile money agents per 1,000 population in Kenya overtime.

Following the introduction of mobile money services in 2007, agent networks expanded rapidly across Kenya, but the pace and scale of expansion differed substantially across counties. Some counties experienced relatively early and dense deployment of agent networks, while others saw slower expansion and lower levels of agent penetration. Figure 1 shows the evolution of mobile money agents and its spread across counties in Kenya over the time period 2007 to 2015. This variation in agent density across counties and years forms the basis of the empirical analysis in this paper.

3.3 County Population

County-level population data are obtained from the Kenya National Bureau of Statistics (Kenya National Bureau of Statistics, 2025). These population counts are used to construct the measure of mobile money access by scaling the number of mobile money agents in each county and year by the county population. Agent density is therefore measured as the number of mobile money agents per 1,000 population.

3.4 Descriptive Statistics

The analysis controls for a set of maternal and child characteristics that may influence infant health outcomes, including maternal age at birth, marital status and the child's gender. Table A1 reports variable definitions and data sources for all variables used in the empirical analysis, while Table 2 presents summary statistics.

Table 1 compares observable characteristics across counties with low and high mobile money access prior to the expansion of mobile money. Counties are classified as low or high access based on whether lagged mobile money agent density falls below or above the median. Maternal and child characteristics are generally well balanced across the two groups. Differences in maternal age, marital status and child gender are small, with normalized differences well below conventional thresholds for imbalance (Imbens and Wooldridge, 2009). Urban residence shows a somewhat larger difference across the two groups, consistent with

greater mobile money access in more urbanized areas.

Figure A1 reports the distribution of births by maternal age. Approximately 73% of births occur among women aged 20–35, while births to teenage mothers account for about 21% of the sample. The analysis therefore focuses on births to women aged 20–35 to reduce heterogeneity associated with high-risk maternal age groups.

4 Empirical Strategy

4.1 Main Specification

Using a difference-in-differences design, I compare siblings born to the same mother who are differentially exposed to mobile money access at birth due to variation in mobile money agent density across counties and over time. In effect, holding fixed all time-invariant maternal characteristics, I estimate how infant survival differs for children born when mobile money access in their county is higher relative to their siblings born when access is lower. To account for unobserved heterogeneity in local conditions, I include province-by-year-of-birth fixed effects and county-specific linear time trends. Approximately 95 percent of mothers remain in the same county across observed births. Mother fixed effects further absorb all time-invariant observed and unobserved maternal characteristics across counties. Identification therefore comes primarily from within-mother variation in exposure to mobile money access across births as agent networks expand over time, net of county-specific trends, observed county characteristics and common province-year shocks.

To estimate the effect of mobile money access on infant mortality, I exploit variation in mobile money agent density across counties and over time. Let i index children, m mothers, c counties, p provinces, and t years. I estimate the following mother fixed effects model:

$$Y_{imcpt} = \beta_1 \,\text{AgentDensity}_{ct} + \beta_2 \,\text{AgentDensity}_{ct}^2 + X_{icpt}'\gamma + Z_{ct}'\delta + \alpha_m + \lambda_t + \theta_c t + \phi_{pt} + \varepsilon_{imcpt}, \tag{1}$$

where Y_{imcpt} is an indicator equal to one if child i of mother m, born in county c in province p and year t, dies before reaching age one. AgentDensity_{ct} measures mobile money access as the number of mobile money agents per 1,000 population in county c and year t. The quadratic term allows for diminishing marginal effects of access as agent penetration increases. The vector X_{imcpt} includes time-varying individual characteristics such as maternal age at birth, marital status, and child gender. The vector Z_{ct} captures time-varying county-level socioeconomic characteristics, including literacy rates, unemployment rates, urbanization, female population share, and total population. Mother fixed effects α_m absorb

all time-invariant maternal characteristics, while year fixed effects λ_t control for national shocks and aggregate trends. Standard errors are clustered at the county level to account for arbitrary correlation in the error term within counties over time

The baseline model includes mother and year fixed effects, along with individual- and county-level time-varying characteristics. I then introduce county-specific linear time trends, $\theta_c t$, to account for unobserved changes in local conditions, such as improvements in health infrastructure or economic development. Finally, I incorporate province-by-year fixed effects, ϕ_{pt}, which absorb shocks common to all counties within a province in a given year, including regional policy changes, health campaigns, and macroeconomic fluctuations. The preferred specification therefore compares changes in infant mortality across counties within the same province and year, net of county-specific trends and observed covariates.

The identifying assumption is that, conditional on mother fixed effects, observed covariates, county-specific trends, and province-specific time trends, changes in mobile money access are uncorrelated with unobserved determinants of infant mortality. Equivalently, absent changes in mobile money access, infant mortality would have followed parallel trends across counties within provinces for a given mother's fertility history.

The main analysis therefore exploits within-mother variation across siblings born under different levels of mobile money access, and all baseline specifications include mother fixed effects. The mechanism outcomes, including place of delivery, antenatal care and childhood vaccination, are observed only for recent births in the DHS. Consequently, mother fixed effects cannot be estimated for these outcomes. Following Bhalotra and Clots-Figueras (2014) and Kudamatsu (2012), I instead estimate specifications with mother-group fixed effects, where mother groups are defined by maternal education, place of residence and maternal birth cohort.

4.2 Dynamic Difference-in-Differences Approach

The expansion of mobile money access across Kenyan counties occurred gradually and unevenly over time, with counties experiencing discrete changes in agent density at different points. In such settings, standard two-way fixed effects estimators may produce biased

estimates when treatment effects are heterogeneous across units or over time (Goodman-Bacon, 2021; Sun and Abraham, 2021). To address this concern, I complement the baseline specifications with the dynamic difference-in-differences estimator proposed by de Chaisemartin and D'Haultfœuille (2020).

This approach allows for heterogeneous and dynamic treatment effects and adjusts for compositional changes in the set of identifying counties over time, ensuring that the same counties contribute to the identification of each post-treatment effect. At the same time, it provides a transparent framework to assess pre-treatment trends and evaluate the plausibility of the identifying assumptions.

Treatment is defined as a change in a county's mobile money access category, measured by quintiles of lagged county-level agent density. Counties are assigned to quintiles based on one-period lagged agent density, and a treatment event occurs when a county moves from one quintile to another. Event time is indexed relative to the last period before the treatment change.

The event-study specification estimates dynamic treatment effects for up to three post-treatment periods ($t = 0, 1, 2, 3$) and includes placebo leads for three pre-treatment periods ($t = -1, -2, -3$). This structure allows the analysis to trace the evolution of infant mortality around changes in mobile money access while providing a direct test for differential pre-treatment trends.

The dynamic specification estimates event-time coefficients for leads and lags of treatment:

$$Y_{imct} = \sum_{k \neq -1} \beta_k \mathbf{1}\{\tau_{ct} = k\} + X'_{imct}\gamma + \alpha_m + \lambda_t + \varepsilon_{imct}. \tag{2}$$

Here, Y_{imct} is an indicator equal to one if child i born to mother m in county c and year t dies before age one. The variable τ_{ct} denotes event time relative to a change in county-level mobile money access. The omitted category is the period immediately before the change in access, $k = -1$. The coefficients β_k measure event-time differences in infant mortality relative to this pre-treatment period. Pre-treatment coefficients provide a test for differential pre-trends, while post-treatment coefficients trace the dynamic effects of increased mobile money access.

4.3 Instrumental Variable Strategy

Although the baseline specifications include mother fixed effects, county-specific linear time trends and province-by-year-of-birth fixed effects, the placement of mobile money agents may still be endogenous. Agent networks may expand more rapidly in areas experiencing favorable economic conditions or improvements in health infrastructure, leading to biased estimates of the effect of mobile money access on infant mortality.

To address this concern, I exploit variation in geographic exposure to the earliest mobile money adoption centers in Kenya combined with national mobile money expansion over time. The instrument follows the logic of shift-share designs that interact predetermined geographic exposure with aggregate national shocks (Erten and Keskin, 2021). It also draws on the broader insight from the infrastructure literature that geographic proximity to early network nodes can generate plausibly exogenous variation in local access as networks expand (Faber, 2014). Counties located closer to early commercial hubs were therefore more likely to experience faster growth in agent networks as the mobile money system expanded nationally.

For each county c, exposure to early mobile money hubs is measured as the average inverse-distance proximity to a set of early adoption centers:

$$Exposure_c = \frac{1}{H} \sum_{h=1}^{H} \frac{1}{1 + Distance_{ch}}, \tag{3}$$

where $Distance_{ch}$ denotes the geodesic distance between county c and hub h, and H is the number of hubs included in the exposure measure. In the preferred specification, $H = 2$, corresponding to Nairobi and Mombasa, the earliest and largest commercial centers in the mobile money network.

The county-specific exposure measure is interacted with aggregate national mobile money rollout to construct a Bartik-style instrument:

$$Z_{ct} = Exposure_c \times Rollout_t, \tag{4}$$

where $Rollout_t$ denotes the total number of mobile money agents operating nationally

in year t.

The first-stage relationship is given by

$$AgentDensity_{ct} = \pi_0 + \pi_1 Z_{ct} + X'_{ct}\pi + \alpha_i + \lambda_t + \varepsilon_{ict}, \tag{5}$$

where X_{ct} includes county-level controls, α_i denotes mother fixed effects, and λ_t denotes year fixed effects.

The second-stage equation replaces observed agent density with its predicted value from the first stage:

$$InfantMortality_{ict} = \beta_0 + \beta_1 \widehat{AgentDensity}_{ct} + \beta_2 \widehat{AgentDensity}^2_{ct} + X'_{ct}\gamma + \alpha_i + \lambda_t + u_{ict}. \tag{6}$$

Identification comes from differential exposure to early mobile money hubs across counties combined with nationwide expansion of the mobile money network over time. Conditional on fixed effects, county controls, county-specific trends, and province-year fixed effects, the identifying assumption is that proximity to early adoption centers affects infant mortality only through its effect on local mobile money access.

4.4 Placebo Test

To assess the validity of the identification strategy, I implement a placebo test using births occurring prior to the introduction of mobile money in Kenya. Specifically, I restrict the sample to births between 1985 and 1999 and assign placebo treatment using the observed future expansion of mobile money agent density. For each county-year in the placebo period, agent density is matched to the corresponding future year in the post-2000 period. This preserves the spatial and temporal variation in treatment intensity while ensuring that mobile money access could not have affected outcomes during the placebo period. The placebo specification mirrors the main empirical design.

5 Results

5.1 Effects of Mobile Money on Infant Mortality

Table 3 reports the main estimates of the relationship between mobile money access and infant mortality using a quadratic specification that allows for nonlinear effects as specified in Equation 1. Across all specifications, the coefficient on agent density is negative, while the squared term is positive and statistically significant, indicating a convex relationship with diminishing marginal effects. Columns (1)–(3) present increasingly saturated specifications, with column (3) corresponding to the preferred model that includes the full set of controls. In this specification, greater agent density is associated with significant reductions in infant mortality at low to moderate levels of access. However, the marginal effect attenuates as access expands, consistent with diminishing returns to mobile money access.

Evaluated at the sample mean, an increase in agent density equal to the average positive within-county increase observed during the sample period (0.130) is associated with a reduction in infant mortality of approximately 0.43 percentage points, corresponding to a decline of about 11.1 percent relative to the sample mean.[7] These findings are consistent with improved financial access reducing barriers to formal maternal healthcare, a mechanism supported by the increase in facility deliveries documented below. The positive quadratic term indicates diminishing marginal effects as agent density increases.

The estimated turning point occurs at approximately 1.61 agents per 1,000 population, which lies substantially above both the sample mean of 0.176 and the 95th percentile of the observed distribution (0.865).[8] Although the turning point lies below the 99th percentile of the distribution (1.818), it remains near the upper tail, indicating that the vast majority of observations in the estimation sample fall within the range where increases in agent density are associated with reductions in infant mortality. Figure A3 presents the cumulative distribution of agent density and illustrates the location of the turning point within the observed distribution.

[7] 0.130 corresponds to the average positive within-county increase in agent density observed during the sample period. See Appendix A.1 for the full marginal effect calculation based on Wooldridge (2010).

[8] See Appendix A.2 for the full calculation based on Wooldridge (2010).

The estimates remain stable across increasingly demanding specifications that account for county-specific trends and province-specific linear time trends, suggesting that the results are not driven by differential regional trends. Instead, the findings point to the importance of early-stage expansion of mobile money infrastructure in improving early-life health outcomes.

5.2 Dynamic Effects

Figure 2 presents event-study estimates based on the estimator proposed by de Chaisemartin and D'Haultfœuille (2020), which is robust to potential biases arising from heterogeneous and dynamic treatment effects. The dynamic estimates highlight three points.

First, the coefficients in the pre-treatment periods are small in magnitude and statistically insignificant, providing no evidence of differential pre-trends prior to changes in mobile money access. The placebo coefficients are jointly close to zero, supporting the identifying assumptions underlying the baseline specification.

Second, declines in infant mortality materialize only after increases in mobile money access. The largest effects occur immediately following treatment, consistent with mechanisms operating through improved liquidity and short-run consumption smoothing. The post-treatment effects attenuate over longer horizons, consistent with diminishing marginal returns to additional mobile money access.

Third, the dynamic event-study estimates based on the estimator proposed by de Chaisemartin and D'Haultfœuille yield similar conclusions. The placebo estimates are jointly insignificant ($p = 0.400$), providing no evidence of differential pre-trends, while the post-treatment effects are jointly significant ($p = 0.032$). The first post-treatment estimate indicates a decline in infant mortality following increases in mobile money access, consistent with the baseline estimates. These results suggest that the main findings are robust to treatment effect heterogeneity and are unlikely to be driven by biases associated with conventional two-way fixed effects estimators.

5.3 Potential Channels

To investigate the mechanisms through which mobile money access affects infant mortality, I first examine whether greater access to mobile money changes maternal health-seeking behavior during pregnancy and childbirth, including place of delivery, antenatal care, iron supplementation and childhood immunization. Because these outcomes are observed only for recent births, I estimate these specifications using mother-group fixed effects, following Bhalotra and Clots-Figueras (2014) and Kudamatsu (2012). Mother groups are defined by maternal education, place of residence and maternal birth cohort. I then examine whether mobile money affects women's labor market outcomes and financial decision-making. Finally, I explore whether the effects differ by household headship, wealth, maternal education and place of residence.

Health care seeking behavior. One mechanism through which mobile money access may improve infant health is by increasing the use of formal maternal healthcare services. By improving households' ability to receive transfers and access funds in response to health shocks, mobile money can relax liquidity constraints that limit the use of maternal healthcare (Jack and Suri, 2014; Suri and Jack, 2016). Greater financial access may therefore increase facility-based deliveries and the use of maternal health services associated with improved birth outcomes (Montgomery et al., 2014; Carroli et al., 2001). Mobile money may also improve infant health by increasing investment in preventive healthcare, such as childhood vaccination, or by improving women's labor market outcomes and control over household resources.

Table 4 examines maternal health-seeking behavior as a potential mechanism through which mobile money access affects infant mortality. Approximately 45 percent of births in the estimation sample occur at home, while 54 percent occur in a health facility. Greater mobile money access significantly reduces the probability of home delivery and increases the probability of facility delivery. The increase in facility delivery appears to be driven primarily by greater use of private facilities, while there is no statistically significant change in deliveries at government facilities. The positive coefficient on the squared term indicates

that these effects diminish as agent density increases.

I next examine antenatal care utilization. There is little evidence that mobile money expansion affects the total number of antenatal visits. However, greater mobile money access increases the probability that women initiate antenatal care during the first trimester. I also find evidence of increased iron supplementation at higher levels of agent density. These results suggest that improved financial access facilitates earlier engagement with maternal healthcare during pregnancy.

Finally, I examine childhood vaccination outcomes, including completion of the recommended polio and DPT vaccination schedules, as well as receipt of the measles and BCG vaccines. The estimated effects are uniformly small and statistically insignificant across all outcomes, suggesting that improvements in childhood immunization are unlikely to explain the reduction in infant mortality.

The evidence points to changes in maternal health-seeking behavior as an important mechanism through which mobile money access improves infant health. Mobile money expansion reduces home deliveries and increases facility-based deliveries, while there is limited evidence of changes in childhood vaccination. Although the evidence for antenatal care is mixed, the findings are consistent with mobile money relaxing short-term liquidity constraints around childbirth, enabling households to substitute away from home births toward facility-based delivery.

Labor market outcomes and women's financial autonomy. Table 5 examines whether mobile money access is associated with women's labor market participation and financial autonomy. Approximately 64 percent of women in the estimation sample are currently employed, 69 percent worked during the previous twelve months and 93 percent report controlling the use of their earnings either alone or jointly with their partner. Greater mobile money access is positively associated with both current employment and employment during the previous twelve months. For both outcomes, the positive linear coefficient and negative quadratic coefficient indicate that the association diminishes as agent density increases. In contrast, there is no statistically significant relationship between mobile money access and women's control over their earnings. These results provide suggestive evidence

that increased labor market participation may be an additional channel through which mobile money access improves infant health, potentially by increasing the resources available to households for health-related expenditures.

Heterogeneity by Household Characteristics The effects of mobile money access may differ across households depending on women's control over financial resources and their ability to use additional financial resources to invest in maternal and child health. Mobile money can increase women's ability to receive and manage transfers directly, potentially strengthening their role in household decision-making (Duflo, 2003). Differences in household headship, wealth, maternal education and place of residence may therefore influence the extent to which improved financial access translates into better infant health outcomes. Table 6 examines whether the effect of mobile money access on infant mortality differs across household characteristics. Panel A presents estimates by household headship. The estimated reduction in infant mortality is larger for female-headed households than for male-headed households. However, the joint heterogeneity test is not statistically significant ($p = 0.634$), indicating that the estimated differences between the two groups are not statistically distinguishable.

Panel B examines heterogeneity across household wealth groups. The estimated effects vary across the wealth distribution, with the largest reduction in infant mortality observed among middle-wealth households. Estimates for the remaining wealth groups are generally imprecise. The joint heterogeneity test rejects equality of the coefficients across wealth groups ($p = 0.001$), providing evidence that the relationship between mobile money access and infant mortality differs across the wealth distribution.

Panel C considers heterogeneity by maternal education. The estimated effects are negative across education groups, although the individual coefficients are imprecisely estimated. The joint heterogeneity test is marginally significant ($p = 0.089$), providing suggestive evidence that the effects of mobile money access differ by maternal education.

Panel D reports estimates by place of residence. The estimated effects are similar across urban and rural areas. Although the coefficients are statistically significant for rural households, the joint heterogeneity test fails to reject equality of the estimates across the two

groups ($p = 0.629$).

Overall, the heterogeneity analysis provides evidence that the effects of mobile money access vary across the wealth distribution and weaker evidence of differences by maternal education. In contrast, there is little evidence of systematic differences by household headship or place of residence.

6 Robustness and Identification Checks

I conduct a series of checks to assess the robustness of the main results and the identifying assumptions underlying the empirical strategy. I first examine whether the results depend on the restriction of the main sample to mothers aged 20–35. I then use an instrumental variables strategy to address potential endogeneity in the expansion of the mobile money agent network. Finally, I conduct a placebo test using births that occurred before the introduction of mobile money in Kenya.

6.1 Alternative Samples

The main analysis restricts the sample to mothers aged 20–35 at the time of birth. Table A2 examines whether this restriction drives the results. Column (1) uses the full sample of births, while columns (2) and (3) consider teenage mothers aged 19 or younger and mothers older than 35, respectively.

The full-sample estimates preserve the pattern in the baseline results: the coefficient on agent density is negative and the squared term is positive. The estimates for teenage and older mothers are also negative at low levels of agent density but are considerably less precise. The lack of precision is consistent with the much smaller samples in these age groups. Overall, the direction of the estimates is similar across the three samples, suggesting that the main findings are not driven by the restriction to mothers aged 20–35.

6.2 Instrumental Variable Results

A remaining concern is that the expansion of the mobile money agent network may be correlated with unobserved changes in local economic conditions or health investments. To address this concern, I instrument county-level agent density using a Bartik instrument that interacts counties' exposure to early mobile money hubs with the national rollout of mobile money agents. Both agent density and its squared term are treated as endogenous and instrumented using the Bartik measure and its square.

Table 8 reports the instrumental variable results. Panel A presents the first-stage estimates, while Panel B reports the corresponding two-stage least squares estimates. The excluded instruments jointly predict both endogenous regressors, with first-stage F-statistics of 18.75 and 10.18. The Sanderson–Windmeijer F-statistics are 46.29 and 44.99, while the Kleibergen–Paap Wald F-statistic for the IV specification is 20.86.

The IV estimates preserve the nonlinear pattern in the baseline results. In the preferred specification in column (3), the coefficient on agent density is negative and statistically significant, while the squared term is positive and statistically significant. The estimates imply that greater mobile money access is associated with lower infant mortality over the relevant range of agent density, with diminishing marginal effects as access expands. The IV estimates are larger in magnitude than the baseline fixed-effects estimates. This difference may reflect measurement error in agent density or differences in the variation used to identify the IV and fixed-effects estimates. Overall, the IV results are consistent with the main finding that greater mobile money access reduces infant mortality.

6.3 Placebo Test

I examine whether the estimated relationship is present before mobile money was introduced in Kenya. If counties that subsequently experienced greater mobile money expansion were already on different infant mortality trajectories, the baseline estimates could capture these pre-existing differences rather than the effect of mobile money access.

Table 7 reports estimates using births that occurred before the introduction of mobile money. The coefficients on both agent density and its squared term are statistically in-

significant across all specifications and there is no consistent pattern in the estimates as the specification is progressively saturated. In contrast to the main results, there is therefore no evidence of a systematic relationship between subsequent mobile money exposure and infant mortality in the pre-mobile-money period.

The placebo results provide evidence against the main estimates being driven by pre-existing differences in infant mortality trends across counties. Together with the alternative-sample and instrumental variable results, these findings support the interpretation that the decline in infant mortality is associated with the expansion of mobile money access rather than differences that predate the rollout.

7 Conclusion

This paper investigates whether the expansion of mobile money access improves early-life health in Kenya. I combine individual birth histories from the Kenya Demographic and Health Surveys with county-level variation in the expansion of mobile money agent networks. The empirical strategy exploits differences in mobile money exposure across siblings born to the same mother at different points in the rollout. I find that greater mobile money access reduces infant mortality. The relationship is nonlinear, with the largest gains occurring at low to moderate levels of agent density and diminishing marginal effects as access expands. An increase in agent density equal to the average positive within-county increase observed during the sample period is associated with a 0.43 percentage point reduction in infant mortality, equivalent to an 11.1 percent decline relative to the sample mean.

The evidence on mechanisms points primarily to maternal health-seeking behavior around childbirth. Greater mobile money access reduces home deliveries and increases facility-based deliveries, with the increase appearing to be concentrated in private facilities. There is also some evidence of earlier antenatal care and increased iron supplementation, while there is little evidence of changes in childhood vaccination. Mobile money access is also positively associated with women's employment and employment during the previous 12 months, although there is no significant relationship with women's control over

earnings. These results are consistent with mobile money relaxing liquidity constraints that limit access to maternal healthcare, particularly around the time of delivery.

A range of identification and robustness checks support the main findings. The estimates are robust to county-specific linear time trends and province $\times$ year-of-birth fixed effects and are similar when alternative maternal age samples are considered. Dynamic treatment effect estimates show no evidence of differential pre-trends, while placebo estimates using births before the introduction of mobile money are statistically insignificant. An instrumental variables strategy based on exposure to early mobile money hubs also produces a negative and nonlinear relationship between mobile money access and infant mortality, with estimates that are larger than the baseline fixed-effects estimates.

The results show that the benefits of financial inclusion extend beyond financial transactions. Access to digital financial infrastructure can improve health by allowing households to mobilize resources when healthcare needs arise. The diminishing marginal effects suggest that the health gains from expanding agent networks are largest where access is initially limited. Expanding mobile money infrastructure may therefore generate larger health benefits in financially underserved areas. These gains, however, depend on households having healthcare services to access. Financial infrastructure should therefore be viewed as a complement to, rather than a substitute for, investments in maternal and child healthcare.

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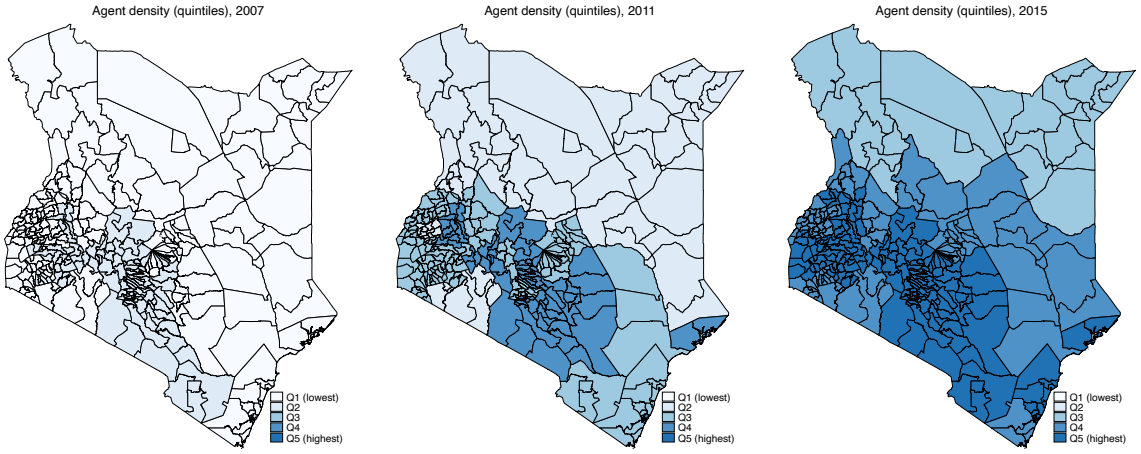
8 Figures and Tables

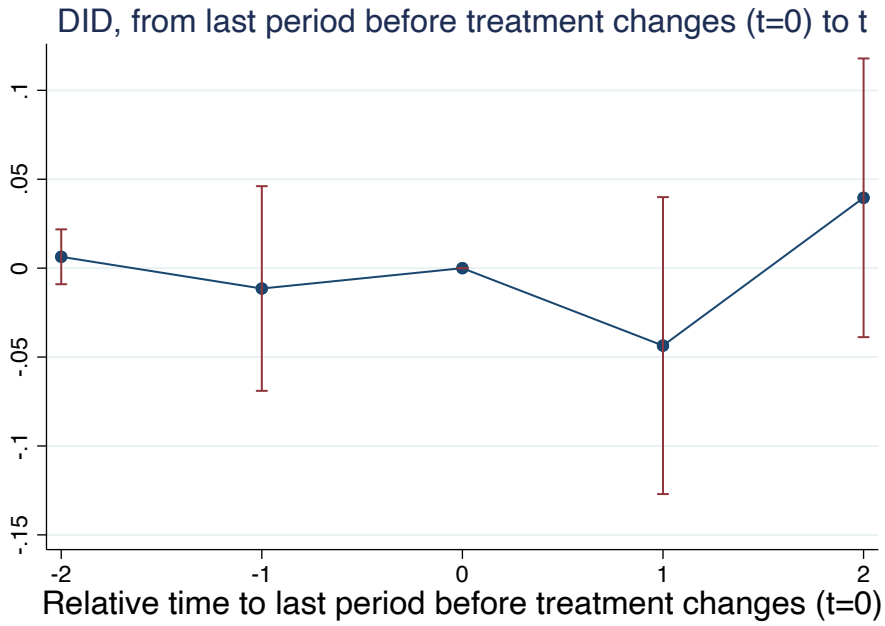
FIGURE 1: EVOLUTION OF MOBILE MONEY AGENT DENSITY ACROSS KENYAN COUNTIES, BY AGENT DENSITY QUINTILE



(A) 2007 (B) 2011 (C) 2015

Note. Shading indicates county-level mobile money agent density, measured as agents per 1,000 population. Counties are classified into quintiles based on the pooled distribution across 2007, 2011, and 2015. The same quintile thresholds are applied in all panels, allowing the spatial expansion of agent density to be compared over time. Darker shading indicates higher agent density. Because the thresholds are fixed across years, some quintiles are not represented in earlier years.

FIGURE 2: DYNAMIC EFFECTS OF MOBILE MONEY AGENT DENSITY ON INFANT MORTALITY (DE CHAISEMARTIN–D'HAULTFŒUILLE ESTIMATOR)



Note. The figure presents dynamic treatment effects estimated using the method of de Chaisemartin and D'Haultfœuille (2020), which is robust to staggered treatment timing and heterogeneous treatment effects. Treatment is defined as a change in the county-level quintile of lagged mobile money agent density. Event time is indexed relative to the last pre-treatment period ($t = 0$), and coefficients are normalized to $t = -1$. The figure reports point estimates and 95 percent confidence intervals for up to three leads and lags. Estimates are weighted by county population and standard errors are clustered at the county level. The estimation sample is restricted to births to women aged 20–35.

TABLE 1: BASELINE CHARACTERISTICS BY MOBILE MONEY ACCESS, BIRTHS PRE-MOBILE MONEY EXPANSION (PRE-2007)

	Low Agent Density		High Agent Density		Normalized
Variable	Mean	S.D.	Mean	S.D.	Difference
Maternal age at birth	25.848	4.170	26.203	4.291	0.084
Married	0.823	0.382	0.838	0.369	0.038
Female child	0.493	0.500	0.494	0.500	0.000
Urban residence	0.283	0.450	0.330	0.470	0.102

Notes: The data come from the 2014 and 2022 Kenya Demographic and Health Surveys (DHS). The table reports means and standard deviations of pre-expansion characteristics for observations with below-median and above-median mobile money agent density. Following (Imbens and Wooldridge, 2009), normalized differences are computed as

$$\Delta = \frac{\bar{X}_1 - \bar{X}_2}{\sqrt{(S_1^2 + S_2^2)/2}},$$

where $\bar{X}_1$ and $\bar{X}_2$ denote the group means and S_1^2 and S_2^2 denote the corresponding variances. A normalized difference of 0.25 is used as the rule-of-thumb threshold (Imbens and Wooldridge, 2009). The sample is restricted to mothers aged 20–35 years at the time of birth.

TABLE 2: SUMMARY STATISTICS

	Mean	SD	Min	Max	N
Panel A: Child Health Outcomes					
Infant mortality	0.036	0.187	0.000	1.000	71,512
Polio complete	0.737	0.440	0.000	1.000	14,805
DPT complete	0.814	0.389	0.000	1.000	14,831
BCG vaccination	0.940	0.238	0.000	1.000	14,837
Measles vaccination	0.732	0.443	0.000	1.000	14,822
Panel B: Maternal Health Care Utilization					
Home delivery	0.448	0.497	0.000	1.000	15,354
Facility delivery	0.543	0.498	0.000	1.000	15,354
Public facility delivery	0.429	0.495	0.000	1.000	15,354
Private facility delivery	0.114	0.318	0.000	1.000	15,354
Antenatal visits	3.790	1.831	0.000	20.000	11,070
Early ANC	0.189	0.391	0.000	1.000	10,459
Iron supplementation	0.687	0.464	0.000	1.000	5,348
Panel C: Labor Market Outcomes					
Currently working	0.630	0.483	0.000	1.000	50,579
Worked last 12 months	0.670	0.470	0.000	1.000	49,532
Controls own earnings	0.931	0.253	0.000	1.000	19,661
Panel D: Mobile Money Access					
Agent density (per 1,000 population)	0.212	0.401	0.000	3.330	71,512
Panel E: Individual Characteristics					
Maternal age at birth	26.026	4.235	20.000	35.000	71,512
Married	0.830	0.375	0.000	1.000	71,512
Female child	0.494	0.500	0.000	1.000	71,512
Urban residence	0.307	0.461	0.000	1.000	71,512

Notes: This table reports summary statistics for the main variables used in the analysis. Infant mortality is an indicator equal to one if the child dies before age one. Agent density is measured as the number of mobile money agents per 1,000 population at the county–year level. The sample is restricted to births to women aged 20–35 at the time of birth.

TABLE 3: EFFECT OF MOBILE MONEY ACCESS ON INFANT MORTALITY

	(1)	(2)	(3)
Agent density	-0.0243***	-0.0378**	-0.0376**
	(0.0065)	(0.0144)	(0.0144)
Agent density^2	0.0092***	0.0116***	0.0117***
	(0.0031)	(0.0039)	(0.0040)
Mother and year-of-birth fixed effects	x	x	x
Time-varying maternal and child characteristics	x	x	x
County-specific linear time trends	—	x	x
Province × year-of-birth fixed effects	—	—	x
Outcome mean	0.039	0.039	0.039
Observations	60,425	60,425	60,425

Notes: This table reports estimates of the effect of mobile money agent density on infant mortality. Agent density is measured as the number of mobile money agents per 1,000 population at the county–year level. All specifications allow for a quadratic relationship between agent density and infant mortality and include mother and year-of-birth fixed effects and time-varying maternal and child characteristics. These characteristics include maternal age at birth, marital status and child gender. Column (2) additionally includes county-specific linear time trends, while Column (3) further includes province × year-of-birth fixed effects. Approximately 95 percent of mothers remain in the same county across observed births, so mother fixed effects absorb time-invariant county characteristics for most mothers. Standard errors clustered at the county level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 4: EFFECT OF MOBILE MONEY ACCESS ON HEALTH-SEEKING BEHAVIOR

Panel A: Place of Delivery				
	Home Delivery	Government Facility	Private Facility	Facility Delivery
Agent density	-0.169**	-0.0407	0.201*	0.160*
	(0.0802)	(0.139)	(0.104)	(0.0802)
Agent density^2	0.0442**	0.0127	-0.0555**	-0.0428**
	(0.0168)	(0.0291)	(0.0269)	(0.0167)
Outcome mean	0.448	0.429	0.114	0.543
Observations	15,351	15,351	15,351	15,351
Panel B: Antenatal Care				
	Number of Antenatal Visits	Started Antenatal Care in First Trimester	Took Iron Supplements	
Agent density	-0.113	0.225*	-0.340	
	(0.456)	(0.124)	(0.239)	
Agent density^2	0.0930	-0.0340	0.119**	
	(0.131)	(0.0238)	(0.0456)	
Outcome mean	3.789	0.189	0.687	
Observations	11,066	10,453	5,341	
Panel C: Child Vaccination				
	Polio	Measles	DPT	BCG
Agent density	-0.0278	-0.139	0.0869	0.0560
	(0.0985)	(0.114)	(0.113)	(0.0602)
Agent density^2	0.0243	0.0308	0.0003	0.0058
	(0.0246)	(0.0371)	(0.0255)	(0.0162)
Outcome mean	0.737	0.732	0.814	0.940
Observations	14,802	14,819	14,828	14,834

Notes: Each column reports estimates using the indicated outcome variable. All specifications include mother-group fixed effects, county fixed effects, province-year fixed effects, individual controls, county-level controls and county-specific linear time trends. Mother groups are defined by maternal education, place of residence and maternal cohort. Standard errors clustered at the county level are reported in parentheses. Home Delivery indicates delivery at the respondent's home or another home. Government Facility indicates delivery at a government or public health facility, while Private Facility indicates delivery at a private health facility. Facility Delivery indicates delivery at either a government/public or private health facility. Number of Antenatal Visits is the total number of antenatal care visits during pregnancy. Started Antenatal Care in First Trimester is an indicator equal to one if the first antenatal care visit occurred during the first trimester. Took Iron Supplements is an indicator equal to one if the mother received or purchased iron tablets or syrup during pregnancy. Polio and DPT indicate completion of the recommended vaccination series, while Measles and BCG indicate receipt of the corresponding vaccine. Outcome means are calculated using the estimation sample for each specification. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

TABLE 5: EFFECT OF MOBILE MONEY ACCESS ON LABOR MARKET OUTCOMES

	Currently Working	Worked in Last 12 Months	Controls Own Earnings
Agent density	0.521***	0.555***	0.0152
	(0.121)	(0.131)	(0.0235)
Agent density^2	-0.136***	-0.143***	-0.00153
	(0.0357)	(0.0383)	(0.00619)
Outcome mean	0.644	0.690	0.934
Observations	26,702	26,095	10,465

Notes: Each column reports woman-level regressions using the indicated outcome variable. Agent density is measured as county-level mobile money agent density in 2015. All specifications include survey-year fixed effects, maternal cohort fixed effects and marital status. Standard errors are clustered at the county level. Currently Working is an indicator equal to one if the respondent was employed at the time of the survey. Worked in Last 12 Months is an indicator equal to one if the respondent worked during the twelve months preceding the survey. Controls Own Earnings is an indicator equal to one if the respondent reports deciding alone or jointly how her earnings are used. Outcome means are calculated using the estimation sample for each specification. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

TABLE 6: HETEROGENEITY IN THE EFFECT OF MOBILE MONEY ACCESS ON INFANT MORTALITY

Panel A: Household Headship					
	(1) Female-headed	(2) Male-headed			
Agent density	-0.0620**	-0.0247			
	(0.0292)	(0.0164)			
Agent density^2	0.0201**	0.0069			
	(0.0084)	(0.0049)			
Outcome mean	0.041	0.038			
p-value: Agent density, (1) = (2)	0.343				
Observations	20,802	39,552			
Panel B: Wealth Quintile					
	(1) Poorest	(2) Poor	(3) Middle	(4) Rich	(5) Richest
Agent density	-0.0322	0.0154	-0.1067*	0.0148	-0.0591
	(0.0355)	(0.0554)	(0.0594)	(0.0490)	(0.0657)
Agent density^2	0.0126	-0.0103	0.0338*	-0.0015	0.0150
	(0.0116)	(0.0201)	(0.0197)	(0.0137)	(0.0137)
Outcome mean	0.037	0.041	0.039	0.042	0.041
p-value: Agent density, (2) = (3)	0.287				
p-value: Agent density, (3) = (4)	0.887				
Observations	20,849	12,632	11,001	9,084	6,296
Panel C: Maternal Education					
	(1) No Education	(2) Primary Education	(3) Secondary or Higher		
Agent density	-0.0093	-0.0448	-0.0299		
	(0.0524)	(0.0317)	(0.0611)		
Agent density^2	0.0046	0.0108	0.0124		
	(0.0157)	(0.0096)	(0.0165)		
Outcome mean	0.037	0.042	0.036		
p-value: Agent density, (1) = (2)	0.506				
p-value: Agent density, (2) = (3)	0.162				
Observations	15,474	31,143	13,341		
Panel D: Place of Residence					
	(1) Urban	(2) Rural			
Agent density	-0.0436	-0.0394**			
	(0.0303)	(0.0172)			
Agent density^2	0.0117	0.0139**			
	(0.0078)	(0.0066)			
Outcome mean	0.041	0.039			
p-value: Agent density, (1) = (2)	0.481				
Observations	17,000	43,087			

Notes: Each column reports estimates from the baseline specification estimated separately for the indicated subgroup. All regressions include mother fixed effects, year-of-birth fixed effects, mother's time-varying characteristics, child gender, county-specific linear time trends and province $\times$ year-of-birth fixed effects. Standard errors clustered at the county level are reported in parentheses. Wealth groups correspond to quintiles of the DHS wealth index. Maternal education is grouped into no education, primary education and secondary or higher education. The reported p-values are obtained from pooled specifications interacting the subgroup indicators with agent density and its squared term. The p-values test the null hypothesis that the coefficient on agent density is equal across the indicated columns. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

TABLE 7: FALSIFICATION TESTS USING DATA FROM PRE-MOBILE MONEY ACCESS PERIOD

	(1)	(2)	(3)
Agent density	0.0169	-0.0484	-0.0525
	(0.0265)	(0.0393)	(0.0400)
Agent density^2	-0.0056	0.0016	0.0022
	(0.0087)	(0.0103)	(0.0104)
Mother and year-of-birth fixed effects	x	x	x
Time-varying mother characteristics	x	x	x
Baseline county characteristics	x	x	x
County-specific linear time trends	—	x	x
Province $\times$ year-of-birth fixed effects	—	—	x
Outcome mean	0.060	0.060	0.060
Observations	12,720	12,720	12,720

Notes: The table reports placebo estimates using births from the pre-mobile money period (1985–1999). The placebo treatment is constructed by assigning to each county the observed agent density from corresponding future years (e.g., 1985 is assigned 2001 values), thereby preserving the spatial and temporal variation in mobile money access while ensuring that the assigned treatment cannot have affected outcomes. The specifications correspond to those in the baseline analysis. All specifications include mother and year-of-birth fixed effects, time-varying maternal and child characteristics and baseline county characteristics. Time-varying maternal and child characteristics include maternal age at birth, marital status and child gender. Baseline county characteristics include average years of schooling, urbanization, unemployment, the female population share and total population. Column (2) additionally includes county-specific linear time trends, while Column (3) further includes province $\times$ year-of-birth fixed effects. Standard errors clustered at the county level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

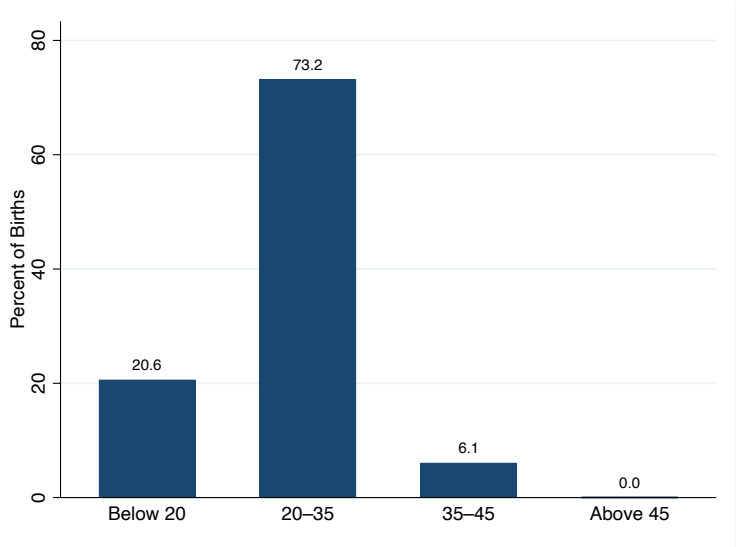
TABLE 8: EFFECT OF MOBILE MONEY ACCESS ON INFANT MORTALITY: IV ESTIMATES

Panel A: First-Stage Estimates			
	Agent Density	Agent Density^2	
Bartik instrument	3.5703***	2.7126	
	(0.8316)	(2.4592)	
Bartik instrument^2	-1.1521	11.4175*	
	(1.0684)	(5.9580)	
Observations	60,425	60,425	
First-stage F statistic	21.55	11.49	
Sanderson–Windmeijer F statistic	43.61	43.22	
Panel B: Second-Stage Estimates			
	(1)	(2)	(3)
Agent density	-0.0327	-0.1260***	-0.1280***
	(0.0264)	(0.0250)	(0.0241)
Agent density^2	0.0096	0.0273***	0.0276***
	(0.0121)	(0.0094)	(0.0092)
Mother and year-of-birth fixed effects	x	x	x
Time-varying mother characteristics	x	x	x
County-specific linear time trends	—	x	x
Province × year-of-birth fixed effects	—	—	x
Outcome mean	0.039	0.039	0.039
Observations	60,425	60,425	60,425

Notes: Panel A reports the first-stage regressions corresponding to the specification with county-specific linear time trends in Panel B. The endogenous variables are county-level mobile money agent density and its squared term. The excluded instruments are the Bartik instrument, constructed from counties' exposure to early mobile money hubs interacted with the national rollout of mobile money agents, and its squared term. The first-stage regressions include mother and year-of-birth fixed effects, time-varying maternal and child characteristics and county-specific linear time trends. The first-stage F statistics test the joint significance of the excluded instruments, while the Sanderson–Windmeijer F statistics assess instrument strength in the presence of multiple endogenous regressors. Panel B reports two-stage least squares estimates of the effect of mobile money access on infant mortality. All specifications include mother and year-of-birth fixed effects and time-varying maternal and child characteristics. Time-varying maternal and child characteristics include maternal age at birth, marital status and child gender. Column (2) additionally includes county-specific linear time trends, while Column (3) further includes province × year-of-birth fixed effects. Standard errors clustered at the county level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

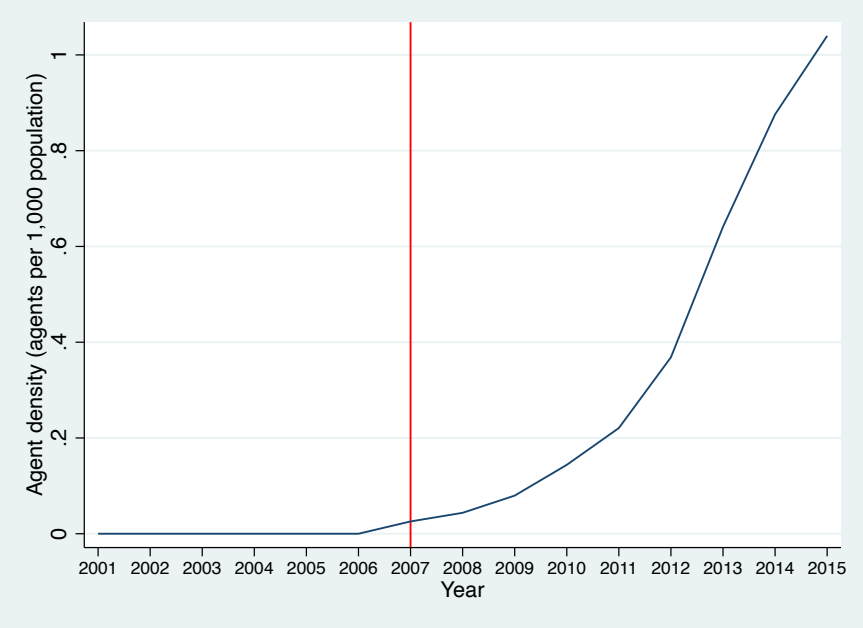
A Additional Tables and Figures

FIGURE A1: DISTRIBUTION OF MATERNAL AGE AT BIRTH



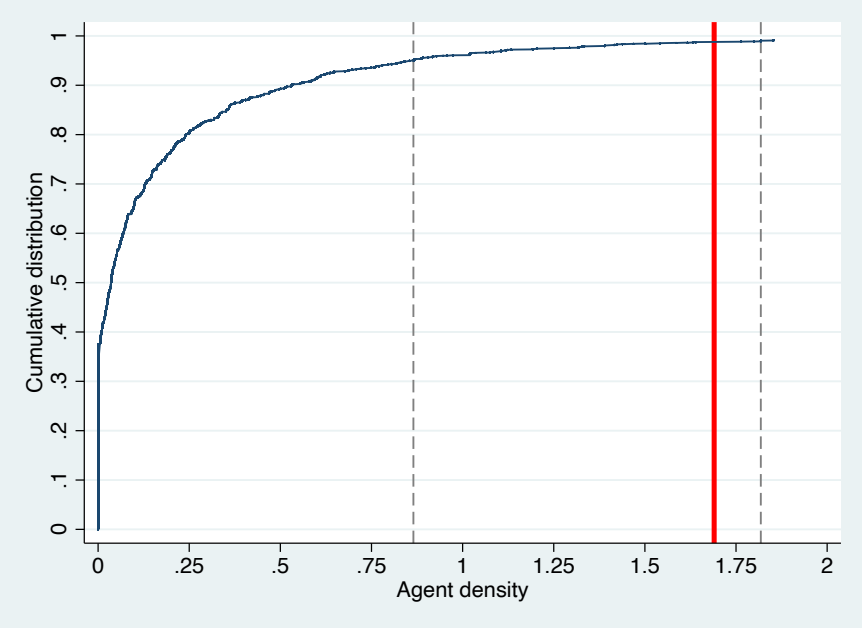
Notes: The figure reports the percentage of births by maternal age group. Most births occur among mothers aged 20–35, which forms the main regression sample.

FIGURE A2: EXPANSION OF MOBILE MONEY AGENT NETWORKS IN KENYA



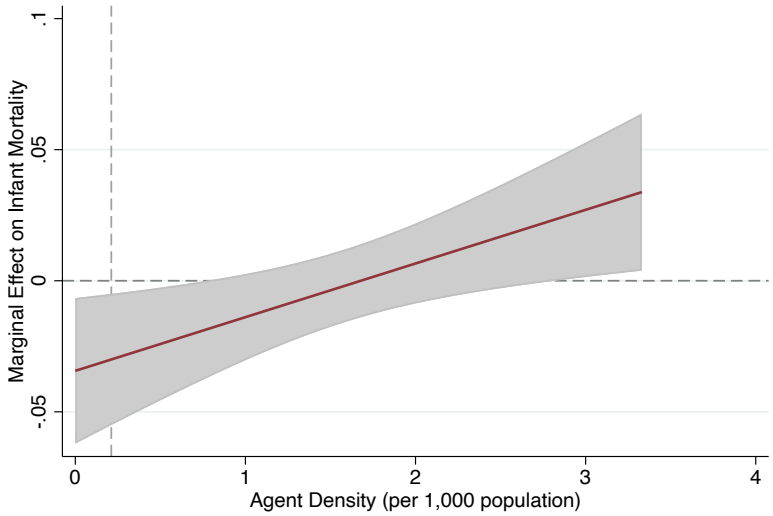
Notes: The figure plots the average number of mobile money agents per 1,000 population in Kenya over time. Agent density remained negligible prior to the introduction of mobile money services but increased rapidly during the early 2010s as providers expanded their agent networks across the country. This gradual rollout generates substantial variation in local access to mobile money infrastructure.

FIGURE A3: DISTRIBUTION OF AGENT DENSITY



Note. The solid vertical line indicates the estimated turning point of 1.61 agents per 1,000 population. Dashed vertical lines indicate the 95th percentile (0.865) and 99th percentile (1.818) of the observed distribution.

FIGURE A4: MARGINAL EFFECTS OF MOBILE MONEY ACCESS ON INFANT MORTALITY



Notes: The figure plots the marginal effect of mobile money agent density on infant mortality implied by the preferred quadratic specification. The marginal effect is computed as $\beta_1 + 2\beta_2 \cdot \text{AgentDensity}$ using the estimates reported in Table 3. Shaded areas represent 95 percent confidence intervals, constructed using the delta method. The dashed horizontal line indicates zero effect, while the vertical line marks the sample mean of agent density.

The marginal effect is negative and statistically significant at low levels of agent density, indicating that initial expansions of mobile money infrastructure are associated with reductions in infant mortality. The effect increases with agent density, consistent with diminishing marginal returns, and becomes statistically indistinguishable from zero at higher levels of access. Although point estimates become positive beyond the turning point, these regions are characterized by wider confidence intervals and limited support in the data. Overall, the figure indicates that the estimated effects are concentrated in the range where increases in agent density reduce infant mortality.

TABLE A1: VARIABLE DEFINITIONS AND DATA SOURCES

Variable	Definition	Data Source
A. Infant Health and Mobile Money Access		
Infant mortality	Indicator equal to one if the child died before reaching 12 months of age.	Kenya DHS (2014, 2022)
Agent density	Number of registered mobile money agents per 1,000 population in a county-year.	FinAccess Geospatial Mapping (2015); KNBS
Agent density^2	Square of county-level mobile money agent density.	Author's calculation
Bartik instrument	County exposure to early mobile money hubs interacted with the national expansion of mobile money agents.	Author's construction using FinAccess Geospatial Mapping (2015)
B. Maternal Healthcare Outcomes		
Home delivery	Indicator equal to one if the child was delivered at home.	Kenya DHS (2014, 2022)
Facility delivery	Indicator equal to one if the child was delivered in a health facility.	Kenya DHS (2014, 2022)
Government facility	Indicator equal to one if the child was delivered in a government health facility.	Kenya DHS (2014, 2022)
Private facility	Indicator equal to one if the child was delivered in a private health facility.	Kenya DHS (2014, 2022)
ANC visits	Number of antenatal care visits during pregnancy.	Kenya DHS (2014, 2022)
Early ANC	Indicator equal to one if the mother initiated antenatal care during the first trimester.	Kenya DHS (2014, 2022)
Iron supplementation	Indicator equal to one if the mother received iron supplementation during pregnancy.	Kenya DHS (2014, 2022)
C. Childhood Vaccination Outcomes		
Polio vaccination	Indicator for completion of the recommended polio vaccination schedule.	Kenya DHS (2014, 2022)
DPT vaccination	Indicator for completion of the recommended DPT vaccination schedule.	Kenya DHS (2014, 2022)
BCG vaccination	Indicator equal to one if the child received the BCG vaccine.	Kenya DHS (2014, 2022)
Measles vaccination	Indicator equal to one if the child received the measles vaccine.	Kenya DHS (2014, 2022)

TABLE A1: VARIABLE DEFINITIONS AND DATA SOURCES (Continued)

Variable	Definition	Data Source
D. Women's Labor Market Outcomes		
Currently working	Indicator equal to one if the woman was employed at the time of the survey.	Kenya DHS (2014, 2022)
Worked in last 12 months	Indicator equal to one if the woman worked during the 12 months preceding the survey.	Kenya DHS (2014, 2022)
Controls own earnings	Indicator equal to one if the woman reports deciding alone or jointly how her earnings are used.	Kenya DHS (2014, 2022)
E. Maternal, Child and Household Characteristics		
Maternal age at birth	Mother's age, in years, at the time of the child's birth.	Kenya DHS (2014, 2022)
Married	Indicator equal to one if the mother is married at the time of the survey.	Kenya DHS (2014, 2022)
Female child	Indicator equal to one if the child is female.	Kenya DHS (2014, 2022)
Urban residence	Indicator equal to one if the household resides in an urban area.	Kenya DHS (2014, 2022)
Female-headed household	Indicator equal to one if the household head is female.	Kenya DHS (2014, 2022)
Household wealth	DHS household wealth category based on the household wealth index.	Kenya DHS (2014, 2022)
Maternal education	Maternal education category used to classify women as having no education, primary education or secondary or higher education.	Kenya DHS (2014, 2022)
F. Fixed Effects and Constructed Variables		
Mother fixed effects	Fixed effects identifying each mother, used to compare siblings born to the same mother.	Author's construction
Mother-group fixed effects	Fixed effects for groups defined by maternal education, residence and birth cohort.	Author's construction
County-specific linear trends	County indicators interacted with a linear time trend.	Author's construction
Province $\times$ year-of-birth fixed effects	Fixed effects for each combination of province and child's year of birth.	Author's construction

Notes: DHS denotes the Kenya Demographic and Health Survey. ANC denotes antenatal care. DPT denotes diphtheria, pertussis and tetanus vaccination. BCG denotes Bacillus Calmette–Guérin vaccination. Agent density is measured as the number of mobile money agents per 1,000 population. County population data used to construct agent density are obtained from the Kenya National Bureau of Statistics (KNBS).

TABLE A2: ROBUSTNESS OF THE BASELINE RESULTS TO ALTERNATIVE MATERNAL AGE SAMPLES

	(1) Full Sample	(2) Teenage Mothers	(3) Older Mothers (>35)
Agent density	-0.0256*	-0.0840	-0.0910
	(0.0142)	(0.0931)	(0.1120)
Agent density^2	0.0087*	0.0323	0.0216
	(0.0045)	(0.0222)	(0.0333)
Mother and year-of-birth fixed effects	x	x	x
Time-varying mother characteristics	x	x	x
County-specific linear time trends	x	x	x
Province $\times$ year-of-birth fixed effects	x	x	x
Outcome mean	0.041	0.059	0.057
Observations	87,446	10,429	3,060

Notes: Column (1) reports estimates for the full sample of births. Columns (2) and (3) restrict the sample to teenage mothers (aged ≤ 19 years) and older mothers (aged >35 years), respectively. The baseline estimation sample is restricted to mothers aged 20–35 years at the time of birth. Agent density is measured as the number of mobile money agents per 1,000 population at the county–year level, and both the linear and quadratic terms are included in each specification. All regressions include mother and year-of-birth fixed effects, time-varying maternal and child characteristics, county-specific linear time trends and province $\times$ year-of-birth fixed effects. Time-varying maternal and child characteristics include maternal age at birth, marital status and child gender. Standard errors clustered at the county level are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

TABLE A3: EXCLUSION RESTRICTION TEST: INSTRUMENT AND WOMEN'S LABOR MARKET OUTCOMES

	(1) Currently Working	(2) Worked Last 12 Months	(3) Controls Own Earnings
Bartik instrument	-0.1490	-0.2740	0.0081
	(0.1320)	(0.1660)	(0.0111)
Bartik instrument^2	0.1060	0.2640	-0.0125
	(0.1110)	(0.1640)	(0.0161)
Outcome mean	0.621	0.659	0.930
Joint test p-value	0.274	0.262	0.737
Observations	42,644	41,770	16,395

Notes: This table reports placebo regressions testing whether the Bartik instrument predicts women's labor market outcomes and control over earnings. The instrument is constructed by interacting county exposure to early mobile money hubs with the national rollout of mobile money agents. The dependent variables are indicators for whether the woman is currently working, whether she worked during the previous 12 months and whether she controls her own earnings. All specifications include mother and year-of-birth fixed effects, time-varying maternal and child characteristics, county-specific linear time trends and province $\times$ year-of-birth fixed effects. Time-varying maternal and child characteristics include maternal age at birth, marital status and child gender. Standard errors clustered at the county level are reported in parentheses. The joint test p-value corresponds to the null hypothesis that the coefficients on the Bartik instrument and its squared term are jointly equal to zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.1 Marginal Effect Calculation

Because the preferred specification includes both a linear and a quadratic term in agent density, the effect of mobile money access on infant mortality varies with the level of agent density. The marginal effect is given by

$$\frac{\partial Y_{ict}}{\partial \text{AgentDensity}_{ct}} = \beta_1 + 2\beta_2 \text{AgentDensity}_{ct}. \tag{7}$$

Using the estimates from the preferred specification in Table 3, $\hat{\beta}_1 = -0.0376$ and $\hat{\beta}_2 = 0.0117$. Evaluating the marginal effect at the sample mean of agent density in the estimation sample, $\overline{\text{AgentDensity}} = 0.176$, yields

$$\frac{\partial Y_{ict}}{\partial \text{AgentDensity}_{ct}} = -0.0376 + 2(0.0117)(0.176) = -0.0335. \tag{8}$$

To provide an economically meaningful interpretation, I evaluate the marginal effect using the average positive within-county increase in agent density observed during the sample period. The average positive within-county increase is 0.130. The implied effect is therefore

$$-0.0335 \times 0.130 = -0.00435. \tag{9}$$

This corresponds to a reduction of approximately 0.43 percentage points in infant mortality. Relative to the sample mean infant mortality rate in the estimation sample, $\overline{Y} = 0.0392$, the implied proportional reduction is

$$\frac{0.00435}{0.0392} \times 100 = 11.1\%. \tag{10}$$

Thus, evaluated at the sample mean, an increase in agent density equal to the average positive within-county increase observed during the sample period is associated with a reduction in infant mortality of approximately 11.1 percent relative to the sample mean.

A.2 Turning Point of Agent Density

The quadratic specification implies that the relationship between agent density and infant mortality follows a convex pattern. The turning point is given by

$$\text{AgentDensity}^* = -\frac{\beta_1}{2\beta_2}. \tag{11}$$

Using the estimates from the preferred specification in Table 3, $\hat{\beta}_1 = -0.0376$ and $\hat{\beta}_2 = 0.0117$, the turning point is

$$\text{AgentDensity}^* = -\frac{-0.0376}{2 \times 0.0117} \approx 1.61. \tag{12}$$

This implies that increases in agent density are associated with reductions in infant mortality up to approximately 1.61 agents per 1,000 population. Beyond this point, the

marginal effect becomes zero and then positive. However, estimates at higher levels of agent density should be interpreted with caution because relatively few observations lie in this range.

The mean agent density in the estimation sample is 0.176, while the 95th and 99th percentiles are 0.865 and 1.818, respectively. Thus, the turning point lies well above the mean and above the 95th percentile of the observed distribution, indicating that for the vast majority of observations in the sample, increases in mobile money access are associated with reductions in infant mortality.

A.3 Dynamic Treatment Effect Calculation

The dynamic treatment effects are estimated using the event-study estimator proposed by de Chaisemartin and D'Haultfœuille (2020), which is robust to heterogeneous and dynamic treatment effects. The estimated average total effect from the preferred specification is

$$\widehat{\text{ATE}} = -0.00417. \tag{13}$$

This estimate implies that increases in mobile money access reduce infant mortality by approximately 0.42 percentage points. Relative to the sample mean infant mortality rate in the estimation sample, $\overline{Y} = 0.039$, the implied proportional reduction is

$$\frac{0.00417}{0.039} \times 100 = 10.7\%. \tag{14}$$

Thus, the dynamic treatment effect estimates imply a reduction in infant mortality of approximately 10.7 percent relative to the sample mean.