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Can Cash Transfers Save Lives? Evidence from a Large-Scale Experiment in Kenya*

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Abstract

We estimate the impacts of large-scale unconditional cash transfers on child survival. One-time transfers of USD 1000 were provided to over 10,500 poor households across 653 randomized villages in Kenya. We collected census data on over 100,000 births, including on mortality and cause of death, and detailed data on household health behaviors. Unconditional cash transfers (accounting for spillovers) lead to 48% fewer infant deaths before age one and 45% fewer child deaths before age five. Detailed data on cause of death, transfer timing relative to birth, and the location of health facilities indicate that unconditional cash transfers and access to delivery care are complements in generating mortality reductions: the largest gains are estimated in neonatal and maternal causes of death largely preventable by appropriate obstetric care and among households living close to physician-staffed facilities and those who receive the transfer around the time of birth, and treatment leads to a large increase in hospital deliveries (by 45%). The infant and child mortality declines are concentrated among poorer households with below median assets or predicted consumption. The transfers also result in a substantial decline of 51% in female labor supply in the three months before and the three months after a birth, and improved child nutrition. Infant and child mortality largely revert to pre-program levels after cash transfers end. Despite not being the main aim of the original program, we show that unconditional cash transfers in this setting may be a cost-effective way to reduce infant and child deaths.

JEL codes: I15, O1, O15

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1 Introduction

The gradient between health and socioeconomic status — across societies and across individuals within societies — is one of the most widely documented correlations in the social sciences ([Preston, 1975](#); [Cutler et al., 2012](#); [Lleras-Muney et al., 2024](#)). Studies often show a concave relationship ([Deaton and Paxson, 2004](#); [Cutler et al., 2012](#)), suggesting that poverty reduction in low-income settings may have particularly important implications for core health outcomes such as mortality. For instance, low and middle income countries (LMICs) still bear a disproportionate burden of child mortality ([Burststein et al., 2019](#)). However, experimental evidence on the poverty-mortality relationship has been limited due to the need for both large-scale data collection (to achieve adequate statistical power) and randomized variation in socioeconomic status.

The rise of unconditional cash transfer (UCT) programs provides an opportunity to study the causal effect of exogenous income gains on mortality. Over 100 LMICs have introduced UCT programs in the past two decades ([Stedman, 2023](#)), and a growing experimental literature has studied the effects of these programs on a wide range of development outcomes ([Bastagli et al., 2016](#); [Crosta et al., 2024](#)). As UCTs become more established as an anti-poverty policy tool, interest has grown in understanding whether the benefits of UCTs accrue to the children of recipients, which could amplify the direct positive effects on recipients that have been documented in the short and medium-run, improving UCTs’ cost effectiveness. Yet despite the proliferation of studies on UCT programs (including many RCTs), it has remained challenging to experimentally examine impacts on a central marker of child well-being: whether they survive infancy and their first five years of life. Randomized evaluations of UCTs to date have typically lacked the large sample size and longitudinal data necessary to precisely estimate impacts on relatively rare but important outcomes such as child mortality.

Previous non-experimental studies estimate an association between UCTs and reduced child mortality ([Richterman et al., 2023](#)), raising the question of whether distributing cash is sufficient on its own to reduce child mortality or if the observed correlation is being driven by other contemporaneous health investments ([Blattman and Niehaus, 2014](#); [Evans and Kosec, 2016](#); [Stedman, 2023](#)). To illustrate, higher levels of income may not lead to major changes in birth outcomes if advanced delivery services and trained medical staff are not available to assist should complications arise. Are complementary health behavior changes or investments in health infrastructure also necessary to improve child survival in low-income settings? Progress in understanding the causal relationship between UCT and child survival, and its underlying mechanisms, is important for policymakers as they allocate scarce resources to improve global health outcomes.

This article exploits large-scale experimental variation from an unconditional cash transfer program in rural Kenya implemented between 2015 to 2017, combined with a new census of over 100,000 births in study villages over more than a decade, to causally identify the effect of UCTs on infant and child mortality. The one-time cash transfer was approximately 75% of average annual expenditure among eligible households, which represented roughly the poorest third of households in the study region, a rural county chosen by the implementing NGO for its high poverty levels. As such, it represented a substantial income shock for both recipients and a stimulus to the broader local economy, resulting in notable living standard improvements for recipient households and others living nearby (Egger et al., 2022). The census of births undertaken by the research team was designed to feature an adequate sample size to more precisely estimate the impact of this large economic shock on child survival.

Specifically, the census data includes detailed birth histories from 107,261 women in the study area covering the period between 2011 to 2023 (both before and after transfers were distributed), as well as information on their children’s mortality and survival. To better understand the underlying mechanisms behind changes in mortality, we collected verbal autopsies (VAs) for child deaths using the World Health Organization’s 2022 methodology and assigned causes of death using the Institute for Health Metrics and Evaluation’s machine-learning classification algorithm (World Health Organization, 2022a; Institute for Health Metrics and Evaluation, 2025).¹ The primary outcomes and experimental design were pre-specified and leverage (as in the previous work of the research team (Egger et al., 2022)) both randomly-assigned treatment status and spatial variation in treatment intensity to study local spillover effects. We further collected extensive data on health facility access, pairing administrative data on the locations of Kenyan hospitals and clinics with new assessments of travel times collected by sending enumerators equipped with speedometers to travel local roads and trails. Additionally, we bring in multiple rounds of household survey data, covering a representative sample of over 10,000 households (both those eligible and ineligible for the cash transfers), in order to further investigate mechanisms. In particular, the most recent survey round (2024-25) collected detailed information on antenatal, delivery, and postnatal healthcare utilization for births during the 2011-2023 period.

The central empirical finding of this study is that the cash transfer treatment substantially reduced infant and child mortality. Results from the study’s primary specification, which accounts for spillover effects within- and across-villages, indicate that infant mortality fell by

¹In settings such as Kenya in which physical autopsies are rare and vital records incomplete, VA is considered the state-of-the-art method for determining causes of death at scale (Gacheri et al., 2014; Serina et al., 2015; Amek et al., 2014, 2018), and was validated in the study area by the Kenya Medical Research Institute (KEMRI), the country’s flagship health research institution. KEMRI staff also trained this project’s field staff in performing VAs.

over 19 deaths per thousand births among recipient households with a pregnancy during the transfer disbursal period. This represents a 48% decline in infant mortality relative to the mean among recipient household births in control villages, and is statistically significant (p -value < 0.01). We find similar results for under-five mortality (a reduction of 45%), as well as when we estimate a reduced-form OLS specification that builds directly on the two-stage research design, which randomizes treatment at both the village level and subregion level (the econometric models are described in detail below). The reduction in mortality does not persist after the end of the transfer disbursal period.

An established literature in public health confirms that child births and deaths are sufficiently consequential to be recalled by mothers and close relatives long after they occur (Rao et al., 2003; Lyons-Amos and Stones, 2017; Nareeba et al., 2021). As such, the birth census, which relies on family members to recall these vital events, utilizes the same methodology as prominent data sources such as the Demographic and Health Surveys (Romero Prieto et al., 2021). Simple memory issues are unlikely to bias the results as they might introduce approximately classical measurement error attenuating estimates. The large-scale longitudinal nature of the census further enables us to assess the validity and robustness of the estimates in several ways. First, in a balance check, we do not estimate any significant differences in infant or child mortality between treatment and control households in the pre-treatment period (2011-14). Next, we document that the census data display internal consistency across both cross-sectional and inter-temporal dimensions (i.e., mortality in the census fits expected patterns with respect to household wealth, seasonality, and aggregate shocks such as droughts and the COVID-19 pandemic). We also find that a meaningful portion of the reported mortality reductions are driven by local cash transfer spillovers (where misreporting due to experimenter demand effects seems unlikely) rather than the direct effects of receiving cash alone. These patterns appear inconsistent with simple explanations such as experimenter demand or measurement concerns.

Furthermore, the analyses suggest that the estimated reductions in child mortality cannot be explained by the changing selection of women into pregnancy. While we observe a modest transient increase in overall birth rates of roughly 10%, several approaches designed to test for changes in the composition of births — made possible by the numerous waves of detailed household surveys previously conducted in the study region — indicate that the characteristics of mothers did not substantially change in cash transfer treatment areas, consistent with previous work on fertility responses to income shocks in developing countries (Chatterjee and Vogl, 2018; Carneiro et al., 2021).

What mechanisms could drive these large reductions? Several analyses indicate that access to antenatal and delivery services may have played an important role. First, when

estimating a dynamic specification that takes into account transfer timing relative to the timing of pregnancy, we find that gains in child survival are concentrated among women who received cash in the month they gave birth or shortly beforehand. Second, analyzing mortality reductions by cause of death (as classified by verbal autopsies), we find declines across most major cause categories but show that the largest share of the overall effect is concentrated in birth complications and neonatal deaths (with mortality in the corresponding cause category falling by 75%). Third, in a representative survey of censused households, we find that transfers are associated with a 45% increase in the rate of hospital deliveries; in this setting, hospitals are typically staffed by physicians and provide more extensive services than local clinics but are far more expensive ([Institute for Health Metrics and Evaluation, 2014](#)). Fourth, in an analysis not pre-specified in the pre-analysis plan, the census data suggests that child mortality reductions were significantly larger in villages located close to physician-staffed health facilities, as assessed using travel time estimates constructed from GPS measurements on local roads. In the preferred specification, which accounts for the full spatial dimension of spillovers and includes LASSO selected covariates, infant mortality falls by an additional 29 deaths per thousand in villages with a below median travel time to a doctor. Taken together, these patterns suggest that cash transfers can complement rather than substitute for investments in rural health infrastructure in this low-income setting.

A related issue is the extent to which cash transfers’ child mortality impacts differ for households with different living standards ([Deaton and Paxson, 2004](#); [Cutler et al., 2012](#)). Along multiple dimensions of poverty – including assets, and predicted consumption and income – child mortality gains in rural Kenya are concentrated among poorer households, and many of these differences between poorer and richer households statistically significant. While recent work in the same Kenyan study setting finds that the poorest households may experience fewer sustained living standards gains from cash than the better-off ([Haushofer et al., 2025](#)), these unconditional cash transfers mainly saved child lives in poorer households.

Another behavioral mechanism examined is the role of maternal labor supply. Kenya has one of the highest female labor force participation rates in the world, with women often performing strenuous physical tasks even during the late stages of pregnancy ([Izugbara and Ngilangwa, 2010](#); [Riang’a et al., 2018](#); [Scorgie et al., 2023](#); [International Labour Organization, 2025](#)). While previous work by the research team did not find general changes in labor supply among transfer recipients overall ([Egger et al., 2022](#)), the effects of the UCTs on work hours may differ for women who are in the late stages of pregnancy or the early months following childbirth. For these women, the transfers arrive at a period when the marginal utility of rest and additional time to invest in child health may be particularly high.² We find a 21

²Time is a critical input in the production of child survival ([Miller and Urdinola, 2010](#)), which is consistent

hour per week reduction (p -value < 0.05) in female labor supply among recipient households with a woman either in the third trimester of pregnancy or the first three months following birth. This sharp decline in labor supply, which represents a 51% decrease compared to the control mean – and is neither observed among men nor among women outside these months – suggests that the cash transfers may have enabled women at a critical period in pregnancy to reallocate work hours to rest or other activities conducive to better child outcomes.

This study makes several contributions. Most directly, it documents the effect of UCTs on one of the most basic indicators of human development, whether children survive their first one to five years of life. Experimental analysis of the impact of UCTs on child mortality has been complicated by the need to collect detailed, long-term data on child survival from a large group of program beneficiaries. This study’s expansive data collection efforts, which entailed visiting 653 villages multiple times to record information on over 100,000 births — combined with an underlying UCT randomized experiment — allow the present study to begin to fill this gap. While previous studies with different research designs and transfer magnitudes have shown at best limited long-run health effects of cash transfers on adult recipients ([Haushofer and Shapiro, 2018](#); [Baird et al., 2019](#); [Blattman et al., 2020](#)), we show that a generous UCT can generate large inter-generational effects by lowering the mortality rates of children.

Prior work has also shown that conditional cash transfers (CCTs) can be effective at lowering child mortality ([Barham, 2011](#)). But because those programs condition cash on behavior changes such as health checks that may directly affect mortality, the role of wealth effects versus behavior change to comply with the conditions for aid is unclear.³ This paper shows that the income effects of cash transfers are important and suggests that cash transfers can be effective without costly monitoring. The substantial child mortality reductions found in this study, coupled with the concentration of effects among mothers receiving cash near the month of birth, suggest that targeting assistance to such mothers may be cost-effective (echoing the anthropometric gains for children documented in [Baird et al., 2019](#)). In fact, a back-of-the-envelope calculation of the cost per child death averted suggests that the UCTs in this study, if targeted to women in the third trimester of pregnancy, are comparably cost effective to a number of WHO-recommended maternal and child health interventions even without taking into account other possible benefits of UCTs (such as consumption gains and multiplier effects on local economic activity).

with the literature on parental leave in industrialized and developing countries ([Ruhm, 2000](#); [Tanaka, 2005](#); [Rossin, 2011](#); [Nandi et al., 2016](#); [Bartel et al., 2023](#)). Previous non-experimental studies suggest that receipt of social assistance is associated with mothers working less ([Amarante et al., 2016](#); [Guldi et al., 2024](#)).

³This includes work on incentivizing deliveries in health facilities or higher-quality facilities (e.g. [Cohen et al., 2017](#); [Grépin et al., 2019](#)).

Second, this paper aims to add to the literature on the relationship between economic status and health (Cutler et al., 2012). The program studied, which provided recipients with USD 1000 (USD 1871 PPP), was multiples larger in magnitude than the average level of assets at baseline held by beneficiaries. As such, it represented a large wealth shock in a high-poverty setting where the marginal return to wealth for health outcomes may be expected to be very large, in contrast to well-resourced settings where even sizable economic shocks, such as winning the lottery, may have smaller impacts on health (Apouey and Clark, 2015; Cesarini et al., 2016; Kim and Koh, 2021; Wyndham-Douds and Cowan, 2024). We provide novel experimental evidence that an income shock in low income settings such as ours can sharply reduce mortality for some of the most vulnerable members of the household, infants and children. Moreover, the effects are concentrated among the poorer households in this region. We document that the socioeconomic gradient in child mortality is causal in nature and not just correlational in this setting. Yet we also document that these large effects dissipate when the cash transfers end, suggesting that while large temporary income shocks can result in considerable health improvements and more surviving children, other interventions may be needed for sustained reductions in infant and child mortality.⁴

Third, these results speak to the complementary roles cash and access to medical services may play in producing population health improvements. Much of the decline in child mortality documented in this study is driven by causes of death which professional obstetric care can help prevent, transfers result in a substantial rise in hospital delivery care, and the improvements in child survival are largely concentrated among households residing near a physician. Taken together, these patterns are consistent with prior work indicating that quality health care has important positive causal impacts in low-income rural settings (Okeke, 2023). Cash transfers may not be enough by themselves to fully realize child health targets in the absence of these other health investments.

2 Intervention and Experimental Design

2.1 Context and Intervention

As part of the Kenya General Equilibrium Study (KGES), the NGO GiveDirectly (GD) provided unconditional cash transfers (UCTs) to poor households in rural Kenya, targeting households living in homes with thatched roofs as a simple proxy means-test for poverty. In

⁴Longer-term consumption and spending impacts of cash transfers – while remaining positive for several years – are much smaller than those observed in the first year after transfers go out (in line with large marginal propensities to consume in this population). Spillover effects on non-recipients similarly are smaller than direct effects in the short run. This may explain the dissipating child mortality effects (and a potential lack of power to detect them). We return to this point in our discussion below.

treatment villages, GD enrolled all households meeting its thatched-roof eligibility criteria (“eligible” households); slightly more than one third of all households were eligible, and existing data indicate that they are indeed poorer on average than other local households (called “ineligible households”). For instance, assets per capita among eligible households at baseline were 64% lower than ineligible households. No households in control villages received transfers.

Eligible households enrolled in GiveDirectly’s program received a one-time series of three transfers totaling USD 1,000 (1,871 2015 USD PPP) via the mobile money system M-PESA, where the three tranches were disbursed over the course of 8 months. This is a one-time program and it was explained that no additional financial assistance would be provided to these households after their final transfer (and in fact none was provided). In total, the transfers constituted a shock of about 75% of household expenditure for eligible households, and of 15% of annual GDP in treated villages at the time that they were distributed.⁵ Egger et al. (2022) show that the marginal propensity to consume from the cash transfer is very high (with estimates in the range of 0.8 to 0.9 in the first one to two years after receipt), perhaps not surprisingly for a low-income rural East African population.

Villages were phased into treatment starting in late 2014 and throughout 2015, and the bulk of the payments were sent out during 2015 and 2016. The years 2015, 2016, and 2017 are thus the years most relevant for understanding the impacts of UCTs on child mortality as many women who received cash transfers while pregnant during 2016 gave birth in 2017.

Background information on the Kenyan health care setting is also useful for understanding the analysis below. Health facilities in Kenya are classified into six levels: (1) community health units and community health workers; (2) primary care provided by dispensaries; (3) primary care provided by health centers; (4) sub-county hospitals (first referral); (5) county hospitals (second referral); and (6) national-level referral hospitals (Miller et al., 2024). Some level 2 facilities can perform deliveries, but they do not have inpatient care services. Level 3 clinics offer basic delivery services, though vary in whether or not they are staffed by physicians (only 15% of such facilities surveyed in this setting had physicians). Level 4 and 5 facilities are hospitals that typically have physicians (over 70% of the time based on survey data) and some specialists; there are no level 6 facilities in the study area.⁶

The Kenyan government has implemented several programs with the goal of reducing user fees for antenatal and delivery care.⁷ However, implementation challenges and a lack of clar-

⁵More details on program design and implementation are described in Egger et al. (2022).

⁶A survey of health facilities targeted at level 3 locations in Siaya and bordering areas of surrounding counties conducted in the KGES project documented 94 level 3 facilities, 49 level 4 facilities, and just 13 level 5 facilities (and 94 level 1 and 2 facilities were also reached).

⁷In 2013, the Kenyan government introduced program to eliminate user fees, which became the “Linda

ity around benefits means that in practice many women still need to pay substantial amounts out-of-pocket for these services (Orangi et al., 2021). In KGES surveys (described below), the total out-of-pocket cost of antenatal, delivery, and postnatal care averaged \$66 (USD 2023 PPP), with 10% of respondents spending over \$156. The average reported delivery cost alone in facilities staffed by a physician stood at over double (\$60) the cost of delivering in facilities without a physician (\$28), highlighting the trade-off faced by households. Additionally, even if some aspects of care are covered by government programs, qualitative work has documented patient concerns around referrals to higher level facilities due to the need to pay for expensive transport back home (Miller et al., 2024). To illustrate the importance of travel costs in this setting (in which few households owned cars or motorcycles), in 23% of control group births respondents themselves reported traveling by foot to a facility for delivery rather than taking any form of transport.

2.2 Experimental Design

Treatment assignment was randomized at two levels, the village level and the sublocation level. Within treatment villages, all households meeting GD’s eligibility requirement received the UCT. The second, sublocation level of randomization provided variation in local treatment intensity. Sublocations, an administrative unit directly above the village including about ten villages on average, were randomly assigned to high or low saturation status: in high-saturation sublocations, two-thirds of villages were treated, while in low-saturation sublocations only one-third of villages were treated. This generated substantial spatial variation in treatment intensity, which is used (as in Egger et al., 2022) to estimate spillover effects. In the analysis, we both directly follow the research design (in terms of village and sublocation assignment) and a spatial instrumental variables (IV) approach that utilizes all variation in local cash transfer exposure, taking advantage of the idiosyncratic variation in local village assignment and the fact that villages could be located near other sublocations with different saturation assignment. Additionally, both treatment and control villages were randomly ordered for the program and for data collection visits, allowing us to assign an “experimental start date” to each village and explore effects related to transfer timing. These approaches are described below in Section 3.2.

Egger et al. (2022) document that the UCTs led to significant increases in living standards for recipient households. Recipient households’ marginal propensity to spend out of the transfer is approximately 0.8-0.9 over the first 1-2 years after the transfer, with the largest increase in spending concentrated over the first months. At endline, an average of 1.5

Mama” program run by the National Health Insurance Fund in 2016. Other work has found that the program did not increase demand for maternal health services (Grépin et al., 2019).

years after the start of the program, they still report a 13% increase in consumption expenditures (including a 9% increase in food expenditures) and 26% increases in asset ownership, associated with substantial increases in food security, and the quality of the home environment (e.g. the quality of roofing materials). The increased spending by recipient households generated local (within 2 km) increases in economic activity for firms, with positive spillover benefits to non-recipient households, who also experience higher expenditure. There is a positive but small increase in inflation in areas that received more cash relative to those that received less. Taken together, [Egger et al. \(2022\)](#) estimate a real transfer multiplier of around 2.5 from the UCTs. These substantial short-term local economic gains from a large cash transfer raise the possibility of child health effects from the program.

3 Data and Estimation

3.1 Data

The primary data for this paper was collected as part of a third endline round (EL3) of data collection for the KGES project. EL3 data collection activities built on the project’s baseline household censuses and surveys (2014-15), endline 1 household surveys (2016-17), and endline 2 household censuses and surveys (2019-22). The infant and child mortality analysis primarily uses EL3 household census data, which included birth histories for adult female household members; we augment the census data with household surveys from a representative sample of eligible and ineligible households to get additional details on birth experiences. We discuss each of these data sources next below. We also make use of data on the location of health facilities and travel times, which we introduce when discussing mechanisms.

3.1.1 Endline 3 household census data

The endline 3 (EL3) household census took place from April to November 2023 in all treatment and control study villages in Siaya, Kenya. The analysis focuses on households that were present at baseline (i.e., the start of the GiveDirectly program) and therefore have a clearly defined program treatment and eligibility status. The household census captures all such households that still resided in the study region in 2023, which encompasses 94% of baseline households. Birth information for the approximately 6% of baseline households that had migrated out of the study area is captured in the representative household survey mentioned above, in which the study team attempted to track all sampled households wherever they had moved across Kenya (and succeeded in locating and surveying slightly over half of them). These observations are then reweighted to maintain baseline sample representativeness.

The pre-analysis plan (PAP) specified two primary outcomes (Egger et al., 2023): infant (under-one) mortality and child (under-five) mortality. For each measure, we focus on children born at least one or five years before the time of data collection, respectively, in order to have a consistent population for both the numerator (children who are deceased) and the denominator (e.g., children who are deceased plus children who have survived until the age of one or five). We thus examine effects on under-five mortality among children born through the end of 2017 (i.e., in the pre-program period and in the treatment period of 2015-17), while for infant mortality we can estimate impacts among children born up to 2021, which also includes the post-cash transfer period of 2018-2021.

In addition to collecting information on fertility and mortality, we also seek to assign causes of death for child mortality instances via verbal autopsies (VA) using the standard World Health Organization 2022 VA Questionnaire (World Health Organization, 2022a). The PAP specified that we would examine whether there were cash transfer treatment effects on the main causes of death (Egger et al., 2023). Verbal autopsy is considered the state-of-the-art survey-based method for determining causes of death based on self-reported information (as physical autopsies are rarely performed in the study region or many other low- and middle-income regions and vital records are incomplete), with previous literature having validated the accuracy of VA methodology and the associated machine-learning classification algorithm, including in the Kenyan study region (Gacheri et al., 2014; Serina et al., 2015; Amek et al., 2014, 2018). Though the literature also notes the limitations of VA relative to the administrative data available in wealthy nations, it is the most accepted approach to determining the distribution of cause of death in populations such as the present one.⁸

Overall follow-up rates were high in the EL3 household census: over 92% of households in the 653 study villages completed the birth history and child mortality modules. The response rate in the census activity was also nearly identical and not statistically different across the treatment and control villages. In total, across the birth census and representative surveys designed to collect information on migrant households, we collected information on 101,405 births. When restricting attention to births in eligible households contemporaneous with the disbursal of cash transfers (2015-17), the population under study is 6,347 births.

Descriptive statistics for the full census of births as well as subsets of interest, such as births to transfer-eligible households, are presented in Appendix Table A.1. Across all births in the census, the infant (under-one) mortality rate was 33.7 deaths per thousand births and the child (under-five) mortality rate was 46.7 deaths per thousand births. These recorded

⁸The VA module includes a categorization for stillbirths in approximately 15% of cases. As noted below, the results are also robust to including only live births, which is sometimes the sample considered in analyses of infant and child mortality.

infant and child mortality rates are thus quite similar to those estimated in Kenya by the United Nations Inter-Agency Group for Child Mortality Estimation (2025). Note that births to households present at baseline – which comprise approximately 77% of total births and are as noted above the primary focus of the analysis – exhibit virtually identical rates of infant and child mortality (as well as a similar average maternal age at birth) to the census data as a whole.

3.1.2 Endline 3 household survey data

Household surveys were conducted at baseline, endline 1, endline 2 and endline 3 with a representative sample of censused households. Specifically, at baseline the KGES team sought to survey eight (8) eligible households and four (4) ineligible households per village; in cases where initially-targeted households were not available at the time of survey, “replacement” households were surveyed instead. Endline 1 sought to survey all households initially selected for surveys, as well as replacement households (see Egger et al., 2022, for details). Endlines 2 and 3 have maintained this sampling frame of households present at baseline, while additionally adding in newly-identified households, as described in Egger et al. (2024). Since the focus of this analysis is on households present at baseline, we exclude those who later moved into the area from the analysis.

Household survey data from the KGES project provides two key benefits to the analysis presented here. First, household surveys tracked individuals that moved outside of the study area, allowing the analysis to account for child births and survival among individuals present in the study area at the time of transfers, but that moved away (and thus were not captured in the census). This accounts for 6% of households from the detailed survey sample. In total, enumerators were able to survey 148 moved-away eligible households that had had at least one birth during this time period. For these households, we conducted the same birth history as in the census (in addition to the other survey data described below), and we include these observations with sampling weights to reflect their proportion of the censused population in the child mortality analysis.

Second, household surveys gathered more detailed data than the census, providing a means to study potential mechanisms. In particular, endline 1 (2016-17) provides short-term data on household living standards, assets, labor supply, nutrition and food security. We highlight some results previously reported in Egger et al. (2022) and conduct further analyses using these data. Additionally, the EL3 household survey data provides detailed information on health behaviors and access that could serve as channels (including, for example, hospital delivery, and antenatal and postnatal care). We pre-specified the specific outcomes we would focus on as mechanisms in a pre-analysis plan (Egger et al., 2024). The EL3 household survey tracking rate was over 90% and was balanced across the treatment and control groups

(Appendix Table A.2).

3.2 Estimation Framework

3.2.1 Primary Econometric Specifications

We first estimate the following reduced-form specification, focusing attention on births in households meeting the eligibility criteria for transfer receipt that were present at the time of the cash transfers:

$$y_{imhvs} = \alpha_1 Treat_v + \alpha_2 HighSat_s + \lambda_{t(i)} + \rho_{g(i)} + \lambda_{t(i)} \times \rho_{g(i)} + A_m + \delta M_i + \epsilon_{imhvs}, \quad (1)$$

where y_{imhvs} is an outcome of interest (i.e., infant mortality) for a birth i in household h , located at baseline in village v and sublocation s . The variable $Treat_v$ is an indicator for residing in a treatment village at baseline and $HighSat_s$ is an indicator for being in a high-saturation sublocation. The specification includes child year of birth fixed effects, denoted by $\lambda_{t(i)}$, and child gender fixed effects $\rho_{g(i)}$. We control for maternal age through the inclusion of indicator variables represented by A_m , where m denotes one of five age groups (under 20, 20-25, 25-30, 30-35, or above 35).⁹ Standard errors are clustered at the sublocation level. While most of the data is derived from the birth census of the study region, as noted above we also survey a representative sample of households that migrated from the region and re-weight those observations by inverse sampling probabilities.

Here α_1 captures the effect of the transfers on eligible households in treatment villages (relative to control villages) from two sources: the direct effect of treatment and the effects of any within-village spillovers. The coefficient α_2 estimates cross-village spillover effects based on the research design of high versus low saturation sublocations. This estimation of cross-village spillovers is relatively coarse as it does not utilize all experimental variation, implicitly assuming that all spillovers are contained within villages and sublocations. The sum of α_1 and α_2 denotes the total effect of the transfers from all three sources (direct effects, within-village and cross-village spillovers). This linear combination of coefficients captures the effect of the transfers on eligible households in treatment villages in high-intensity sublocations relative to eligible households in control villages in low-intensity sublocations.¹⁰

Equation (1) is a straightforward and intuitive benchmark yet it does not capture the

⁹ M_i represents a vector of indicators denoting a missing value for a given covariate, which allows us to retain observations in order to maximize statistical power. Where a covariate is missing, we set the value equal to the covariate's mean.

¹⁰Among other main analyses, we pre-specified a version of Equation (1) that clustered standard errors at the village level and focused on α_1 while considering α_2 as non-primary. We adjust standard errors to cluster at the sublocation level as our analytic focus expanded to include α_2 .

full spatial dimension of spillovers. One-third of villages in low-saturation sublocations still received transfers, and there is additional variation caused by the idiosyncratic placement of treatment and control villages as well as sublocation boundaries. In the following pre-specified analysis, which is based on [Egger et al. \(2022\)](#) and builds on [Miguel and Kremer \(2004\)](#), we make use of the full spatial variation induced by the experimental design. These regressions allow us to compare areas that, due to the randomization, received more cash relative to areas that received less. While we focus on recipients, using these models we can estimate impacts on non-recipients as well. We utilize the following regression specification:

$$y_{imhvs} = \beta_1 Amt_v + \sum_{r=2}^{\bar{R}} \beta_r Amt_{v,r}^{\neg v} + \gamma_1 ShareElig_v + \sum_{r=2}^{\bar{R}} \gamma_r ShareElig_{v,r}^{\neg v} \quad (2)$$

$$+ \lambda_{t(i)} + \rho_{g(i)} + \lambda_{t(i)} \times \rho_{g(i)} + A_m + \delta M_i + \epsilon_{imhvs}$$

The key terms are β_1 , which captures the effect within treatment villages from both direct receipt of the transfers and within-village spillovers (where cash transferred to the village is captured in Amt_v), and the β_r terms, which capture the effects of cash transfers in other villages (not v) at different bands of radius r ($Amt_{v,r}^{\neg v}$) away from village v . $ShareElig$ denotes the share of baseline households eligible for a transfer in an area, which we control for to facilitate more powerful instrumental variable sets as detailed below. The other terms are as in Equation (1). As in [Egger et al. \(2022\)](#), the maximum radius ($\bar{R}$) is found by estimating models with varying radii, then selecting the model that minimizes the Schwarz Bayesian Information Criterion (BIC).¹¹ The Schwarz BIC algorithm indicates, as in the [Egger et al. \(2022\)](#) study of economic impacts, that infant and child mortality effects are locally concentrated within 2 km of cash transfer receipt in nearly all cases.

The amount of cash an area received is a function of the share of households that were eligible at baseline ($ShareElig$), which is endogenous, and the share of eligible households that were treated. We thus instrument for transfer amounts using the share of eligible households treated in an area and the share of eligible households treated interacted with the share of eligible households, which is a valid instrument since the estimates control for the proportion of households that were eligible in an area at baseline.¹² To account for

¹¹For computational reasons, once the BIC increases at a radius, we stop searching and select the minimizing value over the earlier radii searched.

¹²The instrument set in [Egger et al. \(2022\)](#) only uses the share of eligible households treated as an instrument. By logic similar to [Abadie et al. \(2023\)](#), this instrument vector is efficient under homogeneous treatment effects because it captures the true first stage. The original pre-analysis plan specified the instruments from [Egger et al. \(2022\)](#), but we noted that this was an active research area in applied econometrics and as such we might consider estimates based on recent advances. An amendment filed before the household survey data was analyzed (but after data collection had begun) documented our intention to switch to the revised instruments. Standard errors are about 60% larger with the original [Egger et al. \(2022\)](#) approach.

spatial correlation, we calculate spatial heteroskedasticity- and autocorrelation-consistent (HAC) standard errors using a positive definite kernel up to 10 km (Conley, 2008).¹³

The analysis focuses on the “average total effect” of the cash transfers on births in recipient households in high-saturation sublocations, which is defined as:

$$\widehat{\Delta y^r} = \hat{\beta}_1 \cdot (\overline{Amt_v} | i \text{ born in recipient household in high-saturation sublocation}) + \sum_{r=2}^{\bar{R}} \hat{\beta}_r \cdot (\overline{Amt_{v,r}^v} | i \text{ born in recipient household in high-saturation sublocation}) \quad (3)$$

If there are no effects of cash outside of the radius $\bar{R}$ (e.g., due to ambient effects in the study area), this equation estimates the average effect of the cash transfers on recipient households in high-saturation sublocations compared to a counterfactual in which no cash was distributed (to the household or their neighbors). Note that, to the extent that ambient spillover effects have the same sign as the direct effect over all radii, even beyond those chosen for inclusion in Equation (2), then (following the argument in Baird et al., 2016) this quantity is a lower bound on the true effect of a program that would have treated all villages, rather than the two thirds of villages treated in high saturation sublocations here.

As noted in Section 3.1, the dataset contains complete child (under-5) mortality data for all children born through the end of 2017, as all children born in 2017 or earlier were at least five years of age at the time the birth census commenced. We thus estimate effects on child mortality for children born during the cash transfer disbursement period (henceforth the “UCT period”) of 2015-17, as well as in the pre-UCT period of 2011-14 as a form of placebo check. For infant mortality, the other pre-specified primary outcome, we are additionally able to estimate impacts during the years 2018-21, which comprise the period after transfers were sent (henceforth the “post-UCT period”). In addition to examining treatment effects across these three periods, we also examine impacts by birth year in some analyses.

3.2.2 Secondary Econometric Specification

Date-of-birth data from the household census enables us to examine whether the effect of cash varies depending on the timing of receipt. We first restrict the sample to births where the transfer period — the experimental start date plus 8 months, since the three transfers were distributed over that time frame — intersected with an age group G . The relevant age groups G are defined as: (i) 9 months to three years before birth (treatment pre-pregnancy), (ii) within 9 months before birth up to the month before birth (treatment in-utero), (iii) the

¹³We use the kernel $K_{ij} = 1(d_{ij} < 10) \cdot \left(1 - \frac{d_{ij}}{10}\right)^2$ where d_{ij} is the Euclidean distance in kilometers between i and j . We use this kernel, rather than a uniform kernel as in Egger et al. (2022), to ensure that standard errors are not complex-valued.

birth month and up to 28 days after birth (treatment at birth and as a neonate), (iv) 28 days after birth to one year after birth (treatment as an infant), and (v) one to three years after birth (treatment as a young child). For instance, if G is in-utero, then observation i is included in estimates of the effects of receiving cash in-utero if and only if the intersection of the transfer period and individual i 's birth date minus 9 months is not null.

This allows for the estimation of treatment effects among eligible households in treatment and control villages in group G as follows (restricting the sample to households in group G):

$$y_{imhvs} = \alpha_{1,G}Treat_v + \alpha_{2,G}HighSat_s + \lambda_{t(i)} + \rho_{g(i)} + \lambda_{t(i)} \times \rho_{g(i)} + A_m + \delta M_i + \epsilon_{imhvs} \quad (4)$$

We focus on the treatment effect in high-saturation sublocations for each group, namely, $\alpha_{1,G} + \alpha_{2,G}$. In additional analysis, we consider the spatial IV estimator of dynamic effects by similarly restricting the observations into birth timing groups, then estimating equations (2) and (3) separately for each group; both approaches produce similar results.

Intuitively, this specification examines whether there are different effects on children that were exposed to cash at different times either before or after birth. Any effects of pre-birth exposure are more likely to work through effects on mothers (i.e., in terms of their nutrition, health, stress, and medical care received). This perspective helps to motivate the mechanisms examined below through which cash could affect child mortality, and the non-uniform age groups are motivated by key stages of pregnancy and early life.

Because the transfers were distributed over 8 months, many births fall into multiple G groups and so a single birth observation can be included in different estimates. Therefore estimates of the effects of cash when received in-utero may also partially reflect the effect of receiving cash in the birth month, and similarly for other birth timing groups. Estimates should therefore be interpreted as the joint effect of cash exposure in age group G and adjacent pregnancy stages since the design of the experiment does not identify “unbundled” coefficients without additional assumptions.¹⁴

¹⁴The original PAP specified a version of equation (4) that estimated the effect of cash received in each group on mortality jointly by calculating how much cash was transferred in each group G and then estimating one regression across the sample. While in theory this could separately identify the effects of cash at different stages, a recent literature in econometrics such as [Callaway and Sant’Anna \(2020\)](#) shows that such methods can generate biased estimates. We have opted to implement the more robust approach described here and interpret results accordingly.

4 Main Empirical Results

4.1 Graphical Analysis

The results are first presented graphically in Figure 1. Panel A plots simple means of infant mortality by year across 2011-21 for two central groups of interest within the population of eligible households: births in treatment villages located in high-saturation sublocations, and births in control villages located in low-saturation sublocations. The first group comprises censused children born in villages that received the highest average intensity of cash transfers, whereas the second group are those with the lowest average intensity of transfers.

A clear pattern is evident from Panel A: while high-saturation treatment and low-saturation control villages exhibit similar and not statistically distinguishable levels of infant mortality during the pre-period of 2011-14, a marked divergence occurs once cash transfers are distributed. Following the start of transfers, infant mortality in high-saturation treatment villages rapidly falls from 37.5 deaths per thousand births in 2014 to 20.5 deaths per thousand births in 2015, a 45% decline. By contrast, low-saturation control villages continue on their pre-COVID-19 trajectory of gradual improvement over time (absent a severe drought in 2017 which elevated infant mortality). Once the disbursal of transfers ends, however, infant mortality in high-saturation treatment villages swiftly returns to the rates seen in low-intensity villages. Rates for both groups rise in this latter period, and particularly in 2020 and 2021, likely related to the effects of the COVID-19 pandemic.

4.2 Regression Analysis

Turning to the first regression results from the reduced-form specification, Panel B of Figure 1 displays estimates of the effect of living in a high-saturation treatment village (i.e., $\alpha_1 + \alpha_2$ from Equation (1)) on infant mortality by year of birth, where the whiskers represent 95% confidence intervals on each yearly estimate. There are large impacts on infant survival during the period in which UCTs were disbursed. The coefficient estimates range from 15 to 24 infant deaths per thousand births during the three years in the UCT period. Pooling the three years of the UCT period, the effect of transfers is statistically significant at the one percent level (p -value < 0.01), as shown in the regression analysis below. Though we lack the statistical power to detect differences in coefficients across years within the transfer disbursal period, the year with the largest treatment coefficient (2017) is also the year in which a severe drought affected the region, which is consistent with the view that UCTs reduce infant mortality most in settings with the greatest economic adversity, a point we return to below. We do not detect meaningful treatment effects in any of the pre-period years, nor do we find significant persistent impacts in the post-UCT period.

Table 1 reports this study’s main results. Column 1 displays estimates of the reduced-form effect of UCTs on infant (under-one) mortality for eligible households during the period in which transfers were disbursed. Infant mortality declines by 17.9 deaths per thousand births in treatment villages located within high-intensity sublocations, a result statistically significant at the one percent level. This coefficient estimate represents a 44% decline in mortality relative to the low-saturation control mean of 40.2 deaths per thousand births.¹⁵ The total effect appears to be driven both by the direct effect of cash transfer receipt and the effect of cross-village spillovers (α_2); Egger et al. (2022) had shown large effects both of direct cash transfer receipt and of local spillovers in terms of local economic outcomes and living standards. Similarly, when examining child (under-five) mortality in Column 2, there is a reduction of 17.6 deaths per thousand births during the UCT period, a 31% decline relative to the control mean, and once again both the direct effect and spillover estimates are large and negative. This pattern of findings also indicates that most of the reduction in under-5 deaths occurs in the first year of life.

Column 3 of Table 1 presents estimates on infant mortality from Equation (2), the instrumental variables specification which more fully captures the spatial dimension of spillover effects. From this specification, we find an average total effect (including direct, within-village and across-village spillovers) on infant mortality in recipient households of -19.5 deaths per 1000 births across 2015-17. This represents a 48% decline in infant mortality relative to the control mean and is statistically significant at the one percent level. We estimate significant effects both from transfers within one’s own village (β_1) and from other nearby villages (β_2) at the 10% level. Column 4 reports spatial IV estimates of transfer impacts on child mortality, and estimates an average reduction of 25.6 child deaths per 1000 births during the UCT period, a 45% decline relative to control significant at the one percent level, and once again indicating that most of the reduction occurs in the first year of life. These last two effects remain statistically significant (at $p < 0.05$) when accounting for multiple hypothesis testing across the reduced-form and spatial IV results (using the Romano and Wolf (2005) step-down approach as pre-specified). In all, both specifications yield the same striking finding: cash transfers lead to declines of between 31% to 48% in infant and child mortality.

Figure 2 presents the main estimates for infant mortality again, this time aggregating across the main time periods, including the cash transfer period (2015-17) as well as the pre-period (2011-14) and the post-UCT period (2018-21). As noted above in Figure 1, we reassuringly do not find any significant differences in the pre-period, indicating balance (as

¹⁵Distinguishing between stillbirth and neonatal deaths soon after live birth can be challenging outside of medical settings. While we include stillbirths in the main analyses as child deaths, we present the main results excluding stillbirths (as determined by a verbal autopsy classification algorithm) in Appendix Table A.4 and find similar results in terms of magnitude and statistical significance.

shown in the 95% confidence interval whiskers in the figure, which capture the difference across groups). Furthermore, once cash transfers cease, so do the impacts on infant mortality. In the post-UCT period, we do not estimate significant differences in infant survival between treatment and control villages, though the standard errors cannot rule out modest impacts in either direction.¹⁶ We furthermore do not find significant changes over time in infant mortality among ineligible households who did not receive cash transfers. There are also no meaningful treatment effects on the ineligible households, who as noted above were considerably richer at baseline in addition to having far lower infant and child mortality rates (see Appendix Table A.5).¹⁷

The mortality effects estimated during the UCT period are broad-based across various pre-specified child and mother characteristics. As Appendix Figure A.1 reports, the impacts on mortality are nearly identical by child gender and similar by birth order. We find some suggestive evidence of heterogeneity by maternal age, with coefficient estimates about 75% larger in magnitude for older (aged 25 and over) than younger mothers, though we cannot precisely distinguish differences between the two groups, which both exhibit statistically and economically significant declines. As older mothers are more prone to birth complications, this suggestive pattern is also consistent with an important role being played by improved access to delivery services, which relates to the cause of death findings below.

4.3 Effects by Timing of Transfer Receipt

Precise date-of-birth data from the census, paired with administrative records of transfer disbursement from the GiveDirectly program, enable us to study differences in the effect of cash by timing of receipt. We find by far the largest treatment effects among children whose household was receiving cash in the month of their birth. Figure 3 illustrates treatment effects in high-saturation sublocations for the five birth timing groups noted above: (i) children whose household received cash 9 months to three years before their birth (“pre-pregnancy”), (ii) 9 months before birth to birth (“in-utero”), (iii) birth and up to 28 days after birth (“birth month”), (iv) 28 days after birth to one year after birth (“infant ≥ 1

¹⁶We do not estimate effects on under-five mortality for 2018-21 as children born after 2017 may not have reached their fifth birthday at the time of the census activity, as noted above.

¹⁷Results from Egger et al. (2022) and unpublished longer-term analyses show that living standards impacts of cash transfers persist in the medium-term, with consumption increases of 10-13% from 3 to 7 years post-treatment. However, recipients spend a large share of transfers in the first few months, and medium-term consumption gains are thus substantially smaller than in the immediate months after transfers (by a factor of roughly 5). If one were to assume a constant log-log relationship between spending and infant mortality, we would not be statistically powered to detect the mortality impacts of these far smaller changes in spending among cash recipients. The positive but smaller spillover consumption gains experienced by ineligible households would similarly imply smaller infant mortality gains that our design would be underpowered to detect.

month”), and (v) one to three years after birth (“child ≥ 1 year”).¹⁸

Infant mortality declines by 39.7 deaths per thousand births among children receiving cash in their birth month, a finding statistically significant at the one percent level which suggests that mortality for these infants dropped to the low levels seen in industrialized nations. We observe meaningful but more modest mortality declines for infants whose households received cash in-utero (-13.1 deaths per thousand births) and a somewhat smaller effect for children whose family received cash before their pregnancy had occurred (echoing the lack of persistent effects in the post-cash transfer period noted above and consistent with the documented high marginal propensity to consume from the transfer) or after their neonatal period. Spatial IV specifications (Appendix Figure A.2) yield similar results, but with somewhat stronger evidence of effects for children exposed to cash in-utero or as infants.

This stark heterogeneity by transfer timing indicates that factors present at the time of pregnancy and delivery and in the neonatal period may interact with cash receipt (in a setting with a high MPC) to produce particularly large declines in child mortality.

4.4 Effects by Cause of Death: Verbal Autopsies

KGES field staff conducted verbal autopsies (VAs) using the World Health Organization’s 2022 questionnaire to ascertain the likely cause of death for each of the 4,720 recorded under-five deaths in the household birth census. The field team was able to conduct VAs for 91% of child deaths across the period of study, and of these 82% were collected from a family member present at the time of the death, and thus is likely to be particularly knowledgeable about the circumstances. Following VA collection, a likely cause of death was assigned using the Institute for Health Metrics and Evaluation’s SmartVA algorithm, which utilizes the Tariff 2.0 method for machine-learning classification of VAs and which was designed and validated with the Population Health Metrics Research Consortium Gold Standard VA database (Institute for Health Metrics and Evaluation, 2025).¹⁹

We focus on five pre-specified cause of death (COD) groups. The broad COD distribution seen in this study’s VA data is reassuringly similar to other studies in western Kenya (Amek et al., 2014). The leading COD group in control villages is maternal and neonatal causes, which encompasses individual causes such as death from preterm delivery, birth asphyxia, and congenital malformation. Maternal and neonatal causes comprise 37% of deaths with non-missing causes in low-saturation control villages. The second-largest COD group, encompassing 36% of deaths with non-missing causes, is communicable and nutritional diseases

¹⁸Figure 3 presents results using the reduced-form specification presented in Equation (4); we present similar results from the spatial IV specification in Appendix Figure A.2.

¹⁹The likely cause of death is standardized, as defined by the International Classification of Diseases, tenth edition (ICD-10).

such as malaria and malnutrition. Other COD groups include respiratory diseases such as pneumonia (13%), non-communicable diseases (11%), and injuries (2%). A sixth category encompasses completed VAs for which SmartVA was unable to determine a likely cause due to missing or inconsistent answers (18% of completed VAs).

Figure 4 illustrates treatment effects on infant mortality by cause of death estimated using Equation (2) (here grouping together communicable/nutritional causes and respiratory causes, which in both cases are mainly due to infectious disease). The largest reduction in mortality by far is in deaths from maternal and neonatal causes: we estimate a drop of 11.4 deaths in this category per 1000 births, representing a 75% decline relative to the control mean.²⁰ Across other CODs, coefficients are almost always negative (the exception is non-communicable diseases, for which the point estimate is near zero), but mortality reductions across all other CODs combined amount to just half the decline seen within maternal and neonatal causes alone. We do not find evidence that VAs were differentially likely to be undetermined or absent in treatment villages, conditional on a death (Appendix Table A.7). We do find a drop in the overall death rates for undetermined or absent causes in treatment households, accounting for the remainder of the total mortality reduction.

4.5 Heterogeneity by Socioeconomic Status

The hypothesized concave relationship between socioeconomic status and health would imply that socioeconomically deprived households would see the greatest reductions in infant and child mortality due to cash transfers. To test this, we make use of two sources of data: first, baseline values of total household assets and income from the household survey, and second, classifications of surveyed households as “most deprived” in terms of predicted endline 1 outcomes for per capita assets, consumption and income from Haushofer et al. (2025).²¹ More specifically, Haushofer et al. (2025) used detailed baseline household survey data from KGES to predict levels of per capita assets, consumption and income at the first endline (on average 1.5 years after the start of transfers) for eligible households using machine learning ML methods (generalized random forests). The bottom half of households were classified as “most deprived” for each outcome, and Haushofer et al. (2025) contrasts these households with eligible households predicted (again via ML methods) to have the largest cash treatment effects.²² These various measures thus provide complementary information

²⁰This result is consistent with the finding that neonatal mortality, i.e., in the first 30 days of life, declines by 14.6 deaths per 1000 births in the census data, a 63% drop relative to the control mean (Appendix Table A.3). We find similar results for both the VA and neonatal mortality analyses when utilizing Equation (1).

²¹This analysis was not pre-specified. The baseline survey did not collect information about consumption expenditure.

²²As the methodology generates multiple predictions for each household for each outcome, we classify households as most deprived if the share of model runs that classify them as such exceeds the median.

about households' current and expected future living standards.

As both of these sources rely on data from the household survey, we must restrict attention to eligible households in the household survey sample. We first benchmark the infant and child mortality estimates in this sample for reference by re-estimating effects from the infant and child mortality analysis sample (with data from the endline 3 household census plus household surveys for movers outside the study area) but restricting attention to individuals surveyed at baseline. Given the smaller sample size, these results are inherently less powered statistically. Despite this, the infant mortality estimates remain at least marginally statistically significant in the survey sample and somewhat larger in magnitude (reduced form effect of -36.7, p -value < 0.05) as compared to the overall census estimate (-25.3), see Appendix Table A.13, column 1. The proportional reduction versus the control mean is also almost identical to the census data, with the larger treatment effect being matched by a higher control, low-saturation infant mortality rate in this sample (65.8 per 1,000 births).

Figure 5 then presents heterogeneous treatment effects along the dimensions noted above: baseline value of assets and income (from the household survey), and predicted per-capita assets, consumption and income at the first endline from Haushofer et al. (2025).²³ In each case, we split the variable at the median, and report estimated coefficients for above-median (richer) households in red, and below-median (poorer) households in blue. The data present a fairly consistent pattern of larger reductions for poorer households: in four of the five cases, the point estimate for poorer households is larger in magnitude than that of the richer households, and in three of the five cases (the assets and consumption measures), the point estimates for poorer households are statistically significant. For the baseline asset measure and predicted consumption measure, the difference between richer and poorer households is statistically significant at the 5 percent level. Strikingly, for the assets and consumption outcomes nearly the entire reduction in infant mortality appears concentrated among the poorer households. While we do not observe differences in infant mortality effects for baseline nor predicted income, this is also the variable with the least amount of heterogeneity, as described in Haushofer et al. (2025).

These results provide some further support for the view that there is a concave relationship between socioeconomic status and health, and especially that improvements from very low living standards (as in rural Kenya) can be associated with pronounced health gains. This finding also provides another potential rationale for the lack of infant and child mortality effects among ineligible households, despite their documented gains from economic spillovers due to the cash transfer program: as the ineligible households have on average roughly

²³We focus on reduced-form survey sample estimates using Equation (1) here as estimates using Equation (2) are somewhat less precise with the smaller survey sample when carrying out heterogeneity analyses.

twice the value of assets as the eligible households (and far lower baseline infant mortality), dramatic improvements in infant mortality may be more challenging to generate.

Taking the previous subsections together – on transfer timing, the cause of death, and socioeconomic status heterogeneity – suggests that returns to targeting cash could be particularly high if a policymaker were to target pregnant women from the poorest households. As a further policy consideration regarding such targeting: pregnancy is a verifiable condition at relatively low cost, but it is likely to be far more costly to identify more impoverished and deprived households in a rural East African setting like ours where subsistence agriculture and informal employment are widespread. Recall that all of the eligible household possessed the easy to observe grass thatched roofing but the baseline value of household assets and income and the various living standards predictions require far more time-consuming household surveys. This means it may be cheaper in practice to target on pregnancy status than on relative household poverty.

5 Comparison with Non-Experimental Variation

The birth census data enable us to benchmark the experimental cash transfer treatment effect estimates against several dimensions of non-experimental variation in economic circumstances. We provide a summary of these analyses below, with the details of each documented in Appendix B. In short, both the experimental and non-experimental estimates indicate that child survival is very sensitive to economic conditions in rural Kenya.

First, we examine the cross-sectional difference between transfer-eligible and ineligible households in control villages. Across 2015-17, infant mortality is 36% higher among control eligible births than among control ineligibles, a gap robust to controlling for basic birth demographic characteristics. Even within transfer-eligible households, substantial cross-sectional differences exist based on baseline household wealth: infant mortality is more than twice as high among households with below median baseline assets than among households above the median in control villages (Appendix Table A.8).

Second, we study the sensitivity of infant mortality to inter-temporal changes in economic conditions by comparing death rates in the pre-harvest “lean season,” which is defined by Burke et al. (2019) as encompassing April through August, to the relatively prosperous harvest season. We find that across the pre-COVID period of 2011-19, rates of mortality for infants born in August (the peak of the lean season) are 21.9 deaths per thousand births higher than for infants born in the very next month (when the harvest arrives), a difference similar in magnitude to the cash transfer treatment effect we estimate and representing a doubling of infant mortality for births a single month apart.

Third, we investigate how mortality responds to two major economic shocks which affected

Kenya during the period the birth census spans: a severe drought in 2017 and the COVID-19 pandemic in 2020-21. Infant mortality in the census sharply rises across the board during the 2017 drought as well as in 2020-21 (Figure 1, Panel A), and a regression discontinuity analysis indicates that the infant mortality rate doubled the week after Kenya imposed a strict COVID-19 lockdown on March 27, 2020 (Appendix Figure A.3, Panel A.).

Last, we move beyond the birth census and present the cross-country relationship between per capita GDP and infant mortality as a point of comparison (Appendix Figure A.3, Panel B). In 2014, each log point increase in per capita GDP was associated with a 0.79 log point reduction in infant mortality, a relationship which indicates that augmenting the study region’s GDP by the same proportion as the UCTs did for treated households would predict a 36% decline in mortality. By comparison, the experimental estimates find a remarkably similar 44 to 48% decline (depending on the specification).

In sum, across multiple sources of variation, infant and child mortality appears highly sensitive to economic conditions in this context, and non-experimental approaches recover similar magnitudes as the large experimental effects estimated in this study.

6 Mechanisms and Behavioral Change

The main finding of this study is that the disbursal of cash transfers leads to large reductions in infant and child mortality, and that this is concentrated among neonatal deaths. In this section, we turn to exploring potential drivers of these mortality declines. We do so by taking advantage of the numerous waves of detailed KGES household surveys conducted in the study region across over a decade, ranging from baseline data in 2014-2015 to the third endline survey round (2023-2025). We document that multiple channels and behavioral mechanisms appeared to contribute to the overall effect, while noting that it is challenging to decompose exactly how much of the total child mortality reduction can be attributed to any single mechanism, absent strong assumptions.²⁴

6.1 Healthcare Access

Here we present multiple analyses that indicate that improved access to healthcare — in particular, to birth delivery services — may have played an important role.

The first two pieces of evidence were previously presented above. First, when we examine treatment effects by the timing of cash transfer exposure, the impacts on mortality are largely concentrated among infants whose families were receiving a transfer in the month when the

²⁴More broadly, there may be effects on other unmeasured mechanisms that contribute to the mortality reductions, either on their own or in combination with the channels that we document. While the KGES surveys are detailed, it is of course impossible to prove that they capture all relevant behavioral mechanisms.

child was delivered or when the child was a neonate (i.e., the first 28 days of life). Second, when investigating changes in mortality by cause of death using the verbal autopsy data, by far the largest reduction is within the neonatal and maternal cause category (e.g., death from preterm delivery, birth asphyxia, other delivery complications, etc.), for which infant mortality falls 75%. These analyses suggest that conditions in the period around delivery and in the immediate antenatal and neonatal periods play a critical role, as neonatal and maternal causes of death in particular are believed to be largely preventable if quality healthcare is utilized ([World Health Organization, 2015, 2022b](#)).

We next examine direct evidence for these mechanisms collected through the endline 3 long-form household survey, which asks a representative sample of households detailed questions about their antenatal, delivery, and postnatal healthcare utilization during pregnancies which took place between 2015-17. Figure 6 reports estimated effects of the transfers on five pre-specified, WHO-recommended metrics of healthcare utilization (as well as on C-sections, which were not pre-specified but are another potentially important channel), calculated using Equation (2). The results indicate that mothers in eligible households were 20 percentage points more likely to give birth in a hospital if they received the UCT in a high-saturation sublocation, a large and statistically significant 45% increase (over the control mean of 44%).²⁵ These findings are consistent with past research which has argued that cost is a substantial barrier to institutional delivery in Kenya ([Njuguna et al., 2017](#)). Hospital deliveries are particularly expensive: an assessment conducted shortly before the start of this UCT program indicates that the cost of delivering in a hospital stood at well over double that of non-hospital facilities ([Institute for Health Metrics and Evaluation, 2014](#)). For example, they find that delivering in a hospital cost \$137 (in 2011 nominal USD) on average, whereas delivering in a public health center cost \$56 and at a public dispensary just \$17.

We cannot rule out that there were some negative congestion effects due to greater use of local hospitals for deliveries, as some recent research suggests could be relevant in other LMIC settings ([Andrew and Vera-Hernández, 2024](#)). However, any such congestion effects were apparently far smaller than the gains experienced by cash transfer recipients, and recall that there were no adverse net impacts of the program among ineligible households (who did not receive a transfer; see Appendix Table A.5).

Point estimates also suggest that the proportion of pregnancies where the WHO recommended number of at least four antenatal visits occurred may have increased moderately (13%), although this result is not statistically significant at traditional levels. Estimates

²⁵There is some imbalance across treatment arms in the rate of hospital delivery in the pre-period (not shown). It is possible that some of this is driven by recall errors, for instance, if people mistakenly report the same delivery facility used during the cash transfer period for their earlier births. Recall that infant mortality rates are balanced in the pre-period (Figure 2).

also suggest a decline of roughly half in C-section delivery rates, although differences are not statistically significant at conventional levels. Cesarean delivery is positively correlated with mortality in control areas, suggesting that the observed decline in rates may reflect a reduction in emergency surgeries (“crash C-sections”) among cash recipients.

To further examine the potential role of healthcare access, we assemble detailed data on travel times to health facilities across the study region. The coordinates of every registered health facility nationwide were obtained from the Kenya Master Health Facility Registry (KMHFR), a database developed by the Kenya Ministry of Health and the United States Agency for International Development ([Republic of Kenya Ministry of Health, 2025](#)). We then surveyed the facilities to collect information about the care that they offer, including whether the facility is staffed by a physician. We augment this data using surveys of health facilities that household respondents had reported visiting but were not present in the KMHFR data. Field officers equipped with GPS speedometers logged the travel speeds of over 1,100 trips throughout the study area to construct a network of travel times on roads. We then complement this with estimates of walk times off of roads using a procedure previously utilized in the study region ([Ouko et al., 2019](#)) to compute total travel time.

These data enable the estimation of Equation (2) interacted with bins of travel time to delivery facilities. In particular, we focus on two dimensions of heterogeneity in travel times, namely, distance to a physician-staffed facility (which [Okeke \(2023\)](#) suggest is particularly important to birth outcomes) and on proximity to a hospital. These are correlated measures of access since hospitals tend to be staffed by doctors, and both may also proxy for other dimensions of health care quality (i.e., better access to equipment or drugs).

The results of this analysis suggest that the reductions in infant mortality observed during the disbursement of cash transfers may have been more concentrated in households located closer to a physician-staffed health facility in Table 2. We compare households with above median travel times (mean time 55 minutes) to a physician-staffed facility to those with below median travel time (mean 23 minutes). The main analysis is presented in Panel B, which selects regression controls using double partial-out LASSO to account for omitted variable bias (confounding) from place-based factors that might be correlated with the placement of health facilities — including a vector of measures including population density, distance to towns and roads, baseline village wealth, malaria suitability, and rainfall, among others (and interactions of these covariates with the treatment variables are also included). The estimated interaction between cash disbursement and living closer to a physician-staffed facility is large in magnitude (at -29.0) and statistically significant at the 10% level when these controls are included (Table 2, column 1). There is no statistically significant heterogeneity with respect to distance to a hospital (Table 2, column 2), but the sizable point estimate

(-12.7) suggests that effects were also more concentrated in areas near hospitals.²⁶

While some alternative interpretations remain possible, since the location of health facilities staffed with doctors is not randomly allocated, across multiple analyses a consistent pattern emerges: cash matters most for infant survival when delivered in the neonatal period to households near established health infrastructure with good delivery services. Rates of hospital delivery rise, and rates of death from neonatal complications readily prevented by appropriate obstetric care fall. These results are consistent with prior literature including Okeke (2023), which finds in an experiment that access to a physician in rural Nigeria significantly reduces neonatal mortality. Yet even having ample cash in hand may not be enough if hospitals and doctors are difficult to reach, given the challenges of poor road quality and accessing on-demand rapid transportation in Kenyan villages (Floyd et al., 2020), including in our study population where vehicle ownership at baseline was rare. And the timing of the cash transfer also appears decisive given the high marginal propensity to consume cash transfers in this low-income rural population (as documented in Egger et al. (2022)): transfers disbursed far in advance of a delivery have far less impact, presumably because the money is already “gone” and most households have returned to low levels of liquidity.

6.2 Maternal Labor Supply

Parental time is a key input in the production of child health (Ruhm, 2000; Miller and Urdinola, 2010; Rossin, 2011). Many determinants of positive child health outcomes, such as traveling to facilities outside the village for primary care visits and adequate rest during pregnancy, are relatively inexpensive monetarily but highly time-consuming. As background, Kenya is in the top ten countries worldwide for female labor force participation (at 72%), and of all World Bank regions worldwide, Sub-Saharan Africa features the highest rates of women working (International Labour Organization, 2025). While in general increased female labor supply can have a number of positive effects on the well-being of women and children alike (Heath and Jayachandran, 2018), performing strenuous physical tasks for extended periods of time during pregnancy and the initial months postpartum, as is common in Kenya, may have deleterious consequences for infant health (Izugbara and Ngilangwa, 2010; Riang’a et al., 2018; Scorgie et al., 2023).

The previous Egger et al. (2022) paper documented little effects on average household

²⁶We also examine effects on reported delivery in physician-staffed facilities and hospitals by proximity to each of the facility types in Appendix Table A.9. While there is no differential effect with respect to delivery in a physician-staffed location, there is heterogeneity for hospital deliveries: almost all the observed increase in hospital deliveries were reported among households with below median travel time to a hospital ($p < .05$), a relationship that survives controls. This suggests that cash transfers do not appear to be having an impact by allowing households located far from health infrastructure to access it (for instance, by paying for expensive transport) but rather by allowing households living nearby to pay for services.

labor supply due to the cash transfer, with some positive point estimates that were indistinguishable from zero. Families with a woman in late-stage pregnancy or with a newborn at home, however, may exhibit different patterns. For women in these cases, the cash may arrive at a time when the marginal utility of rest and additional time to invest in child health may be particularly high. Detailed data on hours worked by gender combined with the birth census enable analysis of heterogeneity in the cash transfer’s effects on labor supply by gender and the presence of a pregnancy or newborn in the household.

Figure 7 reports labor supply impacts of cash separately by gender, in an analysis based on Equation (2) and including treatment terms interacted with indicators for a pregnancy or newborn present at the time they were surveyed at the first endline.²⁷ In separate regressions, we include indicators for three periods of interest: the first six months in-utero, the third trimester in-utero and first three months postpartum, and the next six months postpartum (i.e., four to nine months after birth). Estimates encompass only those households selected for the long-form surveys which inquired about labor supply; as such, the sample size here is far smaller than the full birth census. Six-month bins are used to increase statistical power under these conditions.

We find quantitatively substantial heterogeneity in labor supply effects among women by whether a late-stage pregnancy or newborn is present within the household. In line with Egger et al. (2022), recipient households without a pregnancy or recent birth exhibit no change in hours of labor supplied as a result of the transfers: for both women and men, the point estimate on weekly hours is statistically insignificant and close to zero. In the three months before and after a birth, however, cash transfers reduce female labor supply in recipient households by 20.79 hours a week, relative to a control group mean of 40 hours; this high control group mean is consistent with other evidence cited above of high rates of labor force participation for pregnant Kenyan women. This result is statistically significant at the five percent level and represents a notable 51% decrease over the control mean. Coefficients are also negative and meaningful for the impact of transfers on female labor supply in the first six months in-utero and 4-9 months after birth, though they are smaller and not statistically significant at traditional confidence levels. By contrast, we estimate much smaller effects close to zero among men across all periods, suggesting that it is women in particular who are able to temporarily reduce labor supply when a pregnancy is present due to the transfers.²⁸

²⁷Note that this analysis was not pre-specified.

²⁸Further heterogeneity analysis based on the gender of the cash recipient within the household could be valuable to further understand these dynamics. However, we observe only the name of the individual who registered the cellphone that cash transfers were sent to. Cellphones are often shared by households and GiveDirectly did not have a policy to target transfers by gender, so in most cases we cannot reliably determine if the cash recipient was female or male.

These results provide suggestive evidence that the cash transfers may have enabled some women to reallocate time from potentially strenuous labor to rest or other activities more beneficial for fetal and child health.²⁹ Reductions in labor supply during pregnancy as a channel lowering infant mortality would be consistent with prior work on the importance of parental time in the production of child health (Ruhm, 2000; Miller and Urdinola, 2010; Rossin, 2011; Nandi et al., 2016; Bartel et al., 2023), and accord with studies in other LMIC settings indicating an association between cash transfers and reductions in maternal labor supply (Novella et al., 2012; Amarante et al., 2016; Garganta et al., 2017; Guldi et al., 2024).

6.3 Nutrition

Another important consideration is the role of nutrition. The first endline survey conducted in 2016-17 indicates that food security for children significantly rose for recipient households: a pre-specified index of child food security, which encompasses household survey questions on whether children skipped meals, went to bed hungry, and went entire days without food over the past week, increased by 0.17 standard deviation units on average among recipients as estimated using Equation (2). This result, reported in Table B.6 of Egger et al. (2022), is statistically significant at the five percent level, and similar results are found using Equation (1). That earlier study also documented significant increases in overall household food consumption, which could plausibly have improved the nutrition of pregnant mothers (although we cannot directly verify this as we do not have direct measures of individual nutritional status, for instance, from anthropometrics, or detailed data on dietary diversity).

6.4 Fertility Patterns

In this final subsection on mechanisms we explore impacts on fertility. We find modest increases in general fertility but no evidence that the characteristics of households and mothers giving birth changed in treatment areas. Specifically, Table 3 reports estimates (from Equation (2)) indicating that among recipient households the share of women giving birth rose by 11% relative to the low-intensity village mean.³⁰ Fertility patterns are indistinguishable

²⁹We lack the statistical power to undertake definitive heterogeneity analyses on the relationship between female labor supply and child survival by whether work occurs close to childbirth, as cell sizes become small, however some suggestive patterns emerge. There is a quantitatively large positive correlation between female labor supply during the third trimester and neonatal mortality, though the interaction is not statistically significant at conventional levels ($p = 0.14$). Female labor supply outside of the third trimester is weakly negatively correlated with neonatal mortality, and for male labor supply, the interaction term is considerably smaller in magnitude and not significant.

³⁰The population of women for which we calculate birth rates is all censused women who were adults in 2023. We did not collect detailed age data for all women who did not give birth, and hence are unable to create fully age-adjusted fertility rates.

by treatment status in the 2011-14 pre-period and again in the 2018-21 post-period. Similar results are obtained in the reduced-form using Equation (1).

Several approaches indicate that the characteristics of the women giving birth in 2015-17 did not change meaningfully as a result of the transfers. To analyze their characteristics, we again turn to the long-form household surveys conducted with a representative subsample as the birth census itself was parsimonious. First, using the sample of transfer-eligible households surveyed at baseline, households with a birth during 2015-17 do not significantly differ by treatment status across six baseline socio-demographic characteristics, namely, education, age, marital status, income, assets, and household size (Appendix Table A.11).

Next, we use these six baseline household characteristics to predict the probability of giving birth over 2015-17 for adult females censused in EL3, and then examine whether treatment status is associated with the predicted probability of birth among women who gave birth during that key period. Birth probabilities are predicted using a random forest model trained with five-fold cross-validation. We find in Column 4 of Table 3 that the predicted probability of birth among actual mothers does not significantly differ by treatment status in the transfer disbursement period (p -value = 0.71). We further find (in Column 5) that treatment status is not associated with predicted birth probability among actual mothers in the post-transfer period of 2018-21 (p -value = 0.88). Predicting birth probability using LASSO from a set of 243 baseline household- and village-level characteristics, as we do in Appendix Table A.12, yields similar results indicating an absence of treatment impacts on the characteristics of women who gave birth (in terms of their individual predicted birth probabilities).

Last, we run Equation (2) augmented with additional baseline controls to test whether the inclusion of household baseline characteristics affects the main results (Appendix Table A.13), again in an attempt to determine if selection into fertility could be driving the main results. These regressions are performed only on households surveyed in the detailed baseline survey, and hence the sample size is smaller. The first column only includes pre-specified controls (e.g., year of birth fixed effects, birth gender, and mother age group), and there are sharp infant mortality declines due to cash transfers in this subsample, with an estimated drop larger than that found in the full census data (as noted above), at 55%. In Column 2, we then include controls for the six baseline socio-demographic characteristics previously considered, and the infant mortality results are again largely unaffected. In Column 3, we augment Equation (2) to include controls selected by post-double selection (PDS) LASSO (Belloni et al., 2014), with a total of 243 baseline household- and village-level characteristics available for selection, and the results are also unaffected, with estimated drops if anything slightly increasing (57%). Panel B of Appendix Table A.13 shows that these results are all robust to estimation using the reduced-form equation (1) rather than the spatial IV design.

A natural conclusion from the above analyses is that any changes in the characteristics of the mothers giving birth (at least on the basis of observables) in the treatment group were at most modest and cannot readily explain the documented infant mortality declines. Whether the transient increase in fertility during the key transfer years (and the corresponding mortality decrease) also raise questions about the program’s effect on lifetime fertility, especially since mothers’ characteristics do not appear to be changing. This will be important to study in future, longer-term research.

7 Cost-Effectiveness Implications

The prior discussion of potential mechanisms prompts two important questions. First, what do the estimated child mortality benefits imply about the welfare gains from UCTs versus other forms of assistance that governments or aid donors may invest in? Second, are cash transfers a cost-effective tool for reducing child mortality rates, and can the mechanism analyses reported above help guide efforts to target transfers to those most likely to benefit?

It is straightforward to estimate the number of child deaths averted due to the UCT program under study. The analysis focuses on recipient households and births during the 2015-17 period of transfer disbursal, as estimated effects after 2017 for this group are near zero and so are estimated impacts among non-recipients across all years (not shown). We estimate the number of child deaths averted among recipient households during the transfer disbursal period using the following “back-of-the-envelope” calculation:³¹

$$\begin{aligned} & \text{(Estimated average treatment effect on recipient child mortality)} \\ & \times \text{(Number of births among recipient treatment households during 2015-17)}. \end{aligned} \tag{5}$$

The estimates reported in Table 1, Column 4 reflect the average total effect in high-saturation sublocations. To estimate lives saved, we therefore apply an estimate of the average treatment effect on child mortality pooled across both high and low-saturation sublocations (instead of focusing on high-saturation cases alone), which is -24.19 (SE 7.81).³² There were 3,533 births for eligible households in treatment villages across 2015-17, so we estimate that approximately $(-24.19/1000) \times (3,533) = 86$ child deaths were averted due to the UCTs. Had all treated households been located in high-saturation sublocations, we estimate that

³¹We focus primarily on child mortality as opposed to infant mortality due to its relevance for policymakers and foundations. For example, the United Nations Sustainable Development Goals refer specifically to under-five mortality but not to under-one mortality (United Nations, 2015). As previously noted, the estimated reductions in child and infant mortality are both large and broadly similar.

³²The result is similar to the high-saturation ATE since most effects are driven by village treatment not cross-village spillovers. We omit a table with these estimates given their similarity and then focus on effects in high-saturation areas, which we view as more policy relevant for scaled up UCT programs.

about 91 lives would have been saved in treated households. This is a substantial reduction: based on the low-saturation control village mean of 57.4 deaths per thousand births, we would have expected approximately 203 child deaths to have occurred in treatment villages across this period, again indicating that the cash transfer treatment led to a drop of nearly half in the number of child deaths.

The leading approach to estimate the welfare gains of these mortality reductions is by recipients’ value of a statistical life (VSL). Revealed preference estimates of VSL, or consumer demand for mortality risk reductions, tend to be low among populations with income levels similar to this study. In fact, most revealed preference studies we are aware of find values below \$5,000 (Killeen, 2025; Kremer et al., 2011; Berry et al., 2020). As documented in Killeen (2025), economic theory only supports applying these VSL’s to balance the trade-off between consumption and health aid to reduce mortality; low values do not imply that resources dedicated to improving health should not be allocated to poor households (since it mainly reflects a high value of consumption). This level of VSL would imply welfare gains from the mortality reductions (of roughly 90 fewer deaths) of less than \$500,000. However, other groups have argued for higher VSL’s in low income settings. For instance, GiveWell, a non-profit charitable giving advisor applies “moral weights” which (based on our understanding) value averting an under-5 death at 116 times the benefit of doubling annual consumption, implying a value of \$87,956 per life saved in this setting. This would value welfare gains from the child mortality reductions at approximately USD 2023 PPP 7.6 million, a sizable portion of the expenditure on UCTs (USD PPP 25.75 million or USD 10.75 million nominal).

To account for the wide range of VSL estimates among the population, we report the estimated benefits of a \$1,000 investment in UCTs (including the multiplier gains documented in Egger et al. (2022)), versus a leading health intervention, malaria medication, by VSL in Figure 8.³³ Four VSL estimates obtained from settings with similar income levels are included in the lower panel. We focus primarily on the range of revealed preference estimates since they are based on choices with real stakes and arguably less prone to social desirability bias, but we additionally include a stated-preference estimate from Redfern et al. (2019), which estimates a VSL of over \$55,000, because it informed GiveWell’s moral weights so is used in important policy decisions.

In the top panel, we present the estimated welfare gains of UCTs across three different scenarios, and contrast them to the gains from the malaria treatment intervention. The first

³³We use GiveWell’s estimate of the cost per life saved through the malaria intervention of \$4,304. We selected this program because it was GiveWell’s top listed charity at the time of writing in March 2025. This estimate accounts for the spillover benefits of treatment.

“base” UCT case excludes any benefits from economic spillovers or child lives saved, and thus values the \$1,000 transfer at exactly \$1,000 (the horizontal green line). In this case, the malaria intervention generates larger welfare gains than cash transfers even at relatively modest levels of the VSL, as the green and red lines intersect at approximately \$4,000.

The second case, denoted by the thin blue line, includes the benefits from both the economic spillovers and child lives saved documented in this study. Recall that [Egger et al. \(2022\)](#) estimate a real transfer multiplier of 2.5 in the study area, leading to an increase in welfare of approximately \$2,500 even at low levels of the VSL. [Egger et al. \(2022\)](#) note that interpreting the transfer multiplier as welfare gains can be problematic if factors such as reduced leisure or savings drive consumption gains, however, they find no evidence of such responses, so we assume the transfer multiplier translates into welfare gains for the purposes of this analysis. We also focus on the high-saturation sublocation estimates of the effects of the UCTs since this scenario more closely matches how a similar policy would likely be scaled up in practice (when no control group is present).

The welfare gains of a UCT program like the one that we study are mainly driven by consumption gains (among recipients and others due to spillovers), and there is a proportionally small difference in welfare when accounting for mortality reductions across most VSLs in the distribution. This is true because the UCTs in the study setting were given to all eligible households and not targeted to pregnant women, so the cost per life saved is relatively high. There is a slight upward slope in the thin blue line at very high VSL levels but it is nearly imperceptible since pregnant women are a small share of all cash recipients.

In our third case, we also plot estimated welfare gains if the UCTs were instead targeted to pregnant women (retaining the same assumptions about the real transfer multiplier of 2.5 as above), in the thick blue line.³⁴ The welfare gains from mortality reductions are much larger in this scenario given that far more births are affected by the transfer: targeted cash transfers yield far higher estimated welfare gains than untargeted transfers for VSL values above about \$20,000 in this scenario.

Across the three cases we consider, the welfare gains from cash transfers are larger than those from malaria medicine for low levels of the VSL corresponding to most of the existing revealed preference VSL estimates (below roughly \$4,000). However, if one values lives saved by the far higher stated preference estimate of \$55,000, then the malaria intervention generates larger welfare gains than any of the UCT estimates. That said, the welfare gains from a UCT program targeted to pregnant women are greater than those generated by the malaria treatment program up to a VSL level of approximately \$11,500, and rises substantially for the higher values in the range considered in the figure.

³⁴Here we abstract away from any potential fertility responses to a targeted UCT program.

We next add structure to the problem and estimate the posterior distribution of the VSL in this population using Bayesian hierarchical meta analysis. The plot reveals that the estimated welfare gains of the targeted UCT program dominate malaria medicine for about 75% of the distribution of VSLs, although the mean estimate of gains from malaria medicine are about \$300 higher. This holds because the [Redfern et al. \(2019\)](#) VSL estimate induces a substantial rightward skew in the distribution. Both the untargeted UCT program (incorporating spillover benefits and child survival benefits) and the targeted UCT program yield higher estimated welfare for the full 95% confidence interval of VSLs if that study is excluded. Thus in cases where UCTs produce the general equilibrium effects documented in [Egger et al. \(2022\)](#), we view UCTs as an attractive form of aid for a wide range of plausible VSL values, especially when they are targeted to pregnant women, even in comparison to highly cost effective health interventions like the malaria program we consider. Details of the estimation of the VSL distribution and the welfare analysis are in Appendix C.³⁵

A second question related to the welfare implications of the child mortality reductions is how cost-effective UCTs are compared to a range of other health interventions while focusing more narrowly on child survival impacts (and excluding consumption gains). As documented in [Killeen \(2025\)](#) and noted above, the prior welfare analysis guides optimal decision making if donors are deciding between various programs to benefit a population such as one in the study, but if funds are specifically earmarked by donors for reduced mortality alone, economic theory does not support the use of recipients' VSL. We therefore benchmark the cost per death averted to other health interventions in Sub-Saharan Africa considered cost-effective by the World Health Organization and other health experts.

In total, the UCT program under study disbursed USD PPP 25.75 million, and given that we estimate the program averted approximately 86 child deaths, this implies a cost of USD PPP 299,418 per death averted. However, this calculation is highly conservative when thinking about cost-effectiveness for at least two reasons. First, unlike most health interventions, the UCTs were intended to affect non-health outcomes such as raising household consumption (as captured in the exercise above). Second, the UCT was not targeted towards pregnant women or households with small children. Note that in practice aid donors or governments attempting to target pregnant women could rely on data being collected by local health clinics and hospitals, or attempt broader outreach via household surveys.

As a first pass, we consider targeting transfers to households with women in the third trimester of pregnancy. Disbursing UCTs to these households would cost a total of USD

³⁵Appendix Figure A.5 reports the results of a decision theoretic model which yields similar results. Namely, broadly targeted UCTs minimize median regret, but malaria medicine narrowly minimizes Bayesian regret when [Redfern et al. \(2019\)](#) is included in posterior estimates of the VSL.

PPP 1.65 million (USD 700,000 nominal) in the study sample and time period, based on the household survey data.

Calculating the number of deaths averted under this scenario is challenging due to at least two opposing factors. On one hand, restricting transfers to a subset of households reduces the total impact of spillovers from other treated households. On another hand, targeting cash to particular subpopulations may result in larger treatment effects among those high-impact groups. We found earlier in Figure 3, for example, that mortality was virtually eliminated for children whose households were receiving the UCT in the month when they were born, in contrast to other timing cases in which there were more modest effects. For simplicity and to take a middle-ground approach, we simply utilize the average high-saturation treatment effect for all recipients (-25.63 deaths per thousand births) and apply this to the back-of-the-envelope calculations here, while noting that these may be conservative.

Targeting UCTs to women in the third trimester of pregnancy under these assumptions would cost about USD PPP 92,000 (or \$39,000 in nominal dollars) per child death averted. We can benchmark these calculations to 37 WHO-recommended maternal and child health interventions in East Africa as estimated by [Stenberg et al. \(2021\)](#). Across interventions and scenarios, the cost per death averted ranges from USD PPP 27 to USD PPP 222,952.³⁶ Hence, even without taking into account any of the other documented benefits of UCTs (such as gains in consumption), the transfers are squarely in the range of cost per death averted among these WHO-recommended interventions.

8 Conclusion

A large-scale unconditional cash transfer program in rural Kenya led to a sharp drop of nearly one half in infant mortality. The largest mortality reductions were observed among those households receiving the cash in the months around a child’s birth, and in households located near physician-staffed health facilities. Concomitant with large mortality reductions among these households, we find that cash leads to far higher rates of hospital deliveries, especially for households who live near hospitals. A rough calculation suggests that transfers targeted to pregnant women are broadly similarly cost-effective in terms of reducing child mortality to a number of child health interventions recommended by the WHO. These documented child mortality reductions represent benefits of UCTs beyond the direct and spillover household consumption gains already documented by [Egger et al. \(2022\)](#).

The large magnitude of the child survival gains documented here underscores the fact

³⁶[Stenberg et al. \(2021\)](#) evaluates cost-effectiveness using three coverage level scenarios: 50%, 80%, and 95%, and report health impacts in terms of healthy life years (HLY) saved. We converted HLYs to deaths averted using WHO data on total and healthy life expectancy in Kenya ([World Health Organization, 2025](#)).

that infant and child mortality appear very sensitive to economic conditions in low-income contexts, such as the rural Kenyan study setting. The socioeconomic gradient in mortality is quite steep in rural Kenya: not only do large UCTs nearly halve infant and under-5 child mortality, but treatment effects are concentrated among the poorer households in the sample, and mortality rates vary substantially by household baseline wealth in the cross-section, as well as inter-temporally by the agricultural harvest season. Furthermore, once the UCTs cease, the mortality reductions do not persist, indicating that contemporaneous income and cash on hand is critical. Interventions such as UCTs can result in substantial child survival gains, but they likely will need to be sustained over time rather than in a one-time program to generate persistent child survival gains. Of course, that does not mean that even one-time cash transfers are worthless, as indicated by the large number of 86 children who we estimate survived in the study sample who otherwise would not have.

Another more speculative implication from this analysis is that cash alone, even if sustained, may not be enough to produce the reductions in infant mortality needed to fully realize public health targets. Health infrastructure appears to complement cash in the present study, with some evidence of larger reductions in mortality apparent among households living near health facilities with a doctor present. While giving birth in a higher quality facility like a hospital is expensive for poor households, even with ample cash on hand it may be arduous to reach such a facility if there is not one nearby. In the short-run, one option for policymakers who wish to maximize the impact of cash transfers on mortality could be to target cash transfers to pregnant women from poor households in places with established health infrastructure. Yet this approach may be undesirable for obvious equity and political considerations. In the longer-term, investments in improved health infrastructure, particularly child delivery services, combined with cash transfers may be an attractive approach to ensure the equitable achievement of child survival goals.

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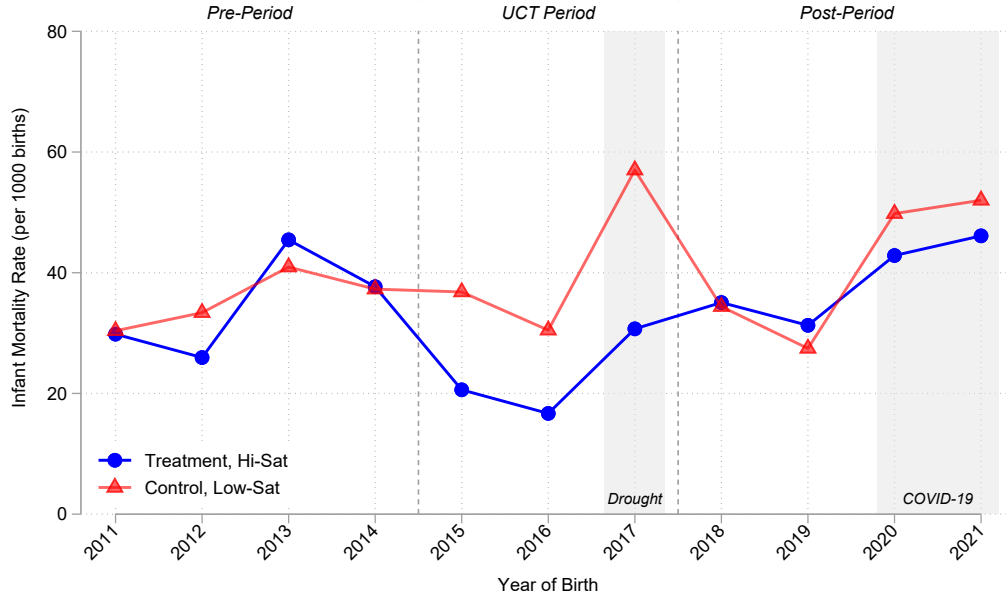
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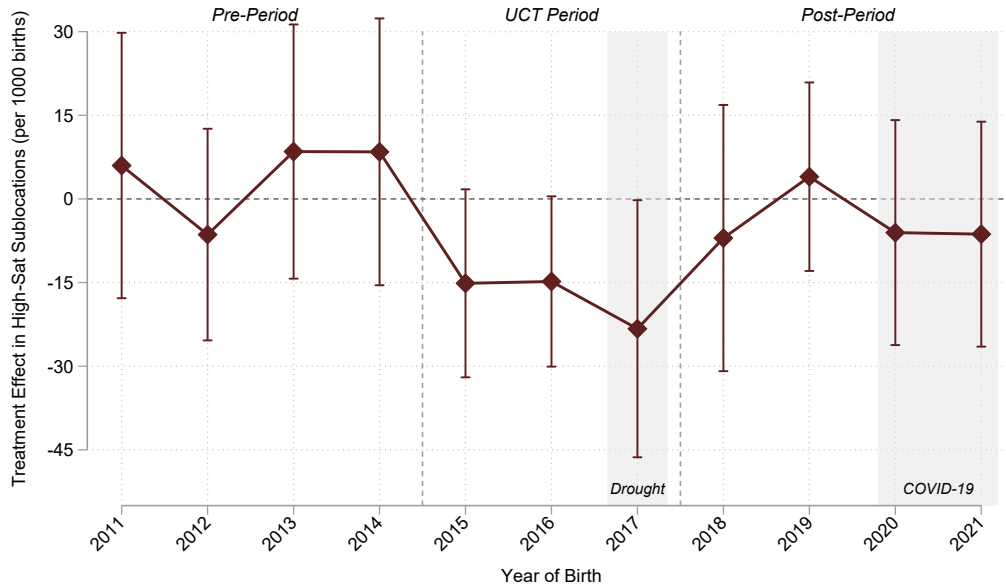
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Figure 1: Unconditional Cash Transfers and Infant Mortality By Year



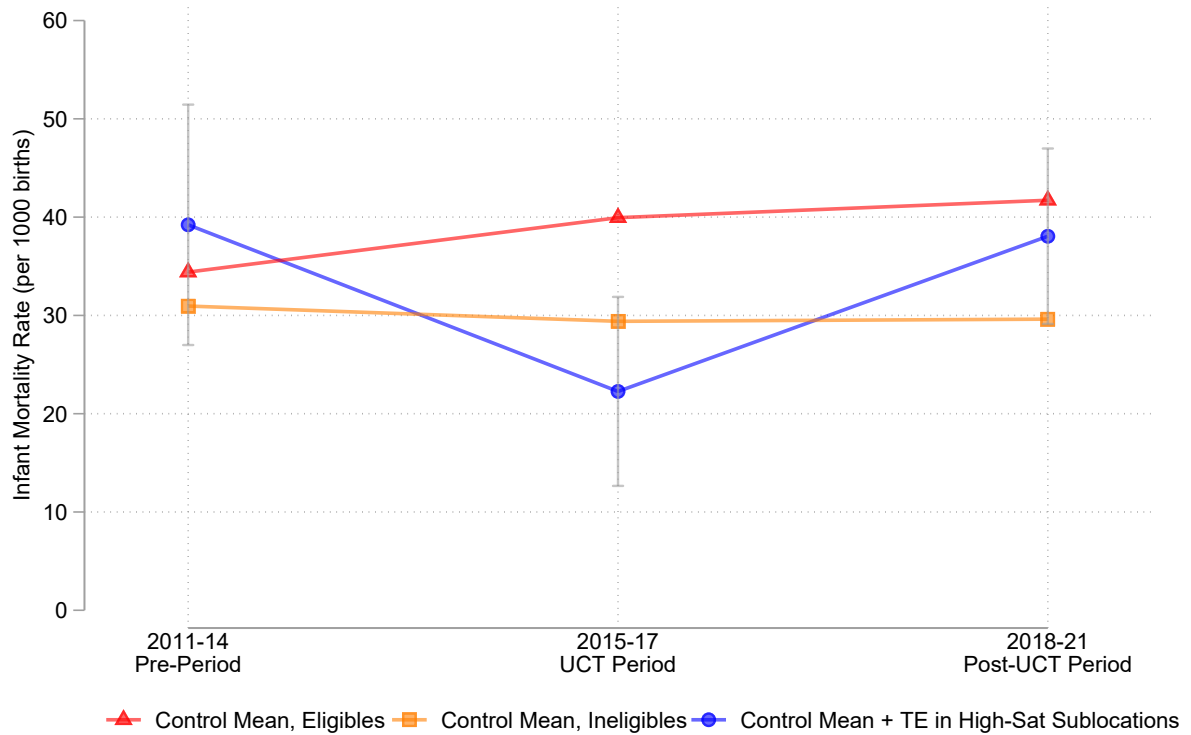
Panel A: Infant Mortality By Year



Panel B: Reduced-Form Impacts by Year

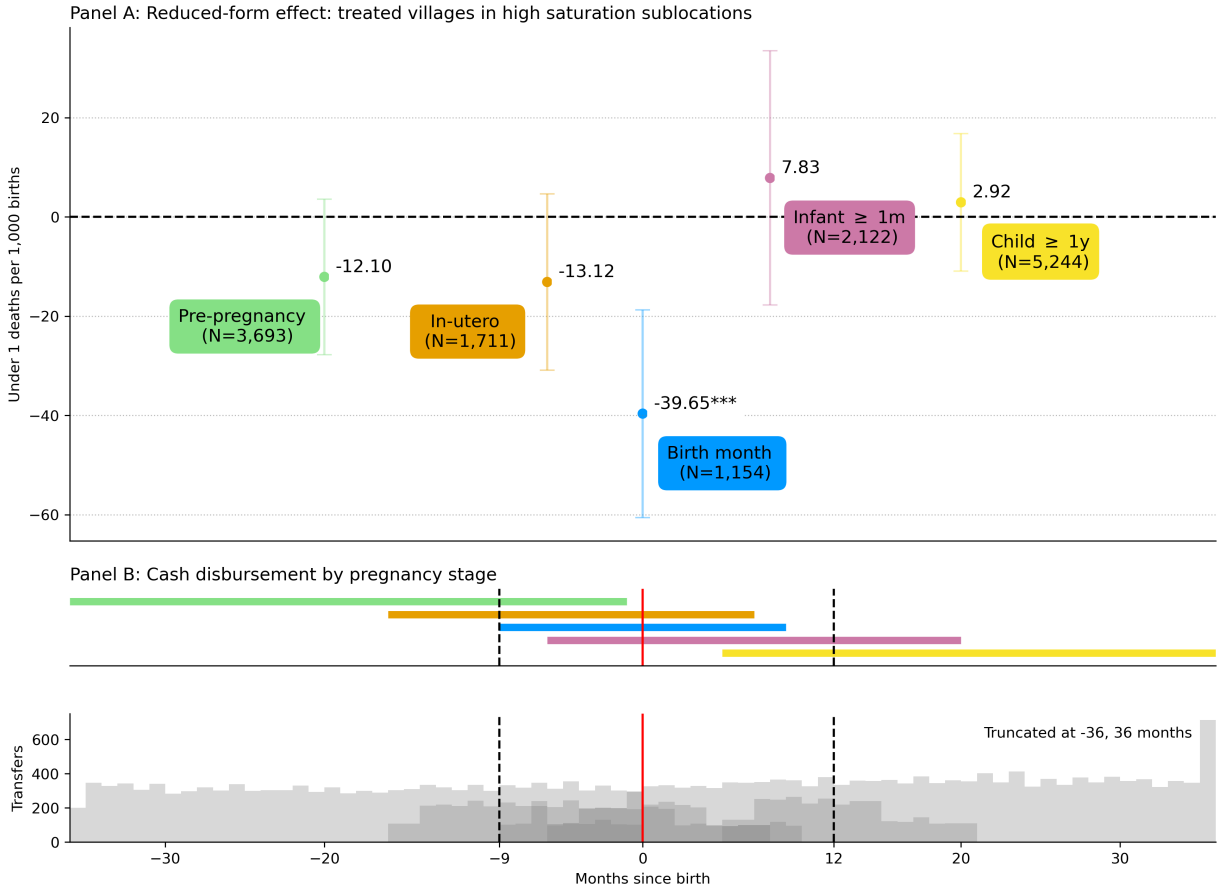
Notes: This figure is based on the main EL3 birth census sample encompassing births from 2011 to 2021. Panel A reports the mean infant mortality rate by year among eligible households for treatment villages in high-saturation areas and control villages in low-saturation areas. Panel B reports the year-by-year reduced-form estimates of infant mortality impacts among eligible households for treatment villages in high-saturation areas. Pre-period births refer to those occurring in the period 2011-14, whereas the unconditional cash transfer (UCT) period refers to 2015-17 and the post-UCT period denotes 2018-21. The COVID-19 pandemic spans 2020-21 and a severe drought affected Kenya in late 2016 and 2017. The whiskers on each yearly estimate denote the 95% confidence interval. Standard errors are clustered at the sublocation level.

Figure 2: Unconditional Cash Transfers and Infant Mortality



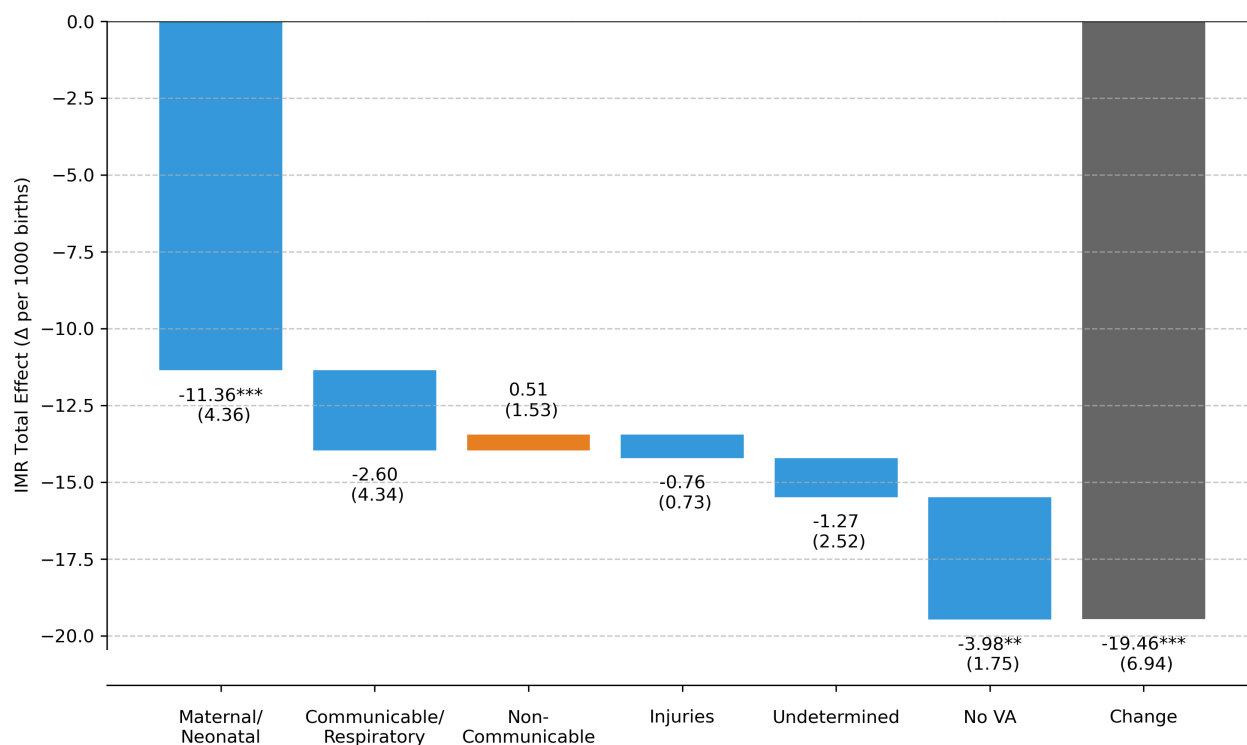
Notes: This figure is based on the main EL3 birth census sample encompassing births from 2011 to 2021. The figure plots the infant (under-1) mortality rate per 1000 births (y-axis) for three different periods of child births: pre-transfers (child birth years of 2011-14), the transfer period (child birth years 2015-17), and the post-transfer period (child birth years 2018-21). The red line reports average rates for transfer-eligible households in control, low saturation villages. The blue line adds in the estimated treatment effects for treatment villages in high saturation areas by period (from Equation (1), reported in Table 1), with 95% confidence intervals shown. The yellow line reports infant mortality rates for transfer-ineligible households residing in control, low saturation villages.

Figure 3: Transfer Effects on Infant Mortality by Timing of Cash Disbursal



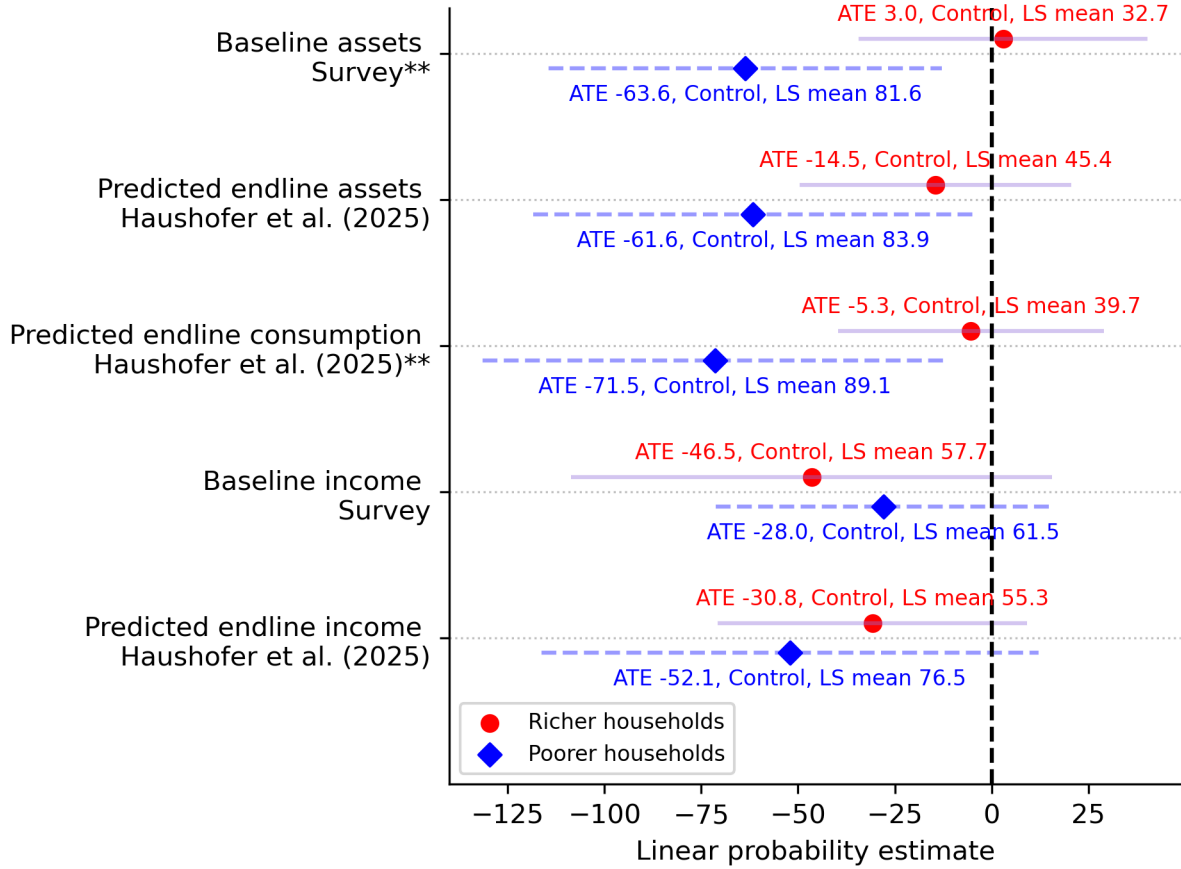
Notes: This figure is based on the main EL3 birth census sample. Panel A plots estimates of dynamics based on the time period of exposure to cash during the child’s life. The transfer timing is defined relative to the “experimental start date”, as this is well-defined for both treatment and control villages. “Pre-pregnancy” includes household exposure 3 years to 10 months before birth. In-utero is 9 to 1 month before birth. Birth month includes cash within the first month of life. Infant includes 1 month to 12 months. Child includes 1 to 3 years (and can be viewed as a placebo check on infant mortality). Estimates are constructed using equation (4), which estimates equation (1) after restricting the sample to those exposed to cash at a particular time relative to the birth month. Observations appear in multiple groups since cash transfers were distributed over 8 months, and we include all observations where the exposure period overlaps with this 8 month window. Panel B plots the range of experimental start dates, relative to birth month, included in each estimate and a histogram of transfers by month in each bin. The spatial IV version of this figure (which is estimated using Equation 2) is presented as Appendix Figure A.2. 95% confidence intervals are shown. * $p < .10$, ** $p < .05$, *** $p < .01$.

Figure 4: Transfer Effects on Infant Mortality by Likely Cause of Death, 2015-17



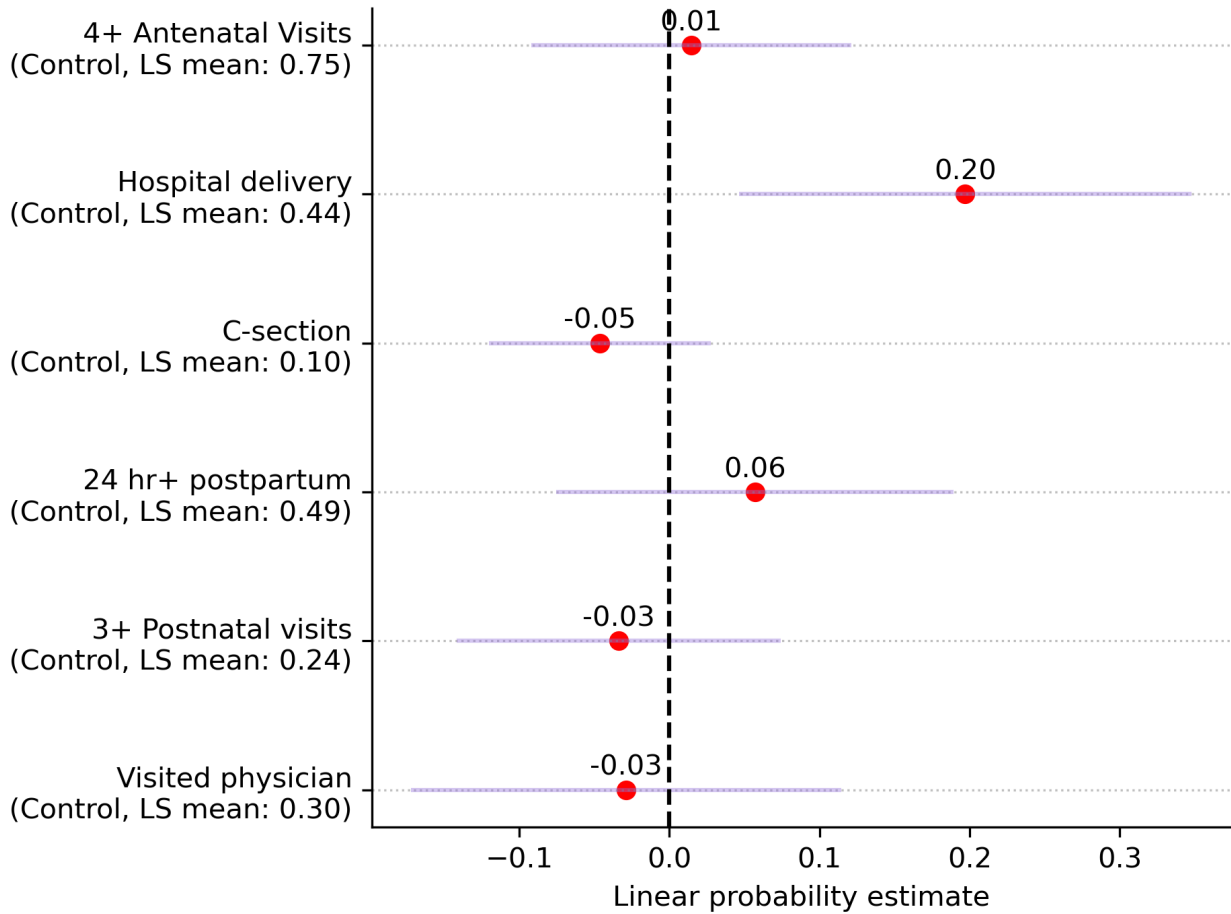
Notes: This figure is based on the main EL3 birth census sample encompassing births from 2015-17 and reports the estimated reduction in infant mortality (per 1000 births) based on cause of death determinations from verbal autopsies (VAs). Treatment effects are estimated using Equation (2). The control, low saturation mean rates per 1000 births for the categories are Maternal/Neonatal: 15.20, Communicable/Respiratory: 10.30, Non-communicable: 2.45, Injuries: 0.49, Undetermined: 7.36, No VA: 4.41, Overall: 40.21. The undetermined category encompasses completed VAs for which the SmartVA algorithm was unable to determine a likely cause due to missing or inconsistent answers. The no VA category includes cases for which no VA was collected. * $p < .10$, ** $p < .05$, *** $p < .01$.

Figure 5: Heterogeneous treatment effects by socioeconomic status (reduced-form)



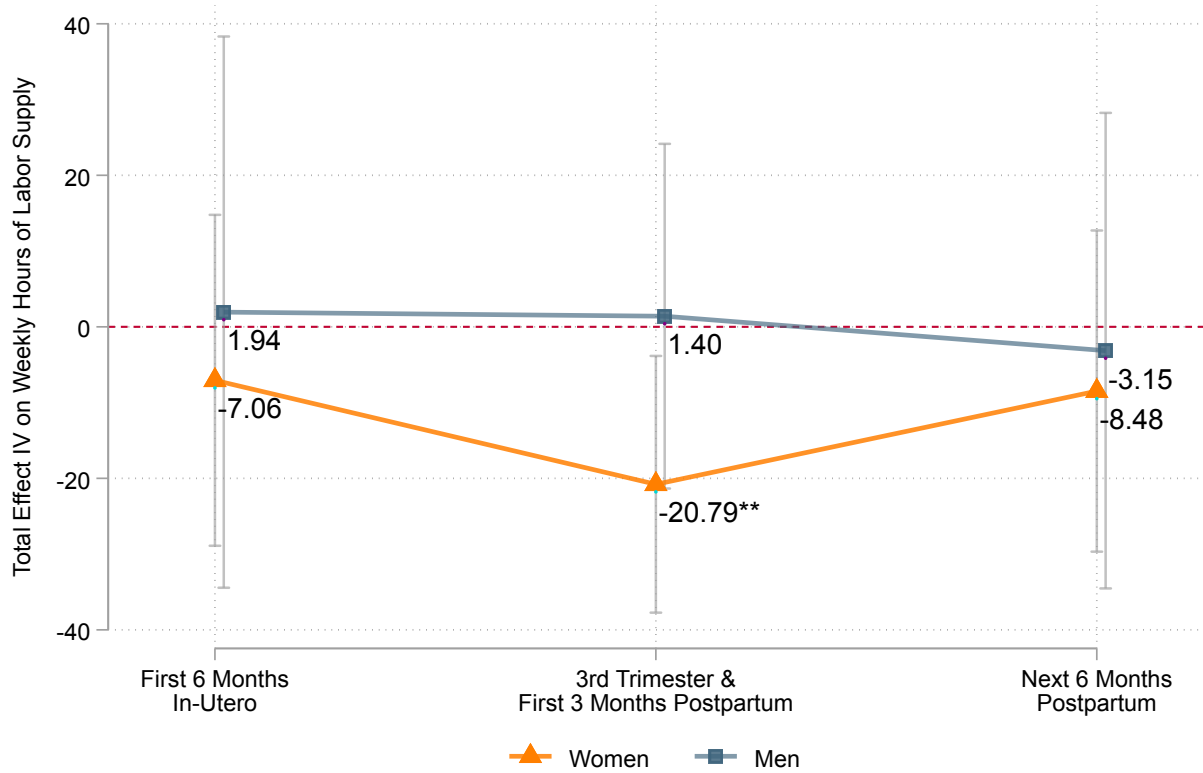
Notes: This figure is based on transfer-eligible households surveyed at both the baseline household survey and the EL3 census. This figure reports the estimated reduction in infant mortality (per 1000 births) based on below vs above-median wealth. Treatment effects are estimated using Equation (1). “Baseline assets” and “baseline income” considers total household assets/income measured in the baseline KGES survey. Predicted endline consumption, assets and income use generalized random forest predictions of the respective per capita measures from Haushofer et al. (2025), which also required households to be surveyed at endline 1. Specifically, we consider the share of model runs in which the household is classified as the “most deprived” according to the respective measure, and define poorer households as those defined as “most deprived” above the median share of the time. The significance of the difference between richer and poorer household estimates is denoted by stars. * $p < .10$, ** $p < .05$, *** $p < .01$.

Figure 6: Unconditional Cash Transfers and Healthcare Utilization, 2015-17



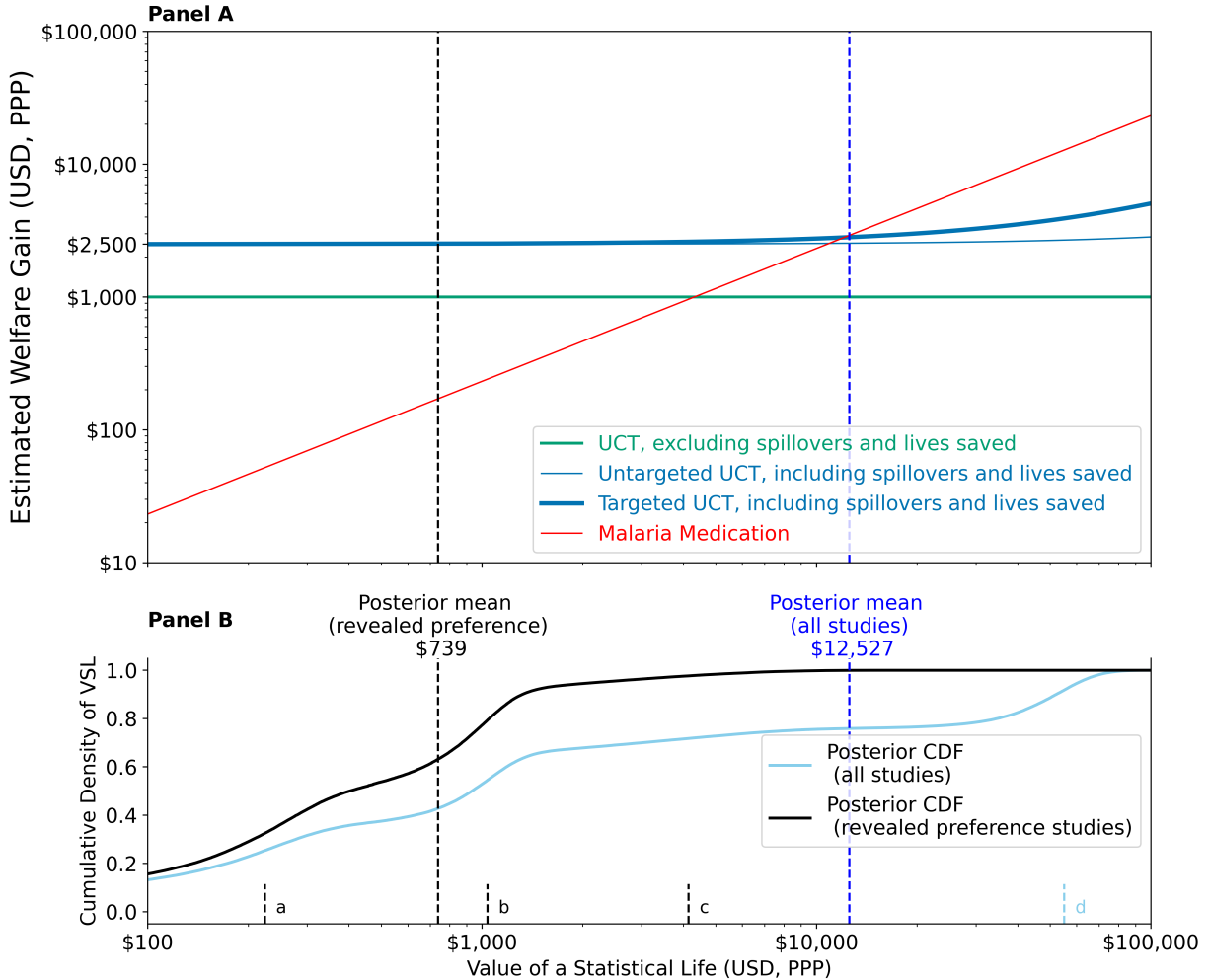
Notes: This figure is based on the sample of transfer-eligible households present at baseline and surveyed in the EL3 long-form survey. It reports treatment effects on indicators for visiting the indicated health service during a pregnancy associated with a birth between 2015 and 2017. Treatment effects are estimated using Equation (2). 95% confidence intervals are constructed using spatial HAC standard errors with a 10km cutoff (Conley, 2008). LS refers to low saturation sublocations. The maximum radius is fixed to 2km to match the value selected in Table 1. Estimates use survey data from eligible households (N=1,154 births). * $p < .10$, ** $p < .05$, *** $p < .01$.

Figure 7: UCTs and Household Labor Supply by Gender Around the Time of Birth



Notes: This figure is based on households surveyed in the first endline survey (2016-17) with a recorded birth in the EL3 census during 2015-17. The measure of labor supply includes hours worked across the household (in agricultural employment, non-agricultural self-employment, and wage employment), as well as hours spent searching for work, in the week prior to the first endline survey. The figure displays the total estimated effect of the cash transfers on labor supply for three groups: households with a woman in the first 6 months of pregnancy when surveyed, households with a woman in the third trimester of pregnancy or who gave birth in the past 3 months when surveyed, and households with a woman who gave birth 3-9 months ago when surveyed. Treatment effects are estimated separately for each group using Equation (2), augmented with interactions with the group of interest as well as indicators for each group. The control means are 40.79 (women) and 46.50 (men). N=876 (women) and N=659 (men). The results shown in this figure are additionally presented in Appendix Table A.10. Spatial HAC standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Figure 8: Welfare Gains from UCTs or Malaria Medicine by Value of a Statistical Life



Notes: Panel A of this figure plots the estimated welfare gains from a \$1,000 investment in UCTs or GiveWell’s top recommended program (as of March 2025), malaria medicine, varying as a function of the value of a statistical life (VSL) on the horizontal axis. We consider three UCT scenarios. First, a UCT excluding spillovers and lives saved (in which \$1,000 of spending generates \$1,000 of benefits), the green line. Second, we plot benefits from a saturated or at-scale UCT assuming the multiplier of 2.5 reported in Egger et al. (2022), and including the benefits of the child mortality reductions estimated in this paper, obtained by multiplying the VSL by 1,000 over the cost per life saved (the thin blue line). Third, We consider a targeted transfer to women in the third trimester of pregnancy, with the same spillover effects from Egger et al. (2022) and child mortality benefits estimated in this paper (the thick blue line). Malaria medicine benefits are estimated using the cost per life saved reported by GiveWell of \$4,304 (“GiveWell directed grants to top charities with impact information (2020 onward),” <https://www.givewell.org/impact-estimates>, accessed June 2025), the red line.

Panel B reports a posterior distribution of the VSL estimates in this sample obtained from Killeen (2025), Kremer et al. (2011), Berry et al. (2020), and Redfern et al. (2019) using Bayesian hierarchical meta analysis with a log-normal prior. The Redfern et al. (2019) estimate is not obtained via revealed preference, so we also report a revealed preference studies cumulative density function.

Table 1: Unconditional Cash Transfers and Mortality, 2015-17

	Reduced-Form		Spatial IV	
	(1) Infant Mortality	(2) Child Mortality	(3) Infant Mortality	(4) Child Mortality
Own village	-5.74 (5.85)	-11.96* (6.38)	-7.98* (4.82)	-12.72** (5.55)
MHT adjusted p-value	[0.234]	[0.110]		
High-saturation spillovers	-12.13** (5.04)	-5.68 (6.66)	-11.49* (6.84)	-12.91 (8.12)
ATE in high-saturation sublocations	-17.87*** (4.94)	-17.64*** (5.86)	-19.46*** (6.94)	-25.63*** (8.54)
MHT adjusted p-value			[0.044]	[0.036]
Percent reduction in HS sublocations	44.44%	30.75%	48.40%	44.67%
Control Mean	40.21	57.37	40.21	57.37
Observations	6,317	6,318	6,317	6,318

Notes: This table is based on the main EL3 birth census sample, which encompasses births from 2015-17 to transfer-eligible households present at baseline. Infant and child mortality estimated effects are reported per 1,000 live births. The ATE in high-intensity villages equals the average total effect of own-village estimates and spillovers in high-saturation sublocations. Columns (1) - (2) report estimates from equation (1) and columns (3) - (4) report estimation from equation (2). MHT corrected p-values in brackets for outcomes that were pre-specified calculated using a [Romano and Wolf \(2005\)](#) step-down correction based on randomization inference with 500 iterations. Reduced form standard errors are clustered at the sublocation level. Spatial HAC standard errors ([Conley, 2008](#)) with a cutoff of 10km are reported for IV estimates. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 2: Heterogeneity: Complementarity with Health Services (Infant Mortality)

	(1) Physician-staffed facility	(2) Hospital
Panel A: Spatial IV, no controls		
Total Effect IV (Time: Above median)	-21.12*** (7.75)	-19.99** (8.41)
Total Effect x Below median time to facility	-2.28 (15.55)	-1.03 (15.28)
Panel B: Spatial IV, double-partial out LASSO controls, with treatment interactions		
Total Effect x Below median time to facility	-29.01* (16.16)	-12.70 (15.73)
Average time to facility (above median, minutes)	54.8	52.1
Average time to facility (below median, minutes)	22.6	19.1
Control Mean, above median time	35.95	37.92
Control Mean, below median time	45.71	43.17
Observations	6,311	6,311

Notes: This table is based on the main EL3 birth census sample for births between 2015-2017. The outcome in both columns is an indicator for infant mortality, scaled to be reported in deaths per 1,000 live births. Column (1) examines the time to a physician-staffed health facility estimated using GPS measurements of travel speeds obtained via the study team during travel throughout the study area. Physician-staffed facilities were measured via clinic surveys in 2024. Estimates in column (2) are similar, but consider time to a hospital. If the facility reported it was open in 2014 and had a physician employed during the survey the facility is included in column 1. Column 2 includes level 4 and higher facilities surveyed, plus those categorized level 4 or higher on the Kenya Master Health Facility List that were unsurveyed. Rows under “Double-partial out LASSO controls, with treatment interactions” include covariates selected by double-partial out LASSO. The possible covariates includes malaria suitability, rainfall, baseline village income and assets, proximity to a road, population, distance to a town, and proximity to a water source. Covariate times treatment interactions are also included. Spatial HAC standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

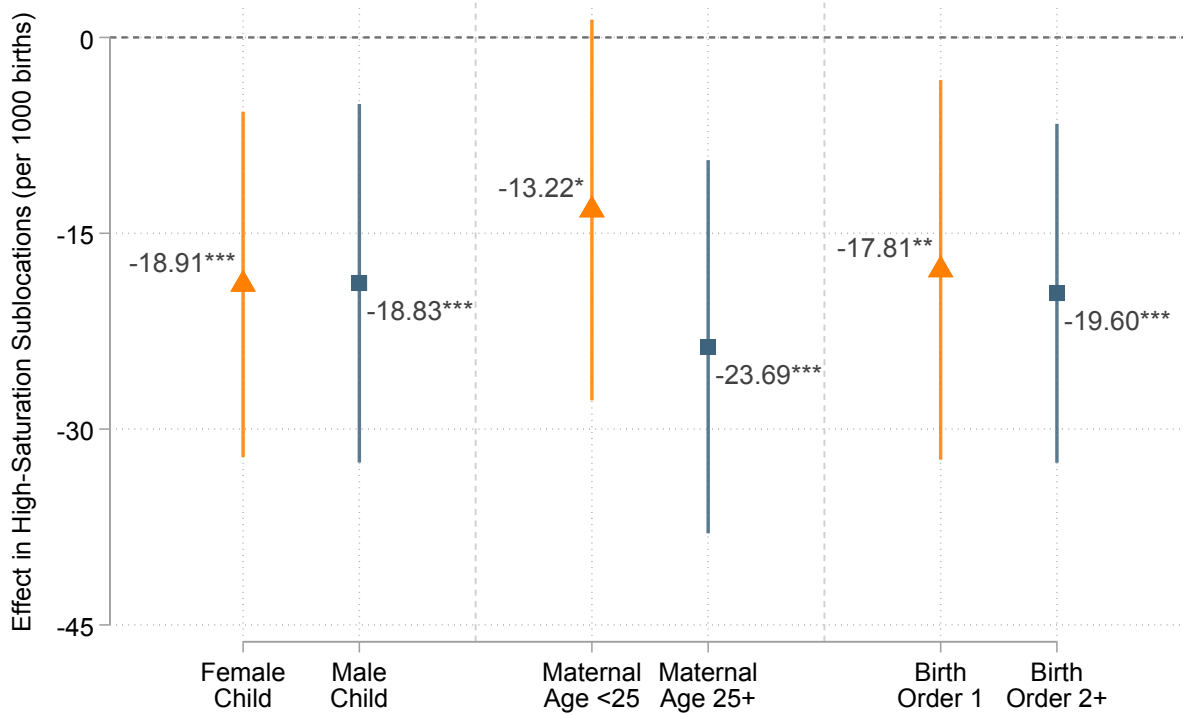
Table 3: Unconditional Cash Transfers and Fertility

	Actual Fertility Among Adult Women			Predicted Fertility Among Mothers	
	(1)	(2)	(3)	(4)	(5)
	2011-14	2015-17	2018-21	2015-17	2018-21
Total Effect IV	0.30 (0.25)	0.80** (0.34)	0.05 (0.27)	0.35 (0.96)	0.11 (0.70)
Control Mean	7.16	7.51	8.28	8.72	9.27
Observations	23662	23662	23662	1354	1354

Notes: Columns 1-3 are estimated on transfer-eligible women from the EL3 census. Columns 4-5 are estimated on transfer-eligible women from the EL3 census from households surveyed at baseline. Fertility outcomes represent the annual probability a woman gives birth to at least one child. The last two columns report predicted fertility among women actually giving birth in a given period, based on a random forest model trained on baseline survey data from women in control, low-saturation villages with six household socio-demographic characteristics used as predictors: baseline household income, baseline household assets, maximum years of education of household members, average household member age, marital status of the primary respondent, and household size. The random forest is trained using five-fold cross-validation, and the model is estimated on indicators capturing all combinations of the six socio-demographic variables, which are each binned into quartiles. Spatial HAC standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

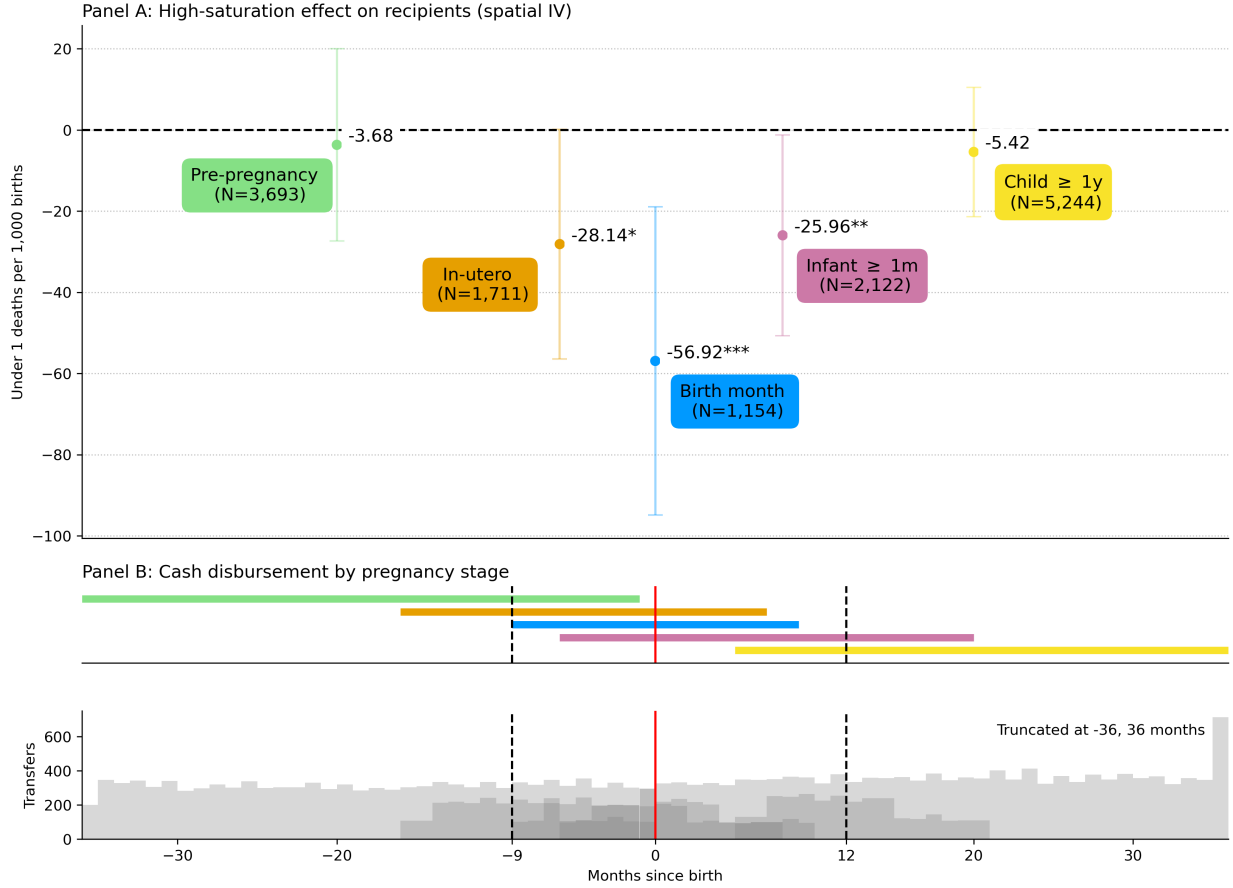
A Additional Exhibits

Figure A.1: Heterogeneity by Gender, Maternal Age, and Birth Order



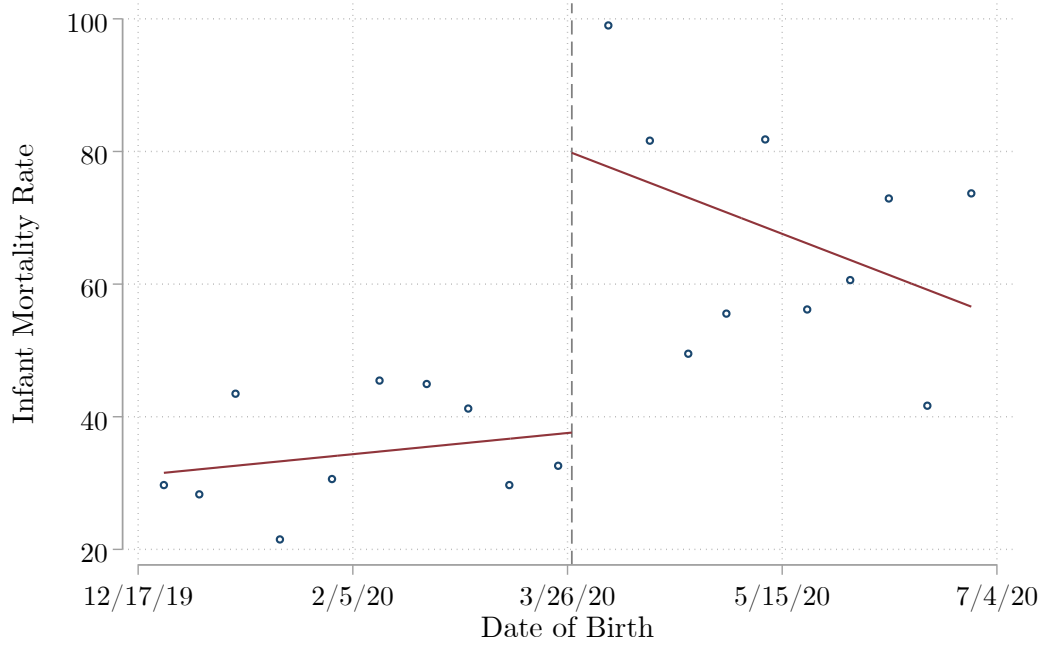
Notes: This figure is based on the main EL3 birth census sample, and plots estimated effects of cash transfers on infant mortality in treatment villages within high-saturation sublocations by gender, maternal age, and birth order. Maternal age at birth is analysed using pre-specified five-year age groups (under 20, 20-24, 25-29, 30-34, and 35 and above); this analysis splits on the median five-year group, with 46% of births having a mother below the age of 25. Child birth order is calculated using the order of births across the 2011-23 period and does not take into account births to mothers prior to 2011 as information on those births was not obtained in the census. Effects are estimated using equation (1) augmented with an interaction with the characteristic of interest. The p -values for the difference across groups (the effect on the interaction term in treatment villages in high-saturation sublocations) is as follows: 0.993 (gender), 0.337 (maternal age), and 0.856 (birth order). The results shown in this figure are additionally presented in Appendix Table A.6. Standard errors are clustered at the sublocation level. 95% confidence intervals are shown. * $p < .10$, ** $p < .05$, *** $p < .01$.

Figure A.2: Transfer Effects on Infant Mortality by Timing of Cash (Spatial IV)

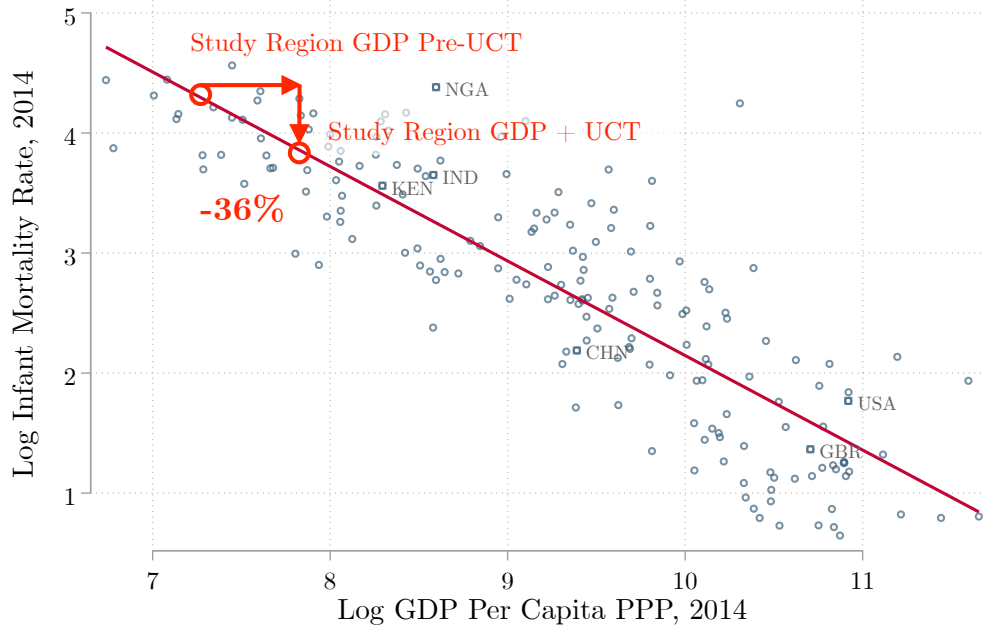


Notes: This figure is based on the main EL3 birth census sample. Panel A plots estimates of dynamics based on the time period of exposure to cash during the child’s life. The transfer timing is defined relative to the “experimental start date”, as this is well-defined for both treatment and control villages. “Pre-pregnancy” includes household exposure 3 years to 10 months before birth. In-utero is 9 to 1 month before birth. Birth month includes cash within the first month of life. Infant includes 1 month to 12 months. Child includes 1 to 3 years. Estimates are constructed using the same approach as equation (4), but using the spatial IV. We estimate equation (2) after restricting the sample to those exposed to cash at a particular time in life. Observations appear in multiple groups since cash transfers were distributed over 8 months. Panel B plots the range of experimental start dates, relative to birth month, included in each estimate and a histogram of transfers by month in each bin. Since the transfers were distributed in a series of three payments, some households in the 1-2 years post-transfer bin were still receiving cash transfers during this period. The spatial IV version of this figure (which is estimated using equation 1) is presented as Figure 3. 95% confidence intervals are shown. * $p < .10$, ** $p < .05$, *** $p < .01$.

Figure A.3: Non-Experimental Variation in Child Mortality



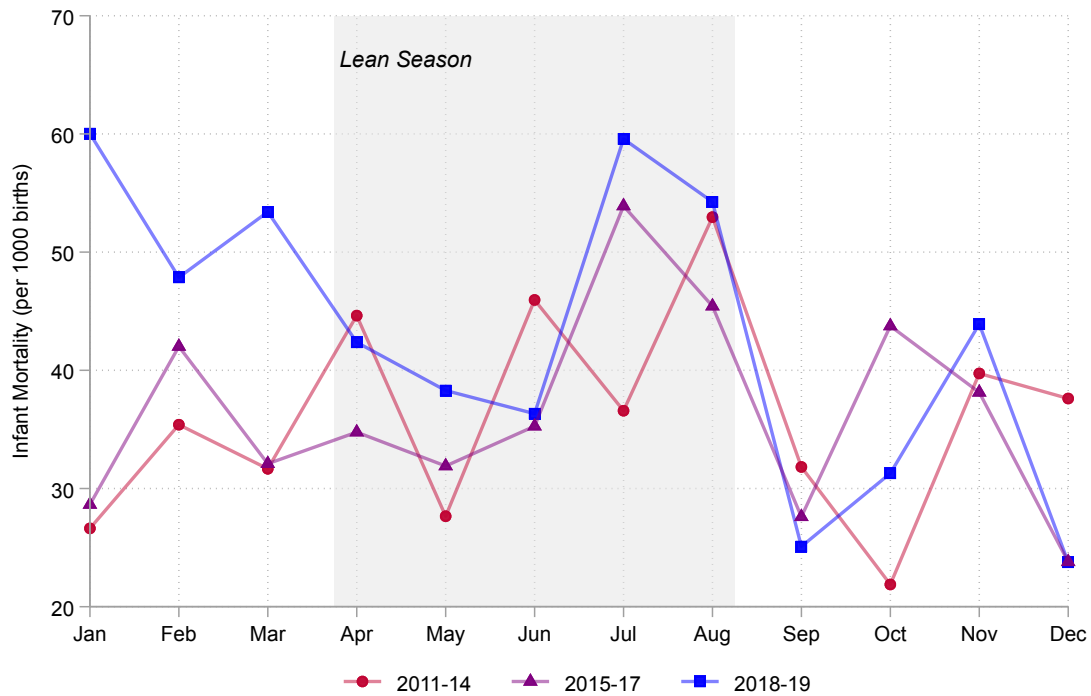
Panel A: COVID-19 Lockdown and Infant Mortality in Study Region



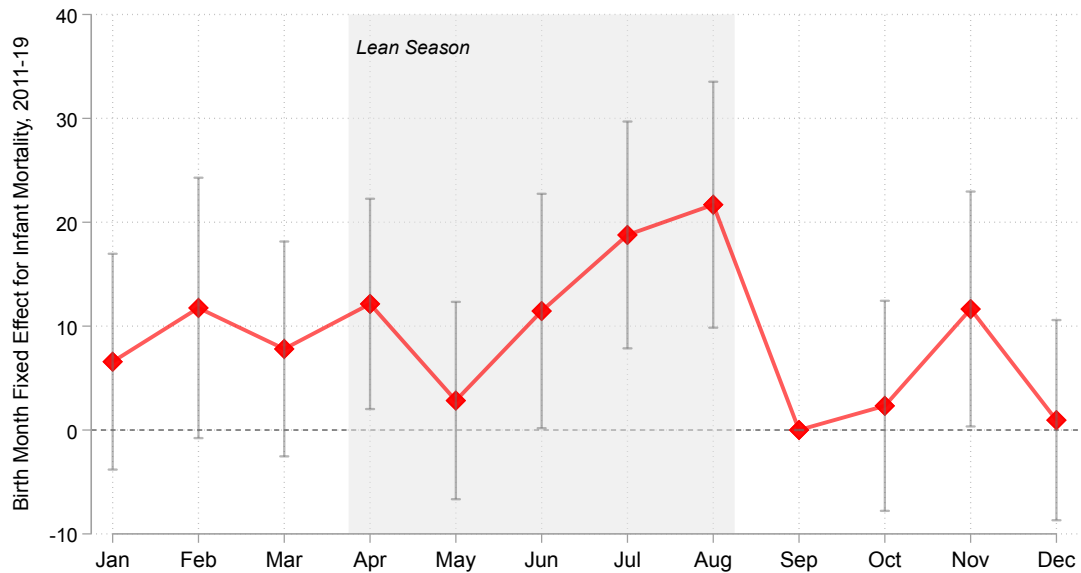
Panel B: Cross-Country GDP - Infant Mortality Relationship

Notes: This figure is based on the main EL3 birth census sample. Panel A reports infant mortality in the study region before and after Kenya's March 2020 COVID-19 lockdown using exact date of birth data from this project's birth census, utilizing all censused births with available data. The data in Panel B are shown as a binned scatter plot. Panel B reports the cross-country association between the natural log of the infant mortality rate (per 1000 births) as reported by the [United Nations \(2024\)](#) and the natural log of GDP per capita (in USD PPP) in 2014 as reported by the [World Bank \(2024\)](#). The "Study Region GDP Pre-UCT" dot highlights the study region's per capita GDP in 2014 as estimated by the [Kenya National Bureau of Statistics \(2019\)](#) as well as predicted infant mortality based on the cross-country relationship. The "Study Region GDP + UCT" dot illustrates the study region's per capita GDP adding a UCT of the same fraction of GDP as the transfer disbursed as a fraction of household consumption (75%) as part of this project (and – consistent with the evidence – assuming it is spent within a year), and the predicted change in infant mortality at that level of income based on the cross-country relationship. Note that the Kenyan national average ("KEN") lies nearly on the regression line and at a higher per capita income level than the rural study region, since the national average also include far richer urban areas like Nairobi.

Figure A.4: Seasonality in Infant Mortality in the Birth Census



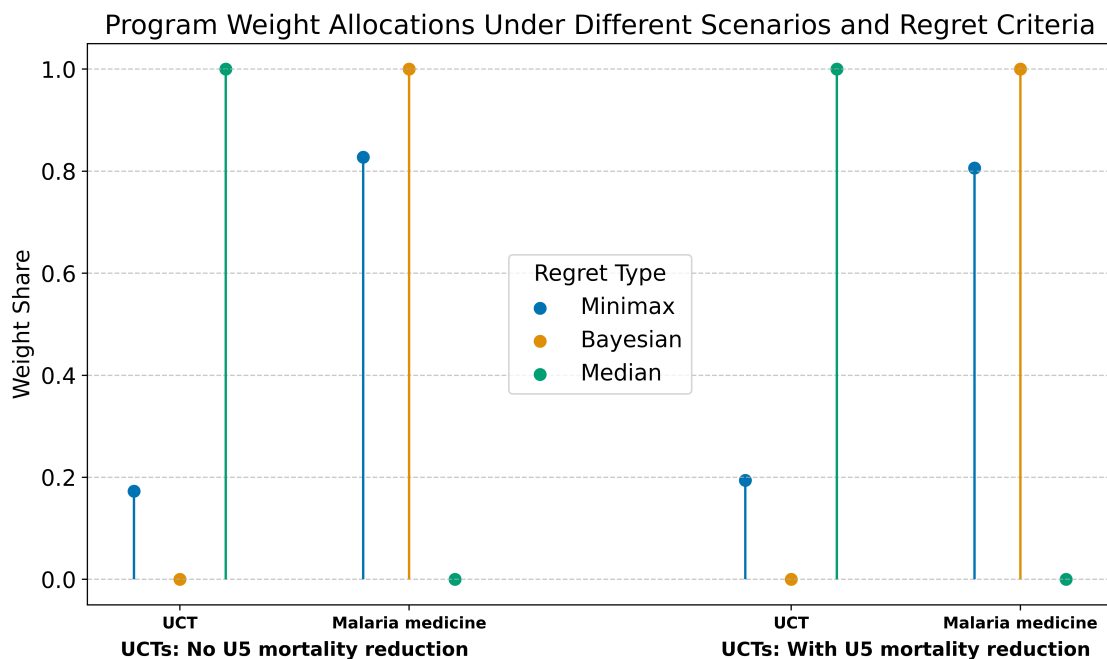
Panel A: Infant Mortality Means By Month



Panel B: Birth Month Fixed Effects

Notes: This figure is based on the main EL3 birth census sample. Panel A reports the mean infant mortality rate by month across 2011-14, 2015-17, and 2018-19. Panel B reports birth month fixed effects controlling for birth year fixed effects, with September as the omitted month. The lean season is defined as April-August using the definition from [Burke et al. \(2019\)](#), whose study area comprised a nearby region in rural Kenya. The years 2020 and 2021 are omitted as COVID-19 pandemic impacted the region, though patterns appear similar if those years are included. 95% confidence intervals are reported in Panel B.

Figure A.5: Share of Funds Allocated to UCTs and Malaria Medicine by Regret Type



Notes: This figure reports the share of funds allocated to UCTs or Malaria medicine by an agent under minimax, Bayesian (mean), and median regret excluding and including the mortality reduction benefits of UCTs. The agent faces uncertainty about the value of a statistical life (VSL), and they minimize the given regret function over the posterior distribution of VSL. We treat other parameters as known. We use GiveWell's estimate of the cost per life saved from Malaria medicine to estimate its returns. The posterior distribution of VSL estimates in this sample obtained from [Killeen \(2025\)](#), [Kremer et al. \(2011\)](#), [Berry et al. \(2020\)](#), and [Redfern et al. \(2019\)](#) using Bayesian hierarchical meta analysis with a log-normal prior.

Table A.1: Descriptive Statistics for the Census of Births

	(1) Infant Mortality	(2) Child Mortality	(3) Neonatal Mortality	(4) Maternal Age at Birth
Births to All Households (2011-23)				
Mean	33.71	46.69	15.11	25.65
Observations	101,336	101,348	100,498	95,353
Births to Households Present at Baseline (2011-23)				
Mean	33.61	46.40	18.50	25.72
Observations	77,847	77,849	77,229	73,247
Births to Eligible Baseline Households, UCT Period (2015-17)				
Mean	34.54	50.68	19.23	25.55
Observations	6,334	6,335	6,240	6,003
Births to Eligible Baseline Households, Pre-UCT Period (2011-14)				
Mean	35.43	54.18	14.11	24.73
Observations	8,634	8,635	8,504	8,208
Births to Eligible Baseline Households, Post-UCT Period (2018-21)				
Mean	37.89	51.54	20.09	26.35
Observations	9,577	9,577	9,457	9,129
Births to Eligible Baseline Households (2011-21)				
Mean	36.15	52.25	17.77	25.57
Observations	24,545	24,547	24,201	23,340
Births to Ineligible Baseline Households (2011-21)				
Mean	31.28	45.44	17.39	25.60
Observations	40,872	40,872	40,712	38,205

Notes: This table reports means for descriptive characteristics and observation counts for groups in the EL3 census of births. The four descriptive characteristics reported are the infant under-one mortality rate (in deaths per thousand births), the child under-five mortality rate (deaths per thousand births), the neonatal mortality rate (deaths per thousand births), and the mean maternal age at birth. The five groups in the census for which these data are presented are as follows: all births recorded in the census (across 2021-23), all births to households present at baseline (2011-23), births to transfer-eligible households present at baseline across 2015-17 (our main sample under study), births to eligible households present at baseline across 2011-14, births to eligible households present at baseline across 2018-21, births to eligible baseline households across 2011-21, and births to ineligible baseline households across 2011-21.

Table A.2: Balance on Tracking, Study Area Presence, and Survey Completion

	(1) Household Tracked (Census)	(2) Present in Study Area (Census)	(3) Completed Census (Census)	(4) Household Tracked (Survey)	(5) Completed Survey (Survey)
Treatment Village	0.001 (0.006)	-0.006 (0.004)	0.006 (0.004)	0.005 (0.005)	0.008 (0.007)
High-Saturation Sublocation	-0.006 (0.008)	0.008* (0.004)	-0.005 (0.006)	-0.003 (0.004)	0.004 (0.007)
Treatment Villages in High-Saturation Sublocations	-0.005 (0.009)	0.002 (0.004)	0.001 (0.007)	0.002 (0.005)	0.012 (0.009)
Control Mean	0.897	0.915	0.905	0.965	0.893
Observations	62742	52582	48125	8372	8075

Notes: This table reports the share of households present at baseline (2014-15) tracked in the third endline (EL3) census, present in the study area at EL3, completed the EL3 census, tracked in the EL3 survey, and completed the EL3 survey, as well as balance by treatment status. The share tracked in the census (column 1) is reported as a share of baseline households non-deceased in the second endline census (EL3). The share present in study area (column 2) is reported as a share of non-deceased baseline households tracked in EL3. The share completing the EL3 census (column 3) is reported as a share of baseline households present in the study area, excluding holiday homes which do not comprise a household's primary residence. The share tracked in the EL3 survey (column 4) is reported as a share of baseline households sampled for the EL3 survey. The share completing the EL3 survey (column 5) is reported as a share of baseline households tracked in the survey. Observations encompass split households, which were created since 2014-15 from the 65,383 parent households originally present at baseline. Standard errors are clustered at the sublocation level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.3: Transfers and Mortality Including Secondary Outcomes, 2015-17

	Primary outcomes		Secondary outcomes	
	(1) Infant Mortality 2015-17	(2) Under-5 Mortality 2015-17	(3) Neonatal Mortality 2015-17	(4) Days survived under-5 2015-17
Panel A: Reduced-form results				
Treatment village	-5.74 (5.85)	-11.96* (6.38)	-3.56 (4.00)	17.63* (10.22)
MHT adjusted p-value	[0.234]	[0.110]		
High-saturation sublocation	-12.13** (5.04)	-5.68 (6.66)	-6.00 (3.75)	11.78 (10.50)
Treatment village in high-saturation sublocation	-17.87*** (4.94)	-17.64*** (5.86)	-9.56** (3.80)	29.41*** (9.75)
Panel B: Spatial IV results				
Spatial IV: Own village effect	-7.98* (4.82)	-12.72** (5.55)	-3.56 (3.62)	17.83* (9.55)
Spillover effect	-11.49* (6.84)	-12.91 (8.12)	-11.00* (6.16)	26.94** (13.12)
ATE on eligibles	-19.46*** (6.94)	-25.63*** (8.54)	-14.57** (5.75)	44.78*** (13.47)
MHT adjusted p-value	[0.044]	[0.036]		
Control Mean	40.21	57.37	23.05	1,735.17
Observations	6,317	6,318	6,237	6,257

Notes: This table is based on the main EL3 birth census sample and includes estimated effects on pre-specified secondary outcomes. Reduced form standard errors are clustered at the sublocation level. Spatial HAC standard errors (Conley, 2008) with a cutoff of 10km are reported for IV estimates. MHT corrected p-values in brackets. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.4: Transfers and Mortality Excluding Stillbirths, 2015-17

	Reduced-form		Spatial IV	
	(1) Infant Mortality 2015-17	(2) Under-5 Mortality 2015-17	(3) Infant Mortality 2015-17	(4) Under-5 Mortality 2015-17
Own village	-6.85 (5.53)	-13.10** (6.13)	-9.02** (4.44)	-13.79*** (5.26)
High-saturation spillovers	-9.63** (4.84)	-3.18 (6.52)	-8.35 (5.47)	-9.84 (7.33)
ATE in high-saturation sublocations	-16.48*** (4.64)	-16.28*** (5.60)	-17.38*** (5.73)	-23.63*** (8.11)
Percent reduction in HS sublocations	52.04%	33.24%	54.88%	48.24%
Control Mean	31.66	48.98	31.66	48.98
Observations	6,266	6,267	6,266	6,267

Notes: This table is based on the main EL3 birth census sample and estimates effects on infant and child mortality excluding stillbirths. The sample excludes 51 stillbirths that were classified as such by the SmartVA verbal autopsy algorithm as opposed to directly reported by the household as a stillbirth. Reduced form standard errors are clustered at the sublocation level. Spatial HAC standard errors ([Conley, 2008](#)) with a cutoff of 10km are reported for IV estimates. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.5: Mortality Effects of Cash Transfer Spillovers on Non-Recipients

	(1) Infant Mortality 2015-17	(2) Under-5 Mortality 2015-17	(3) Neonatal Mortality 2015-17	(4) Days survived under-5 2015-17
Total effect IV	0.74 (3.84)	2.20 (4.96)	-4.04 (3.01)	-1.15 (7.27)
Control Mean	31.82	46.84	19.21	1,753.45
Observations	13,191	13,192	13,102	13,154

Notes: This table reports estimates of spillover effects of the cash transfers on non-recipients of cash (both ineligible households and eligible households in control villages) on mortality for births across 2015-2017. Estimates are constructed using a spatial instrumental variable approach similar to Equation 2. Spatial HAC standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.6: Heterogeneity by Gender, Maternal Age, and Birth Order

	Infant Mortality			Child Mortality		
	(1) Male	(2) Maternal Age 25+	(3) Birth Order 2+	(4) Male	(5) Maternal Age 25+	(6) Birth Order 2+
Treatment Village	-12.80* (7.02)	-5.80 (7.26)	-14.34* (8.39)	-12.92* (7.09)	-12.92* (7.09)	-22.77*** (8.27)
High-Saturation Sublocation	-6.11 (6.98)	-7.42 (6.06)	-2.34 (7.62)	1.35 (7.56)	1.35 (7.56)	2.74 (9.32)
Treatment Village x Heterogeneity	8.53 (9.04)	-4.57 (9.57)	9.98 (9.26)	-3.06 (10.32)	-3.06 (10.32)	13.63 (10.54)
High Saturation Sublocation x Heterogeneity	-8.45 (9.09)	-5.90 (9.71)	-13.68 (10.64)	-10.36 (11.00)	-10.36 (11.00)	-11.34 (11.51)
Treatment Villages in High-Saturation Sublocations	-18.91*** (6.75)	-13.22* (7.44)	-16.68** (7.52)	-11.56 (8.52)	-11.56 (8.52)	-20.03** (8.96)
Treatment in High-Saturation Sublocations Including Heterogeneity Characteristic	-18.83*** (7.01)	-23.69*** (7.29)	-20.39*** (6.70)	-24.99*** (8.48)	-24.99*** (8.48)	-17.74** (7.62)
Control Mean	39.95	39.95	39.95	57.00	57.00	57.00
Observations	6256	6331	6256	6332	6332	6257

Notes: This table is based on the main EL3 birth census sample. Effects are estimated using Equation (1) augmented with an interaction with the characteristic of interest. Maternal age at birth is analysed using pre-specified five-year age groups (under 20, 20-24, 25-29, 30-34, and 35 and above); this analysis splits on the median five-year group, with 46% of births having a mother below the age of 25. Child birth order is calculated using the order of births by mother across the 2011-23 period and does not take into account births to mothers prior to 2011 as information on those births was not obtained in the census. Standard errors clustered at the sublocation level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.7: Unclassified Verbal Autopsies by Treatment Status

	(1) Undetermined VA (2011-14)	(2) Undetermined VA (2015-17)	(3) Undetermined VA (2018-21)
Treatment Village	0.06 (0.05)	-0.10* (0.05)	-0.03 (0.05)
High-Saturation Sublocation	0.03 (0.05)	0.08 (0.06)	0.03 (0.05)
Treatment Villages in High-Saturation Sublocations	0.09 (0.06)	-0.02 (0.07)	0.00 (0.05)
Control Mean	0.11	0.21	0.16
Observations	263	201	325

Notes: This table reports regressions exhibiting the association between treatment status and the probability that a completed verbal autopsy (VA) had an undetermined cause by the SmartVA classification algorithm for transfer-eligible under-5 deaths across three periods: 2011-14 (pre-UCT), 2015-17 (the period contemporaneous with UCT disbursement), and 2018-21 (post-UCT). Standard errors are clustered at the sublocation level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.8: Cross-Sectional Relationship Between Socio-Demographic Status and Infant Mortality in Control Eligible Households

	(1) Under-1 Mortality	(2) Under-1 Mortality	(3) Under-1 Mortality	(4) Under-1 Mortality
Transfer-Eligible	10.54** (5.25)	10.58** (5.31)		
Below-Median Baseline Assets			50.88** (23.74)	60.51** (25.34)
Mean in Low SES Group	39.95	39.95	81.38	81.38
Mean in High SES Group	29.40	29.40	30.50	30.50
Observations	5202	5202	489	489
Birth Characteristic Controls	NO	YES	NO	YES

Notes: This table reports regressions exhibiting the cross-sectional association between infant mortality and markers of socio-demographic status for births to eligible households in control low-saturation villages across 2015-17. Columns 1-2 present the association between eligibility for the unconditional cash transfers (determined by having a thatched roof) and infant mortality. Columns 3-4 present the association between holding a level of assets at baseline below the median level and infant mortality within the sample of transfer-eligible households surveyed in the representative baseline survey sample. Columns 1 and 3 present bivariate relationships, whereas Columns 2 and 4 include pre-specified controls for birth and maternal characteristics (i.e., gender, year of birth, and maternal age). Robust standard errors are utilized. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.9: Heterogeneity: Complementarity with Health Services (Delivery in indicated facility type)

	(1) Physician-staffed facility	(2) Hospital
Panel A: Spatial IV, no controls		
Total Effect IV (Time: Above median)	-0.04 (0.07)	0.11 (0.09)
Total Effect x Below median time to facility	0.07 (0.12)	0.27** (0.14)
Panel B: Spatial IV, double-partial out LASSO controls, with treatment interactions		
Total Effect x Below median time to facility	0.04 (0.14)	0.33** (0.13)
Average time to facility (above median, minutes)	55.5	52.6
Average time to facility (below median, minutes)	22.5	18.8
Control Mean, above median time	0.26	0.39
Control Mean, below median time	0.30	0.45
Observations	1,075	1,110

Notes: The outcome is an indicator for delivery in a physician-staffed facility in column (1) and a hospital in column (2). Estimates in column (1) use the time to a physician-staffed health facility estimated using GPS measurements of travel speeds obtained via the study team during travel throughout the study area. Physician-staffed facilities were measured via clinic surveys in 2024. Estimates in column (2) are similar, but consider time to a hospital. If the facility reported it was open in 2014 and had a physician employed during the survey the facility is included in column 1. Column 2 includes level 4 and higher facilities surveyed, plus those categorized level 4 or higher on the Kenya Master Health Facility List that were unsurveyed. Rows under “Double-partial out LASSO controls, with treatment interactions” include covariates selected by double-partial out LASSO. The possible covariates includes malaria suitability, rainfall, baseline village income and assets, proximity to a road, population, distance to a town, and proximity to a water source. Covariate times treatment interactions are also included. Spatial HAC standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.10: UCTs and Household Labor Supply by Gender Around the Time of Birth

	(1) Women Hours Worked Last Week	(2) Men Hours Worked Last Week
<i>Panel A: First 6 Months In-Utero</i>		
Total Effect IV	-7.06 (11.14)	1.94 (18.56)
<i>Panel B: 3rd Trimester & First 3 Months Postpartum</i>		
Total Effect IV	-20.79** (8.64)	1.40 (11.61)
<i>Panel C: Next 6 Months Postpartum</i>		
Total Effect IV	-8.48 (10.82)	-3.15 (16.01)
Control Mean	40.79	46.50
Observations	876	659

Notes: This table encompasses households surveyed in the first endline survey (2016-17) with a recorded birth in the EL3 census during 2015-17. Labor supply encompasses hours worked across the household (in agricultural employment, non-agricultural self-employment, and wage employment), as well as hours spent searching for work, in the week prior to the first endline survey. The table displays the total estimated effect of the cash transfers on labor supply for three groups: households with a woman in the first 6 months of pregnancy when surveyed, households with a woman in the third trimester of pregnancy or who gave birth in the past 3 months when surveyed, and households with a woman who gave birth 3-9 months ago when surveyed. Treatment effects are estimated separately for each group using Equation (2), augmented with interactions with the group of interest as well as indicators for each group. Spatial HAC standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.11: Association Between Treatment Status and Baseline Characteristics For Eligible Households with 2015-17 Births

	(1) Education	(2) Age	(3) Marital Status	(4) Income	(5) Assets	(6) Household Size
Total Effect IV	0.05 (0.33)	-5.07 (4.18)	0.05 (0.04)	-1642.99 (2452.42)	-7399.24 (4573.36)	0.09 (0.28)
Control Mean	8.50	26.69	0.72	12326.22	33837.74	4.34
Observations	1354	1354	1354	1354	1354	1354

Notes: This table encompasses transfer-eligible households which reported a birth from 2015-17 in the EL3 census and were surveyed at baseline. Regressions are reported exhibiting the association between treatment status and six baseline household socio-demographic characteristics (maximum years of education of household members, average household age, number of adults, number of children, baseline household assets, and baseline household income). Spatial HAC standard errors (Conley, 2008) with a cutoff of 10km are reported for IV estimates. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.12: Association Between Treatment Status and Predicted Probability of Giving Birth Among Mothers Giving Birth Over 2015-17

	(1) Probability of Birth Among Actual Mothers Socioeconomic Variables	(2) Probability of Birth Among Actual Mothers LASSO Variables
Total Effect IV	0.35 (0.96)	0.11 (0.10)
Control Mean	8.72	7.10
Observations	1354	1354

Notes: This table encompasses mothers in transfer-eligible households present at baseline sampled in the baseline survey, and reports regressions exhibiting the association between treatment status and the probability of birth among mothers who actually gave birth over 2015-17. In column 1, the probability of birth is predicted using six baseline household socio-demographic characteristics (maximum years of education of household members, average household member age, marital status of primary respondent, baseline household income, baseline household assets, and household size) using a random forest trained on women in control, low-saturation villages. In column 2, the probability of birth is predicted linearly using LASSO from a set of 243 baseline household- and village-level characteristics. Spatial HAC standard errors ([Conley, 2008](#)) with a cutoff of 10km are reported for IV estimates. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.13: Impacts on Infant Mortality in Household Survey Sample

	(1) Pre-Specified Controls	(2) Socioeconomic Controls	(3) PDS LASSO- Selected Controls
<i>Panel A: Spatial IV Estimates</i>			
Total Effect IV	-36.16* (18.81)	-35.41* (19.16)	-37.83** (18.70)
<i>Panel B: Reduced-Form Estimates</i>			
Treatment Village	-25.76* (12.97)	-26.13* (13.24)	-26.48** (12.94)
High-Saturation Sublocation	-10.97 (11.81)	-8.10 (11.83)	-11.96 (11.60)
Treatment Villages in High-Saturation Sublocations	-36.73** (17.91)	-34.23* (17.90)	-38.44 (17.95)
Control Mean	65.83	65.83	65.83
Observations	1486	1486	1486

Notes: This table is based on births reported in the EL3 birth census to transfer-eligible households present at baseline and surveyed in the baseline household survey. Effects estimated by Equation (2) (Panel A) and Equation (1) (Panel B) of the cash transfer on infant mortality rates are reported. Mortality data are sourced from the endline 3 census whereas household- and village-level control variables are sourced from the baseline survey. Column 1 presents infant mortality results in the baseline survey sample with only pre-specified (e.g., year of birth fixed effects, birth gender, mother age group) covariates. Column 2 additionally includes controls for six baseline household socioeconomic characteristics (maximum years of education of household members, primary respondent age, marital status of primary respondent, income, assets, and household size), along with the pre-specified controls. Column 3 includes controls selected by PDS LASSO, with a total of 243 baseline variables available for selection, along with pre-specified controls. For consistency across the two equations, we use the PDS LASSO covariates selected for Equation (1) for Equation (2) as well. Spatial HAC standard errors (Conley, 2008) with a cutoff of 10km are reported for IV estimates, and standard errors clustered at the sublocation level are reported for reduced-form estimates.* $p < .10$, ** $p < .05$, *** $p < .01$.

B Comparison with Non-Experimental Variation

The existence of birth census data over the period 2011-2023 enables us to benchmark the experimental cash transfer treatment effect estimates against several dimensions of non-experimental variation in economic circumstances. In short, across multiple sources of variation, child survival appears to respond substantially to economic conditions in rural Kenya, with non-experimental differences exhibiting the same sign and similar magnitudes as the main cash transfer experimental results reported above.

B.1 Cross-Sectional Variation

One form of variation is cross-sectional: transfer-eligible households were notably poorer on average than ineligible households (as noted above), as measured by either consumption or assets. As illustrated by Columns 1-2 of Appendix Table A.8, the birth census indicates that infants born to ineligible mothers in control villages were substantially less likely to die in their first year of life than children from eligible households in these same villages, but that in the 2015-17 period of transfer disbursal, the treatment effect from transfers is sufficient to close the infant mortality gap between treated eligible and ineligible households. Across 2015-17, under-one mortality among control eligible births was 10.5 deaths per thousand higher than among control ineligibles, a 36% difference statistically significant at the five percent level. This socioeconomic gradient in mortality also holds even when controlling for birth characteristics such as maternal age and gender, and a similar pattern is evident when examining under-one mortality or all the years across the birth census as opposed to the main cash transfer period. Furthermore, even within eligible households, births to households with below-median assets at baseline are substantially less likely to survive their first year in control villages: Columns 3-4 of Appendix Table A.8 show that infant mortality among households with below-median assets is 2.7 times higher (81.4 deaths per thousand births) than among households with above-median assets (30.5 deaths per thousand). Of course, there are many other interpretations of this pattern and many potential omitted variables or confounders — which is precisely why the current study exploits experimental variation to estimate causal impacts — but we note that these cross-sectional observational patterns remain broadly consistent with the large experimental estimates described above.

B.2 Seasonality in Infant Mortality

Child mortality displays a similarly high degree of sensitivity to inter-temporal changes in economic conditions. The information on exact month of birth for most children in the birth census allows us to estimate month of birth effects for both infant and child mortality (controlling for several other characteristics such as birth year, gender, and maternal age). [Burke et al. \(2019\)](#) define the pre-harvest “lean season” in rural western Kenya as encompassing the months of April through August, after which the main grain harvest occurs. The analysis of the birth census during the pre-COVID period of 2011-19 reveals a meaningful degree of seasonality in infant mortality: rates of mortality for infants born in August (often the very peak of the lean season just before the harvest) are 21.9 deaths per thousand births higher than for infants born in the next month (Appendix Figure A.4). This difference — which is similar in magnitude to the cash transfer treatment effect we estimate — is statistically significant at the one percent level, and forms part of a larger pattern of mortality rising

as the lean season progresses: from May through August, infant mortality rates rise by an average of 6.44 deaths per thousand each month. Under-five survival rates display virtually identical patterns.

B.3 Aggregate Economic Shocks

Mortality in the birth census also responds to major aggregate economic shocks: as is evident in Panel A of Figure 1, infant mortality rises across the board in 2017, a year in which a severe drought affected rural Kenya (BBC, 2017). Furthermore, the COVID-19 pandemic struck Kenya in early 2020, leading to a large recession as well as reduced access to many public services. The birth census data indicates that infant mortality nearly doubled from 2019 to 2020, with the largest jump occurring precisely in April 2020, the first full month of pandemic related lockdowns. Mortality remains elevated in 2021, a year in which the COVID-19 pandemic continued to affect Kenya and another severe drought impacted the region (World Bank, 2022).

As data were collected on exact date of birth for the majority of children censused, we can more precisely examine the impact of the COVID-19 shock on child health outcomes. On March 27, 2020, the Kenyan government imposed a strict lockdown intended to combat the spread of COVID-19, including a nationwide curfew during evening and night hours (Achuka and Gisesa, 2020). Individuals who left their homes during the curfew were subject to a maximum of three months imprisonment, a Ksh. 1000 (USD PPP 23.5) fine, or both, and local media reported numerous instances of police brutality meted out on Kenyans breaking curfew (Mireri, 2020).

A regression discontinuity analysis indicates that the infant mortality rate sharply increased immediately after the lockdown. Panel B of Figure A.3 illustrates that infant mortality doubled in the study region the week after the lockdown, a result statistically significant at the five percent level. These effects are likely caused by changes to health care access rather than COVID-19 deaths per se: reported COVID-19 deaths exhibit no such discontinuity at the lockdown date, and mortality from COVID-19 is largely concentrated among the elderly as opposed to children (Ma et al., 2021). Furthermore, the jump in infant mortality is particularly high in areas near a physician-staffed health facility, suggesting that a decline in access to care may have contributed to part of the deterioration in child health outcomes. The sharp rise in infant mortality in the study setting during the COVID-19 lockdown is consistent with non-experimental evidence from India suggesting that mortality among infants at six months rose 44% during India’s lockdown, with the authors positing that reduced healthcare access and utilization may have driven this decline in child survival (Asker et al., 2025).

B.4 Cross-Country Association Between Income and Health

We last examine the cross-country relationship between per capita GDP and infant mortality. As previously documented by others, income and health are closely associated with each other (Preston, 1975; Cutler et al., 2012). Data on per capita GDP is obtained from the World Bank (2024), whereas infant mortality estimates are sourced from the United Nations (2024).³⁷ In 2014, the year in which cash transfers in our study region commenced,

³⁷We also utilize an estimate of the study region’s GDP, which is obtained from the Kenya National Bureau of Statistics (2019).

each log point increase in GDP per capita was associated with a 0.79 log point reduction in infant mortality, a relationship statistically significant at the one percent level and with an R-squared of 0.74. As seen in Panel A of Figure A.3, augmenting the study region’s GDP by the same proportion as the cash transfers did for treated households would result in a 36% decline in infant mortality as predicted by the global cross-country GDP-mortality relationship. While the cross-country association is correlational rather than causal in nature, the magnitude of this predicted reduction is notably similar to our experimental estimates.

In summary, child mortality appears to be highly sensitive to economic conditions in this study’s context, irrespective of the source of variation. Non-experimental approaches recover similar magnitudes as the large experimental effects estimated in this study.

C Details of Benefit-Cost Calculations

This appendix details the estimates presented in Section 7. We first describe how the estimated posterior distribution of VSLs presented in Figure 8 is constructed and detail the benefit curves in the figure. We then describe the econometric decision theory problem producing the weights plotted in Figure A.5.

The posterior distribution of VSLs is constructed using Bayesian hierarchical meta analysis. We begin by positing that VSLs in countries with similar income levels follow a log-normal distribution with weakly informative priors. The log-normal distribution imposes that VSL is never negative and captures the possibility of a large right tail. We assume that the location parameter of the distribution is normally distributed with a mean of 10 and standard deviation of 10, and the scale parameter is 5 times a half-Cauchy distribution with location parameter 0 and scale parameter 0.5. A half-Cauchy distribution with a scale parameter of 0 and scale of 0.5 is a recommended best practice for imposing weakly informative priors (Reis et al., 2022). We multiply these draws by 5 to ensure priors do not favor VSL values near 0. Estimates of the posteriors are not sensitive to these decisions, consistent with the fact that the prior is only weakly informative.

We then use Markov Chain Monte Carlo methods in the Python package “pymc” to estimate the posterior distribution. We do so by matching the likelihood to estimates from Killeen (2025), Kremer et al. (2011), Berry et al. (2020), and Redfern et al. (2019). We utilize these estimates because they were obtained from populations with similar income levels. For instance, we exclude León and Miguel (2017) because the population studied has a much higher income level, and Killeen (2025) shows that this predicts a VSL orders of magnitude higher. We also restricted our focus to revealed preference estimates, other than Redfern et al. (2019), since they reflect real world choices rather than hypothetical questions that may be prone to social desirability bias.

Figure 8 then plots welfare gains of UCTs and other programs by VSL. For UCTs, we plot estimates without child mortality reductions priced in imposing the 2.5 consumption multiplier from Egger et al. (2022), the same with our estimates of the cost per life saved of the UCTs, and targeted transfers to pregnant women that have a consumption multiplier of 1 with and without lives saved priced in. We also plot the benefit curve by VSL of GiveWell’s top recommendation, the Malaria Consortium, since this is one of the most effective health interventions and GiveWell reports estimates of the cost per life saved. We assume no direct consumption benefit and therefore all benefits are $(1,000/\text{cost per life saved}) \times \text{VSL}$.

Figure A.5 plots the share of funds that a decision maker would invest in UCTs or the Malaria Consortium by regret type in the following econometric decision problem.

A social planner allocates a budget across J programs, each characterized by its consumption gain and expected lives saved per dollar spent. The planner’s objective is to minimize regret given uncertainty about the true VSL, which we will denote by $\theta \in \Theta$.

Each program $j \in \{1, \dots, J\}$ has two attributes: g_j , the consumption gain per dollar spent, and S_j , the expected number of lives saved per dollar spent.

The planner chooses a weight vector $\mathbf{w} = (w_1, \dots, w_J)$ such that $\sum_{j=1}^J w_j = 1$, where w_j represents the share of the budget allocated to program j .

The true VSL, denoted θ , is uncertain and follows a distribution $\theta \sim F(\theta)$. We use the estimated posterior distribution of θ in calculations, but in minimax regret impose that it falls within the 95% confidence interval since one would otherwise mechanically invest only in the most effective health program since VSL has no upper bound in the log-normal prior assumed. For simplicity, and since we were unable to find confidence intervals on the cost per life saved of the Malaria Consortium program, we take point estimates of consumption multiplier and cost per life saved as given.

For a given realization of θ , the optimal allocation $\mathbf{w}^*(\theta)$ maximizes total benefit:

$$\mathbf{w}^*(\theta, \mathbf{S}) = \arg \max_{\mathbf{w}} \sum_{j=1}^J w_j (S_j \theta + g_j).$$

The regret for a given $(\theta, \mathbf{S})$ and choice $\mathbf{w}$ is:

$$R(\mathbf{w}, \theta) = \max_{\mathbf{w}'} \sum_{j=1}^J w'_j (S_j \theta + g_j) - \sum_{j=1}^J w_j (S_j \theta + g_j).$$

We consider three regret criterion. First, the minimax regret criterion selects $\mathbf{w}$ to minimize the worst-case regret:

$$\min_{\mathbf{w}} \sup_{\theta} R(\mathbf{w}, \theta).$$

where we use the 5th and 95th percentile of VSL values estimated in the posterior distribution to define the respective worst case draws of θ for each program. This case reflects the optimal allocation for an extremely risk averse decision maker that wishes to maximize welfare in the worst possible case.

Second, the Bayesian (average) regret criterion minimizes the expected regret given the posterior distribution of VSL:

$$\min_{\mathbf{w}} \mathbb{E}_{\theta} [R(\mathbf{w}, \theta)].$$

In practice, we use 1,000 posterior draws from the posterior to estimate this value. This value would be optimal for a risk neutral decision maker that wishes to maximize expected welfare gains, but is heavily influenced by the outlying right tail of VSL values. In practice, this results in the Malaria Consortium’s program receiving 100% of investment in all cases, even though it is dominated by UCTs for 75% of VSL values simulated from the posterior distribution.

Third, we consider the allocation that minimizes the median regret over θ :

$$\min_{\mathbf{w}} \text{median}_{\theta} R(\mathbf{w}, \theta).$$

This provides a regret measure that is less sensitive to outliers in VSL.

We construct these estimates including and excluding the benefits of mortality reductions from UCTs. In practice, this has little effect on results since the cost per life saved is lower under the Malaria Consortium than UCTs.