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Mobile phone coverage and infant mortality in sub-Saharan Africa[☆]

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ABSTRACT

Infant mortality is still high in Sub-Saharan Africa. Mobile phone technology has the potential to reduce mortality by facilitating the exchange of information. To test for effects, we combine georeferenced information on mobile phone signal coverage with infant mortality data on 1,268,041 children born in 30 Sub-Saharan African countries between 1999–2016. Our results reveal that infant mortality risk drops substantially as mobile phone coverage expands. Infants are 0.9 percentage points less likely to die within the first year after birth compared to their sibling(s) when mobile phone signal is available. In line with this result, we also find that fertility rates decline with the rollout of mobile coverage. Suggestive evidence indicates that improved health knowledge is relevant in explaining our findings. Mobile-phone related changes in access to in-person healthcare services or improvements in income opportunities, on the other hand, are unlikely to play an important role.

1. Introduction

There is wide-shared optimism that mobile technology can improve health outcomes in less developed countries (WHO, 2011). First and foremost, mobile phones facilitate the exchange of information. This could directly improve parents' health knowledge thereby informing how to care for their children, particularly in potentially life-threatening situations (e.g., illness or accident). Mobile technologies can also help overcome access barriers to healthcare services (e.g. Dammert et al., 2014; Opoku et al., 2017; Betjeman et al., 2013). Furthermore, mobile technologies can generate income opportunities and provide access to credit markets (Aker and Mbiti, 2010) which can translate into improved health outcomes. Despite the promising potential, large-scale causal empirical evidence on the impact of mobile coverage on infant mortality does not exist. This paper contributes to filling this gap.

Our study draws on birth history records collected as part of the Demographic and Health Surveys (DHS). In total, we observe 1,268,041 births. These cover 30 Sub-Saharan African countries and the time period 1999–2016. To each birth event, we match georeferenced data on mobile phone coverage (compiled by GSMA and Collins Bartholomew). Specifically, we identify whether mobile phone signal is available in the mother's location of residence in the year a child is born. We then

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test if the expansion of mobile phone coverage influences infant mortality using a linear probability model. Our baseline empirical specification includes a variety of birth- and mother-level characteristics, location-of-residence dummies as well as country×birthyear fixed effects. Because we observe multiple births for the majority of women, we can further include mother fixed effects, thus restricting comparison to siblings. In this restrictive setup we compare mortality risks of infants born to the same mother in years with and without mobile phone coverage while holding time-invariant parental and geographical characteristics constant.

The regression results produce a consistent picture. Across all specifications, we find that mobile phone signal expansion statistically significantly reduces infant mortality risk. In the most demanding specification (including mother fixed effects) infant mortality decreases by 0.9 percentage points when phone coverage becomes available. The economic magnitude of this effect is substantial. Evaluated at the average mortality risk of 7.8%, this represents a reduction of about 11%. Looking at heterogeneities, we find that the mortality-reducing effects are particularly pronounced in rural areas as well as in situations in which infant mortality risk is elevated.

Using a woman×year panel constructed from the DHS birth records we further document that—in line with the reduction in mortality risk—mobile phone rollout also leads to a decline in fertility rates. Controlling for individual and time fixed effects as well as various time-varying controls, the estimates show that women residing in locations with mobile signal are 0.5 percentage points less likely to give birth compared to residents in areas without coverage.

We draw on a range of alternative DHS survey data to gauge the plausibility of various candidate mechanisms by which individuals could leverage access to GSM coverage to reduce infant mortality risks. We find that the respondents' general health knowledge improves with mobile coverage rollout, suggesting better caring ability. However, there is no indication coverage leads to improved access to (in-person) healthcare services. Likewise, mobile-phone induced changes in income opportunities and credit market access are not driving our results.

The identifying assumption underlying our empirical analysis is that changes in mobile phone coverage in a given location and year are—conditional on control variables—as good as random. The primary concern is that this assumption is violated due to the rollout of phone coverage being correlated with unobserved time-varying characteristics, such as local economic shocks or coinciding expansion of public services. As a first step in supporting the validity of our estimates, we use the event study model developed in Sun and Abraham (2021) and document the absence of pre-trends. We then conduct a number of robustness and falsification tests to further document that unobservables are unlikely sources of bias. To additionally assuage worries related to the possibility that correlation with local shocks drive our results, we employ the well-established instrumental variable strategy which exploits the fact that frequent lightning strikes delay the adoption of mobile phone technology (cf. Andersen et al., 2012; Manacorda and Tesei, 2020; Guriev et al., 2021). Specifically, we predict mobile phone signal availability in a given location using the time-trend-interacted local average lightning frequency. The resulting IV estimate is statistically indistinguishable from the OLS coefficient. Drawing on Afrobarometer data, we further show that mobile phone signal expansion is unrelated to changes in access to infrastructure and public goods, such as healthcare clinics, sanitation facilities and safe water. Together, this set of results strongly suggests that our estimates are, in fact, capturing the effects of mobile phone coverage on infant mortality.

Our study builds on and contributes to several literatures. Most directly, our work speaks to the body of work that correlates mobile phone usage and socio-economic outcomes (Billari et al., 2020; Rotondi et al., 2020). Estimates from these studies, however, are likely biased due to the presence of unobserved variables. Our contribution is to develop an empirical approach that allows for the causal assessment of effects. In this regard, our paper is also related to case studies that look at the effects of mobile-phone-based e-health interventions on maternal and child health in Sub-Saharan Africa.[1] The results of these studies are mixed, even though often indicating promising potential (see Obasola et al. (2015) for a review). The breadth and reach of such mobile phone based e-health interventions, however, has so far been limited. In surveys covering 24 sites in 3 Sub-Saharan countries, for example, Hampshire et al. (2015) document that awareness of, and participation in, m-health programmes is extremely low. Contrasting this result, they find that mobile phones are utilised extensively in informal ways for health-related purposes. For example, healthcare workers and relatives are often asked via phone for advice. Our results lend additional support for this view.

Our study also speaks to research that investigates the effects of mobile phones on health and economic development.[2] A number of papers document that mobile phone coverage can increase welfare by facilitating access to information and new technologies (Gupta et al., 2020; Aker and Fafchamps, 2014; Aker, 2010; Jensen, 2007) or by enabling access to banking and risk sharing platforms (Jack and Suri, 2014). Positive effects of mobile phone availability have further been documented for educational outcomes (Aker et al., 2012). Mobile technology can also improve the quality and efficiency of healthcare services, facilitate remote disease diagnosis and increase healthcare utilisation (Agarwal et al., 2015; D'Ambrosio et al., 2015; Braun et al., 2013). Dammert et al. (2014) further show that mobile phones can be effective in spreading information and thereby changing health behaviour.

More generally, we contribute to the research on causes of child mortality in less developed countries. Factors identified as being important are, amongst others, economic and climatic shocks (e.g., Adhvaryu et al., 2020; Benshaul-Tolonen, 2019;

[1] Interventions include the establishment of helplines, sending out SMS containing information on topics such as child nutrition, or setting up SMS reminders for patients with appointments.

[2] See Aker and Mbiti (2010) for an overview.

Flückiger and Ludwig, 2018; Carleton and Hsiang, 2016; Baird et al., 2011a), ethnic and political cleavages (Franck and Rainer, 2012; Kudamatsu, 2012), infectious diseases (e.g., WHO, 2019, Liu et al., 2016), and closely associated, lack of access to sanitation and safe water (e.g., Geruso and Spears, 2018; Troeger et al., 2018). Our results suggest that expanding mobile phone coverage can potentially mitigate the impact of some of these factors.

Finally, our findings relate to the literature stressing the importance of the demographic transition—the decline in mortality and fertility rates—and quantity-quality trade-off as a key mechanism in the shift to a modern economic growth regime (e.g. Galor, 2011; Becker and Barro, 1988). Our findings indicate that the rollout of mobile phone coverage may speed up this process in less developed regions.

The remainder of this paper is structured as follows: In the next section, we outline our estimation strategy. The data to which it is applied is then presented in Section 3. We present our main results in Section 4 and discuss threats to identification in the subsequent Section. In Section 6 we look at the plausibility of various candidate mechanisms and investigate the effects of mobile phone coverage on alternative outcomes. Finally, Section 7 concludes.

2. Empirical strategy

The basis for our empirical analysis is the following OLS regression model:

$$o_{i,m,k,c,t} = \beta P_{k,c,t} + \gamma' \mathbf{X}_{i,m,k,c,t} + \kappa_{k,c} + \pi_{c,t} + \mu_m + \varepsilon_{i,m,k,c,t}, \tag{1}$$

where outcome $o_{i,m,k,c,t}$ is an indicator variable that takes the value one if child i, born to mother m who resides in DHS cluster k (situated in country c) in year t has died before the age of one.[3] The main regressor of interest, $P_{k,c,t}$, is a dummy that reflects whether mobile phone coverage is available in a given location and year. The coefficient β thus captures the reduced-form effects, i.e., the net impact of all channels through which coverage expansion influences infant mortality risk.

The vector $\mathbf{X}$ is a set of child and mother controls. In its most parsimonious version, this set consists of fixed effects for gender and birth order of the child, for the mother's age at birth as well as for the number of children per birth event. Cluster fixed effects ($\kappa_{k,c}$) absorb any time-invariant local characteristics—such as distance to population centres or reliance on traditional medicine—that influence child health. The country×birthyear fixed effects $\pi_{c,t}$ account for birthyear-specific mortality shocks within a given country (e.g., country-wide recessions Baird et al., 2011b).[4] Additionally, we are able to control for any time-invariant differences in parental and household characteristics by including mother fixed effects (μ_m). When accounting for these fixed effects, we only compare the mortality risks between siblings, i.e., infants born to the same mother in years with and without mobile phone coverage.[5] Throughout, we cluster error terms ($\varepsilon_{i,m,k,c,t}$) at the DHS cluster level (i.e., the level of exposure).

The main identifying assumption underlying regression Eq. (1) is that *changes* in mobile phone coverage in a given location are—conditional on control variables—as good as random. The primary worry is that the expansion of mobile phone coverage is correlated with local economic development or the rollout of other infrastructure projects, both of which could influence health outcomes. In Sections 4 and 5, we provide four complementary pieces of evidence to document that this is unlikely to be the case. First, we use the recent event study methodology developed in Sun and Abraham (2021) to document that there are no differential mortality trends prior to the rollout of the GSM signal. Second, we demonstrate that our estimates remain very stable when we control for a wide range of locational characteristics, including rainfall shocks and night-time light intensity as proxies for local economic development. Third, we reproduce our OLS results using a well-established instrumental variable approach. Therein, we predict mobile phone signal availability in a given location using the time-trend-interacted local average lightning frequency (cf. Andersen et al., 2012; Manacorda and Tesei, 2020; Guriev et al., 2021). The resulting point estimate is very similar to the one produced by the OLS approach. Fourth, we document that changes in mobile phone signal availability are uncorrelated with the rollout of public infrastructure, such as hospitals, piped water, sewerage, or roads.

3. Data

Information on infant mortality along with other birth-, mother-, and household-level characteristics are taken from the Demographic and Health Surveys (DHS). The primary target population of the surveys are women aged 15–49. Among many other aspects, the DHS record the complete birth history of female respondents as well as all instances of child deaths. Based on this information, we construct our main outcome variable: an indicator that takes the value one if a child died before the age of one, and zero otherwise. To facilitate interpretation of point estimates, this dummy is multiplied by 100 throughout.

Georeferenced information on mobile phone coverage is compiled in a joint effort between the GSM Association (GSMA) and Collins Bartholomew.[6] The database offers spatially explicit information (shapefiles) on availability of Global System

[3] DHS clusters—i.e. the primary sampling units—approximate villages.

[4] We have within-DHS-cluster variation in mortality outcomes due to the fact that, for a given cluster, we typically observe multiple women who give birth to children in different years.

[5] Note that the mother fixed effects also pin down the location. This implies that the cluster fixed effects ($\pi_{c,t}$) become redundant in regressions where we account for mother fixed effects.

[6] The GSMA represents the interests of mobile operators worldwide; Collins Bartholomew is a map publishing company.

Table 1
Descriptive Statistics key variables.

Variable	Mean	Std. Dev.	Min.	Max.	Obs.
Death before age 1 (percentage points)	7.367	26.123	0	100	1,268,041
Mobile phone coverage	0.527	0.499	0	1	1,268,041
Male	0.508	0.500	0	1	1,268,041
Birth order	3.554	2.355	1	18	1,268,041
Mother age at birth	25.923	6.646	5	49	1,268,041
Number of children per birth event	1.036	0.191	1	4	1,268,041

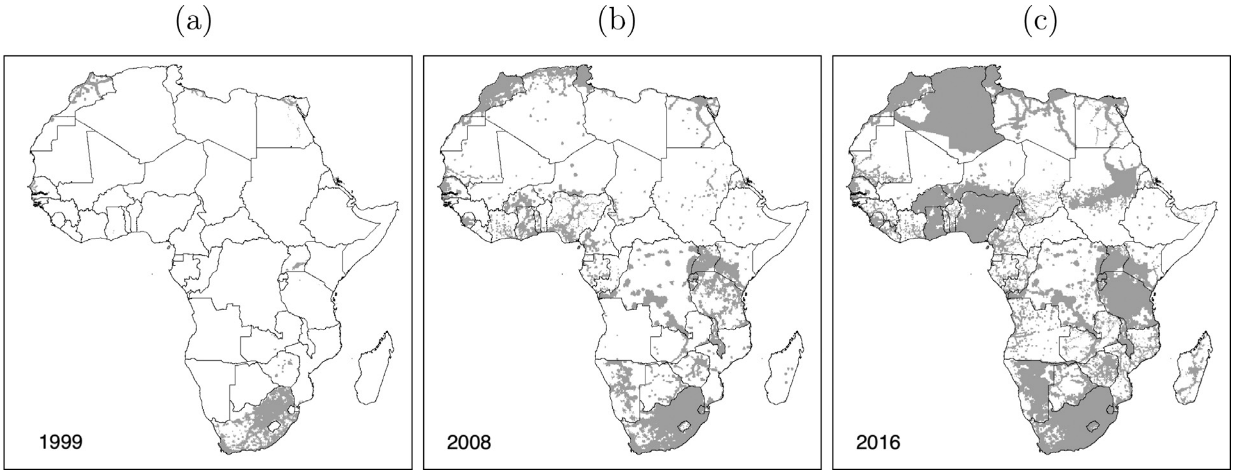

Fig. 1. Panels (a)–(c) depict GSM coverage in year 1999, 2008, and 2016, respectively.

for Mobile Communications (GSM) digital cellular networks. Coverage data is submitted by mobile network operators from around the world. Information is gathered separately for the different protocols (2G, 3G, and 4G). Coverage in Sub-Saharan Africa (during our sample period) is overwhelmingly restricted to the 2G technology.[7] Our dataset, commercially available from the GSMA, spans the years 1999–2016, with information not available for 2005 and 2010. For each of the available years, we can thus identify the regions with GSM coverage.[8]

We link mobile phone signal coverage to the DHS surveys based on the geocoded location of residence (i.e. DHS cluster) of the mother and the birthyear of the child. The main explanatory variable is then defined as a dummy variable that captures whether any generation of GSM signal (i.e., 2G or 3G) is available within 10 km of that DHS cluster and birthyear.[9] In Section 4.2, we investigate whether effects vary depending on the generation of mobile technology.

An implicit assumption we make is that the mother has not moved since the birth of the child. In robustness tests, we show that our results remain unchanged if we run our regressions using only the subset of mothers for which we know that they have not migrated during our sample period (see Table C.1).[10]

Our main estimating dataset consists of 1,268,041 birth-level observations gathered in 74 DHS surveys. These were conducted between 1999 and 2016 and cover 30 Sub-Saharan African countries.[11] Table 1 reports summary statistics for the key variables. The first row documents that infant mortality in Sub-Saharan Africa is high. For every 100 children born, more than 7 had died before the age of one. GSM signal availability, on the other hand, is limited by international standards. On average, 53% of children in our sample were born to mothers that resided in locations with coverage. However, this number masks considerable spatial and temporal variation. As illustrated in Fig. 1, GSM coverage varies markedly over time and space. The aim of this study is to determine whether this variation influences infant mortality risk.

[7] Within our sample, 3G signal is available in 2.5% of the cases. This proportion is much lower for 4G (0.07%).

[8] In a robustness check, we illustrate that we obtain similar estimates if we interpolate GSM coverage linearly across neighbouring years (see Table C.1).

[9] Defining coverage based on a 10 km buffer is motivated by the fact that the geographical coordinates of the DHS clusters are offset by 10 km in order to preserve anonymity of respondents. Our results are robust to varying the width of the buffer (see Fig. C.2).

[10] For a subset of survey waves, the DHS elicit information on mobility of the respondent. In these waves, we can identify the individuals that did not move within our sample period using variable V4: "Number of years the respondent has lived in the village, town, or city where she was interviewed".

[11] Table A.1 lists the included survey waves. The geographical scope of our analysis, along with the location of DHS clusters, is depicted in Fig. A.4.

Table 2
Mobile phone coverage and infant mortality.

	Infant Mortality			
	(1)	(2)	(3)	(4)
Phone coverage	−0.624***	−0.624***	−0.891***	−0.864***
	(0.112)	(0.110)	(0.126)	(0.127)
DHS cluster FE	yes	yes	yes	yes
Country×birthyear FE	yes	yes	yes	yes
Birth controls	no	yes	yes	yes
Mother FE	no	no	yes	yes
Locational controls	no	no	no	yes
Obs.	1,268,041	1,268,040	1,069,780	1,069,780
R-squared	0.052	0.070	0.397	0.397
Mean of Dep. Var.	7.367	7.367	7.802	7.802

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at DHS cluster level. Dependent variable is multiplied by hundred to facilitate interpretation. I.e., Infant mortality is 100 if the child died within the first 12 months of life, and 0 otherwise. Birth controls include fixed effects for gender and birth order of the child, for the mother's age at birth as well as for the number of children per birth event. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables is provided in Appendix A.

4. Mobile phone coverage and infant mortality

In this section, we first document that the rollout of GSM coverage reduces infant mortality risk. We then look at heterogeneities to try to gain an understanding for situations in which the effect is particularly pronounced.

4.1. Main results

We start investigating the effect of mobile phone coverage on infant mortality using a parsimonious version of regression Eq. (1) in which we only account for country×birthyear and DHS cluster fixed effects. The resulting point estimate—presented in column (1) of Table 2—implies that infant mortality declines by 0.6 percentage points when access to mobile phone coverage is gained. Evaluated at the sample mean of 7.4%, this corresponds to a drop of around 8%. The estimate remains unchanged when we control for mother and child characteristics in column (2). In column (3), we restrict comparison to siblings by including mother fixed effects. That is, we compare mortality risks of infants born to the same mother in years before and after the rollout of GSM coverage. The resulting point estimate is highly statistically significant and slightly larger than the coefficients without mother fixed effects. Mortality risk is 0.9 percentage points lower—11.5% evaluated at the baseline risk—when mobile signal expands to the location the mother resides in. The economic magnitude of the mobile coverage effect is substantial and similar to the impact of democratisation. Using a regression setup analogous to ours, Kudamatsu (2012) finds that an infant's risk of dying relative to its sibling drops by 1.2 percentage points (12% evaluated at the sample mean) after a country has transitioned to democracy. In another study that exclusively exploits within-mother variation, Kotsadam et al. (2018) show that infant mortality risk in Nigeria decreases by 1 percentage point when official development projects start near the location of residence.

In column (4), we extend the set of controls to include local climatic, economic, and geographic characteristics. Changes in the climatic environment, particularly rainfall shocks, could influence both the likelihood of gaining access to mobile phone signal and infant mortality.[12] To account for this possibility, we construct a measure of negative rainfall shocks akin to Burke et al. (2015). Specifically, for each DHS cluster and year we compute the share of months in which precipitation is below the 15th percentile of the historical distribution of rainfall. In addition to this climatic control, we also include population numbers and local night-time light intensity as measures for economic activity Henderson et al. (e.g. 2012). Finally, we account for the possibility that the effects of time-invariant local characteristics change over time (cf. Manacorda and Tesei, 2020). We do so by creating interactions between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city.[13] The inclusion of the time-varying controls in column (4) leaves the point estimate of mobile phone coverage virtually unchanged.[14] This provides a first indication that omitted variables are unlikely to bias our estimates (Altonji et al., 2005). To further substantiate this

[12] Plausible, for example, is that negative rainfall shocks reduce local income levels (e.g., Burke et al., 2015) and thereby lower demand for phone coverage and/or curtail financial resources of local authorities. At the same time, negative income shocks could increase infant mortality rates (e.g., Flückiger and Ludwig, 2018; Baird et al., 2011a). Similarly, shocks (such as floods) could directly hamper the establishment of mobile phone infrastructure in a given location, whilst also influencing infant survival probabilities.

[13] A more detailed definition of these locational controls along with their sources is listed in Table A.2.

[14] In Appendix B we construct a DHS cluster×year panel to explicitly test if there is a relationship between nighttime light intensity and the rollout of GSM coverage. Reflecting the stability of the GSM coefficient when controlling for night-time light intensity (columns (3)–(4) of Table 2), we do detect any association between the two (see Table B.1).

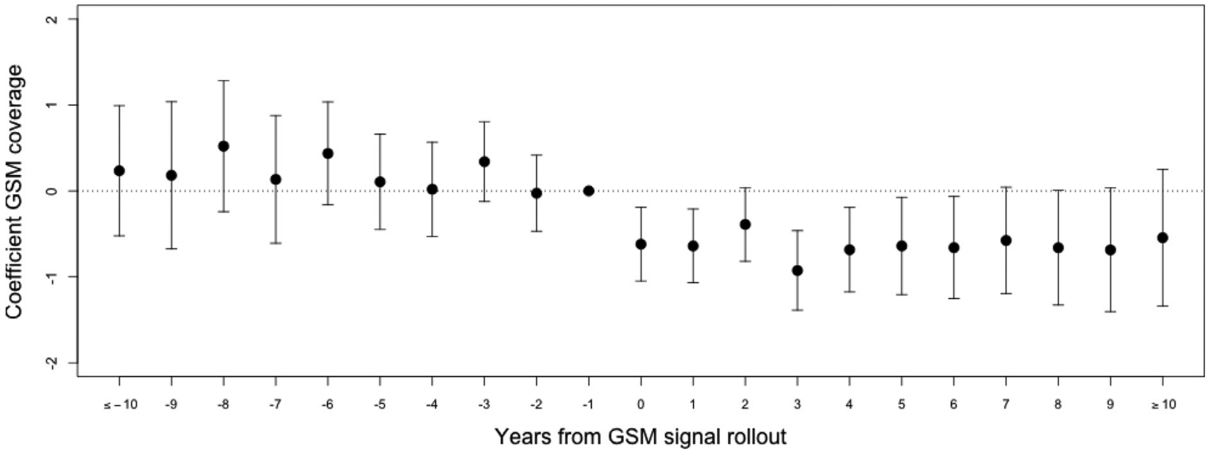

Fig. 2. Figure depicts the event study point estimates and 95% confidence intervals of the dynamic effect of GSM coverage on infant mortality. Results produced using Sun and Abraham (2021)'s event study model with full set of controls (i.e., mother fixed effects and locational controls).

notion, we estimate an event study model using the method recently developed in Sun and Abraham (2021). This method specifically accounts for the staggered nature of the rollout and allows us to test for the presence of pre-trends and—more generally—analyse the temporal dynamics of the GSM expansion. To account for the fact that we have relatively few observations with distant leads (lags), we group these together and define a dummy for observations with leads (lags) of ten years or more.[15]

Fig. 2 visualises the result of the event study model. Reassuringly, pre-trends cannot be detected. In years prior to the rollout of GSM coverage, point estimates are statistically non-significant and close to zero. Starting with the first year of availability, however, we observe that mobile phone signal reduces infant mortality. Interestingly, the size of the effect remains very stable in successive years, implying the absence of cumulative or slow-moving processes. On the flip side, this also means that the effect does not drop off over time.

4.2. Heterogeneities

We first test for differences between rural and urban areas. The positive effects of coverage rollout is likely concentrated in the former, where access to health services, information, and markets is relatively poor. We test if this is the case by interacting GSM coverage with an indicator for rural residence. The results are presented in Table 3, column (1). They show that the mortality-reducing effect of signal expansion is predominantly driven by rural areas. The baseline effect of coverage is statistically non-significant and (relatively) small. Interacted with the rural residence dummy, however, the rollout of coverage reduces infant mortality risk considerably.

We next analyse if mobile phone signal availability differentially impacts poor households.[16] We surmise that the effects of GSM expansion are particularly strong among these households as access to information—and related access to health-care services, labour and credit markets—is relatively good for wealthier households prior signal availability. Therefore, the mobile-phone induced improvements are potentially smaller for the relatively wealthy group of population as they are relatively well-equipped to mitigate infant mortality risks.[17] In line with this argument, we find that children born to poor households experience a particularly strong decline in mortality risk as coverage expands (column (2)). This suggests that the rollout of mobile signal is a cost-effective way of reducing infant mortality among the poorest part of the population.

Column (3) further shows that the beneficial effects of mobile phone technology is more pronounced for young mothers (aged 20 or younger), for which the risk of infant death is elevated (e.g., Neal et al., 2018).[18] In column (4) we look at the interaction between mobile phone coverage and gender. The positive impact of GSM coverage is somewhat larger for male offspring, for whom the mortality risk is typically higher (e.g., Kraemer, 2000).

The results of columns (1)–(4) of Table 3 suggest that gaining access to mobile phone signal is especially beneficial in rural areas and situations in which child mortality risk is elevated. To gauge whether such risk-mitigating effects also exist in the presence of negative income shocks, we interact our index of negative rainfall shocks with mobile phone coverage

[15] We obtain very similar results if we include all distant leads and lags separately into the regression (see Fig. C.1).

[16] We define households as poor if they fall into the bottom two quintiles of the DHS wealth classification.

[17] As mentioned earlier, mobile phones are widely shared with friends and relatives (e.g. Hampshire et al., 2015; Aker and Mbiti, 2010). Purchasing a mobile phone—which may potentially be financially infeasible for poor households—is therefore not a prerequisite for benefiting from the expansion of GSM signal.

[18] We have investigated whether this effect is driven by first-born children. This is not the case. There is no coverage-related reduction in mortality risk for first-born children compared to later born siblings.

Table 3
Heterogeneous effects of mobile phone coverage.

	Infant Mortality					
	(1)	(2)	(3)	(4)	(5)	(6)
Phone coverage	−0.295	−0.593***	−0.741***	−0.731***	−0.786***	−0.860***
	(0.195)	(0.152)	(0.131)	(0.142)	(0.133)	(0.127)
Phone coverage × rural residence	−0.763***					
	(0.206)					
Phone coverage × poor household		−0.557***				
		(0.182)				
Phone coverage × young mother			−0.642***			
			(0.187)			
Phone coverage × male				−0.263**		
				(0.126)		
Negative Rainfall shock					2.190**	
					(0.873)	
Phone coverage × negative rainfall shock					−2.289**	
					(1.083)	
3G coverage						0.216
						(0.290)
DHS cluster FE	yes	yes	yes	yes	yes	yes
Country×birthyear FE	yes	yes	yes	yes	yes	yes
Birth controls	yes	yes	yes	yes	yes	yes
Mother FE	yes	yes	yes	yes	yes	yes
Locational controls	yes	yes	yes	yes	yes	yes
Obs.	1,069,780	1,069,780	1,069,780	1,069,780	1,069,780	1,069,780
R-squared	0.397	0.397	0.397	0.397	0.397	0.397
Mean of Dep. Var.	7.802	7.802	7.802	7.802	7.802	7.802

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at DHS cluster level. Dependent variables are multiplied by hundred to facilitate interpretation. Birth controls include fixed effects for gender and birth order of the child, for the mother's age at birth as well as for the number of children per birth event. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables is provided in Appendix A.

availability. The rainfall shock measure is part of our standard set of controls and therefore included in all regressions of Table 3. In column (5), we explicitly report its point estimate. Consistent with the literature, we find that negative climate shocks raise infant mortality risk (e.g., Flückiger and Ludwig, 2018; Kudamatsu et al., 2016). The point estimate implies that the mortality risk increases by 2.2 percentage points if levels of rainfall are below the 15th percentile of the historical distribution in all 12 months of the birthyear. However, when mobile phone signal is available, the rainfall-shock-induced risk is mitigated entirely. This indicates that mobile signal availability improves the ability to absorb negative income shocks.[19]

In column (6), we analyse whether the rollout of 3G technology differentially impacts infant mortality.[20] Compared to 2G, this follow-up generation of wireless mobile technology offers a faster information transfer rate, enabling fast internet browsing and data downloading. This, in turn, could create new pathways of impact for mobile phones. Our estimates, however, document that there is no additional benefit of having access to 3G coverage (compared to 2G). This suggests that our results are driven by channels working through the 'traditional' use of mobile phones: voice calling and text messaging.

As a last type of heterogeneity, we investigate whether the effect of GSM coverage on mortality risk changes with the age of the child. To this end, define separate dummy variables for different ages at death. Specifically, we create the following outcome variables: death within the first month of birth, between months 2–12, between years 1–2, between years 2–3, between years 3–4, and between years 4–5. Fig. 3 depicts the point estimates for the effect of mobile phone signal on mortality risk during the respective phase of life. Reflecting earlier results, we find that the mortality-reducing impact is strongest for children below the age of one. That is, during the period in which mortality risk is particularly high (e.g. UNIGME, 2018). For older children, the GSM effect rapidly dissipates.[21]

Summing up, the results presented in this section illustrate that the mortality-reducing effects are particularly pronounced in rural areas as well as in situations in which infant mortality risk is elevated. This is compatible with a range of mechanisms. Before outlining these, we first discuss potential threats to our identification strategy.

[19] A similar effect is also documented for the rollout of transport infrastructure in regions that rely on rain-fed agriculture. For example, Burgess and Donaldson (2010) show the arrival of railroads in Indian districts dramatically reduced the likelihood that rainfall shocks caused famines in colonial India.

[20] 4G coverage is extremely limited. Within our sample only 0.07% of observations have access to this generation of wireless mobile telecommunications technology.

[21] These findings should be interpreted with caution as information on death of children after age 1 can be prone to measurement error. Possible sources are survival bias of the mother, recall bias, or truncation bias (i.e., bias due to the upper age limit of the respondents (49 years)).

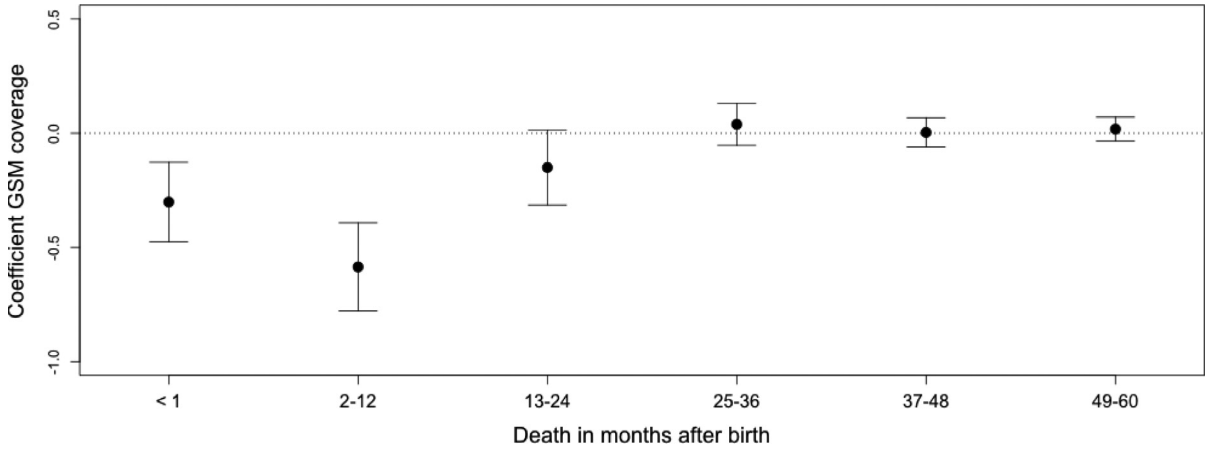

Fig. 3. Figure depicts the point estimates and 95% confidence intervals of the effects of mobile phone coverage on the risk of death: within first month of birth, between months 2–12, between years 1–2, between years 2–3, between years 3–4, and between years 4–5. Results produced using regression Eq. (1) with full set of controls (i.e., mother fixed effects and locational controls). Table B.2 reports the point estimates and standard errors corresponding to Fig. 3.

5. Threats to identification

The main threat to the validity of our empirical approach is that unobserved time-varying factors bias our results. More precisely, that the rollout of mobile phone coverage coincides with (correlated) shocks which themselves influence infant survival probabilities.[22] The absence of pre-trends (Fig. 2) and the stability of point estimates in Table 2 across different regression specifications are first indications that this is not the case. To provide additional support for the validity of our estimates, we employ three complementary approaches. First, we conduct a number of robustness and falsification exercises. Second, we develop an instrumental variable strategy. Third, we document that the expansion of GSM coverage is unrelated to the timing of infrastructure projects.

5.1. Robustness and falsification

As an initial step in mitigating the worry that correlated local economic shocks bias our results, Table C.1 shows that estimates remain very stable when we include more stringent sets of administrative region×birthyear fixed effects or country×climate-zone×birthyear fixed effects (columns (1)–(2)). This is also the case when we restrict our analysis to mothers that did not move during our sample period (column (3)).[23] This documents that our results are not due to selective migration, i.e., the possibility that mothers with lower infant mortality risks migrate to areas with GSM coverage. We further show that our estimates remain very stable if we drop observations in the top and bottom 5% of the age-at-birth distribution (column (4)).[24] This addresses worries related to the possibility that there are coding errors in the data or that our findings could be driven by birth outcomes for extremely young or old mothers.

In a recent article Ackermann et al. (2021) show that mobile phone coverage can increase the risk of conflict in Africa. Similarly, Manacorda and Tesei (2020) document that protests become more likely when GSM coverage expands. This raises concerns that our results may be biased due to the coexistence of violence. Reassuringly, however, we find that the GSM point estimate remains unchanged if we control for the presence of violent conflict within 50 km of the DHS cluster and year (column (5)).[25] Likewise, we find that clustering standard errors by administrative regions rather than DHS clusters has little influence on results (column (6)).

To address the worry that our results are driven by overrepresentation of countries for which multiple survey rounds are available (see Section 3 and Table A.1), we re-run our regressions using only the most recent survey round available for each country. The point estimate remains very stable (column 7). In a further robustness check, we illustrate that our results do not depend on the omission of births in the two years—2005 and 2010—for which we do not have GSM coverage data (see Section 3 for more details). We obtain similar estimates if we interpolate GSM coverage linearly across neighbouring years and use all births between 1999–2016 (column (8)).

[22] For example, correlated resource windfalls could influence both the demand for mobile phone coverage and infant mortality. Similarly, concurrent rollout of infrastructure programs (e.g. roads) could affect both of our key variables.

[23] Information on migration is only available for a relatively small subset of survey waves.

[24] This subsample is restricted to mothers aged 17 to 38.

[25] In analogy to Ackermann et al. (2021), we draw the data from the Uppsala Conflict Data Program (UCDP). The cutoff of 50 km is also inspired by their study. However, our results are unchanged if we use distance to the nearest conflict as an alternative conflict exposure measure (results available upon request).

As a final robustness check, we look at the sensitivity of our estimates with respect to the size of the buffer used to define our main explanatory variable. As our baseline we define coverage as the availability of coverage within 10 km of the location of residence (see Section 3). In Fig. C.2, we vary this cutoff between zero and 20 km. Throughout, we find a negative and statistically significant effect of mobile phone coverage on infant mortality. However, the effects are somewhat smaller and less precisely estimated when we move away from the 10 km cutoff in either direction. The fact that our results get noisier when using buffers that are not corresponding to the (maximum) random displacement of the DHS coordinates is an indication that our coverage data accurately captures signal availability. Widening the buffers increases the number of DHS clusters that are falsely assigned GSM coverage. Analogously, choosing very narrow buffers leads to DHS clusters not being assigned GSM coverage even though they have coverage in reality.

Complementing the robustness tests, we illustrate that our findings are unlikely the result of chance. To this end, we randomly permutate the geographical coordinates—and associated mobile coverage—across DHS clusters and then re-run regression Eq. (1). We repeat this exercise 1000 times and present the results in Panel (a) of Fig. C.3. Point estimates are centred around zero and orders of magnitude smaller than the coefficients reported in Table 2. Results are very similar for a second, more demanding, falsification test in which the starting year of coverage is randomly chosen for clusters that ever gained access to mobile phone signal. For all other clusters, signal availability is set to zero in all years (as observed in the data). The distribution of point estimates, shown in Panel (b) of Fig. C.3, is again centred around zero. This documents that our results specifically reflect the onset of mobile coverage and not some general local time trends.

5.2. Instrumental variable approach

As a second approach to assuaging worries related to the possibility that unobserved local shocks drive our results, we employ an instrumental variable approach that exploits the fact that frequent lightning strikes delay the adoption of mobile phone technologies (see Manacorda and Tesei, 2020; Guriev et al., 2021, for analogous approaches). The primary reason behind the lag in diffusion is that mobile-phone infrastructure is sensitive to voltage surges caused by the lightning strikes (Martin, 2016; Zeddam and Day, 2014). In the IV approach—outlined in more detail in Appendix D—we use local lightning frequency interacted with a time trend as an instrument for coverage. Due to the binary nature of the (potentially) endogenous coverage indicator, we operationalise the IV approach using the three-step procedure proposed by Wooldridge (2002, p. 236ff.).[26,27] The resulting IV estimate of −1.283 is reported in column (1) of Table D.1 (Panel C).[28] Reassuringly, it is very similar compared to the OLS coefficients. If anything, the IV point estimate is slightly larger (confidence intervals overlap however).

5.3. Mobile phone coverage, infrastructure and mobile phone use

As a final piece of evidence supporting the validity of our estimation strategy, we show that we are not conflating mobile signal expansion with concurrent rollout of mortality-risk-influencing public infrastructure. The DHS surveys do not contain this type of information. We therefore alternatively draw on Afrobarometer data. These surveys also allow us to check whether mobile phone usage increases as coverage expands. This constitutes an implicit assumption underlying our analysis.

Afrobarometer surveys are nationally representative repeated cross-sections that are conducted every two to three years. The first round started in 1999 while the latests available round was completed in 2018. We restrict our analysis to rounds 2–6, where fieldwork was conducted between 2002 and 2015. These surveys contain information on availability of infrastructure and public goods in a given enumeration area (EA).[29] Starting with round 4 (year 2008) information on mobile phone use is additionally elicited.

The EAs typically change between survey rounds. We therefore cannot use a standard panel data approach in which EA fixed effects are accounted for. As an alternative way of controlling for time-invariant unobservables, we implement a regression strategy akin to Hjort and Poulsen (2019). Therein, we divide Sub-Saharan Africa into individual grid cells of 0.1 × 0.1 degrees (approximately 10 × 10 km) and assign each EA to the grid cell into which it falls (where multiple EAs can fall into the same grid cell). This enables us to control for 10 × 10 km grid cell fixed effects and consequently account for unobserved time-invariant factors within this narrowly defined area. Additionally, we can include country×year dummies; these absorb any aggregate country-level shocks.

Formally, the pooled cross-sectional regression is given by:

$$y_{k,c,t} = \beta P_{k,c,t} + \mu_{g(k,c)} + \pi_{c,t} + \varepsilon_{k,c,t}, \tag{2}$$

[26] This approach has important advantages over a standard 2SLS-IV strategy. For example, it explicitly takes into account the binary nature of the endogenous variable and does not require the first stage to be correctly specified.

[27] In the first step, we use the probit model with the coverage indicator as binary outcome variable and time-trend-interacted local lightning frequency as explanatory variable. This produces predictions for coverage at the DHS cluster level. In the second and third step, these predictions are then used to instrument for observed coverage in a standard 2SLS-IV approach. See Maffioli (2021) for a similar application of the three-step procedure.

[28] The results of the first two steps are reported also in Table D.1 (Panels A and B).

[29] Information on EA-level infrastructure was not collected in round 1. We exclude round 7 because the sampling period does not overlap with the temporal coverage of our GSM data.

Table 4
Mobile phone coverage, infrastructure and phone use.

	Infrastructure and public goods in enumeration area								Mobile phone use	
	Health clinic (1)	Piped water (2)	Sewerage (3)	Electricity (4)	Market (5)	Police station (6)	Post Office (7)	Paved road (8)	Usage frequency (9)	Share never use (10)
Phone coverage	−0.056	−0.014	0.057	0.070	0.032	−0.017	0.001	0.084	0.316**	−0.073**
	(0.061)	(0.059)	(0.053)	(0.045)	(0.059)	(0.056)	(0.055)	(0.056)	(0.140)	(0.034)
Grid cell FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country×year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	4280	4280	4280	4280	4280	4279	4279	4280	3526	3526
R-squared	0.477	0.666	0.680	0.750	0.467	0.485	0.529	0.651	0.777	0.760
Mean of Dep. Var.	0.648	0.748	0.485	0.786	0.671	0.449	0.376	0.647	2.933	0.149

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at the 10 × 10 km grid cell level. Data are from Afrobarometer waves 2–6 (columns (1)–(8)) and waves 4–6 (columns (9)–(10)). Only observations with precise information on location of the enumeration area are used (precision code equal to 1).

where $y_{k,c,t}$ is the outcome for EA k (located in country c) in year t; and $P_{k,c,t}$ is the dummy reflecting whether mobile signal is available in the EA in a given year. The 10 × 10 km grid cell fixed effects are symbolised by $\mu_{g(k,c)}$, and the country×year dummies by $\pi_{c,t}$. The idiosyncratic error terms ($\varepsilon_{k,c,t}$) are clustered at the grid cell level.

We start by looking at the correlation between GSM coverage and health-related infrastructure. Columns (1)–(3) of Table 4 show that the likelihood that a health clinic, piped water or sewerage—all of which can influence infant mortality (e.g. Alsan and Goldin, 2019; Baranov and Kohler, 2018)—is available in an EA does not increase with mobile signal expansion. The point estimates are statistically non-significant and even negative in sign in some cases. Similarly, we find no systematic relationship between coverage and presence of infrastructure or public goods that are less directly related to infant health. Neither the probability of having access to electricity, markets, police stations, post offices, or paved roads changes with phone coverage (columns (4)–(8)). This lack of comovement between signal expansion and infrastructure availability reinforces confidence in the validity of our main results.[30]

As stated previously, our results represent reduced-form effects, i.e., the net impact of all channels through which GSM coverage expansion influences infant mortality risk. Before turning to the discussion of potential mechanisms, it is important to note that an implicit assumption underlying our analysis is that the effects of mobile phone coverage work through the (increased) use of mobile phones. Using information reported in the Afrobarometer surveys, we can provide suggestive evidence for the validity of this assumption.[31] In column (9) of Table 4 we investigate whether the frequency of mobile phone use increases as GSM signal becomes available. Reassuringly, this is the case. The average usage index among residents—ranging from zero (no usage) to 4 (daily usage)—increases by 0.316 points when signal is available in the location of residence. Similarly, the proportion of individuals that never use mobile phones decreases as coverage expands (column (10)). Evaluated at the sample mean, this share drops by 50%.[32] In Table C.3 we show that this effect is homogeneous along the gender dimension. That is, both female and male respondents are more likely to use mobile phones once signal becomes available.

6. Potential mechanisms and other outcomes

The results so far document that the rollout of mobile phone coverage reduces infant mortality in Sub-Saharan Africa. They have, however, been silent about the specific mechanisms underlying this reduced-form effect. Generally speaking, mobile phone coverage facilitates acquiring and providing information. This could, for example, improve health knowledge or open up economic opportunities, thereby reducing infant mortality rates (e.g., Gupta et al., 2020; Aker and Mbiti, 2010; Bhalotra, 2010). To shed some light on the (relative) importance of candidate mechanisms, we use a range of alternative outcome variables drawn from a variety of DHS survey types. In this section, we give an overview of our exploratory analysis. The data, along with the regression methodologies and resulting estimates are discussed in more detail in Appendix E. All subsequently presented results should be interpreted with caution as the available data do not allow for an analysis of the same standard of empirical rigorousness as in previous sections.

[30] One potential concern is that these results are not representative for the area covered by the DHS surveys as the spatial coverage of the Afrobarometer is not as complete as in the DHS. To mitigate these concerns, we re-run regressions presented in Table 2, restricting observations to DHS clusters that are no farther away than 20 km from an Afrobarometer EA (see Table C.2). Reassuringly, the pattern of results remains unchanged.

[31] The DHS do not elicit information on mobile phone usage. A limited number of DHS surveys collect data on mobile phone *ownership* at the household level. Using this data, we find that GSM rollout statistically significantly increases the likelihood that a households owns a mobile phone (the point estimate is 4.938; the mean of the dependent variable 62.236). However, given the widespread practice of phone sharing, ownership may only be a poor proxy for actual access and usage of mobile technology (cf. Hampshire et al., 2015; Aker and Mbiti, 2010).

[32] The dependent variables in columns (9)–(10) are based on individual-level responses and represent average values for each EA and year. We can therefore run the regressions at the individual level and investigate whether the effect of GSM coverage on mobile phone usage differs across genders.

We start by testing if parents' general health knowledge improves as mobile phone signal expands. The idea being, that this leads to more informed decisions and thereby increases infant survival probabilities.[33] Consistent with mobile phone coverage facilitating the exchange of information, we find structural differences in levels of knowledge. Respondents are better informed about modern family planning methods as well the existence of AIDS and other sexually transmitted diseases when coverage expands (see Table E.1 in Appendix E.1). To gauge if individuals act upon the acquired information, we test whether the likelihood of using modern contraceptive methods changes with access to GSM signal. We do, in fact, find a large and statistically significant positive effect. This is also reflected in a mobile phone coverage-induced reduction in fertility (see Table E.2). These results suggest that health knowledge improves with mobile phone coverage, prompting individuals to adapt their behaviour accordingly. This, in turn, implies that knowledge-driven changes in caring behaviour of parents may (partially) explain why we observe a reduction in infant mortality as GSM coverage expands.

We next analyse if parents leverage GSM coverage to access healthcare services around the time of birth. There is ample evidence that improved access to in-person healthcare services—both pre- and postnatal—reduces infant mortality WHO, 2020. However, the results presented in Table E.3 do not provide any indication for the relevance of such effects in the context of our study. Neither the probability of being seen by a healthcare professional prior to the birth or the likelihood of delivering in a healthcare setting increases with mobile phone signal. There are no discernible differences in birth weights, further indicating the absence of differential prenatal preventative measures. Uptake of postnatal preventative healthcare, as measured by vaccination rates, is also unaffected by GSM rollout.[34]

Improved economic conditions is an alternative channel through which the rollout of GSM coverage and the facilitated exchange of information could reduce infant mortality. In Appendix E.3, we investigate this possibility empirically. Absent any direct measures of income, we test whether individuals are more likely to work at a job or business when mobile signal expands. As shown in Table E.5, this is not the case.[35] Furthermore, we do not detect any effect of mobile phone coverage on the probability that households have a bank account. However, there is evidence that mobile signal rollout raises the probability that respondents have health insurance. This suggests that the improved ability to absorb health-related financial shocks contributes to explaining why mobile technologies reduce infant mortality. The size of the effect, however, is modest.

In a final step, we investigate if the health status of children at the time of interview varies with mobile phone coverage. This could provide some insights into the relative importance of preventative versus curative effects of mobile coverage. In line with the previous null results, we do not see any differences in the probability of a child having anaemia, diarrhoea, fever or a cough at the time of the interview (Table E.7). This set of results provides suggestive evidence that our main results—the mortality-reducing effect of GSM coverage—is not driven by differences in preventative actions or effects, but by differential reactions to health issues, once they arise.

In sum, the results of our exploratory analysis of mechanisms outlined in this section suggest that the expansion of mobile phone coverage improves health knowledge. We find little evidence that parents (can) leverage access to mobile phone coverage beyond that. A qualifying remark relates to the fact that the outcome variables primarily capture the extensive margin (e.g., access to healthcare services). We cannot investigate whether GSM rollout influences the intensive margin (e.g., quantity or quality of healthcare services).

7. Conclusion

This paper shows that the expansion of mobile phone coverage substantially reduces child mortality in Sub-Saharan Africa. These effects are particularly pronounced in rural areas and situations in which mortality risks are elevated. Our findings imply that expanding mobile phone coverage may be an effective way of increasing survival probabilities of infants in regions where health care systems are not (yet) well-developed, particularly through the promotion of health-related information.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

[33] Unfortunately, the DHS surveys overlapping our sample period only elicit information on the parents' knowledge about general health issues. Knowledge about infant-health-related issues is elicited in the most recent rounds of the DHS survey (which do not/minimally) overlap with the period for which mobile phone coverage data is available to us. Furthermore, the surveys do not elicit information on the cause of death. This would have allowed us to test if information-intensive illnesses/causes of death disproportionately decline after the rollout of GSM signal.

[34] This holds when we look at all vaccines combined but also when we separately look at Polio 0, Polio 1, Measles and DPT vaccines, all of which are advised to be administered within the first year of life due to their morbidity and mortality reducing effects. See Table E.4.

[35] This lack of correlation is consistent with the absence of a relationship between GSM coverage and nighttime light intensity (see Section 4).

Appendix A. Data

Table A.1
DHS Waves included in sample.

Angola 2015	Ghana 2008	Malawi 2015	Sierra Leone 2008
Benin 2001	Ghana 2014	Mali 2001	Sierra Leone 2013
Benin 2012	Guinea 1999	Mali 2006	South Africa 2016
Burkina Faso 1999	Guinea 2005	Mali 2012	Tanzania 1999
Burkina Faso 2003	Guinea 2012	Mozambique 2011	Tanzania 2010
Burkina Faso 2010	Ivory Coast 1998	Namibia 2000	Tanzania 2015
Burundi 2010	Ivory Coast 2012	Namibia 2006	Togo 2013
Burundi 2016	Kenya 2003	Namibia 2013	Uganda 2000
Cameroon 2004	Kenya 2008	Nigeria 2003	Uganda 2006
Cameroon 2011	Kenya 2014	Nigeria 2008	Uganda 2011
Chad 2014	Lesotho 2004	Nigeria 2013	Uganda 2016
Congo Democratic Republic 2007	Lesotho 2009	Rwanda 2005	Zambia 2007
Congo Democratic Republic 2013	Lesotho 2014	Rwanda 2010	Zambia 2013
Ethiopia 2000	Liberia 2007	Rwanda 2014	Zimbabwe 1999
Ethiopia 2005	Liberia 2013	Senegal 2005	Zimbabwe 2005
Ethiopia 2010	Madagascar 2008	Senegal 2010	Zimbabwe 2010
Ethiopia 2016	Malawi 2000	Senegal 2012	Zimbabwe 2015
Gabon 2012	Malawi 2004	Senegal 2015	
Ghana 2003	Malawi 2010	Senegal 2016	

Table A.2
Locational controls: sources and definitions.

Variable	Source	Definition
Population	Gridded Population of the World (GPW), v. 4	Total population within 10km buffer of DHS cluster
Frequency of negative rainfall shocks	CPC Global Temperature database	Share of months in a given year in which precipitation is below the 15th percentile of the historical distribution of rainfall for a DHS cluster
Night-time lights	Li et al. (2020)	Total night-time lights within 10km buffer of DHS cluster
Elevation	WorldClim, v. 2.1	Average elevation within 10km buffer of DHS cluster
Elevation	WorldClim, v. 2.1	Ruggedness index calculated using the index devised in Riley et al. (1999)

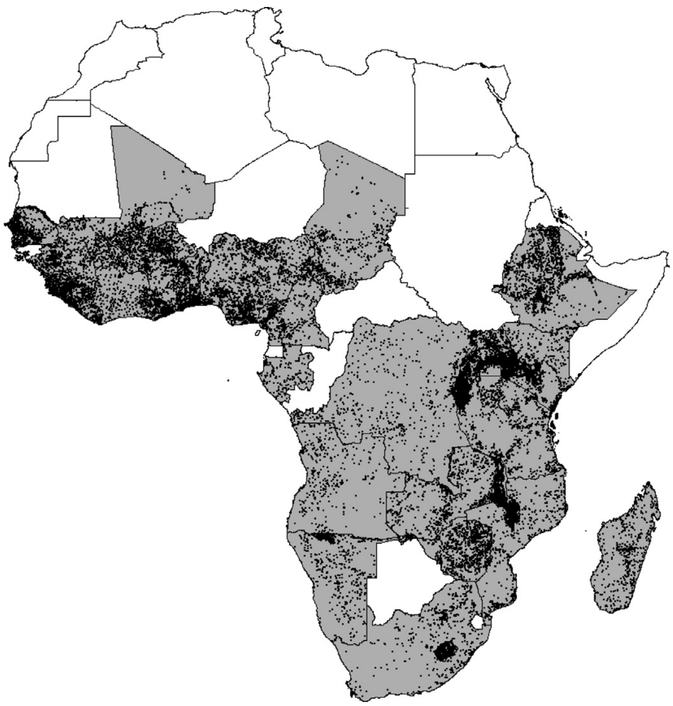

Fig. A1. Figure depicts the countries covered in our sample (shaded grey) as well the locations of the individual DHS clusters (dots).

Appendix B. Additional and supporting evidence

B1. Mobile phone coverage and night-time light intensity

To investigate if night-time light intensity is related to the rollout of mobile phone coverage, we construct a cluster×year panel dataset. The lights data are provided by the Defense Meteorological Satellite Program-Optical Line Scanner (DMSP-OLS) sensor and represent an index ranging from 0 to 63. Following the literature, we log-transform the data (c.f. Michalopoulos and Papaioannou, 2014).[36] We combine this data with annual information on GSM coverage for each cluster (see Section 3) and run the following regression model:

$$y_{k,c,t} = \beta P_{k,c,t} + \gamma' \mathbf{X}_{k,c,t} + \kappa_{k,c} + \pi_{c,t} + \varepsilon_{k,c,t}. \tag{B.1}$$

The dependent variable is logarithmised night-time light intensity index for DHS cluster k (located in country c) in year t. The GSM coverage indicator is represented by $P_{k,c,t}$ and the vectore$\mathbf{X}$ is the set of locational controls, as defined in the main part. The cluster ($\kappa_{k,c}$) and country×year ($\pi_{c,t}$) fixed absorb any location and time-specific effects. Finally, $\varepsilon_{k,c,t}$ symbolises the error term; standard errors are clustered at the DHS cluster level.

Table B.1 reports the regression results. In column (1), we use contemporaneous GSM coverage as the main regressor. In column (2) and (3), we lag coverage by one and two years, respectively. We do not detect any statistically significant relationship between night-time light intensity and GSM coverage, irrespective of the lag structure employed. This is also the case when we simultaneously include all three coverage indicators (column (4)).

B2. Regression table for Fig. 3

Table B.2 reports coefficients and standard errors corresponding to Fig. 3

Table B.1
Mobile phone coverage and night-time light intensity.

	Log night-time light intensity			
	(1)	(2)	(3)	(4)
Phone coverage t	−0.006			−0.004
	(0.009)			(0.009)
Phone coverage $t-1$		−0.007		−0.008
		(0.009)		(0.009)
Phone coverage $t-2$			0.000	0.006
			(0.008)	(0.008)
DHS cluster FE	yes	yes	yes	yes
Country×year FE	yes	yes	yes	yes
Locational controls	yes	yes	yes	yes
Obs.	233,751	233,751	233,751	233,751
R-squared	0.951	0.951	0.951	0.951

Notes: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at DHS cluster level. Locational controls include population, frequency of negative rainfall shocks, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables are provided in Appendix A.

[36] Again following common practice, we add a small number in order to avoid losing observations with an index value of zero (i.e., $y = \ln(lights + 0.01)$).

Table B.2
Mobile phone coverage and mortality by age.

	Mortality by Age (in Months)					
	< 1	2–12	13–24	25–36	37–48	49–60
	(1)	(2)	(3)	(4)	(5)	(6)
Phone coverage	−0.301***	−0.585***	−0.150*	0.038	0.003	0.018
	(0.089)	(0.098)	(0.084)	(0.047)	(0.033)	(0.027)
DHS cluster FE	yes	yes	yes	yes	yes	yes
Country×birthyear FE	yes	yes	yes	yes	yes	yes
Birth controls	yes	yes	yes	yes	yes	yes
Mother FE	yes	yes	yes	yes	yes	yes
Locational controls	yes	yes	yes	yes	yes	yes
Obs.	1,069,780	1,017,522	967,544	940,111	932,022	928,128
R-squared	0.399	0.373	0.359	0.340	0.335	0.333
Mean of Dep. Var.	3.988	3.966	2.270	0.683	0.332	0.213

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at DHS cluster level. Dependent variable is multiplied by hundred to facilitate interpretation. I.e., mortality is 100 if the child died within the first specified months of life, and 0 otherwise. Birth controls include fixed effects for gender and birth order of the child, for the mother's age at birth as well as for the number of children per birth event. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables is provided in Appendix A.

Appendix C. Robustness

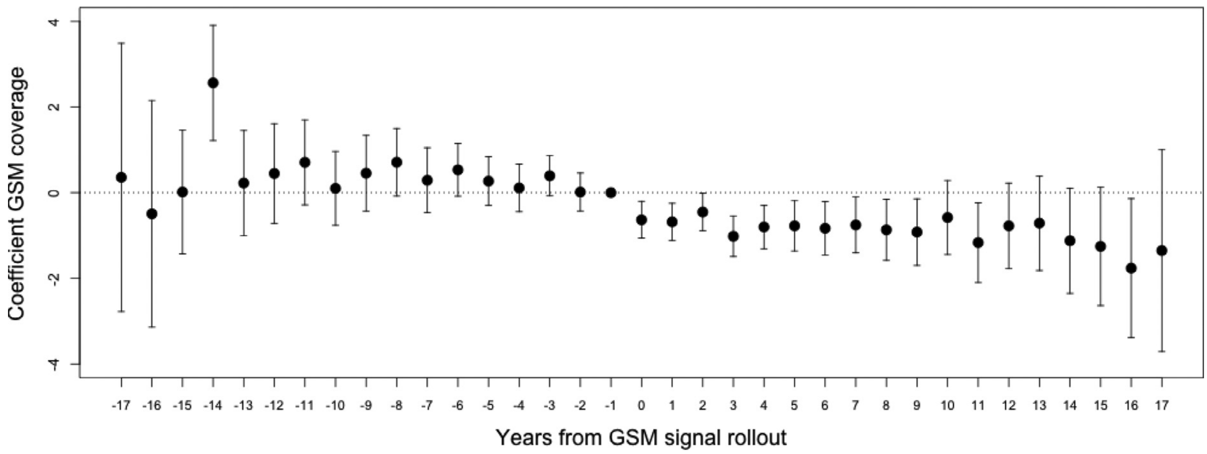

Fig. C.1. Figure depicts the event study point estimates and 95% confidence intervals of the dynamic effect of GSM coverage on infant mortality. Results produced using Sun and Abraham (2021)'s event study model with full set of controls (i.e., mother fixed effects and locational controls).

Table C.1
Robustness: Mobile phone coverage and infant mortality.

	Infant Mortality							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Phone coverage	-0.906^{***}	-0.928^{***}	-0.752^{***}	-0.930^{***}	-0.864^{***}	-0.864^{***}	-0.967^{***}	-0.716^{***}
	(0.151)	(0.116)	(0.231)	(0.134)	(0.127)	(0.137)	(0.151)	(0.112)
Robustness	admin region× birthyear FE	country×climate-zone ×birthyear FE	No movers	Drop top/bottom 5% age at birth	Control for conflict	clustering at admin 1 region	most recent survey wave	Interpolate GSM coverage
DHS cluster FE	yes	yes	yes	yes	yes	yes	yes	yes
Country×birthyear FE	no	no	yes	yes	yes	yes	yes	yes
Birth controls	yes	yes	yes	yes	yes	yes	yes	yes
Mother FE	yes	yes	yes	yes	yes	yes	yes	yes
Locational controls	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	1,069,740	1,065,494	277,697	953,493	1,069,780	1,069,780	719,191	1,276,312
R-squared	0.403	0.398	0.418	0.404	0.397	0.397	0.372	0.373
Mean of Dep. Var.	7.802	7.805	8.063	7.574	7.802	7.802	7.202	6.694

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at DHS cluster level. Dependent variable is multiplied by hundred to facilitate interpretation. I.e., Infant mortality is 100 if the child died within the first 12 months of life, and 0 otherwise. Birth controls include fixed effects for gender and birth order of the child, for the mother's age at birth as well as for the number of children per birth event. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables is provided in Appendix A.

Table C.2
Mobile phone coverage and infant mortality (within 20km of Afrobarometer EA).

	Infant Mortality				
	OLS				IV
	(1)	(2)	(3)	(4)	(5)
Phone coverage	-0.795^{***}	-0.812^{***}	-0.944^{***}	-0.845^{***}	-2.004^{**}
	(0.212)	(0.208)	(0.252)	(0.256)	(0.939)
DHS cluster FE	yes	yes	yes	yes	yes
Country×birthyear FE	yes	yes	yes	yes	yes
Birth controls	no	yes	yes	yes	yes
Mother FE	no	no	yes	yes	yes
Locational controls	no	no	no	yes	yes
Obs.	410,355	410,352	331,150	331,150	331,150
R-squared	0.055	0.074	0.409	0.409	
Mean of Dep. Var.	6.731	6.731	7.241	7.241	7.241

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at DHS cluster level in columns (1)–(4) and bootstrapped in column (5). Dependent variable is multiplied by hundred to facilitate interpretation. I.e., Infant mortality is 100 if the child died within the first 12 months of life, and 0 otherwise. Birth controls include fixed effects for gender and birth order of the child, for the mother's age at birth as well as for the number of children per birth event. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables is provided in Appendix A.

Table C.3
Individual-level regressions: mobile phone coverage and phone use.

	Mobile phone use					
	All respondents		Female		Male	
	Usage	Share never use	Usage	Share never use	Usage	Share never use
	(1)	(2)	(3)	(4)	(5)	(6)
Phone coverage	0.350^{**}	-0.080^{**}	0.394^{**}	-0.080^{**}	0.283^{*}	-0.076^{**}
	(0.138)	(0.033)	(0.155)	(0.039)	(0.152)	(0.036)
Grid cell FE	yes	yes	yes	yes	yes	yes
Country×year FE	yes	yes	yes	yes	yes	yes
Obs.	55,743	55,743	27,919	27,919	27,761	27,761
R-squared	0.431	0.381	0.510	0.459	0.484	0.439
Mean of Dep. Var.	2.644	0.207	2.453	0.244	2.834	0.170

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at the 10 × 10 km grid cell level. Data are from Afrobarometer waves 4–6. Only observations with precise information on location of the enumeration area are used (precision code equal to 1).

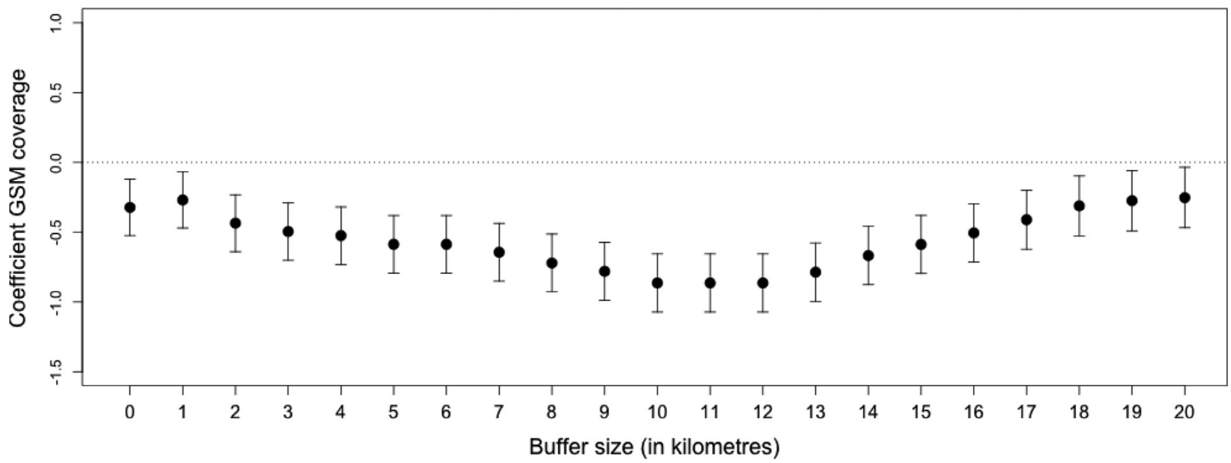

Fig. C.2. Figure depicts the point estimates and 95% confidence intervals of GSM coverage on infant mortality. GSM coverage is defined as an indicator variable that captures whether any generation of GSM signal (i.e., 2G or 3G) is available within a given buffer (where we vary the size of the buffer between 0 and 20 km).

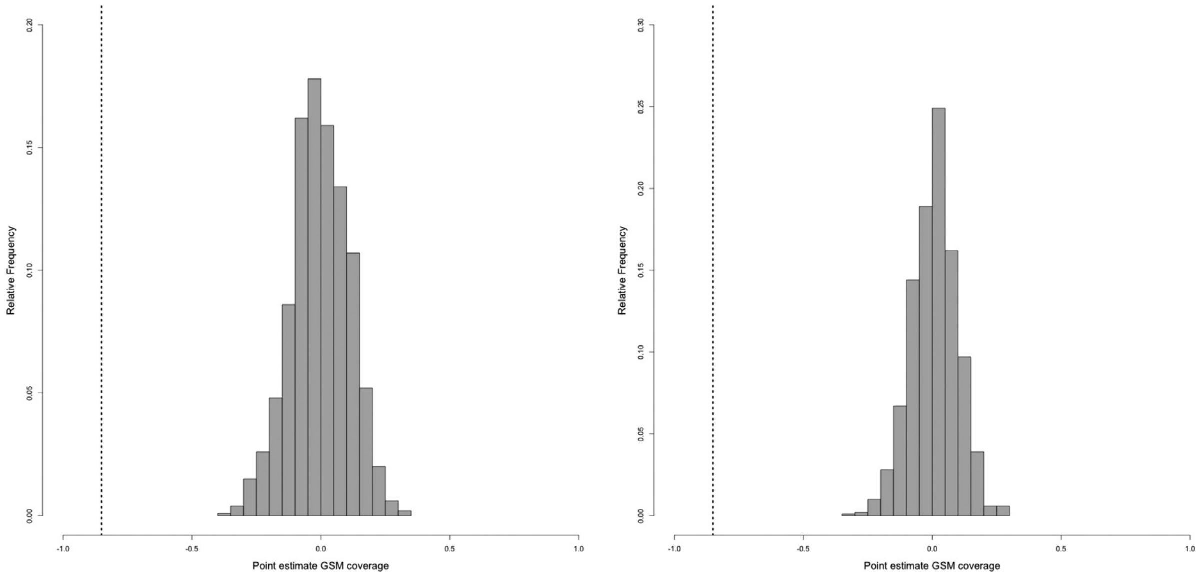

Fig. C.3. Panel (a) plots the distribution of point estimates obtained from 1000 random permutation of geographical coordinates of DHS clusters and associated mobile coverage information. The dashed black vertical line at −0.891 represents point estimate obtained using the actual coverage data (see Table 2, column (3)). Panel (b) represents the distribution of 1000 point estimates obtained from regressions in which the starting year of coverage is set randomly for clusters that ever gain access to mobile phone coverage. Coverage is always set to zero for clusters that never have coverage.

Appendix D. Instrumental variable approach

A primary threat to the validity of our OLS estimation strategy is that unobserved local shocks are correlated with the rollout of mobile phone coverage. To address this concern, we employ an instrumental variable strategy that exploits the fact that frequent lightning strikes delay the adoption of mobile phone technology. As outlined in the main part, the lag in diffusion is due to the fact that mobile-phone infrastructure is sensitive to the voltage surges induced by the lightning strikes. Furthermore, lightning storms can negatively impact connectivity, thereby reducing (and delaying) demand and supply of mobile phone infrastructure (Martin, 2016; Zeddam and Day, 2014; Andersen et al., 2012).

We implement the instrumental variable strategy by predicting mobile phone signal availability in a given location using the time-trend-interacted local average lightning frequency (see Andersen et al., 2012; Manacorda and Tesei, 2020 for similar applications). The data on average lightning strike intensity are taken from Kaplan and Lau (2019). Due to the binary nature of the (potentially) endogenous coverage indicator, we operationalise the IV approach using the three-step procedure proposed by Wooldridge (2002, p. 236ff.) (see Maffioli (2021) for a similar application). In the first step, we use a probit model to predict coverage at the DHS cluster level using lightning frequency. That is, we use the coverage indicator as a binary outcome variable and the time-trend-interacted local average lightning frequency as the explanatory variable. Formally, the probit model is represented as:

$$P_{k,c,t} = \Phi\big(\beta_1 L_{k,c} \times t + \gamma_{\mathbf{1}}{}' \mathbf{X}_{k,c,t} + \pi_t + \zeta_{k,c,t}\big). \tag{D.1}$$

The dependent variable $P_{k,c,t}$ is a dummy variable taking the value one when the DHS cluster k (situated in country c) has mobile phone coverage in year t; Φ is the cumulative distribution function of the regression equation. The included regressors are: time-interacted local lightning frequency ($L_{k,c} \times t$) as well as the locational controls introduced in Section 4 (subsumed in $\mathbf{X}_{k,c,t}$), and year fixed effects (π_t). The idiosyncratic error is clustered at the DHS cluster level. In an extension, we further account for country×birthyear fixed effects. When this more stringent set of dummies is included, the number of observations decreases due to the fact that the fixed effects perfectly predict the outcome (i.e. coverage) in some cases. These perfectly separated observations prevent the probit estimator from converging and therefore have to be excluded from regressions (e.g., Correia et al., 2019). As documented below, the results do not depend on the choice of fixed effects.

Panel A of Table D.1 reports the results of the Probit regressions. In column (1) we control for the set of locational characteristics, and year fixed effects. The statistically highly significant and negative coefficient documents that higher frequency of lightning strikes reduces the probability that the signal expands into a DHS cluster. The results remain very stable when we control for a more extensive set of fixed effects in column (2).

In step two and three, we use the predicted probabilities from model (D.1)—denoted by $\widehat{\Phi(\cdot)}$— as the excluded instrument in a standard 2SLS-IV setup. In analogy to the main approach described in Section 2, we run the regressions at the birth-event level. Formally, the next two steps can be written as:

$$P_{i,m,k,c,t} = \beta_2 \widehat{\Phi(\cdot)} + \gamma_{\mathbf{2}}{}' \mathbf{X}_{i,m,k,c,t} + \chi_{c,t} + \psi_m + \nu_{i,m,k,c,t} \tag{D.2}$$

Table D.1
3-step instrumental variable procedure.

Panel A: Cluster-level probit regressions		
	Coverage	
	(1)	(2)
Lightning strike frequency	-0.002^{***}	-0.0003^{***}
× time trend	(0.000)	(0.000)
Birthyear FE	yes	no
Country×birthyear FE	no	yes
Locational controls	yes	yes
Observations	314,906	283,584
Panel B: First stage regression 2SLS		
	Coverage	
	(1)	(2)
Coverage	0.904^{***}	0.980^{***}
(probit predictions)	(0.005)	(0.002)
First-stage F-statistic	20,362	46,738
DHS cluster FE	yes	yes
Country×birthyear FE	yes	yes
Birth controls	yes	yes
Mother FE	yes	yes
Locational controls	yes	yes
Obs.	1,268,041	958,346
Panel B: Second stage regression 2SLS		
	Infant Mortality	
	(1)	(2)
Coverage	-1.283^{***}	-1.319^{***}
	(0.488)	(0.311)
DHS cluster FE	yes	yes
Country×birthyear FE	yes	yes
Birth controls	yes	yes
Mother FE	yes	yes
Locational controls	yes	yes
Obs.	1,268,041	958,346

Note: $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Birth controls include fixed effects for gender and birth order of the child, for the mother's age at birth as well as for the number of children per birth event. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. Panel A: Probit regressions run at the DHS cluster level, standard errors (reported in parentheses) are clustered at the DHS cluster level. Panel B & C: 2SLS-IV regressions. Standard errors (reported in parentheses) are bootstrapped.

$$o_{i,m,k,c,t} = \beta_3 \widehat{\widehat{\Phi(\cdot)}} + \gamma_3{}' \mathbf{X}_{i,m,k,c,t} + \kappa_{k,c} + \mu_m + \varepsilon_{i,m,k,c,t}, \tag{D.3}$$

where $\widehat{\widehat{\Phi(\cdot)}}$ are the predicted values derived from the results of regression (D.2). $P_{i,m,k,c,t}$ and $o_{i,m,k,c,t}$ is a dummy for coverage and death within the first year of life, respectively. In keeping with the OLS approach, we account for birth and mother characteristics, locational controls, mother fixed effects as well as country×birthyear fixed effects. Because we employ a three step approach, standard errors Eqs. (D.2)–(D.3) are bootstrapped (100 repetitions).

Panel B reports the result of step 2 (i.e., regression Eq. (D.2)). The probit predictions obtained from step 1 strongly influence observed coverage, irrespective of the fixed effects included in the probit regression. Panel C shows the final IV estimates (i.e., regression Eq. (D.3)). The point estimates are very similar compared to the OLS coefficients in all three columns.

Appendix E. Mechanisms and further outcomes

This appendix provides a more detailed description of the data, regression methodologies, and results discussed in Section 6.

E1. Health knowledge and fertility

Health knowledge

As GSM signal becomes available, obtaining and distributing infant-health-related and general information is greatly facilitated. On average, parents could therefore be more knowledgeable about how to care for their children, particularly in potentially life-threatening situations (e.g., illness or accident). This should then lead to more informed decisions, thereby

Table E.1
Mobile phone coverage and health knowledge.

	Know modern family planning method (1)	Heard of Aids or other STI (2)	Use modern contraceptive (3)
Phone coverage	2.158***	1.857***	1.845***
	(0.620)	(0.535)	(0.656)
Grid cell FE	yes	yes	yes
Country×year FE	yes	yes	yes
Respondent controls	yes	yes	yes
Locational controls	yes	yes	yes
Obs.	848,612	761,081	595,677
R-squared	0.479	0.242	0.188
Mean of Dep. Var.	74.35	95.26	17.06
Unit of observation	adult	adult	adult
Time of GSM exposure	interview	interview	interview

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at the 10 × 10 km grid cell level. Dependent variables are multiplied by hundred to facilitate interpretation. Respondent controls include fixed effects for gender and age of respondent as well as a dummy for urban residence. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables is provided in Appendix A.

increasing infant survival probabilities (Dammert et al., 2014). Unfortunately, the DHS surveys that overlap the time-span for which we have information on mobile phone coverage do not specifically ask about the parents' knowledge of infant-health-related issues or whether mobile phones are used to acquire medical advice.[37] Furthermore, the surveys do not elicit information on the cause of death. This would have allowed us to test if information-intensive illnesses/causes of death disproportionately decline after the rollout of GSM signal.

As an indirect way of testing for differential patterns in infant health-related knowledge, we investigate whether knowledge about more general health issues changes as mobile phone signal expands. To this end we draw on adult- and household-level data. These are also collected as part of the DHS.[38] In contrast to the analysis presented in the main part (Section 4), these data solely capture a snapshot of information at the time of the interview and are therefore purely cross-sectional in nature. As the location of enumeration areas (i.e., DHS clusters) changes across survey waves, this implies that we cannot control for location-of-residence dummies. To nevertheless account for time-invariant unobservables, we follow the strategy outlined in Section 5.3 and control for 10 × 10 km grid cell fixed effects (see Eq. (2)).[39]

In the following, we use two proxies: an indicator variable taking the value one if the respondent knows any modern family planning methods and a dummy variable that captures whether a respondent had heard of AIDS or other sexually transmitted diseases (STIs).[40] The results presented in columns (1) and (2) of Table E.1 show that health-related knowledge—as measured by these two proxies—increases with the rollout of GSM signal. In order for this improved knowledge to affect health outcomes, individuals need to act upon the acquired information. In the last column of Table E.5, we provide some indicative evidence that this is indeed the case. Respondents residing in areas with mobile phone signal coverage are, in line with the findings of column (2), more likely to use modern contraceptive methods than individuals living outside the reach of GSM signal. We next investigate if this effect is also reflected in a decline in fertility.

Fertility

The positive effects of mobile technologies on knowledge about and use of modern family planning methods implies that the availability of GSM coverage should also influence fertility decisions. Furthermore, it is a stylised fact that mortality and fertility are closely interconnected. For example, key feature of the demographic transition—a process that is still ongoing in Sub-Saharan Africa—is that the reduction in mortality is followed by a decline in total fertility (e.g. Galor, 2005; Chesnais, 1992). To investigate whether the mortality-risk reducing effect of the mobile coverage rollout is also reflected in a decline in total fertility, we construct a mother×year panel from the birth history records collected as part of the DHS. This dataset

[37] This type of information is elicited in the most recent rounds of the DHS survey (which do not/minimally) overlap with the period for which mobile phone coverage data is availability to us.

[38] Specifically, the data are taken from the 'Individual Recodes', 'Male Recodes', and 'Household Recodes'.

[39] Formally, the regression equation can be written as:

$$o_{i,k,c,t} = \beta P_{k,c,t} + \gamma' \mathbf{X}_{i,k,c,t} + \mu_{g(k,c)} + \pi_{c,t} + +\varepsilon_{i,k,c,t},$$

where $o_{i,k,c,t}$ is the outcome for respondent i, residing in DHS cluster k (located in country c) and interviewed in year t. The dummy $P_{k,c,t}$, captures whether mobile phone coverage is available in a given location and year. $\mathbf{X}$ is a set of control variables including age, gender and urban residence of the respondent as well as locational controls. The 10 × 10 km grid cell fixed effects are symbolised by $\mu_{g(k,c)}$, and the country×year dummies by $\pi_{c,t}$. The idiosyncratic error terms ($\varepsilon_{i,k,c,t}$) are clustered at the grid cell level.

[40] For details on definition of variables, see www.idhsdata.org/idhs-action/variables/FPKNOTYP and www.idhsdata.org/idhs-action/variables/STIHEARD.

Table E.2
Mobile phone coverage and total fertility.

	Birth		
	(1)	(2)	(3)
Phone coverage	−0.527***	−0.531***	−0.520***
	(0.087)	(0.085)	(0.085)
Mother FE	yes	yes	yes
Country×birthyear FE	yes	yes	yes
Birth controls	no	yes	yes
Locational controls	no	no	yes
Obs.	5,171,444	5,171,444	5,171,444
R-squared	0.085	0.104	0.104
Mean of Dep. Var.	23.77	23.77	23.77

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at DHS cluster level. Dependent variable is multiplied by hundred to facilitate interpretation. I.e., birth is 100 if a woman gave birth to a child in a given year, and 0 otherwise. Birth controls include fixed effects for the mother's age at birth. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables are provided in Appendix A. Woman aged 15–49 are included in regressions.

encompasses 5,171,444 observations and is analysed using a regression setup analogous to Equation(1). The dependent variable now captures whether a woman gave birth to child in a given year.[41] Again, we multiply the dummy by 100 to facilitate interpretation of coefficient sizes. Table E.2 reports the results.

Starting with a parsimonious setup that only accounts for individual and country×year fixed effects, column (1) shows that the availability of GSM signal is reflected in a reduction of total fertility rates. The likelihood of a woman giving birth in a given year declines by 0.5 percentage points. Evaluated at the sample mean, this represents a drop of 2%. We obtain similar point estimates when we add birth and time-varying locational controls in columns (2)–(3). Thus, consistent with the demographic transition and the associated move to a regime of sustained growth, we find that the effects of mobile phone coverage on infant mortality are also reflected in fertility decisions. This, in turn, has positive implications for long-run economic growth if the decline in (total) fertility is larger than the drop in mortality (for example in presence of hoarding motives Doepke, 2005).

Two qualifying remarks relating to the results presented in Table E.2 are necessary. First, it is important to note that our results pertain to total fertility rates. Whether the rollout of mobile phone coverage leads—or will lead—to a reduction in net fertility rates cannot be analysed within the framework of our analysis.[42] Second, we cannot assess the extent to which the mobile-phone-related decline in fertility is attributable to the reduction in infant mortality or the improved knowledge about family planning methods.

E2. Access to care

To investigate if parents (can) leverage mobile phone coverage to access healthcare services around the time of birth, we test whether there are differences in pre-and postnatal care. This information is only reported for a subset of births.[43] In many cases, only one observation per mother is available. In the following analysis, we therefore exchange the mother fixed effects in regression Eq. (1) for DHS cluster dummies. This implies that we compare infants born in the same location in years with and without signal. Table E.3 presents the results.

In the first three columns, we focus on antenatal care visits by a skilled provider which is a strong predictor of infant mortality (e.g. Tekelab et al., 2019) The regression results show that the probability that a woman ever receives antenatal care does not change with signal expansion (column (1)). As documented in columns (2)–(3), the qualification of the person delivering care is also unaffected by coverage. Neither the likelihood of receiving care by a doctor or a nurse increases when mobile signal is available. These results indicate that mobile phone signal rollout does not improve access to (in-person) healthcare services.[44]

[41] The modified regression Eq. (1) can be written as:

$$b_{m,k,c,t} = \beta P_{k,c,t} + \gamma' \mathbf{X}_{m,k,c,t} + \pi_{c,t} + \mu_m + \varepsilon_{m,k,c,t},$$

where $b_{m,k,c,t}$ is a dummy capturing whether woman m residing in DHS cluster k (located in country c) gave birth in year t. All explanatory variables are defined in analogy to the previous analysis (see Section 2 for details). Note that the DHS cluster fixed effects are entirely absorbed by the inclusion of the individual (i.e., woman) fixed effects.

[42] We observe total fertility and mortality, but not the net effect on fertility.

[43] This set of questions is collected as part of the 'Children Recodes' and only available for children born in the five years preceding the survey.

[44] However, we cannot rule out that there is a shift in the circumstances under which infants are admitted to hospital. For example, the ability to contact health care professionals prior to transport could result in a more targeted hospitalisation. I.e., infants are brought only if they are in need of acute care.

Table E.3
Mobile phone coverage and ante- and postnatal outcomes.

	Antenatal Outcomes			Postnatal Outcomes				
	Any antenatal care (1)	Antenatal care by physician (2)	Antenatal care by nurse (3)	Birth at health facility (4)	Delivery by physician (5)	Delivery by nurse (6)	Any vaccine (7)	Birth weight (8)
Phone coverage	0.081	0.294	−0.200	0.423	0.203	−0.009	−0.011	−14.959
	(0.392)	(0.310)	(0.466)	(0.366)	(0.178)	(0.351)	(0.530)	(10.983)
DHS cluster FE	yes	yes	yes	yes	yes	yes	yes	yes
Country×birthyear FE	yes	yes	yes	yes	yes	yes	yes	yes
Birth controls	yes	yes	yes	yes	yes	yes	yes	yes
Locational controls	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	371,451	322,473	338,357	530,537	470,729	484,603	205,956	263,536
R-squared	0.451	0.342	0.491	0.438	0.254	0.424	0.436	0.235
Mean of Dep. Var.	88.16	11.07	68.22	50.83	7.200	45.67	78.30	3213
Unit of observation	infant	infant	infant	infant	infant	infant	infant	infant
Time of GSM exposure	birth	birth	birth	birth	birth	birth	birth	birth

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at DHS cluster level. Dependent variables in columns (1)–(8) are multiplied by hundred to facilitate interpretation. Birth controls include fixed effects for gender and birth order of the child, for the mother's age at birth as well as for the number of children per birth event. Locational controls include population, include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables are provided in Appendix A.

Table E.4
Mobile phone coverage and vaccination.

	Vaccine			
	Polio 0 (1)	Polio 1 (2)	Diphteria, pertussis tetanus (3)	Measles (4)
Phone coverage	−0.014	0.263	−0.241	−0.144
	(0.180)	(0.353)	(0.405)	(0.431)
DHS cluster FE	yes	yes	yes	yes
Country×birthyear FE	yes	yes	yes	yes
Birth controls	yes	yes	yes	yes
Locational controls	yes	yes	yes	yes
Obs.	619,457	466,102	291,440	463,542
R-squared	0.626	0.337	0.443	0.491
Mean of Dep. Var.	37.64	83.51	71.92	61.62

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at DHS cluster level. Birth controls include fixed effects for gender and birth order of the child, for the mother's age at birth as well as for the number of children per birth event. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables are provided in Appendix A.

In columns (4)–(8), we investigate if postnatal outcomes vary with phone coverage. This is not the case for the likelihood of giving birth in a healthcare facility does not increase with coverage. Columns (5)–(6) further indicate that the qualification of the person assisting the birth is not influenced by signal availability. The result of column (7) documents that the likelihood of a child ever being vaccinated does not change with the rollout of GSM signal. This is also the case when we separately look at Polio 0, Polio 1, Measles and DPT vaccines, all of which are advised to be administered within the first year of life due to their morbidity and mortality reducing effects (see Table E.4 below). In the final column of Table E.3, we test if birth weight varies in dependence of GSM coverage. This is not the case.

Taken together, the results presented in this subsection suggest that improved access to (in-person) healthcare services is not a primary mechanism underlying the mobile-phone-related reduction in infant mortality.

E3. Income opportunities, credit and insurance markets

Income opportunities

To investigate if mobile phone-driven improvements in income opportunities play a role in explaining our results, we draw on adult- and household-level data. These again represent a snapshot at the time of interview.[45] Unfortunately, the DHS does not directly elicit information on income (opportunities). We therefore use the probability of working at a job or business as a proxy for improvements in income opportunities. Column (1) of Table E.5 documents that gaining access to

[45] Consequently, we use the regression setup in which time-invariant characteristics are accounted for via inclusion of grid-cell fixed effects (cf. Appendix E.1).

Table E.5
Mobile phone coverage and economic outcomes.

	Income generation	Smoothing	
	Working	Bank account	Health insurance
	(1)	(2)	(3)
Phone coverage	−1.374	−3.068	2.003**
	(0.967)	(2.508)	(0.855)
Grid cell FE	yes	yes	yes
Country×year FE	yes	yes	yes
Respondent controls	yes	yes	yes
Locational controls	yes	yes	yes
Obs.	831,336	279,438	383,792
R-squared	0.290	0.333	0.316
Mean of Dep. Var.	64.05	28.03	7.756
Unit of observation	adult	household	adult
Time of GSM exposure	interview	interview	interview

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at the 10 × 10 km grid cell level. Dependent variables are multiplied by hundred to facilitate interpretation. Respondent controls include fixed effects for gender and age of respondent as well as a dummy for urban residence. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables is provided in Appendix A.

GSM coverage does not influence this income proxy. This result is in line with the fact that the effect of phone coverage on infant mortality does not change when we control for changes in local levels of development (see Table 2 column (4) and Appendix B.1). It is important to note that our results reflect short-run effects. In the longer run, income-related effects could materialise.

Income and risk smoothing

We next investigate the plausibility of income and risk smoothing as mechanisms, proxied by the probability of having a bank account and health insurance, respectively. This information has only been elicited in relatively recent DHS survey waves. The temporal variation that can be exploited is thus limited compared to the previous analysis.

Column (2) of Table E.5 shows that there is no indication that the rollout of mobile phone signal increases the likelihood that households have a bank account. The point estimate is statistically non-significant and even negative in sign (although small relative to the sample mean). This is somewhat surprising given our earlier results[46] and the growing evidence that mobile phone technologies can improve access to banking (see Suri, 2017, for an overview). One possible explanation is that we are not able to pick these effects up due to the restricted temporal and spatial variation. A further possibility is that there are substantial heterogeneities across regions. We do, in fact, find evidence that the average effect is masking existing heterogeneity. When restricting our sample to Kenya, for which there is abundant evidence that mobile phones are extensively used for financial transactions (Suri, 2017), the effect of GSM rollout on the probability of having a bank account becomes positive and statistically significant (see Table E.6). A third possibility is that mobile technology increases the share of the population that gains access to informal, rather than formal, types of banking (c.f Suri, 2017). Information on the former, however, is not collected in the DHS.

In contrast to access to credit markets, we find evidence that mobile signal rollout raises the probability that respondents have health insurance. The phone coverage coefficient in column (3) is statistically significant. Its size implies that gaining access to GSM signal increases the likelihood of being covered by health insurance by two percentage points. This suggests that the improved ability to absorb health-related financial shocks contributes to explaining why mobile technologies reduce infant mortality.

E4. Child-level health outcomes

In a final step, we investigate if the health status of children at the time of interview varies with (contemporaneous) mobile phone coverage.[47,48] More specifically, we test if the probability of having had diarrhoea, fever or a cough in the two weeks leading up to the interview depends on GSM coverage. Columns (1)–(3) in Table E.7 shows that this is not the case. None of the coefficients are statistically significant and their size is small relative to the sample mean. There is also no effect of mobile phone coverage on the probability that a child has anaemia (column (4)). This series of null results provides

[46] See differential impact of phone coverage in the presence of rainfall shocks (Table 3, column (5)).

[47] This information is collected as part of the 'Children Recodes' and only available for children born in the five years preceding the survey.

[48] Representing only a snapshot at the time of interview, we cannot account for DHS-cluster fixed effects. As an alternative way of controlling for local unobservables, we again employ the regression setup in which we exchange the cluster fixed effects for grid-cell fixed effects (cf. Appendix E.1).

Table E.6
Kenya: Mobile phone coverage and access to banking.

	Bank account (1)
Phone coverage	21.706***
	(2.311)
Grid cell FE	yes
Country×year FE	yes
Respondent controls	yes
Locational controls	yes
Obs.	19,922
R-squared	0.257
Mean of Dep. Var.	39.50
Unit of observation	household
Time of GSM exposure	interview

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at the 10 × 10 km grid cell level. Dependent variable is multiplied by hundred to facilitate interpretation. Respondent controls include fixed effects for gender and age of respondent as well as a dummy for urban residence. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables are provided in Appendix A.

Table E.7
Mobile phone coverage child health outcomes at time of interview.

	Health outcome			
	Diarrhoea (1)	Fever (2)	Cough (3)	Anaemia (4)
Phone coverage	−0.558	0.301	−1.584	0.606
	(0.939)	(1.262)	(1.266)	(2.411)
DHS cluster FE	yes	yes	yes	yes
Country×birthyear FE	yes	yes	yes	yes
Birth controls	yes	yes	yes	yes
Locational controls	yes	yes	yes	yes
Obs.	255,203	255,220	255,221	84,048
R-squared	0.119	0.163	0.172	0.223
Mean of Dep. Var.	16.24	23.76	23.31	66.02
Time of GSM exposure	interview	interview	interview	interview

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors (reported in parentheses) are clustered at the 10 × 10 km grid cell level. Dependent variables are multiplied by hundred to facilitate interpretation. Respondent controls include fixed effects for gender and age of child as well as a dummy for urban residence. Locational controls include population, frequency of negative rainfall shocks, (log) night-time lights, as well as the interaction between a linear time trend and the following characteristics: elevation, ruggedness, distance to coastline, distance to land border, and distance to the capital city. More details on control variables is provided in Appendix A.

suggestive evidence that our main results—the mortality-reducing effect of GSM coverage—is not driven by differences in preventative actions and effects, but by differential reactions to health issues, once they arise.

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