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CAN CASH TRANSFERS SAVE LIVES? EVIDENCE FROM A LARGE-SCALE  
EXPERIMENT IN KENYA

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



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
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
Can Cash Transfers Save Lives? Evidence from a Large-Scale Experiment in Kenya Michael W. Walker  Nick Shankar  Edward Miguel  Dennis Egger  Grady Killeen  
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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 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. These improvements appear to arise from better maternal health and nutrition, a 53% reduction in labor supply around the time of childbirth, and a 45% increase in hospital delivery. There is a transient 10% increase in fertility, but mortality reductions are largest in households receiving transfers after pregnancies began, ruling out selection bias. Mortality effects dissipate rapidly when cash is distributed more than three months from the birth. Mortality reductions are concentrated among households with limited (monetary) consumption gains, suggesting a trade-off between financial and health investments.

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A data appendix is available at <http://www.nber.org/data-appendix/w34152>

A randomized controlled trials registry entry is available at <https://www.socialscisceregistry.org/trials/505>

# 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 (Burstein et al., 2019). However, causal 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 credible exogenous 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 their effects 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 benefits 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.

This article presents one of the first experimental studies of the effects of unconditional cash transfers on child mortality. We evaluate the mortality effects of a program that distributed one-time UCTs, equal to 75% of average annual household expenditure, to over 10,000 households from randomly selected villages in western Kenya between 2015 and 2017.<sup>1</sup> This study leverages new census data recording over 100,000 births spanning more than a decade to examine the short- and medium-run effects of these UCTs on child survival.

The central empirical finding of this study, which was pre-specified, is that the cash transfer treatment led to a large and significant reduction in infant and child mortality during the cash transfer period. Results from the study’s primary specification, which accounts for spillover

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<sup>1</sup>Egger et al. (2022) document large economic gains among recipient and non-recipient households and local firms one and a half years on average after the transfers were implemented.

effects within- and across-villages, indicate that infant mortality fell by over 19 deaths per thousand births among recipient households with a pregnancy during the transfer disbursal period. This represents a statistically significant 48% decline in infant mortality relative to the mean in control villages ( $p$ -value  $< 0.01$ , multiple-hypothesis testing (MHT) adjusted  $p = .04$ ). We find similar results for under-five mortality (a reduction of 45%, MHT adjusted  $p = .04$ ), 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 sublocation level (the econometric models are described in detail below). However, impacts are transitory: effects on mortality dissipate rapidly after the UCT period ends or, at the household level, when a household received cash before a female member became pregnant. This result aligns with prior evidence that households’ marginal propensity to consume is high in this setting (Egger et al., 2022), suggesting that UCTs need to be delivered near the time of birth to yield mortality benefits in settings where saving is limited.

Several features of the research design and additional analyses indicate that the main result is unlikely to be confounded by factors such as measurement error or experimenter demand effects. The birth census — based on family recall of vital events — follows the same methodology used by prominent data sources such as the Demographic and Health Surveys (Romero Prieto et al., 2021). Evidence from the public health literature further supports the reliability of such data, as births and deaths are sufficiently salient to be accurately reported (Rao et al., 2003; Lyons-Amos and Stones, 2017; Nareeba et al., 2021). The large-scale, longitudinal design of the census also allows for extensive validity checks. We find no significant pre-treatment differences in infant or child mortality between treatment and control households during 2011–2014, or in households receiving cash just after a child turns 1 (or 5 respectively). In addition, the data are internally consistent across both cross-sectional and intertemporal dimensions: mortality varies as expected with household wealth, seasonality, and aggregate shocks. Finally, the estimates align closely with magnitudes implied by non-experimental variation, such as the cross-country income–mortality gradient, providing further reassurance about their validity.

The second part of the paper examines the mechanisms that might explain these reductions in child mortality. We first assess whether selection into fertility, an important determinant of child health in other settings (e.g., Baird et al., 2019), can account for effects. We document a modest, transient 10% increase in the share of women giving birth during the cash transfer period that dissipates thereafter and leaves completed lifetime fertility as well as total fertility over the entire post-UCT period unchanged.<sup>2</sup> Importantly, selective fertility choices cannot

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<sup>2</sup>This pattern aligns with prior evidence on limited fertility responses to income shocks in poor countries (Chatterjee and Vogl, 2018; Carneiro et al., 2021).

account for the decrease in mortality: the fertility effects appear mostly *after* the peak of the reduction in child mortality, which is concentrated among households whose eight-month window of transfer disbursement overlapped with an infant’s birth month. Because households were informed of their eligibility within a month of their first payment, treatment status was unknown to these households at the time these pregnancies began, a clean test ruling out selection effects. Consistent with this interpretation, we find no significant change in fertility within nine months of a household’s start date, nor any significant differences in mothers’ observable characteristics.

Second, we examine changes in healthcare utilization. To do so, we bring in multiple rounds of household survey data (covering a representative sample of over 10,000 households), surveys of local health clinics and hospitals, and a dataset of drive times to health facilities obtained by equipping surveyors with GPS devices over the course of hundreds of trips throughout the study area. The most recent survey round (2024-25) collected particularly detailed information on antenatal, delivery, and postnatal healthcare utilization for births during the 2011-2023 period. During the census, we also completed verbal autopsies (VAs) for child deaths using the current World Health Organization methodology and assigned causes of death using a machine-learning classification algorithm ([World Health Organization, 2022](#); [Institute for Health Metrics and Evaluation, 2025](#)).<sup>3</sup>

Several analyses indicate that access to and utilization of antenatal and delivery services played an important role in increasing child survival. First, 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 driven by birth complications and neonatal deaths, with mortality in the corresponding cause category falling by 75%. Second, 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 delivery services than local clinics but are far more expensive ([Institute for Health Metrics and Evaluation, 2014](#)). The increase in hospital deliveries occurred mainly among households with below-median travel time to such a facility. However, while mortality reductions are also somewhat larger in areas near hospitals, they remain large (and not statistically distinguishable) in regions of the study area far from hospitals where there is no significant evidence of greater healthcare use. Healthcare utilization alone, therefore, cannot fully explain the observed child mortality reductions.

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<sup>3</sup>In 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.

Third, we turn to improved maternal health and nutrition as a plausible additional mechanism. [Egger et al. \(2022\)](#) document large overall improvements in nutrition and food security in recipient households for both adults and children. In this paper, we show that women around pregnancy particularly benefited in terms of improved health. Among female respondents in a household with a child aged one or younger, the transfers increase a pre-specified health index by 0.28 standard deviation units ( $p$ -value  $< 0.05$ ) at the first endline survey. Compared to the control group, these women experienced 66% fewer major health problems since baseline ( $p$ -value  $< 0.05$ ). In contrast, there is no significant impact on the health index among male respondents with infants in the household, nor among female respondents without infants in the household.<sup>4</sup> Several additional results support the view that increases in maternal health likely played a role. First, as noted above, the VA data shows that the overall reductions in mortality are driven overwhelmingly by a decrease in maternal and neonatal causes. In particular, reductions in stillbirths and pre-term delivery drive a meaningful share of the fall in mortality, and these are strongly linked to maternal health ([László et al., 2013](#); [Premji, 2014](#); [Buffa et al., 2018](#)). Second, the estimated mortality reductions are almost twice as large among older mothers, for whom birth complications are more prevalent, although differences by age are not significant.

Last, we examine maternal leisure, which may have reduced physical strain and contributed to improved maternal health during pregnancy. We estimate a large and significant 24 hour per week reduction ( $p$ -value  $< 0.05$ ) in female labor supply among recipient households with a woman in the third trimester of pregnancy or the first three months following birth. This sharp decline – a 53% decrease compared to the control mean – is neither observed among men nor among women outside these months. This suggests that cash transfers may have enabled women at a critical period in pregnancy to reallocate work hours to rest (or other non-work activities). 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](#)). Leisure is thought to be a critical input in the production of child survival ([Miller and Urdinola, 2010](#)), which is consistent with the literature on the positive impacts of parental leave documented in both rich and poor countries ([Ruhm, 2000](#); [Tanaka, 2005](#); [Rossin, 2011](#); [Nandi et al., 2016](#); [Bartel et al., 2023](#)).

This study provides some of the first *causal* evidence that income affects child mortality. Previous non-experimental studies of UCTs have documented a relationship ([Richterman et al., 2023](#)), raising the question of whether distributing cash is sufficient on its own to reduce

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<sup>4</sup>The absence of significant impacts on self-reported health among the full sample of adult recipients was reported by [Egger et al. \(2022\)](#).

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). Furthermore, McIntosh and Zeitlin (2024) report the results of a randomized evaluation of a UCT program in Rwanda suggesting a 70% decline in child mortality among a sample of children that were already born when transfers were distributed, although the result is not significant once corrections for multiple hypothesis testing are applied (and is based on a relatively small sample, with 13 deaths in the control and 2 in the treatment group).

The expansive data analyzed in this study also allows us to understand the timing of and mechanisms underlying mortality effects, which may have policy implications. First, we find that effects are transitory, highlighting the importance of appropriately timing aid to yield infant mortality reductions. Second, evidence suggests that receipt of cash during pregnancy is important to reduce maternal physical strain, indicating that schemes that deliver cash to women only at or after birth may be sub-optimal. Third, we examine whether UCTs are effective as a substitute or complement to health infrastructure investments. We find some evidence that effects may be larger when health infrastructure is available, consistent with evidence on the importance of health infrastructure quality (Okeke, 2023). Yet effects are large and significant even far from such resources, where survey evidence shows little evidence of increased utilization. This suggests that UCTs may also be an effective tool to increase child health even in settings where health infrastructure is relatively limited. Further, 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 the many other possible benefits of UCTs, i.e., on consumption.

This study also contributes to the literature on the timing of cash transfers and savings in low- and middle-income countries. Research shows that poor households often struggle to save without commitment devices (Ashraf et al., 2006) and that improving savings can enhance health investments (Dupas and Robinson, 2013). These features have influenced the design of UCT programs such as the one studied in this paper, which often distribute cash in large lump sums to facilitate costly investments. In line with such savings barriers mattering for UCT efficacy, recipients often prefer lump sum transfers and sometimes prefer to delay them, suggesting attempts to align them with investment needs (Kansikas et al., 2025). Yet little direct evidence exists about whether aligning UCT timing with actual investment opportunities consequentially affects program impacts, in part because it is challenging to identify exactly when lump sums are most needed. Our study setting of investment in child health therefore offers a useful opportunity since the time at which investments are needed, around the birth of a child, is well-defined. We document that cash receipt aligned

precisely with critical stages of pregnancy leads to large reductions in infant mortality, but the effect vanishes if a household’s final cash transfer was received before the third trimester of pregnancy. This finding suggests that the timing of transfers is important to their efficacy and that the high marginal propensity to consume observed for UCTs in low-income settings — which contributes to large transfer multipliers (Egger et al., 2022) — may, at the same time, limit child survival gains.

Finally, the results indicate that many households face a trade-off between using cash transfers to increase monetary consumption versus the consumption of non-monetary amenities, such as rest time, that have consequential effects on health but are infrequently captured in surveys. Economists have long recognized that GDP and poverty metrics omit consumption of non-monetary amenities such as leisure (Jones and Klenow, 2016; Deaton, 2016), and that such goods may be especially consequential for health in developing countries (Gertler and Gruber, 2002). A small applied microeconomic literature also shows that the value of non-work time can represent a large share of the hourly wage in LMIC settings (Agness et al., 2025). Yet the extent to which such omissions change core results drawn from consumption measures is not well documented. We provide evidence that pregnant women increase consumption of non-work time (leisure) in response to UCT income gains, likely contributing to the substantial inter-generational health benefits that we document. Moreover, improvements in child survival are found precisely among those households with the smallest predicted monetary gains from transfers (over 1.5 years on average), suggesting a tradeoff between investments in maternal and child health versus future household income-generating activities. This pattern suggests that targeting UCTs solely based on their later monetary impacts (i.e., consumption, income or assets), as is common in practice (e.g., Haushofer et al., 2025), may lead to significant misallocation by neglecting the welfare gains from non-monetary investments that are consequential to recipient health and long-term inter-generational outcomes including child survival.

## 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 treatment villages, GD enrolled all households meeting its thatched-roof eligibility criteria (“eligible” households); slightly more than one third of all households were eligible. Data from our surveys indicate that they are indeed poorer on average than other local house-

holds (“ineligible households”). For instance, assets per capita among eligible households at baseline were 64% lower than ineligible households.

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: USD 80 a few weeks after enrollment and two equal sized tranches of USD 460 after two and eight months respectively. 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 they were distributed.<sup>5</sup>

Villages were phased into treatment starting in late 2014 and throughout 2015. The bulk of the payments were sent out during 2015 and 2016. The years 2015-2017 are thus most relevant for understanding the short-term impacts of UCTs on child mortality as many women who received cash transfers while pregnant during 2016 gave birth in 2017. Births from subsequent years allow us to test whether the effects of UCTs are persistent.

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.<sup>6</sup>

The Kenyan government has implemented several programs with the goal of reducing user fees for antenatal and delivery care.<sup>7</sup> However, implementation challenges and a lack of clarity around benefits means that in practice many women still pay substantial amounts out-of-pocket for these services (Orangi et al., 2021). In KGES surveys (described below), the

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<sup>5</sup>More details on program design and implementation are in Egger et al. (2022).

<sup>6</sup>A 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). Note that this implies that the median village has no health facility.

<sup>7</sup>In 2013, the Kenyan government introduced program to eliminate user fees, which became the “Linda 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).

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 transport (Miller et al., 2024). To illustrate the importance of travel costs in this setting (in which few households own cars or motorcycles), in 23% of control group births respondent mothers reported traveling by foot to a facility for delivery.

## 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 comprised of 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 local 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 cash transfer exposure over space, taking advantage of the idiosyncratic variation in local village assignments 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, and the vast majority of spending occurring locally within the study area. At endline, an average of 1.5 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% increase 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, higher profits for firms, and higher wages, income and expenditure for non-recipient households. There is a positive but small 0.2% average increase in prices 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. This 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 and healthcare utilization. We discuss each of these data sources 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 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 movers). 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 (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, 2022](#)). 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 many LMICs 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.<sup>8</sup>

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 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 disbursement 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/1,000 births and the child (under-five) mortality rate was 46.7 deaths/1,000 births. These recorded infant and child mortality rates are similar to those estimated in Kenya by the United Nations Inter-Agency Group for Child Mortality Estimation ([2025](#)).<sup>9</sup>

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<sup>8</sup>The 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.

<sup>9</sup>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 maintained this sampling frame of households present at baseline while 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. The EL3 household survey tracking rate was over 90% and balanced across treatment and control groups (Appendix Table [A.2](#)).

Household survey data from the KGES project provides two main benefits to the analysis. 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 questions 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. Endline 1 (2016-17) provides short-term data on household living standards, assets, labor supply, health, 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 (e.g. hospital delivery, and antenatal and postnatal care). We specified the outcomes we would focus on as mechanisms in a pre-analysis plan ([Egger et al., 2024](#)).

## 3.2 Estimation Framework

### 3.2.1 Primary Econometric Specifications

We first estimate the following reduced-form specification, focusing attention on births in eligible households that were present at the time of the cash transfers:

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

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, 26-30, 31-35, or above 35).<sup>10</sup> 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.

The coefficient  $\alpha_1$  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 any within-village spillover effects. The coefficient  $\alpha_2$  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  $\alpha_1$  and  $\alpha_2$  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.<sup>11</sup>

Equation (1) is a straightforward and intuitive benchmark yet it does not capture the full spatial dimension of spillovers. One-third of villages in low-saturation sublocations still received transfers, and there is additional variation stemming from by the idiosyncratic placement of treatment and control villages as well as sublocation boundaries. In the following

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<sup>10</sup> $M_i$  is a vector of indicators for a missing value of a covariate. Where a covariate is missing, we set the value equal to the covariate's mean.

<sup>11</sup>Among other main analyses, we pre-specified a version of Equation (1) that clustered standard errors at the village level and focused on  $\alpha_1$  while considering  $\alpha_2$  as non-primary. We adjust standard errors to cluster more conservatively at the sublocation level as our analytic focus expanded to include  $\alpha_2$ .

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. We utilize the following regression specification:

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

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

The key coefficients are  $\beta_1$ , 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  $\beta_r$  terms, which capture the effects of cash transfers in other villages (not  $v$ ) at different bands of radius  $r$  ( $Amt_{v,r}^{-v}$ ) away from village  $v$ .  $ShareElig$  denotes the share of baseline households eligible for a transfer in an area. 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).<sup>12</sup> The Schwarz BIC algorithm indicates that infant and child mortality effects are locally concentrated within 2 km of cash transfer receipt in nearly all cases, as was also the case for most outcomes in [Egger et al. \(2022\)](#).

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.<sup>13</sup> To account for spatial correlation, we calculate spatial heteroskedasticity- and autocorrelation-consistent (HAC) standard errors using a positive definite kernel up to 10 km ([Conley, 2008](#)).<sup>14</sup>

The analysis focuses on the “average total effect” of the cash transfers on births in recipient

<sup>12</sup>For computational reasons, once the BIC increases at a radius, we stop searching and select the minimizing value over the earlier radii searched.

<sup>13</sup>[Egger et al. \(2022\)](#) only uses the share of eligible households treated as an instrument. By logic similar to [Abadie et al. \(2023\)](#), the revised instrument vector is efficient under homogeneous treatment effects because it captures the true first stage. The original PAP specified the instruments from [Egger et al. \(2022\)](#), but we noted that this was an active research area and we might consider estimates based on recent advances. An amendment filed before the data was analyzed (but after data collection began) documented our intent to switch to the specification presented here. Standard errors are somewhat larger with the original approach.

<sup>14</sup>We 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 km 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.

households in high-saturation sublocations, which is defined as:

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

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. 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 this quantity is a lower bound on the true effect of the program.

When working with the full census sample of births to estimate overall effects, we mainly focus on Equation 2 since it captures all variation in cash exposure. However, this estimator relies on asymptotic consistency which, combined with the spatial dependence of the data, may yield biased estimates in finite samples. We therefore focus on reduced-form estimates from Equation 1, which have better finite sample properties, when considering heterogeneity estimates that effectively divide the sample into much smaller subgroups. We present equivalent heterogeneity results for census outcomes using Equation 2 in the Appendix, but omit similar heterogeneity results for the household survey data (given that the effective subgroup sample sizes fall considerably).

### 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 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 group  $G$  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  $G$ ):

$$y_{imhvs} = \alpha_{1,G}Treat_v + \alpha_{2,G}HighSat_s \tag{4}$$

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

We focus on the treatment effect in high-saturation sublocations for each age group, namely,  $\alpha_{1,G} + \alpha_{2,G}$ , since some cells end up with modest sample sizes, favoring the better finite sample properties of this reduced-form specification (similar to the issues around estimating heterogeneity across subgroups noted above). 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 sized 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.<sup>15</sup>

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<sup>15</sup>The 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 this could in principle separately identify the effects of cash at different stages, a recent literature in econometrics such as [Callaway and Sant’Anna \(2020\)](#) — which studies dynamic difference-in-differences but generalizes to this setting where observations that received cash in different stages serve as controls in joint estimation — shows that such methods can generate biased estimates. In fact, when we estimate the PAP specification, coefficient estimates suggest that cash *increased* deaths in each pregnancy stage, which is soundly rejected by our overall estimates and consistent with consequential bias. 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 drought in 2017 which elevated infant mortality).<sup>16</sup> Once the disbursement 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. There are large impacts on infant survival during the period in which UCTs were disbursed. The coefficient estimates range from 15 to 24 fewer 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  $< 0.01$ , MHT adjusted  $p = .04$ ), as shown in the regression analysis below. Though we lack the statistical power to detect differences in coefficients across years within the transfer disbursement period, the year with the largest treatment coefficient (2017) is also the year in which a drought affected the region, which is consistent with the view that UCTs reduce infant mortality most in settings with the

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<sup>16</sup>Note that droughts have become increasingly frequent due to climate change (Nkiaka et al., 2022). Within East Africa, the frequency of droughts has doubled since 2005, from every six years to every three years (United Nations Office for Disaster Risk Reduction, 2023). This could jeopardize the long-standing trend toward reduced infant and child mortality in the region.

greatest economic adversity, a point we return to below. The magnitude is similar and the statistical significance of the result remains unchanged ( $p$ -value  $< 0.01$ ), however, even when the sample is restricted to the more economically favorable years of 2015 and 2016. 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.<sup>17</sup>

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.<sup>18</sup> The overall effect is driven both by the direct effect of cash transfer receipt including spillovers within treated villages ( $\alpha_1$ ), and the effect of cross-village spillovers ( $\alpha_2$ ); 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 ( $\beta_1$ ) and from other nearby villages ( $\beta_2$ ) 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

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<sup>17</sup>Appendix Figure A.1 presents the main estimates for infant mortality 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). Appendix Table A.4 reports results separately for 2011-14, 2015-16, 2017, and 2018-21.

<sup>18</sup>Distinguishing 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.5 and find similar results in terms of magnitude and statistical significance.

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.

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.<sup>19</sup> 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 these non-recipient households overall, who as noted above were, on average, considerably richer at baseline in addition to having substantially lower infant and child mortality rates (see Appendix Table [A.6](#)).<sup>20</sup> We do, however, find some evidence of infant mortality reductions among the poorest ineligible households who at baseline possessed a non-thatched roof but walls and floors made of mud (Appendix Table [A.7](#)), which is consistent with the finding that a non-negligible share of the impacts on eligible households was driven by spillovers.<sup>21</sup>

The mortality effects estimated during the UCT period are broad-based across various pre-specified child and mother characteristics. As Appendix Figure [A.2](#) 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 both exhibit statistically and economically significant declines. As older mothers are more prone to birth complications triggered by strain ([Yaman et al., 2025](#)), this suggestive pattern is consistent with an important role being played by maternal health, which relates to the cause of death findings and evidence on mechanisms presented below.

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<sup>19</sup>We 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.

<sup>20</sup>Results 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, on average, be underpowered to detect. We did not pre-specify heterogeneity analyses within the ineligible population.

<sup>21</sup>Estimated spillovers for eligibles, as well as for the poor ineligibles, are not significant in the 2011-14 pre-UCT period, which serves as a useful balance check. In Appendix Table [A.4](#), the spillover coefficient for eligibles in the pre-period estimated using Equation [1](#) is in fact positive in sign (+5.08, SE 4.79), whereas pre-period spillovers estimated using Equation [2](#) are similarly modest and not significant (-5.22, SE 5.01).

### 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 2 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  $\geq 1$  month”), and (v) one to three years after birth (“child  $\geq 1$  year”). We focus on reduced-form estimates since the sample sizes in some groups are modest; Appendix Figure A.3 reports results using the spatial IV, which yields similar results.

Infant mortality declines by 39.7 deaths/1,000 births among children receiving cash in their birth month ( $p < .01$ ), suggesting that mortality fell to the low levels seen in industrialized nations. There is some evidence of mortality declines when cash is received in-utero (-13.1 deaths/1,000 births), although many children in this group were also exposed to cash during their birth month since the transfer window was 8 months. Strikingly, we estimate essentially no reduction in mortality in cases where a household received transfers before the pregnancy began. This result holds even if one considers the set of births where the last cash transfer was distributed 12 to 4 months before birth (before the last trimester of pregnancy), and we can reject the null hypothesis that the coefficient is equal to that of birth month receipt with 95% confidence (Appendix Figure A.4). Echoing the limited child mortality reductions after the UCT window, this result indicates that transfers are only effective when aligned with critical periods of pregnancy and a child’s life, and suggests that households likely did not save a meaningful share of transfers, even when a pregnancy was already known. There are also limited effects when cash was distributed in a child’s first year of life.<sup>22</sup> Moreover, and consistent with the absence of pre-treatment differences, there is no estimated effect of the UCT on infant (under-1) survival if a household received the transfer after the child’s first year of life, which again serves as a useful specification check. Spatial IV results are similar, but with somewhat stronger evidence of effects for children exposed to cash in-utero or as infants (Appendix Figure A.3).

How is the apparent lack of savings which constrains effects of UCTs on infant mortality

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<sup>22</sup>In Appendix Figure A.4, we additionally consider cases where the first transfer was received 4-12 months after birth and again find little evidence of a mortality reduction and can reject equality with the “birth year” coefficients. This result stands in contrast to the findings of McIntosh and Zeitlin (2024), which estimated a 70% reduction in mortality among children already born in households that received UCTs in Rwanda (although that result does not survive multiple hypothesis corrections and is based on a very small sample).

documented here reconciled with evidence that consumption is higher even 1.5 years on average after the transfers went out (Egger et al., 2022)? For one, the consumption effects dissipate over time, so households may lack resources for lumpy investments in health further removed from the transfers even if living standards are somewhat higher. Second, longer-run consumption gains may reflect investments in productive assets that modestly raise consumption but do not ease liquidity constraints as dramatically as UCT receipt. Third, the households with longer lasting consumption gains may differ in various ways from those that make health investments to reduce child mortality. In fact, we provide evidence below that child mortality reductions are concentrated among households with the lowest average 1.5 year consumption gains, consistent with this view.

In all, 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, and we return to these issues below.

#### 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).<sup>23</sup>

We focus on five pre-specified cause of death (COD) groups. The broad COD distribution seen in this study’s VA data is 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. Pre-term delivery and stillbirths (if included) are the leading maternal and neonatal causes, accounting for 60% of this category. The second-largest COD group, encompassing 36% of deaths with non-missing causes,

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<sup>23</sup>The likely cause of death is standardized, as defined by the International Classification of Diseases, tenth edition (ICD-10).

is communicable and nutritional diseases 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 the algorithm was unable to determine a likely cause due to missing or inconsistent answers (18% of completed VAs).

Figure 3 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/1,000 births in this category, a 75% decline relative to the control mean.<sup>24</sup> 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.9). 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.

These patterns confirm the central role of the birth event and the circumstances around it, and the importance of alignment of cash with birth timing. They also highlight the potential role of maternal health, a key correlate of neonatal deaths discussed further below.

## 4.5 Heterogeneity by Socioeconomic Status and Economic Treatment Effects

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 from the household survey, and second, classifications of surveyed households as “most deprived” in terms of baseline consumption (which was not collected in the baseline KGES survey) from Haushofer et al. (2025).<sup>25</sup> More specifically, Haushofer et al. (2025) used detailed baseline household survey data from KGES to predict levels of per capita consumption using machine learning (ML) methods (generalized random forests). The bottom half of households were classified as “most deprived”.

<sup>24</sup>This result is consistent with the finding that neonatal mortality, i.e., in the first 30 days of life, declines by 14.6 deaths/1,000 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).

<sup>25</sup>This analysis was not pre-specified. We focus on assets and consumption since Haushofer et al. (2025) find the most evidence of predictable heterogeneity along these dimensions

A related question is whether households with the largest positive treatment effects on medium-term economic outcomes (assets and consumption) experience differential impacts on child mortality. The hypothesis is that households in rural Kenya may face a trade-off between investing the cash transfers in child and maternal health versus investing in medium-term income generating activities or savings. [Haushofer et al. \(2025\)](#) classifies households by predicted treatment effects on assets and consumptions at endline (1.5 years on average after transfers) into those ‘most vs. least impacted’, again via ML methods.<sup>26</sup> They show that there is a strong correlation between being ‘most deprived’ and ‘least impacted’, i.e., the poorest households tend to gain least in terms of future consumption and assets.

As ML classification by deprivation and impact rely on data from the household survey, we 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 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.18](#) (column 1). The proportional reduction on 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 the household survey subsample (65.8 per 1,000 births).

Figure [4](#) then presents heterogeneous treatment effects along the dimensions noted above: Panel A examines heterogeneity by deprivation, namely, the baseline value of assets and predicted per-capita consumption (absent treatment) from [Haushofer et al. \(2025\)](#).<sup>27</sup> 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 consistent pattern of larger reductions for poorer households: by both measures, the point estimate for poorer households is larger in magnitude than that of the richer households and the differences are statistically significant at the 5% level. Strikingly, nearly the entire reduction in infant mortality appears concentrated among the poorer households.<sup>28</sup> This pattern

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<sup>26</sup>As the methodology generates multiple predictions for each household and outcome, we classify households as more deprived or impacted if the share of model runs that classify them as such exceeds the median.

<sup>27</sup>We 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.

<sup>28</sup>In Appendix Figure [A.7](#), we report similar tests using predicted endline assets, which yields similar results, and by observed baseline and predicted endline income. [Haushofer et al. \(2025\)](#) find less evidence of predictable heterogeneity with respect to household income, which is more challenging to measure and fluctuates substantially in this setting. Consistent with heterogeneity being driven by true difference in household wealth, we find little evidence of heterogeneity along these income dimensions.

also persists when controlling for the other dimension of heterogeneity. Estimates show evidence of substantial heterogeneity by deprivation even controlling for the corresponding impacted classifications interacted with treatment assignment, and vice-versa. However, because the deprivation and impact measures are correlated, i.e., the most-deprived are more likely to be less-impacted (see [Haushofer et al. \(2025\)](#)), standard errors increase somewhat when these controls are added (see Appendix Figure [A.8](#)).

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 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.

Figure [4](#) Panel B, shifts the focus to heterogeneity based on predicted impacts on endline 1 assets and consumption. This analysis indicates that infant mortality effects are largest in the groups with the smallest estimated economic impacts at endline 1. In fact, point estimates are essentially zero for the “most impacted” groups, and differences between the most versus least impacted are significant at the 10% and 5% levels for assets and consumptions, respectively. This result has two implications. First, this finding supports concerns that welfare measures based on consumption and assets typically omit non-monetary goods such as leisure or health that are challenging to measure ([Deaton, 2016](#); [Jones and Klenow, 2016](#)), which may cause targeting policies to misallocate transfers towards households whose gains are easier to measure. Second, the fact that the households experiencing the largest reductions in child mortality had smaller medium-term economic gains suggests that they may have faced a stark trade-off between savings and investment in future economic gains versus investment into maternal and child health (possibly also through increases in leisure consumption by mothers shortly before and after birth, which we document below).

Taking the previous subsections together – on transfer timing, cause of death, and socioeconomic heterogeneity – suggests that returns to targeting cash could be particularly high if a policymaker were to target pregnant women from the poorest households. But in practice, pregnancy is a verifiable condition at relatively low cost, whereas it is likely far more expensive to identify more impoverished 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 easy-to-observe grass thatched roofing but the baseline value of household assets and income and the various living standards predictions require

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.11).

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 has largely arrived), 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 major 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.5, Panel A.).

Lastly, 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.5, 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.

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 multiple 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 begin by examining the selection of mothers into giving birth, and find that this cannot explain the main result. Second, we examine changes in healthcare utilization. We document large increases in hospital delivery and somewhat larger declines in child mortality for households near hospitals. Yet it is noteworthy that mortality reductions remain large in places far from hospitals where healthcare utilization does not increase. Healthcare utilization may thus amplify the impact of UCTs on child mortality but cannot fully explain them. We then turn to channels that are directly linked to maternal and child health: maternal labor supply, maternal self-reported health, and both maternal and child nutrition. We find large improvements in each measure consistent with these improvements playing a role in driving the mortality reductions.

### 6.1 Fertility Patterns

Prior research examining the effects of cash transfers has found that selection into fertility can play an important role in child health (e.g., [Baird et al., 2019](#)). While we do find evidence that the cash transfers resulted in a modest transitory increase in fertility, features of the experimental design and analyses of the composition of women giving birth are inconsistent with fertility driving the reductions in child mortality observed in this paper.

First, we turn to the fertility effects of cash. We observe modest increases in births in the 2015-17 cash transfer period. Appendix Table [A.10](#) reports estimates (from Equation (2)) indicating that among recipient households in high-saturation sublocations the share of women giving birth rose by 10% and births per 100 women grew 13% relative to the low-saturation control mean. Fertility patterns are indistinguishable by treatment status in the 2011-14 pre-period and again in the 2018-23 post-UCT period.<sup>29</sup> The impacts in the

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<sup>29</sup>Similar results are obtained using the reduced-form Equation (1).

UCT period are transient: fertility remains unchanged when we look at the entire 2015-23 post-transfer period, and completed lifetime fertility among women who had reached age 45 in 2023 is similarly unmoved. Cash thus appears to have shifted the timing of births to a degree, but not increased the overall number of children born in the medium to long run.<sup>30</sup>

The timing of the transient fertility gains, however, does not align with the timing of the infant mortality reductions. As reported in Panel A of Figure 5, the rise in fertility is a gradual one. Consistent with the lack of selection bias, the treatment coefficient in the first nine months after transfers begin is not statistically significant and is in line with pre-period coefficients. It is only 27 to 45 months after transfers start that fertility exhibits a significant increase. By contrast, the fall in infant mortality occurs immediately after transfers commence, and in fact the largest estimated effect is observed in the first nine months following the start of transfers (Figure 5, Panel B). These births represent a useful test for selection effects: cash transfers were distributed over an eight month period, with households learning their treatment status less than a month before transfers started in their village. Mothers giving birth within 9 months of *any* transfers starting in the village therefore did not know their treatment status when pregnancy began, ruling out any endogenous selection into pregnancy. There are also significant reductions in infant mortality during the latter months of the cash transfer treatment period in which selection into fertility is a more distinct possibility.<sup>31</sup>

Several additional analyses indicate that differential selection into fertility is not a first-order determinant of child survival in this setting. We analyze the characteristics of women giving birth in 2015-17 in the representative survey sample. First, households with a birth during 2015-17 do not significantly differ by treatment status across six baseline socio-demographic characteristics: education, age, marital status, income, assets, and household size (Appendix Table A.15). Next, we use these six baseline household characteristics to predict the probability of giving birth over 2015-17 for all adult females in the census, 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

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<sup>30</sup>The fertility effects we estimate are not adjusted for age. Given the finding that fertility was shifted somewhat forward on average, controlling for age could introduce post-treatment bias, attenuating the estimated impact on fertility. Furthermore, the EL3 census did not collect detailed age data for all women who did not give birth, and hence we are unable to create fully age-adjusted fertility rates. We did collect full age data for all women who gave birth and therefore control for maternal age in the main child mortality regressions in an attempt to partly account for selection.

<sup>31</sup>Infant mortality remains lower by 10 to 17 per 1000 births — about a 30% reduction relative to the control mean — in the cash transfer period more than 9 after UCTs start, while fertility effects peak at 13%. Such a modest increase in fertility seems quantitatively unlikely to explain these large infant mortality reductions.

of Appendix Table A.16 that the predicted probability of birth among actual mothers does not significantly differ by treatment status in the transfer disbursement period ( $p$ -value = 0.47). 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 = 0.78). Predicting birth probability using LASSO from a set of 243 baseline household- and village-level characteristics, as we do in Appendix Table A.17, yields similar results indicating an absence of treatment impacts on the characteristics of women who gave birth.

Finally, we run Equation (2) for the representative baseline survey sample only, augmented with additional baseline controls to test whether the inclusion of household baseline characteristics affects the main results (Appendix Table A.18). 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 inclusion, and the results are also unaffected, with estimated drops if anything slightly larger (57%). Panel B of Appendix Table A.18 shows that these results are all robust to estimation using equation (1) rather than the spatial IV design.

Overall, selective fertility thus does not appear able to explain the large drops in infant and child mortality documented in this study.

## 6.2 Healthcare Access

We next examine whether the transfers allowed households to increase healthcare utilization. The endline 3 long-form household survey asked 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 caesarean sections, C-sections, which were not pre-specified but are another potentially important channel), obtained from 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%).<sup>32</sup> These

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<sup>32</sup>There is a small 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

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.<sup>33</sup>

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, we tabulate only 4.3 births per week at the average hospital in our study area during the 2015-17 period, suggesting limited scope for congestion. Moreover, the results show that any such congestion effects are smaller than the gains experienced by cash transfer recipients. Specifically, we do not document any adverse net impacts of the program among ineligible households who did not receive a transfer (Appendix Table A.6). In fact, Appendix Table A.7 documents that the poorest ineligible households — who experienced more modest but still sizable reductions in infant mortality, as noted above — were also able to deliver in hospitals at a significantly higher rate during the cash transfer period.

Does the expansion in institutional delivery alone explain the large reduction in child mortality? 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; 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 actual 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 to each facility.

Table 2 column (1) shows that reported increases in hospital delivery are concentrated

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mortality rates are balanced in the pre-period (Appendix Figure A.1).

<sup>33</sup>In terms of other health care utilization outcomes, the table shows that cash transfers do not significantly affect the proportion of pregnancies where the WHO recommended number of at least four antenatal visits occurred. Point estimates also suggest a decline of roughly half in C-section delivery rates, although differences are not statistically significant. 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.

among households with below-median travel time (mean travel time 23 minutes) to such a facility, with no significant effect in areas far from hospitals (mean travel time 55 minutes). If hospital delivery were the main driver of lower mortality, one would therefore expect mortality reductions to likewise be concentrated in areas near hospitals. But Table 2 column (3) shows that there is a large and statistically significant reduction of 20 infant deaths per 1,000 births even in areas that are farther from hospitals. Panel B further 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). There is stronger evidence that access to care may increase effect sizes in this specification, although differences are not significant. We repeat this analysis using distance to a physician-staffed facility (which [Okeke \(2023\)](#) suggests is particularly important to birth outcomes). Effects remain large and significant far from such facilities, although the estimated interaction with controls in Panel B again suggests that effects are stronger when households can more easily access care ( $p < 0.1$ ).

The evidence therefore suggests that healthcare access can be complementary to UCTs and lead to larger child survival gains, but that the effect of transfers remains large and significant even when such resources are not locally available or utilized. Other mechanisms thus have to explain a meaningful share of the reductions in child mortality, and we turn to some of these next.

### 6.3 Maternal Labor Supply

Above, we showed that infant mortality reductions are largest among households that experienced the smallest monetary gains over the 1.5 years after cash transfer receipt. One explanation consistent with this result is that households may have traded off investments into child health with those into future income-generating activities. We test this prediction by examining changes in maternal rest before and after birth. Parental time is believed to be a key input in the production of child health ([Ruhm, 2000](#); [Miller and Urdinola, 2010](#); [Rossin, 2011](#); [Nandi et al., 2016](#); [Bartel et al., 2023](#)). Many determinants of child health, such as traveling to primary care visits and rest during pregnancy, are inexpensive monetarily but time-consuming, creating a high opportunity cost. Kenya is in the top ten countries worldwide for female labor force participation (at 72%), and of all global regions, Sub-Saharan Africa features the highest rates of women working ([International Labour Organization, 2025](#)). While in general high female labor supply can have a number of positive effects on the well-being of women and children ([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). As background on the study setting, women in control villages engage in nearly 40 hours of productive work on average per week in the last trimester and first three months after birth.

Egger et al. (2022) document little effect of the transfers on average household labor supply, with positive point estimates statistically indistinguishable from zero. Families with a woman in late-stage pregnancy or with a newborn, however, may exhibit different patterns. For women in these cases, the cash arrives at a time when the marginal utility of rest and time to invest in child health may be particularly high. Figure 7 reports labor supply impacts of cash separately by gender, based on Equation (2) and including treatment terms interacted with indicators for a pregnancy or newborn present at the time they were surveyed at endline 1.<sup>34</sup> 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 (four to nine months after birth).<sup>35</sup>

We find substantial heterogeneity in labor supply effects among women by whether a late-stage pregnancy or newborn is present in 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: for both women and men, the point estimate on weekly hours is not statistically significant and close to zero. In the three months before and after a birth, however, cash transfers reduce female labor supply in high-saturation recipient households by 24.48 hours a week, a 53% decrease relative to the control group mean ( $p < .05$ ). By contrast, among men we estimate effects that are near zero and not significant 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.<sup>36</sup>

These results provide 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. Reductions in labor supply during pregnancy as a channel lowering infant mortality is consistent with prior work on the importance of parental time in the production of child health, and accord with studies in other LMIC settings indicating an

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<sup>34</sup>This analysis was not pre-specified.

<sup>35</sup>See Appendix Table A.12 for the estimates from Figure 7 in table format. Estimates encompass only households selected for long-form surveys which inquired about labor supply; as such, the sample size is smaller than the full birth census. Six-month bins are thus used to increase statistical power.

<sup>36</sup>Further heterogeneity analysis based on the gender of the cash recipient within the household could be valuable. However, we observe only the name of the individual who registered the cellphone that transfers were sent to. Cellphones are often shared by households and GiveDirectly did not have a policy to target transfers by gender, so we cannot reliably determine if the actual cash recipient was female.

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). It is also consistent with evidence of extensive labor supply during women’s final trimester of pregnancy likely resulting in increases in mortality that align closely with the causes of death we identify. Cai et al. (2020) report the results of a meta-analysis finding that lifting objects, physically demanding labor, and prolonged periods of standing significantly increase the odds of preterm delivery, low birth-weight, and pre-eclampsia. The health literature also documents that excessive work hours, such as those reported in our setting, are a particularly high risk factor for neonatal mortality. For instance, Kader et al. (2021) find that women who worked one 40 hour week during their third trimester of pregnancy doubled their risk of a preterm delivery, with additional risk from frequently working long hours.

In all, the large decline in work hours documented in this study, generally in physically demanding professions, from approximately 40 hours to around 20 hours per week, may thus plausibly translate into meaningful reductions in child mortality.

## 6.4 Maternal Health and Nutrition

Lastly, we examine the effects of transfers on the health of mothers and the nutrition of children. Measures of adult health from the first endline survey conducted in 2016-17 are consistent with recent and expectant mothers benefiting most from the cash transfers. Adult health was measured using a pre-specified standardized average of three components: an index of recent symptoms for health conditions (e.g., fever, persistent cough, blood in stool, stomach pain), a binary variable capturing whether the respondent reported experiencing a major health problem that affected their work or life since the baseline survey, and the respondent’s self-reported level of current general health rated on a scale of one to five.

Across the general population of adult recipients, estimates of treatment effects are close to zero and not statistically significant, as reported in Egger et al. (2022). As with labor supply, however, treatment effects are different for new mothers. Among female survey respondents in a household with a child aged one or younger, column 1 of Appendix Table A.13 reports that UCTs increased the health index by 0.28 standard deviation units ( $p$ -value  $< 0.05$ ). In contrast, we find no changes in physical health for eligible men with infants in the household nor eligible female respondents without infants.

An examination of the components of the health index suggests that the increase in the index is driven by fewer major adverse health episodes as opposed to current perceptions of health. Appendix Table A.14 shows that transfers reduced the share of recipient women with infants having experienced major health problems that seriously affected their life or work since baseline by 9.1 percentage points, a 66% decrease over the control mean of

13.8 percentage points. In contrast, self-reported current health is virtually unchanged, though the point estimate is positive. The third component of the index, which is itself a standardized index of health symptoms in the last four weeks, exhibits a 0.22 standard deviation unit decrease ( $p$ -value  $< 0.05$ ). Last, column 5 of Appendix Table A.14 shows that conditional on having experienced a major health episode, transfers do not render it more likely that the health problem was resolved. Rather, transfers reduce the likelihood there was a major health problem in the first place.

Another important consideration is the role of maternal and child 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 0.18 standard deviation units on average among recipient baseline households. This result, which is reported in Appendix Table A.13, is statistically significant at the one percent level. The final column of Appendix Table A.13 reports a marginally significant positive rise in the analogous adult food security index among recipient baseline households of approximately 0.10 standard deviation units.<sup>37</sup>

These nutrition gains, as well as the reductions in maternal labor supply, do not necessarily depend on improved access to healthcare. These mechanisms are therefore consistent with the finding that infant and child mortality reductions remain large even absent observable increases in healthcare utilization (i.e., among households living far from a hospital). UCTs thus may yield meaningful benefits even in settings where such infrastructure is limited if they can directly improve the health and nutrition of mothers and children.

## 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? 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?

We estimate the number of child deaths averted among recipient households during the

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<sup>37</sup>Estimates are of a similar magnitude among recipient women with an infant present in the household, though we lack the statistical power to examine these subgroup effects with much precision.

transfer disbursement period using the following “back-of-the-envelope” calculation:<sup>38</sup>

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

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). 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. 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.

The leading approach to estimate the welfare gains of mortality reductions is by recipients’ value of a statistical life (VSL). Revealed preference estimates of VSL, consumers’ demand for mortality risk reduction, 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 VSLs to balance the trade-off between consumption and aid to reduce mortality; low values do not imply that resources dedicated to improving health should not be allocated to poor households. This level of VSL would imply welfare gains from the mortality reductions below \$500,000. However, others have argued for higher VSLs 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 (and aggregate gains of USD 2023 PPP 7.6 million).

To account for the wide range of VSL estimates, 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.<sup>39</sup> Four VSL estimates obtained from settings with similar income levels are included in the lower panel. We focus on the range of revealed preference estimates since they are based

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<sup>38</sup>We focus 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).

<sup>39</sup>We 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 malaria treatment.

on choices with real stakes, 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 and 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 “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. The previously presented results suggest that such gains largely accrue in different households: those with the largest mortality reductions are those with the weakest evidence of economic gains. We thus interpret these figures as the average gain across households. Recall that [Egger et al. \(2022\)](#) estimate a real transfer multiplier of approximately 2.5 in the study area, leading to an increase in welfare of approximately \$2,500 at low VSLs.<sup>40</sup>

The welfare gains of a UCT program like the one that we study are mainly driven by consumption gains, and there is a proportionally small marginal gain in welfare when accounting for mortality reductions across most VSLs in the distribution. This is true because the UCTs were not targeted to pregnant women, so the cost per life saved is relatively high.

Third, we plot estimated welfare gains if the UCTs were targeted to pregnant women (retaining the assumption of a transfer multiplier of 2.5), in the thick blue line.<sup>41</sup> 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.

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. 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.

We next add structure to the problem and estimate the posterior distribution of the

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<sup>40</sup>[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 this analysis.

<sup>41</sup>Here we abstract away from any potential fertility responses to a targeted UCT program.

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 \$300 higher. This holds because the [Redfern et al. \(2019\)](#) VSL estimate induces a rightward skew in the distribution. Both the untargeted UCT program and the targeted UCT program yield higher estimated welfare for the full 95% confidence interval of VSLs if only revealed preference VSL studies are examined. 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. Details of the estimation of the VSL distribution and the welfare analysis are in Appendix C.<sup>42</sup>

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 (and excluding consumption gains). As documented in [Killeen \(2025\)](#) and noted above, the prior welfare analysis guides optimal policy if donors are deciding between various programs to benefit a fixed population, but if funds are specifically earmarked by donors for health, 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.

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, as discussed above, the UCTs were primarily intended to raise consumption and not targeted to pregnant women. We therefore consider targeting transfers to households with women in the third trimester of pregnancy. Disbursing UCTs to these households would cost a total of USD 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 two opposing factors. On one hand, restricting transfers to a subset of households reduces the spillovers from other treated households. But targeting cash to particular subpopulations may result in larger treatment effects among those high-impact groups. We found earlier in [Figure 2](#), 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. To be conservative, we therefore utilize the average

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<sup>42</sup>Appendix [Figure A.9](#) 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.

high-saturation treatment effect for all recipients (-25.63 deaths/1,000 births).

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 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.<sup>43</sup> 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.

One could also imagine targeting UCTs based on baseline socio-economic status or predicted impact, as results show that mortality gains are concentrated among households with the lowest consumption levels and smallest consumption gains. For instance, the estimated cost per life saved among households with below median household consumption is about USD PPP 33,000 (\$14,000 in nominal dollars). However, while such targeting efforts may be cost-effective, they depend on rich data that is not typically readily available.

## 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 households that received cash near a child’s birth month, in poorer households, and among households with smaller economic gains from the transfers in the medium term. Concomitant with large mortality reductions, cash leads to substantial reductions in maternal labor supply, improvements in mothers’ health, and nutritional gains for mothers and children. We find evidence that access to higher quality health facilities like hospitals may amplify these gains, but there remain sizable reductions in mortality even among households living far from health infrastructure. A rough calculation suggests that transfers targeted to pregnant women are similarly cost-effective to a number of child health interventions recommended by the WHO. These child mortality reductions represent benefits of UCTs beyond the direct and spillover household consumption gains already documented ([Egger et al., 2022](#)).

The large magnitude of the child survival gains underscores the fact that infant and child mortality appear sensitive to economic conditions in low-income contexts, such as the rural Kenyan study setting. The socioeconomic gradient in mortality is steep in rural Kenya: not only do large UCTs nearly halve infant mortality, but treatment effects are concen-

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<sup>43</sup>[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](#)).

trated 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.

An important implication of these results is that cash can be effective at reducing infant and child mortality even absent health infrastructure because it improves the health of mothers and children through channels such as increased maternal rest and improved household nutrition. This suggests that such programs may yield meaningful child survival gains in other low-income settings that have more limited healthcare infrastructure than Kenya. The timing of transfers is also highly consequential: households receiving cash near the time a child is born see by far the largest resulting reductions in mortality. Outside of public health policy, this finding supports the view that allowing households to select the distribution time of cash transfers may increase their efficacy by aligning available liquidity with high-return investment opportunities (Kansikas et al., 2025).

Finally, this paper suggests that typical measures of consumption, which focus on market goods that are relatively easily measured, may miss important welfare gains from the consumption of non-market amenities such as leisure and health. Economists have long noted that consumption of such amenities is frequently omitted from consumption surveys and GDP. We find that child mortality reductions are concentrated overwhelmingly among the households with the smallest documented consumption gains following cash transfer receipt, suggesting that poor households may face a stark trade-off between monetary consumption versus non-monetary investments in health.

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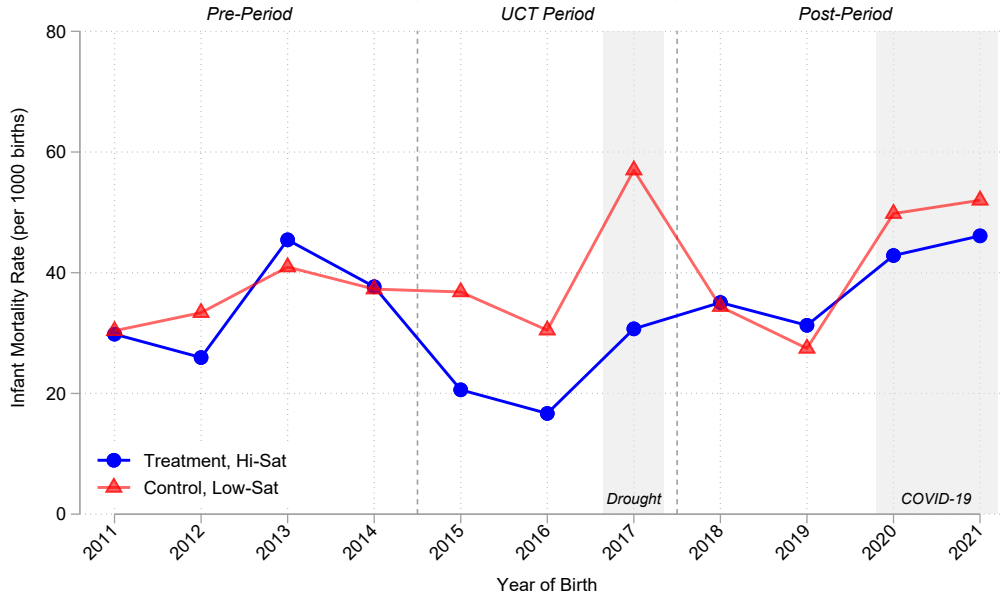
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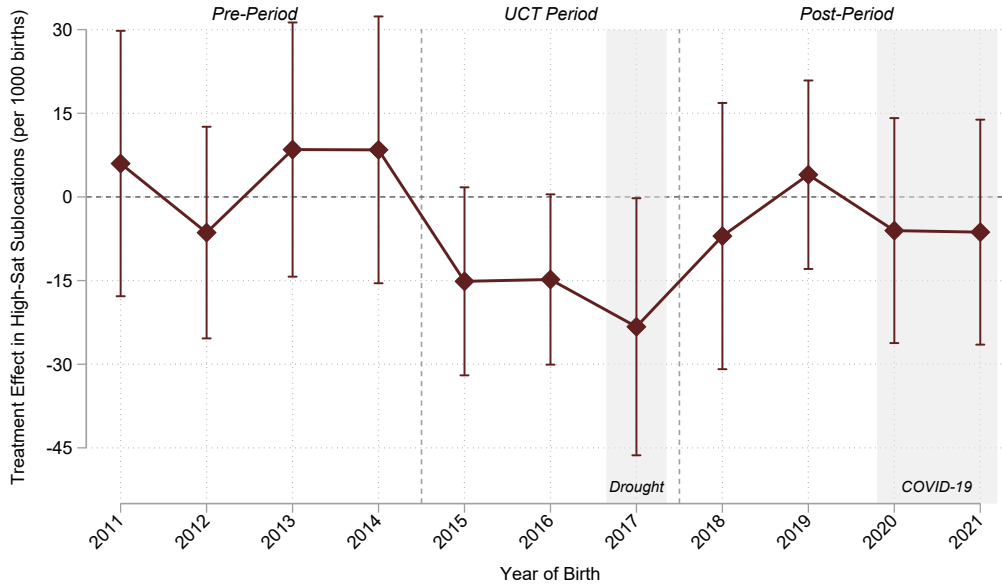
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Figure 1: Unconditional Cash Transfers and Infant Mortality By Year



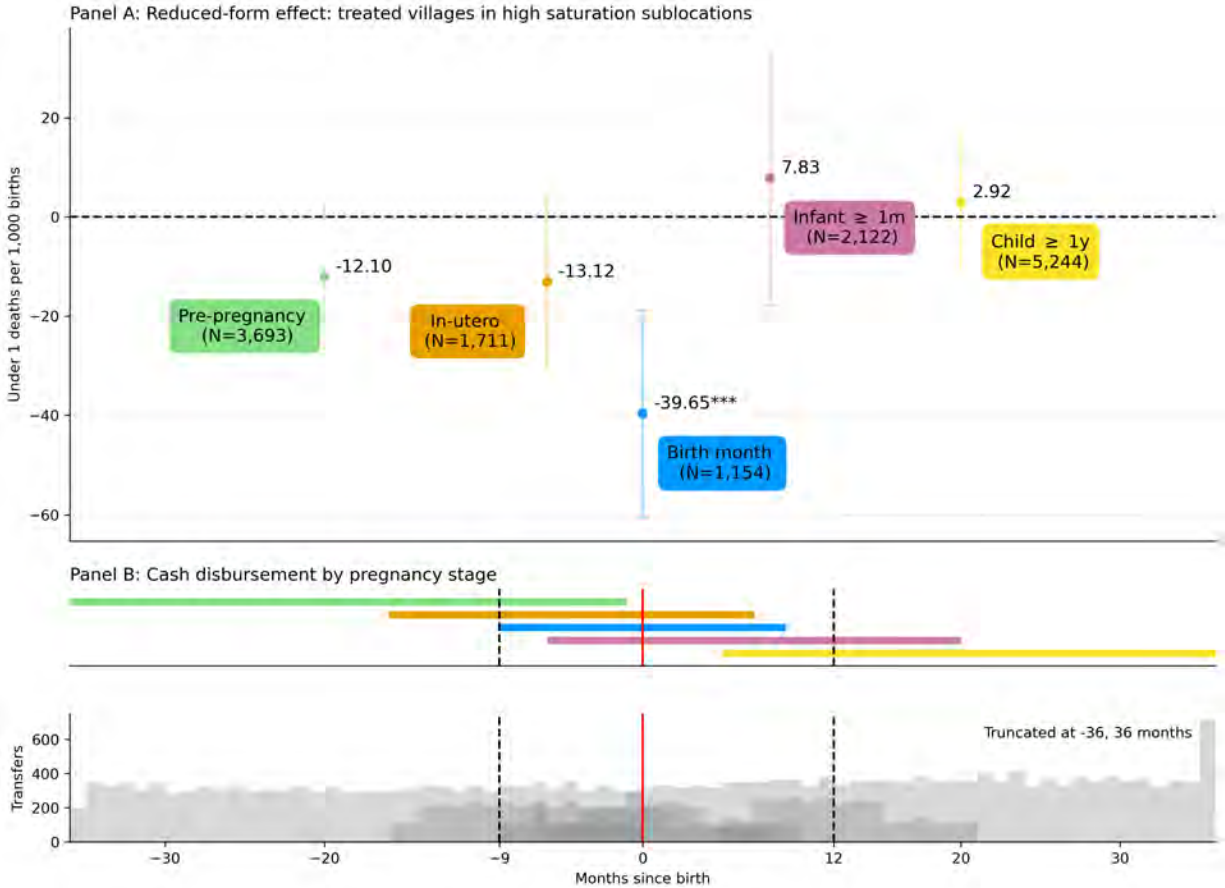
Panel A: Infant Mortality By Year



Panel B: Reduced-Form Impacts by Year

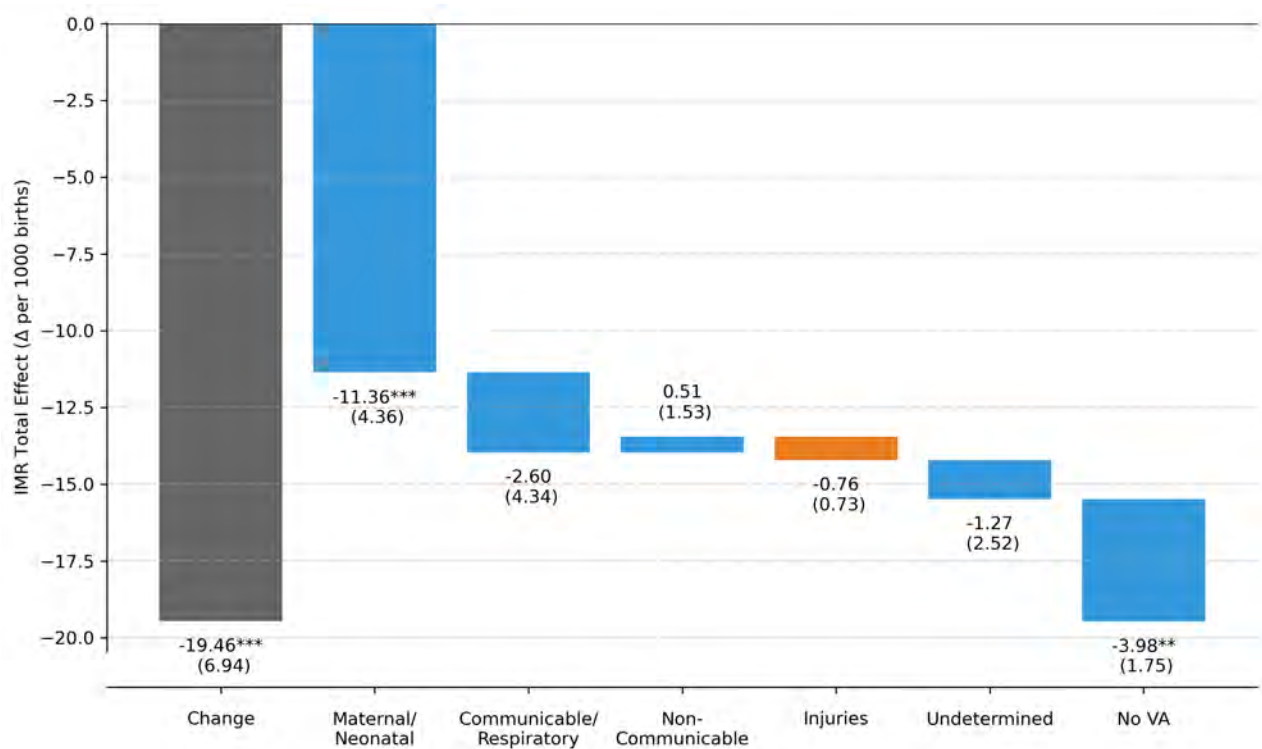
*Notes:* This figure is based on the main EL3 birth census sample encompassing births from 2011 to 2021. Panel A reports the mean infant mortality rate by year among eligible households for treatment villages in high-saturation areas and control villages in low-saturation areas. Panel B reports the year-by-year 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 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: Transfer Effects on Infant Mortality by Timing of Cash Disbursal



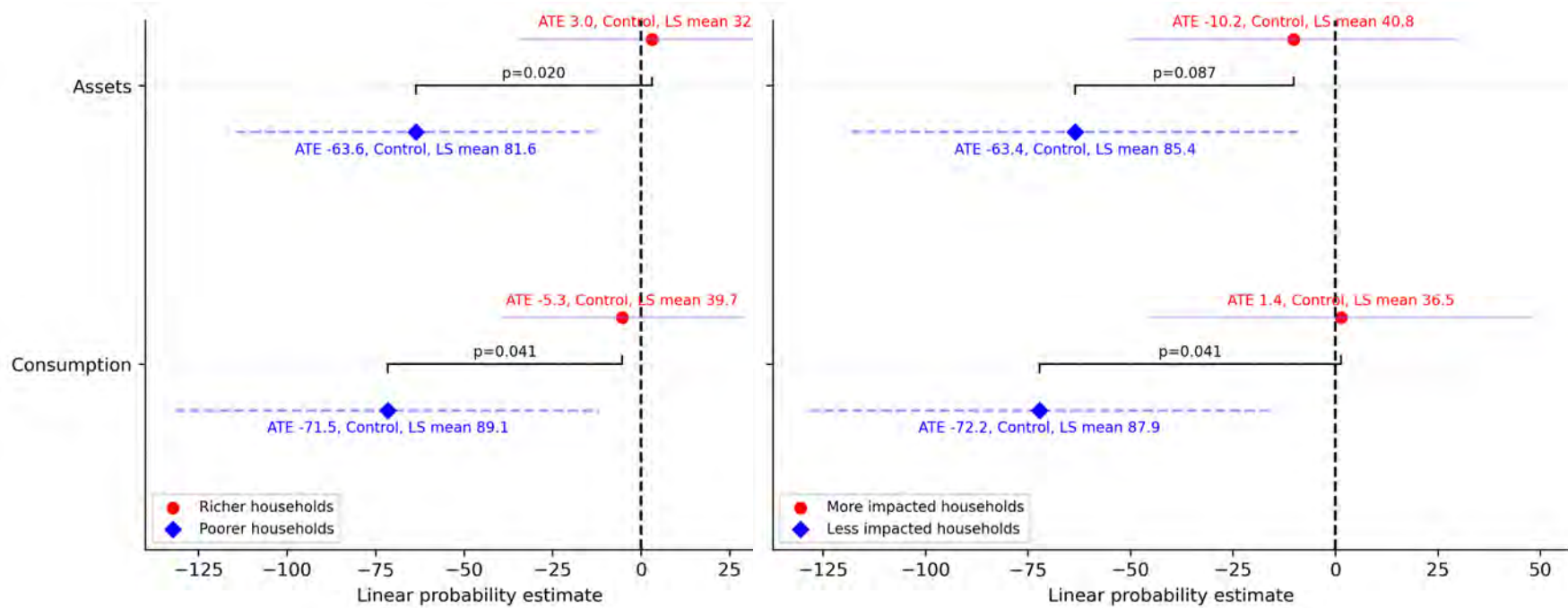
*Notes:* This figure is based on the main EL3 birth census sample. Panel A plots estimates of dynamics based on the time period of exposure to cash during the child’s life. The transfer timing is defined relative to the “experimental start date”, as this is well-defined for both treatment and control 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.3. 95% confidence intervals are shown. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Figure 3: 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 4: Heterogeneous treatment effects on infant mortality by deprivation and impact (reduced-form)

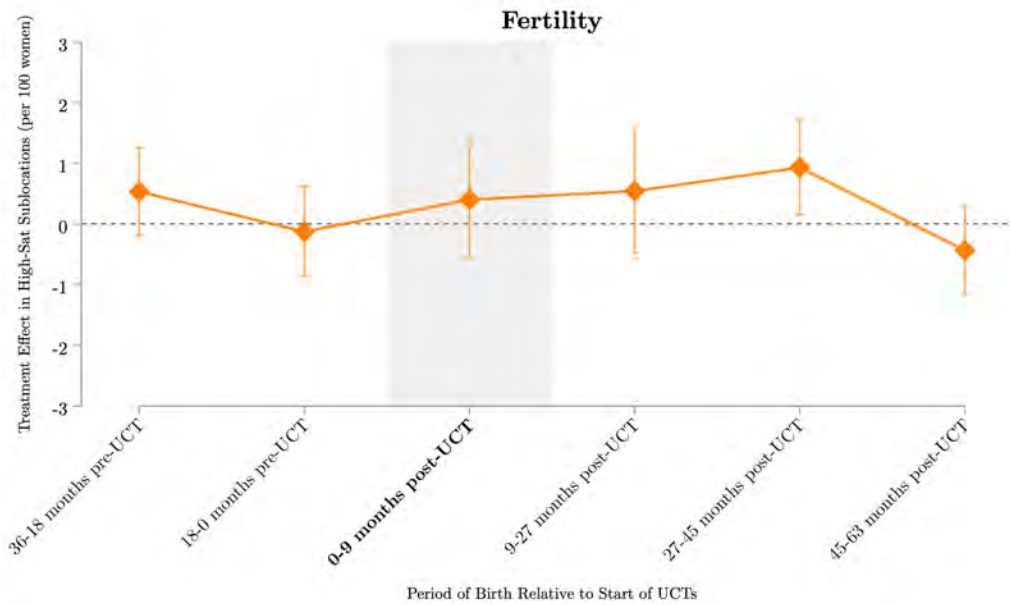


Panel A: Heterogeneity by baseline socioeconomic status

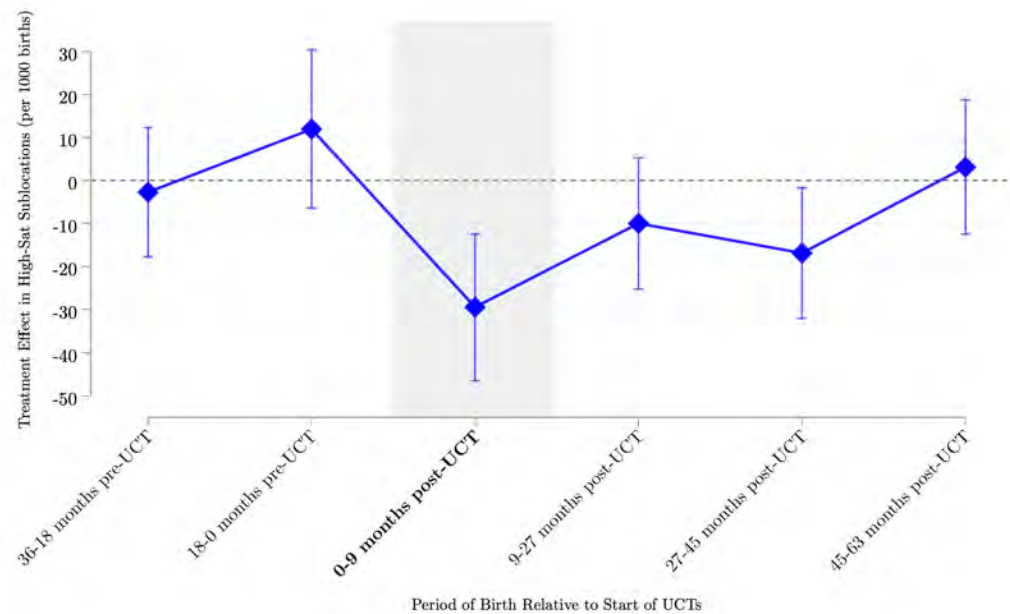
Panel B: Heterogeneity by predicted conditional average treatment effect (CATE)

*Notes:* Transfer-eligible households visited in the baseline survey and EL3 census are included. Panel A reports effects on infant mortality (per 1000 births) by below vs above-median wealth, estimated using Eq. (1). In Panel A, richer households by the measure of “assets” reported above median baseline household assets, while “consumption” uses the predicted endline per capita generalized random forest (GRF) prediction from Haushofer et al. (2025). Consumption is predicted since it was not measured at baseline. A household is defined as “richer” if the share of GRF runs where it was defined as “most deprived” per the Haushofer et al. (2025) was lower than average. Panel B reports effects on infant mortality (per 1000) births based on whether the predicted conditional average treatment effect (CATE) of per capita assets or consumption was lower or higher. Specifically, a household is defined as “more impacted” if the share of GRF splits in Haushofer et al. (2025) that classified it as “most impacted” exceeds the sample median, across the full sample of births among eligible HHs. Income is excluded from this figure since Haushofer et al. (2025) find less evidence of predictable heterogeneity on that dimension. 244 observations fall in above-median BL assets and predicted asset CATE, 335 had below median assets but above median predicted CATE, 209 had above median BL assets but below median predicted CATE, and 473 were below median on both dimensions. With respect to consumption, these counts are 438, 137, 179, and 507 respectively. Stars denote the significance of the difference between richer/poorer or less/more impacted households. Figure A.7 reports effects by income and predicted (rather than observed) baseline assets. P-values on the probability of equality of the groups are plotted in black.

Figure 5: Transfer Effects on Fertility and Infant Mortality by Timing of Birth



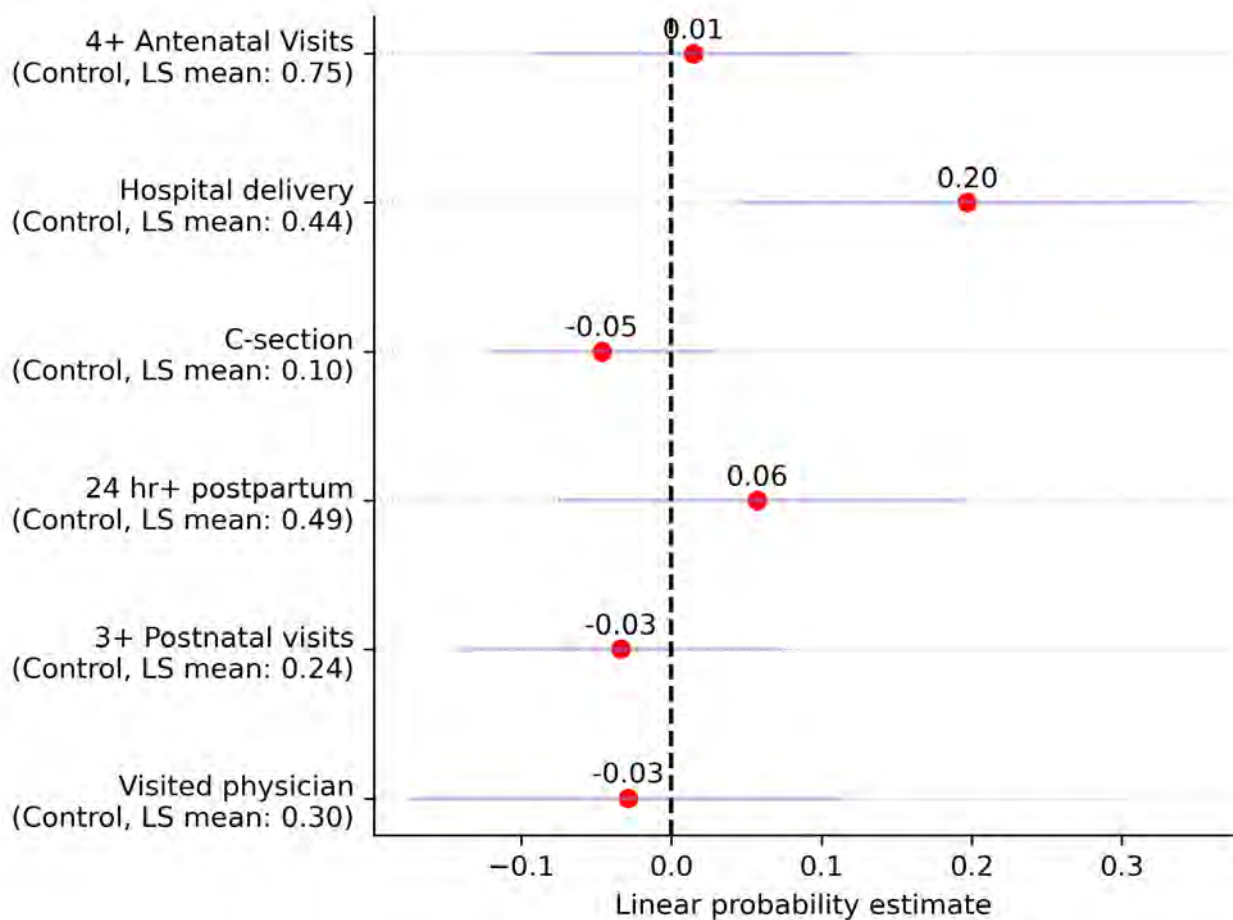
Panel A: Fertility by Period Relative to UCT Start



Panel B: IMR by Period Relative to UCT Start

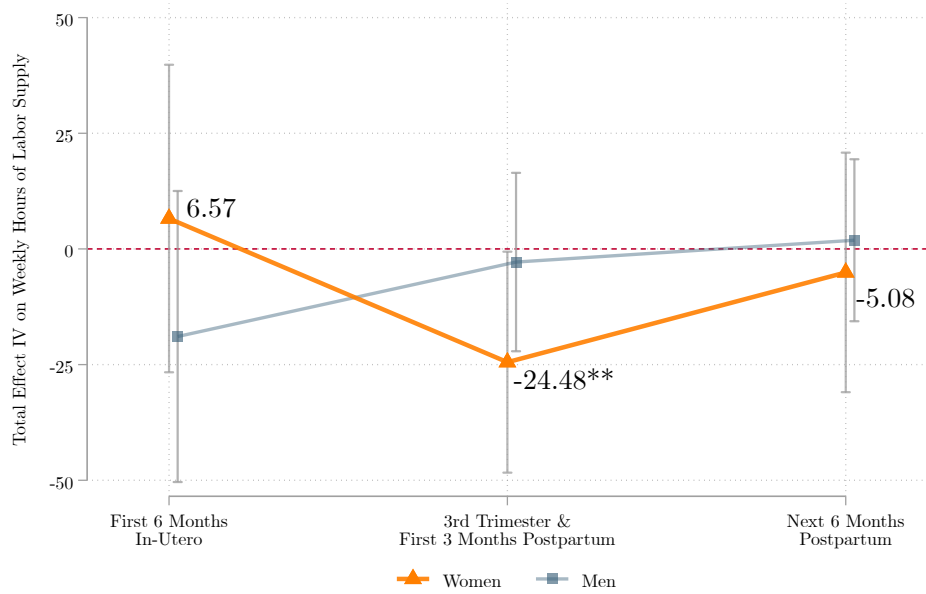
*Notes:* This figure is based on the sample of transfer-eligible women from the EL3 census living in households present at baseline (Panel A) and the main EL3 birth census sample, which comprises births in transfer-eligible households present at baseline (Panel B). All treatment effects are estimated using Equation (1). Panel A reports treatment effects in high-saturation areas on the number of births per 100 women in a given period, with birth rates standardized to a nine-month window to facilitate comparisons. Panel B reports treatment effects in high-saturation areas on infant mortality in a given period. The whiskers on each estimate denote the 95% confidence interval. Standard errors are clustered at the sublocation level.

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

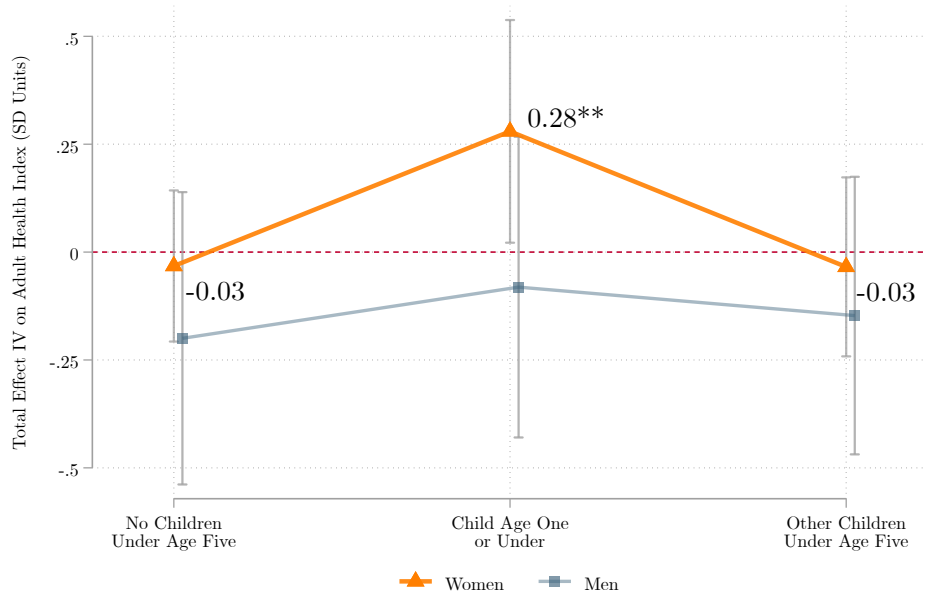


*Notes:* This figure is based on the sample of transfer-eligible households present at baseline and surveyed in the EL3 long-form survey. It reports treatment effects on indicators for visiting the indicated health service during a pregnancy associated with a birth between 2015 and 2017. Treatment effects 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: Transfer Effects on Household Labor Supply and Health by Gender



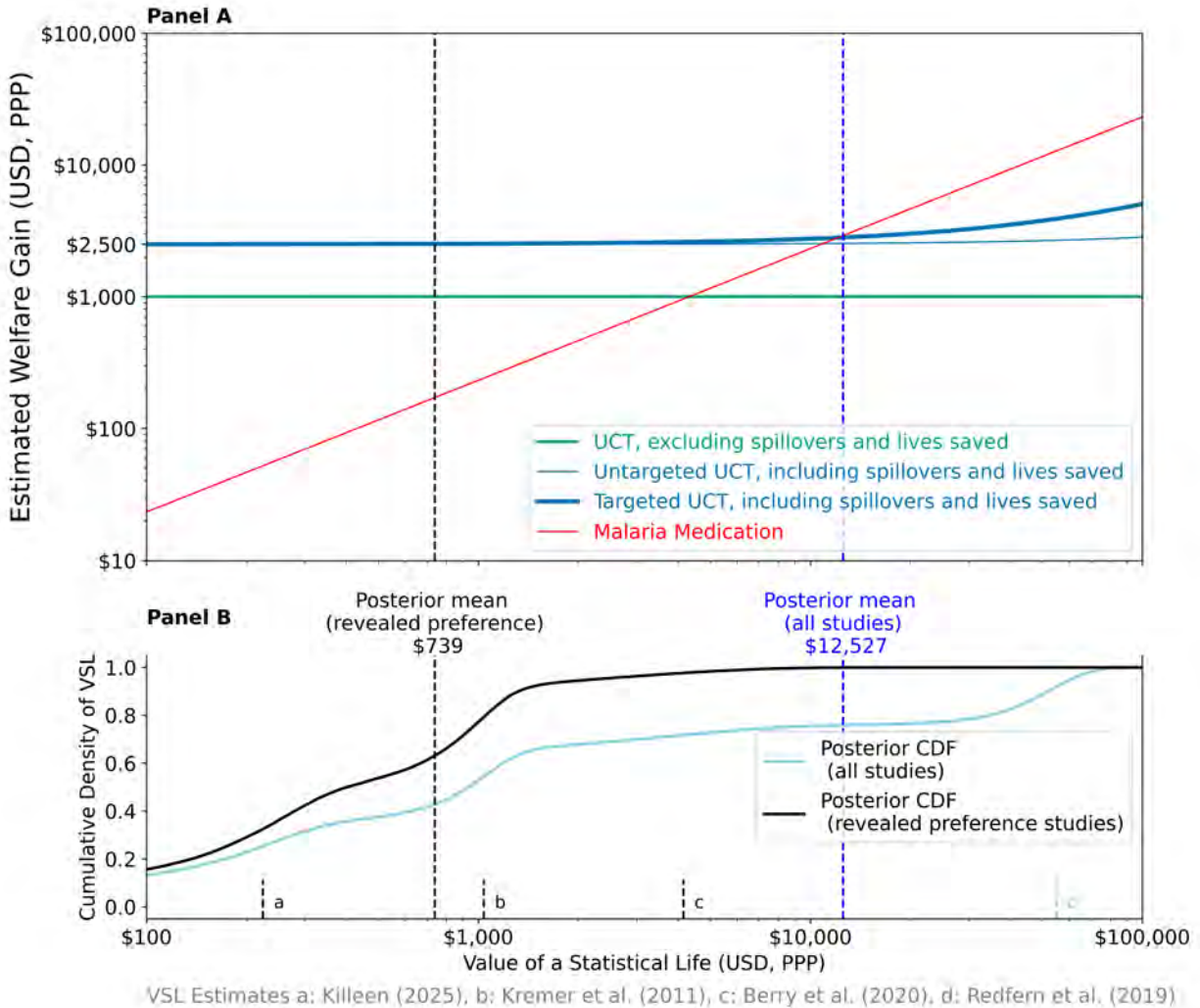
Panel A: UCTs and Labor Supply by Gender Around the Time of Birth



Panel B: UCTs and Adult Health by Gender and Presence of Recent Birth

*Notes:* Panel A is based on households surveyed in the first endline survey (2016-17) with a recorded birth in the EL3 census. The labor supply measures 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. Panel A displays the estimated differential 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. The control means are 45.85 (women) and 34.77 (men). N=1766 (women) and N=1677 (men). The results shown in this figure are additionally presented in Appendix Table A.12. Panel B is based on households surveyed in the first endline survey. The outcome of interest is a pre-specified, standardized index of adult health. Panel B displays the estimated differential effect of transfers on adult health for three groups: households of reproductive age with no children under age five, households with a child age one or under, and households with a child between age one and five. 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)

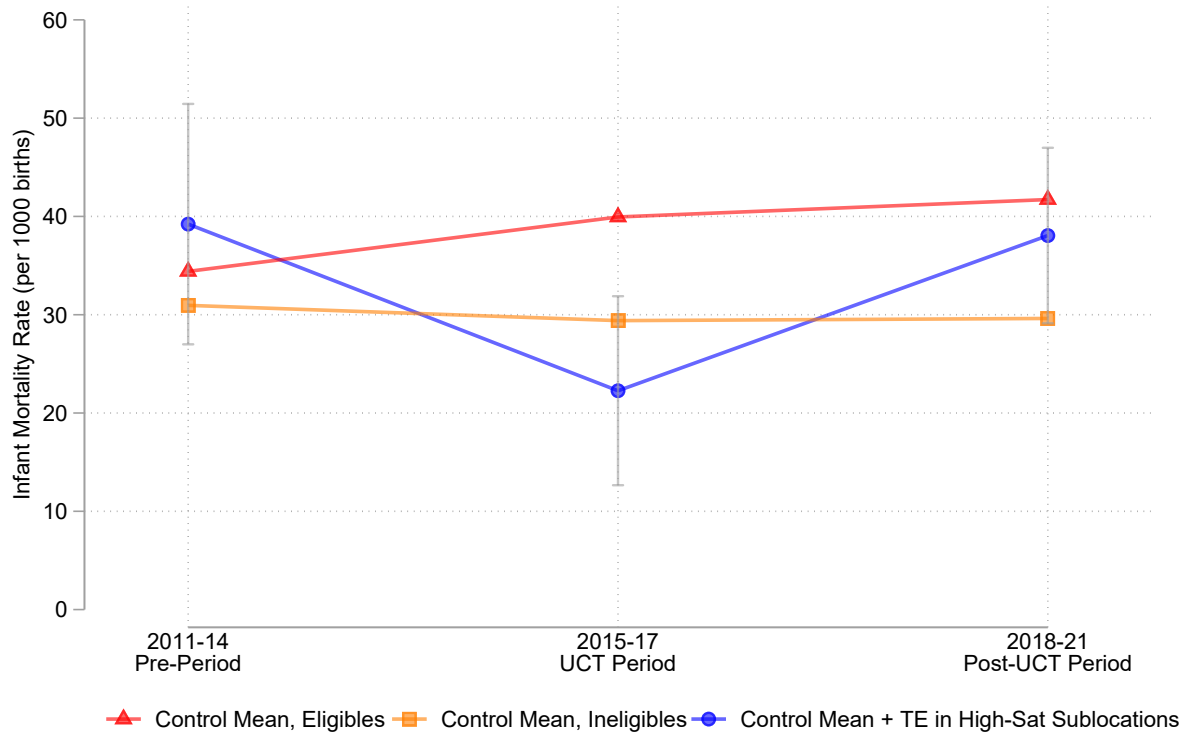
	Delivery in facility type (Survey data)		Infant mortality (Census data)	
	(1) Hospital	(2) Physician-staffed facility	(3) Hospital	(4) Physician-staffed facility
<b>Panel A: Spatial IV, no controls</b>				
Total Effect IV (Time: Above median)	0.11 (0.09)	-0.04 (0.07)	-19.99** (8.41)	-21.12*** (7.75)
Total Effect x Below median time to facility	0.27** (0.14)	0.07 (0.12)	-1.03 (15.28)	-2.28 (15.55)
<b>Panel B: Spatial IV, double-partial out LASSO controls + treatment interactions</b>				
Total Effect x Below median time to facility	0.33** (0.13)	0.04 (0.14)	-12.70 (15.73)	-29.01* (16.16)
Mean time to facility (above median, minutes)	52.6	55.5	52.1	54.8
Mean time to facility (below median, minutes)	18.8	22.5	19.1	22.6
Control Mean, > med. time	0.39	0.26	37.92	35.95
Control Mean, ≤ med. time	0.45	0.30	43.17	45.71
Observations	1,110	1,075	6,311	6,311

*Notes:* This table uses data from both the EL3 survey (columns 1-2) and the EL3 census sample (columns 3-4) for births between 2015-2017. In column (1), the outcome is birth in a hospital, based on below vs above-median travel to a hospital. Column (2) considers is similar but based on delivery in and time to a physician-staffed facility. Columns (3) and (4) consider an indicator for infant mortality, scaled to be reported in deaths per 1,000 live births, based on time to a hospital and physician-staffed facility respectively. Column (1) and (3) includes level 4 and higher facilities surveyed, plus those categorized level 4 or higher on the Kenya Master Health Facility List that were unsurveyed. If the facility reported it was open in 2014 and had a physician employed during the survey the facility is included in columns (2) and (4). 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.

\*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

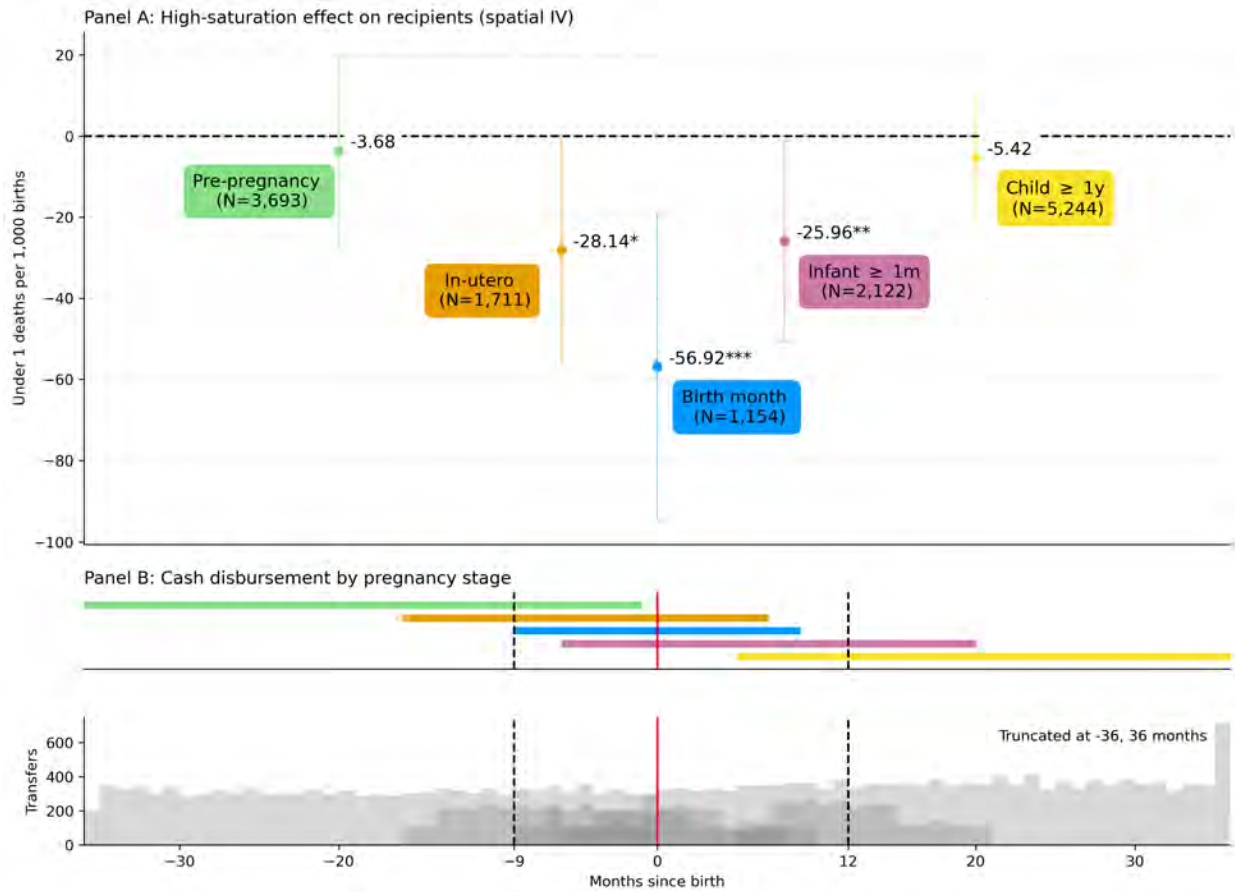
## A Additional Exhibits

Figure A.1: Unconditional Cash Transfers and Infant Mortality



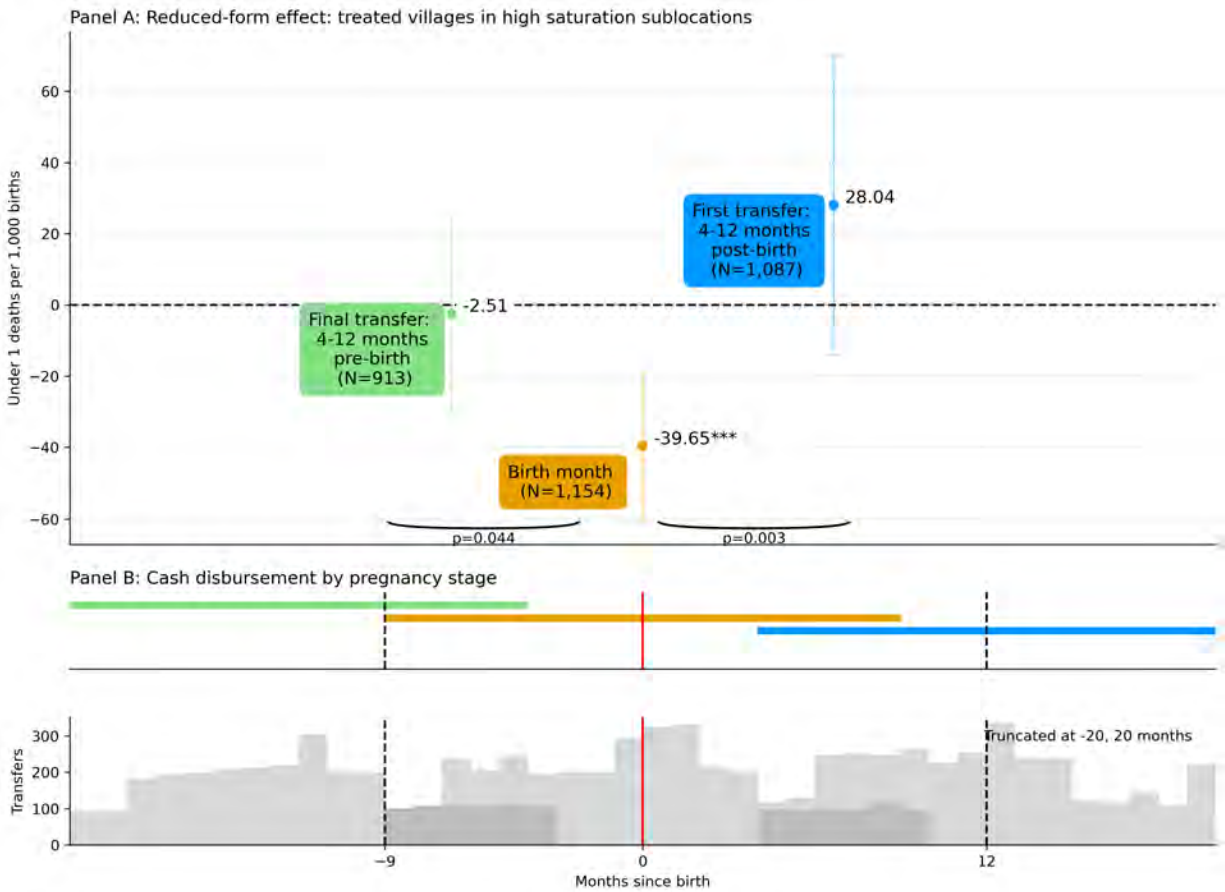
*Notes:* This figure is based on the main EL3 birth census sample encompassing births from 2011 to 2021. The figure plots the infant (under-1) mortality rate per 1000 births (y-axis) for three different periods of child births: pre-transfers (child birth years of 2011-14), the transfer period (child 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 A.3: Transfer Effects on Infant Mortality by Timing of Cash (Spatial IV)



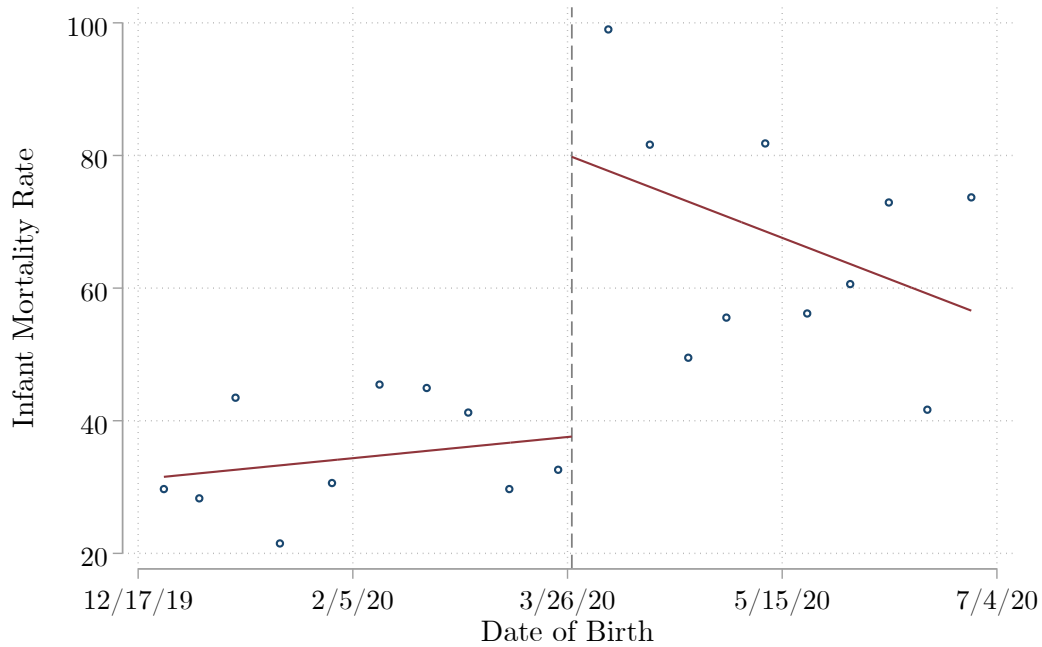
*Notes:* This figure is based on the main EL3 birth census sample. Panel A plots estimates of dynamics based on the time period of exposure to cash during the child’s life. The transfer timing is defined relative to the “experimental start date”, as this is well-defined for both treatment and control 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 reduced-form version of this figure (which is estimated using equation 1) is presented as Figure 2. 95% confidence intervals are shown. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Figure A.4: Effects of Cash Receipt in Birth Month vs Neighboring Times on Infant Mortality(Reduced-form)

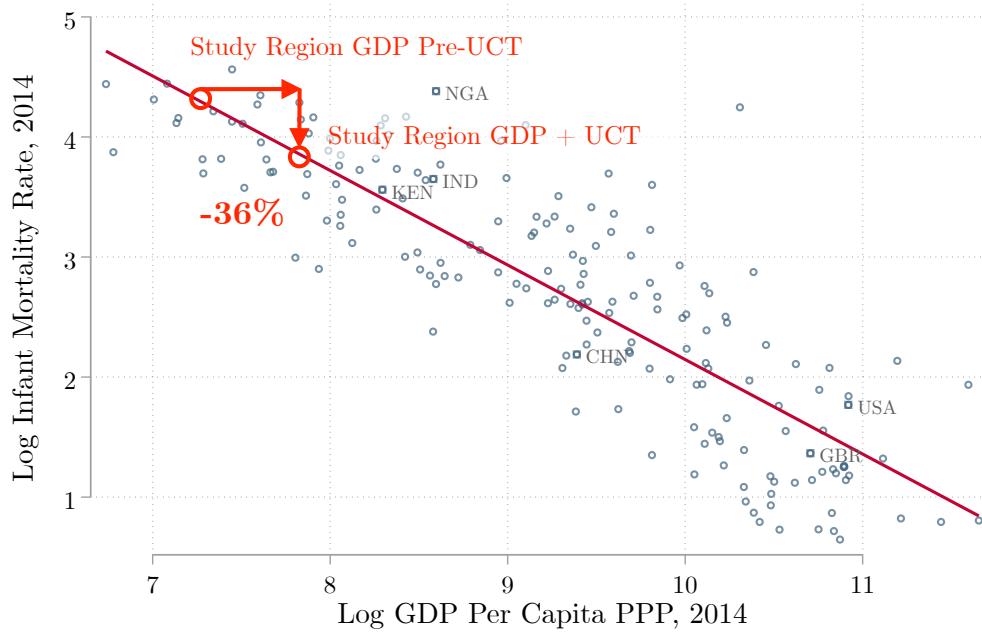


Notes: This figure is based on the main EL3 birth census sample. Panel A plots estimates of dynamics based on the time period of exposure to cash during the child’s life. The transfer timing is defined relative to the “experimental start date”, as this is well-defined for both treatment and control villages. The first group includes households where the last transfer was received between 12 and 4 months before birth (or would have been if they are control). Birth month includes cash within the first month of life. The third group includes birth where the experimental start date was 4-12 months post-birth. Estimates are constructed using the same approach as equation (4). We estimate equation (1) after restricting the sample to those exposed to cash at a particular time in life. The bottom of Panel A reports p-values for equality to the birth month effect based on an estimate which includes both groups indicates, and interacts the treatment effects with cash receipt timing. 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. 95% confidence intervals are shown. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Figure A.5: Non-Experimental Variation in Child Mortality



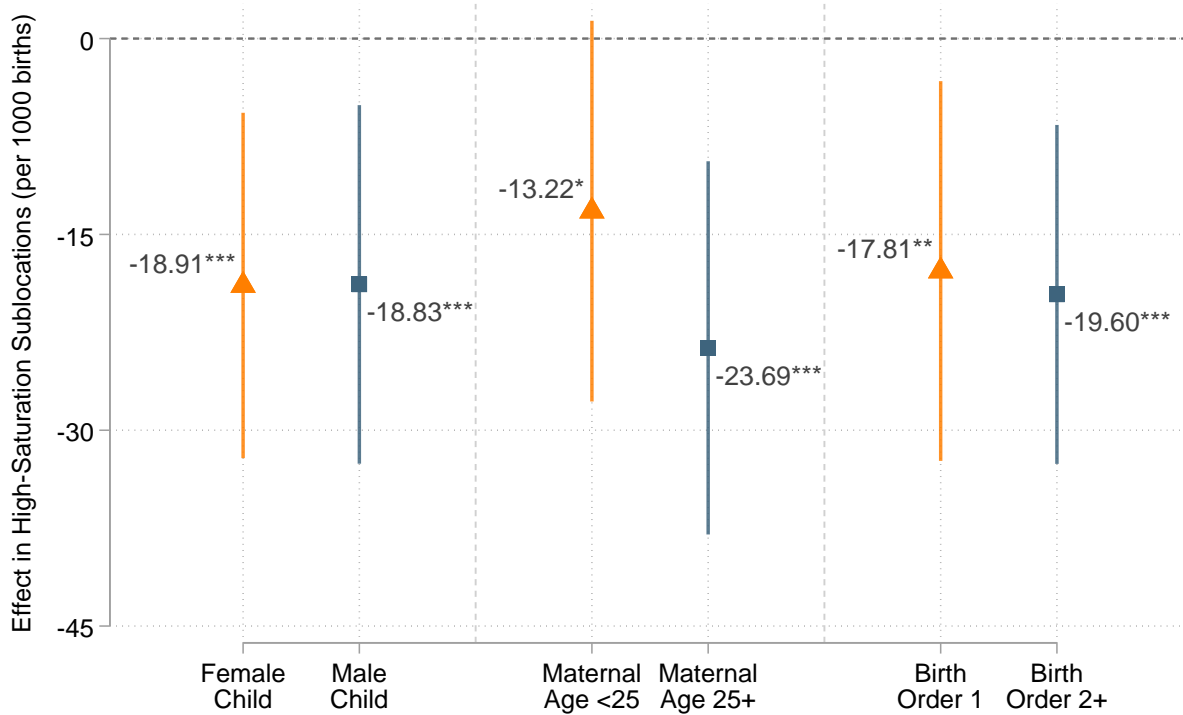
Panel A: COVID-19 Lockdown and Infant Mortality in Study Region



Panel B: Cross-Country GDP - Infant Mortality Relationship

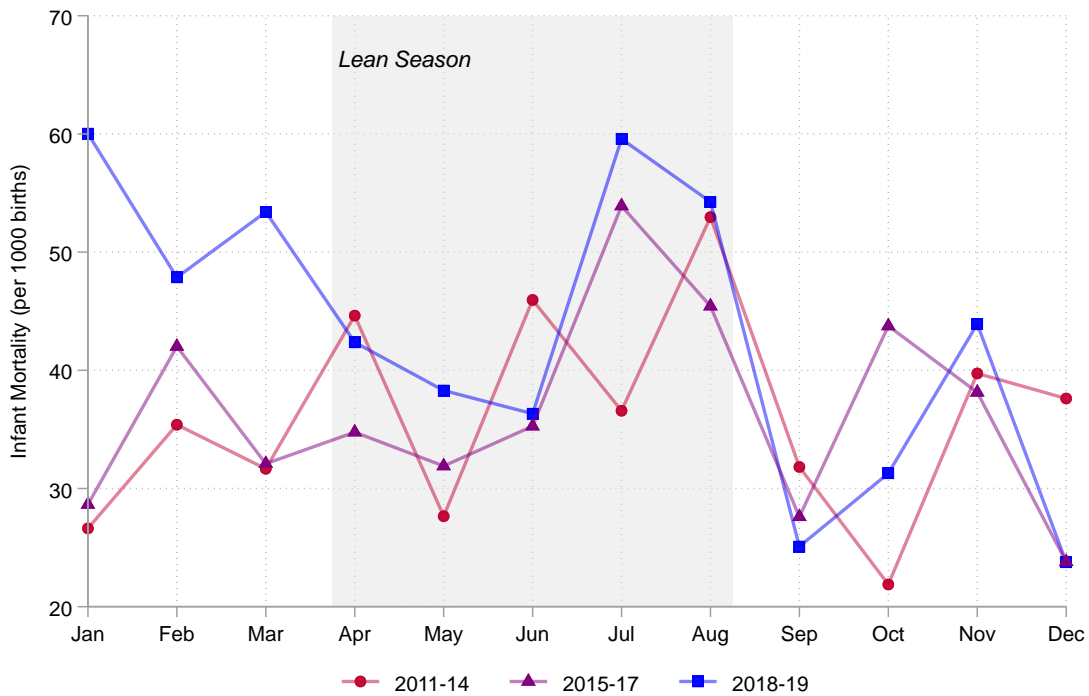
*Notes:* This figure is based on the main EL3 birth census sample. Panel A reports infant mortality in the study region before and after Kenya’s March 2020 COVID-19 lockdown using exact date of birth data from this project’s birth census, utilizing all censused births with available data. The data in 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.2: Heterogeneity by Gender, Maternal Age, and Birth Order

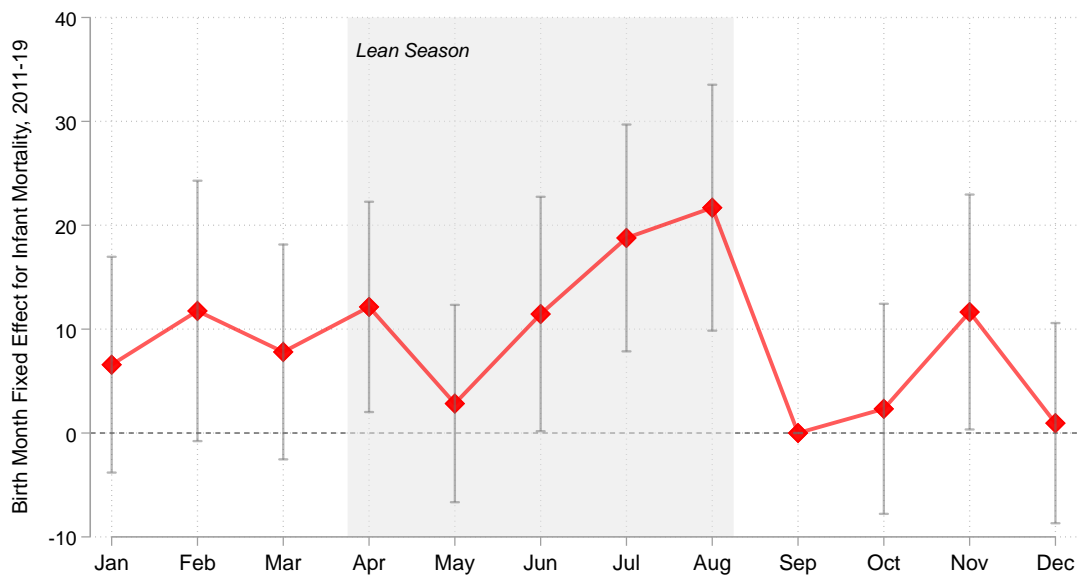


*Notes:* This figure is based on the main EL3 birth census sample, and plots estimated effects of cash transfers on infant mortality in treatment villages within high-saturation sublocations by gender, maternal age, and birth order. Maternal age at birth is analysed using pre-specified five-year age 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.8. Standard errors are clustered at the sublocation level. 95% confidence intervals are shown. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Figure A.6: Seasonality in Infant Mortality in the Birth Census



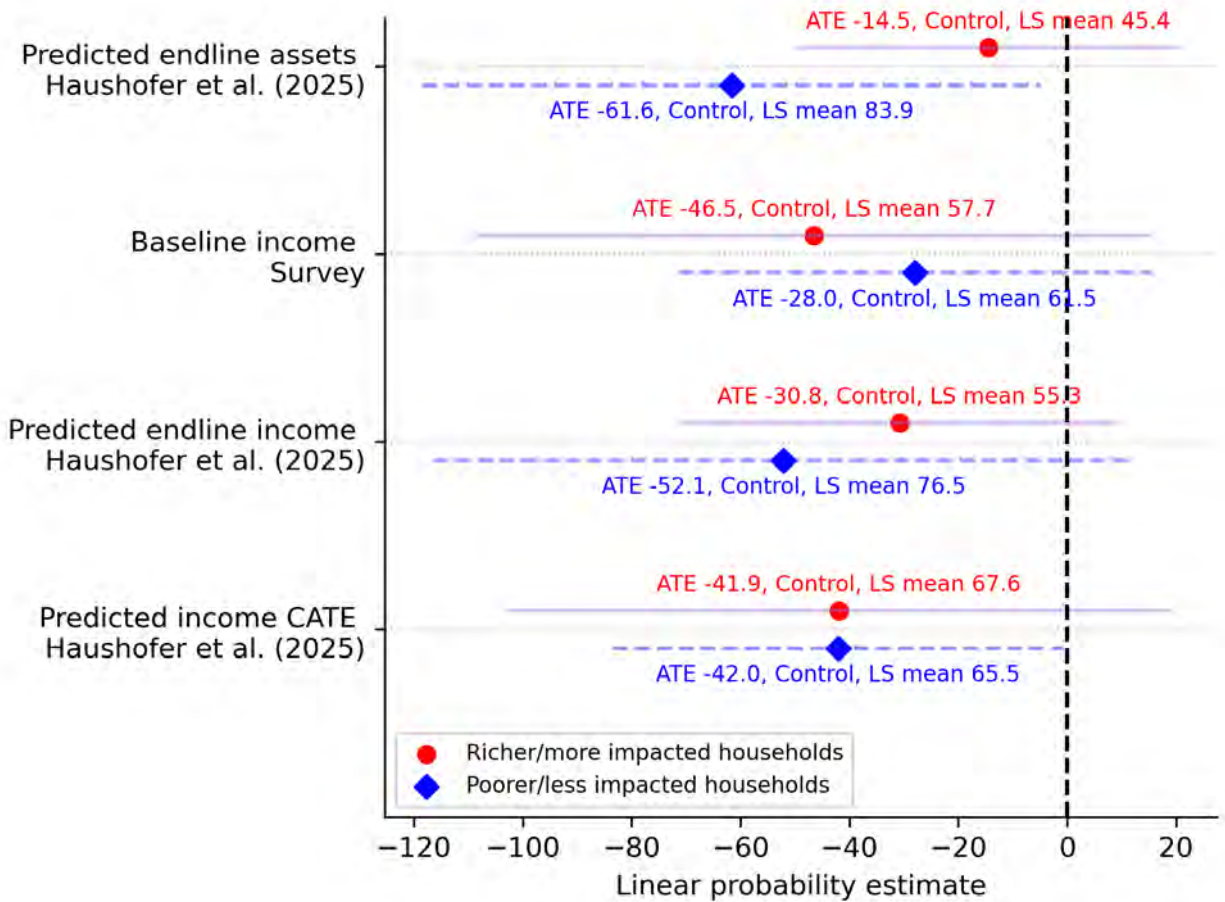
Panel A: Infant Mortality Means By Month



Panel B: Birth Month Fixed Effects

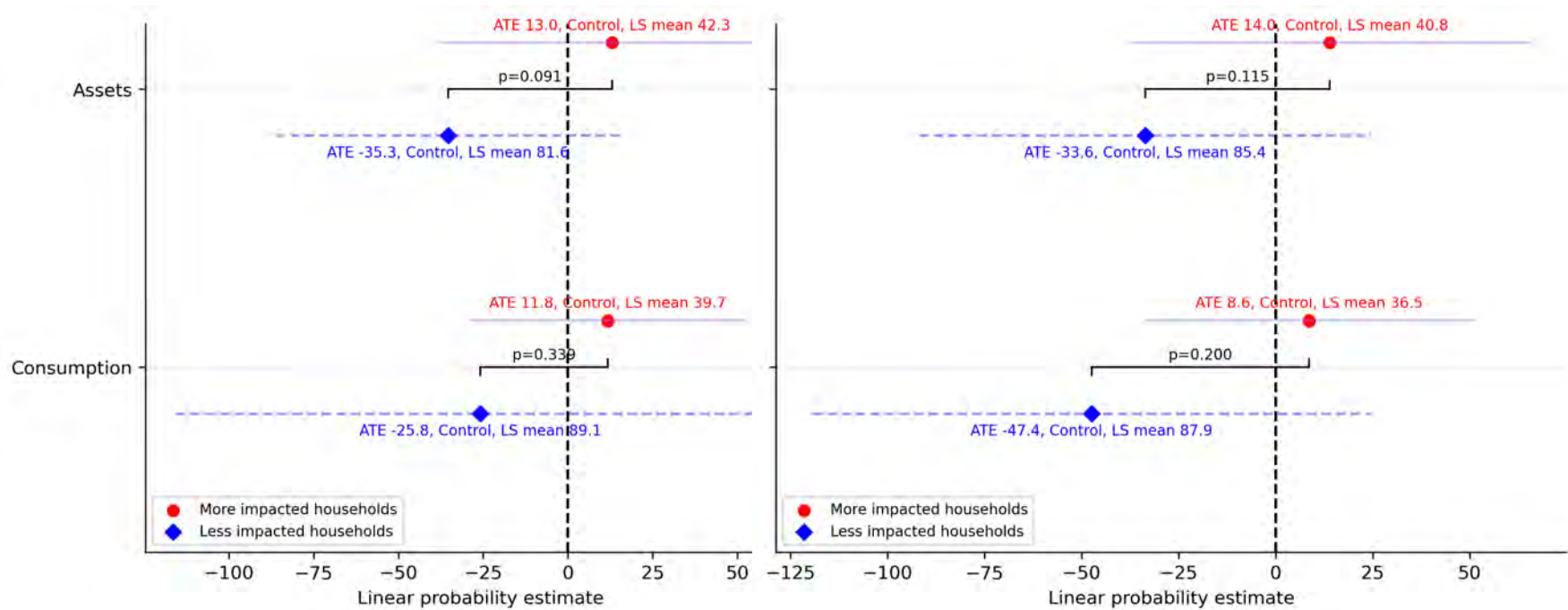
*Notes:* This figure is based on the main EL3 birth census sample. Panel A reports the mean infant mortality rate by month across 2011-14, 2015-17, and 2018-19. Panel B reports birth month fixed effects controlling for birth year fixed effects, with September as the omitted month. The lean season is 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.7: Heterogeneous effects by deprivation and impact: additional measures



*Notes:* Transfer-eligible households visited in the baseline survey and EL3 census are included. The plotted effects are treatment effects on infant mortality (per 1000) births, based on the indicated heterogeneity cuts. Predicted endline assets and income use per-capita generalized random forest (GRF) predictions from [Haushofer et al. \(2025\)](#), where “poorer” households are defined as those where the share of time the household is classified as “most deprived” exceeds the median. Baseline income refers to baseline household income measured by the survey, where poorer households are those where this value falls below the median. The predicted income CATE row classifies households as “more impacted” if the share of time they were classified as “most impacted” in terms of per capita income CATEs in [Haushofer et al. \(2025\)](#) exceeds the median. Stars denote the significance of the difference between richer/poorer or less/more impacted households. Figure 4 reports effects in terms of consumption and assets, where there is more predictable heterogeneity.

Figure A.8: Heterogeneous effects by deprivation and impact: controlling for left out heterogeneity dimension

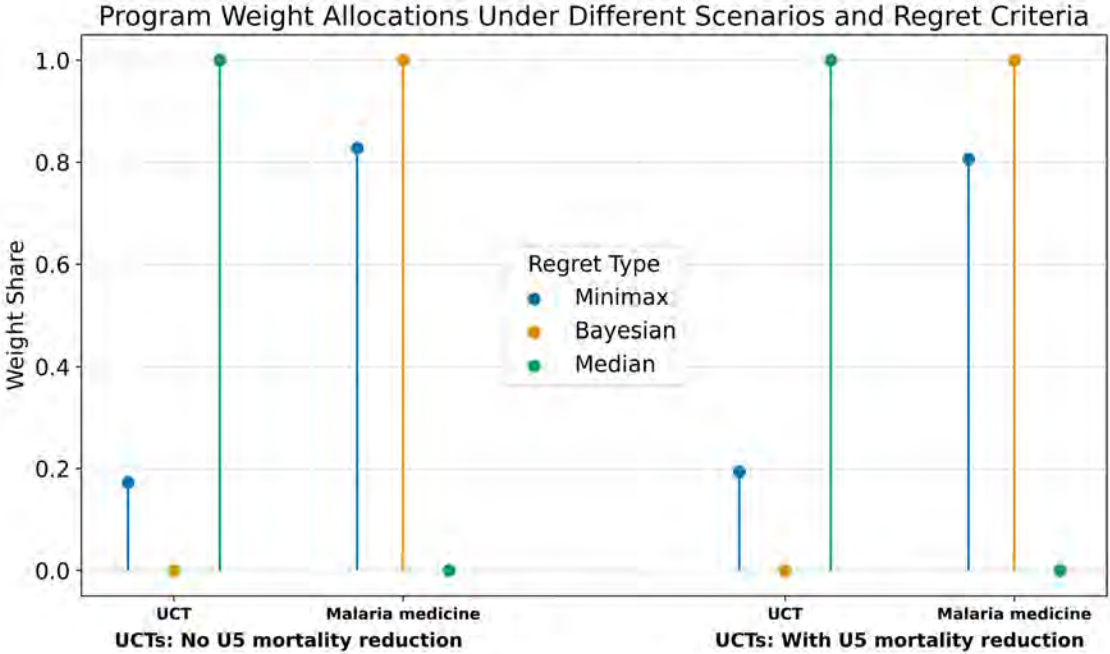


Panel A: Heterogeneity by baseline socioeconomic status

Panel B: Heterogeneity by predicted conditional average treatment effect (CATE)

*Notes:* This figure estimates the same regressions as Figure 4, but estimates of heterogeneity by deprivation control for impacted and impacted interacted with treatment and vice-versa. Transfer-eligible households visited in the baseline survey and EL3 census are included. Panel A reports effects on infant mortality (per 1000 births) by below vs above-median wealth, estimated using Eq. (1). In Panel A, richer households by the measure of “assets” reported above median baseline household assets, while “consumption” uses the predicted endline per capita generalized random forest (GRF) prediction from Haushofer et al. (2025). Consumption is predicted since it was not measured at baseline. A household is defined as “richer” if the share of GRF runs where it was defined as “most deprived” per the Haushofer et al. (2025) was lower than average. Panel B reports effects on infant mortality (per 1000) births based on whether the predicted conditional average treatment effect (CATE) of per capita assets or consumption was lower or higher. Specifically, a household is defined as “more impacted” if the share of GRF splits in Haushofer et al. (2025) that classified it as “most impacted” exceeds the sample median, across the full sample of births among eligible HHs. Panel A controls for the corresponding measures (and their interactions with treatment) in Panel B, and Panel B controls for the corresponding measures and treatment interactions in Panel A.

Figure A.9: 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
<b>Births to All Households (2011-23)</b>				
Mean	33.71	46.69	15.11	25.65
Observations	101,336	101,348	100,498	95,353
<b>Births to Households Present at Baseline (2011-23)</b>				
Mean	33.61	46.40	18.50	25.72
Observations	77,847	77,849	77,229	73,247
<b>Births to Eligible Baseline Households, UCT Period (2015-17)</b>				
Mean	34.54	50.68	19.23	25.55
Observations	6,334	6,335	6,240	6,003
<b>Births to Eligible Baseline Households, Pre-UCT Period (2011-14)</b>				
Mean	35.43	54.18	14.11	24.73
Observations	8,634	8,635	8,504	8,208
<b>Births to Eligible Baseline Households, Post-UCT Period (2018-21)</b>				
Mean	37.89	51.54	20.09	26.35
Observations	9,577	9,577	9,457	9,129
<b>Births to Eligible Baseline Households (2011-21)</b>				
Mean	36.15	52.25	17.77	25.57
Observations	24,545	24,547	24,201	23,340
<b>Births to Ineligible Baseline Households (2011-21)</b>				
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)	(2)	(3)	(4)	(5)
	Household Tracked (Census)	Present in Study Area (Census)	Completed Census (Census)	Household Tracked (Survey)	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
<b>Panel A: Reduced-form results</b>				
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)
<b>Panel B: Spatial IV results</b>				
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: Unconditional Cash Transfers and Infant Mortality, 2011-21

	(1) 2011-14	(2) 2015-16	(3) 2017 (Drought)	(4) 2018-21
<b><i>Panel A: Reduced-Form Estimates</i></b>				
Own village	0.03 (4.66)	-3.61 (7.87)	-9.84 (9.01)	-4.11 (4.42)
High-saturation spillovers	5.08 (4.79)	-11.44 (7.18)	-13.50 (8.19)	0.72 (4.44)
ATE in high-saturation sublocations	5.11 (6.31)	-15.04*** (5.69)	-23.35** (10.39)	-3.39 (4.60)
Control mean	34.53	32.97	54.87	41.92
Observations	8605	4216	2101	9544
<b><i>Panel B: Spatial IV Estimates</i></b>				
Own village	1.09 (4.68)	-6.28 (6.26)	-13.08 (8.63)	-4.28 (4.35)
High-saturation spillovers	-5.22 (5.01)	-6.38 (7.97)	-17.05 (10.67)	4.85 (5.71)
ATE in high-saturation sublocations	-4.13 (6.42)	-12.66 (8.34)	-30.13*** (11.59)	0.57 (6.82)
Control mean	34.53	32.97	54.87	41.92
Observations	8605	4215	2101	9544

*Notes:* Effects are estimated using Equation (1) (Panel A) and (2) (Panel B) on transfer-eligible women from the main EL3 census sample. Infant mortality estimated effects are reported per 1,000 live births. The periods examined are 2011-14 in column 1 (the period before UCTs were distributed), 2015-16 (the first two years of the UCT period), 2017 (the final year of the UCT period, during which there was a major drought in Kenya), and 2018-21 (the period following UCT disbursal). The main results for the pre-specified 2015-17 period of central interest are reported in Table 1. The ATE in high-intensity villages equals the average total effect of own-village estimates and spillovers in high-saturation sublocations. Reduced form standard errors are clustered at the sublocation level. Spatial HAC standard errors with a cutoff of 10km are reported for IV estimates. \*  $p < .10$ , \*\*  $p < .05$ ,

Table A.5: 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.6: 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.7: Spillover Effects of Transfers for Eligibles and Poor Ineligibles

	Infant Mortality		Hospital Delivery	
	(1) Eligibles	(2) Poor Inelig.	(3) Eligibles	(4) Poor Inelig.
Total effect IV	-19.46*** (6.50)	-8.51* (5.09)	0.20** (0.07)	0.23** (0.09)
Direct	-7.98* (5.21)		0.01 (0.03)	
Spillovers	-11.49* (5.98)	-8.51* (5.09)	0.18*** (0.07)	0.23** (0.09)
Control Mean	40.21	32.23	0.44	0.53
Observations	6317	7150	1154	290

*Notes:* Effects are estimated using Equation (2) augmented with an interaction with the characteristic of interest. Poor ineligibles are defined as ineligible households (i.e., home does not have a thatched roof) whose home has both mud walls and a mud floor. Information on home attributes is sourced from the baseline census, and approximately 67% of ineligible households fulfill these criteria. The median eligible household in the control group possessed USD PPP 435 in net non-land, non-housing assets, whereas the median poor ineligible household in the control group held USD PPP 643 in assets (48% higher than eligibles). In contrast, the median control household among other ineligibles possessed USD PPP 1335 in assets (108% higher than poor ineligibles and 207% higher than eligibles). All outcome variables are for births and households across 2015-17. Spatial HAC standard errors (Conley, 2008) with a cutoff of 10km are reported for IV estimates. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table A.8: Heterogeneity by Gender, Maternal Age, and Birth Order

	Infant Mortality			Child Mortality		
	(1)	(2)	(3)	(4)	(5)	(6)
	Male	Maternal Age 25+	Birth Order 2+	Male	Maternal Age 25+	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.9: 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.10: Unconditional Cash Transfers and Fertility

	(1)	(2)	(3)	(4)	(5)
	Births Per 100 Women 2011-14	Births Per 100 Women 2015-17	Births Per 100 Women 2018-23	Births Per 100 Women 2015-23	Completed Fertility Women 45+ in 2023 2011-23
<b><i>Panel A: Probability of Birth</i></b>					
Total Effect IV	0.28 (0.25)	0.79** (0.34)	-0.00 (0.18)	0.13 (0.13)	-0.08 (0.07)
Control Mean	7.19	7.54	7.01	5.58	7.69
Observations	23597	23597	23597	23597	979
<b><i>Panel B: Number of Births</i></b>					
Total Effect IV	0.22 (0.34)	1.13*** (0.38)	-0.15 (0.31)	0.27 (0.28)	0.28 (0.78)
Control Mean	9.12	8.48	9.66	9.27	11.31
Observations	23597	23597	23597	23597	979

*Notes:* Effects are estimated using Equation (2) on transfer-eligible women from the main EL3 census sample. Fertility outcomes represent the annual probability a woman gives birth to at least one child (Panel A), and the number of births per 100 women in a given period, with birth rates standardized to a one-year window to facilitate comparisons (Panel B). Column 5 restricts the sample to transfer-eligible women who were aged 45 year or over in 2023, the year in which the EL3 census was conducted. Spatial HAC standard errors in parentheses. \*  $p < .10$ , \*\*  $p < .05$ ,

Table A.11: 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.12: UCTs and Household Labor Supply by Gender Around the Time of Birth

	Spatial IV		Reduced-Form	
	(1) Women Hours Worked Last Week	(2) Men Hours Worked Last Week	(3) Women Hours Worked Last Week	(4) Men Hours Worked Last Week
<b><i>Panel A: First 6 Months In-Utero</i></b>				
Treated High-Saturation Estimate	6.57 (16.96)	-18.93 (16.05)	-0.79 (6.13)	-0.03 (7.00)
Control Mean in Period	38.44	46.23	38.44	46.23
<b><i>Panel B: 3rd Trimester and First 3 Months Postpartum</i></b>				
Treated High-Saturation Estimate	-24.48** (12.18)	-2.83 (9.84)	-15.06*** (5.29)	0.53 (5.00)
Control Mean in Period	39.51	39.30	39.51	39.30
<b><i>Panel C: Next 6 Months Postpartum</i></b>				
Treated High-Saturation Estimate	-5.08 (13.21)	1.88 (8.93)	9.98 (8.20)	-5.44 (5.11)
Control Mean in Period	53.26	26.49	53.26	26.49
Control Mean Overall	45.85	34.77	45.85	34.77
Total Observations	1766	1677	1766	1677

*Notes:* This table encompasses households surveyed in the first endline survey (2016-17) with a recorded birth in the EL3 census. 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 estimated differential 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 (columns 1-2) and Equation 1 (columns 3-4) augmented with interactions with the group of interest. Spatial HAC standard errors (columns 1-2) and robust standard errors clustered at the sublocation level (columns 3-4) in parentheses. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table A.13: UCTs and Effects on Maternal Health and Nutrition

	(1)	(2)	(3)	(4)	(5)	(6)
	Health Index Female All	Health Index Female Child $\leq 1$	Health Index Male All	Health Index Male Child $\leq$ Age 1	Child Food Security All	Adult Food Security All
Total Effect IV	0.06 (0.06)	0.28** (0.11)	0.00 (0.07)	-0.08 (0.12)	0.17*** (0.06)	0.09* (0.05)
Control Mean	-0.02	0.04	0.14	0.25	-0.10	0.03
Observations	3234	730	1521	299	3673	4768

*Notes:* \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ . The health-status index is a weighted, standardized average of self-reported health, index of health symptoms, and experienced a major health problem since baseline. The food security index is a weighted, standardized average of food security outcomes including number of days in the past week meals were skipped, gone entire days without food, and gone to bed hungry. The construction of both indices was pre-specified. The sample is restricted to eligible households.

Table A.14: UCTs and Effects on the Health of Women in Households with Infants

	(1)	(2)	(3)	(4)	(5)
	Health Index Female Child $\leq 1$	Major Health Episode Female Child $\leq 1$	Recent Health Symptoms Child $\leq 1$	Current Self- Reported Health Female Child $\leq 1$	Health Episode Resolved Female Child $\leq 1$
Total Effect IV	0.28** (0.11)	-0.09** (0.04)	-0.22** (0.11)	0.11 (0.15)	-0.10 (0.15)
Control Mean	0.04	0.14	0.00	3.65	0.32
Observations	730	730	730	730	85

*Notes:* \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ . The health-status index is a weighted, standardized average of self-reported health, index of health symptoms, and experienced a major health problem since baseline. The food security index is a weighted, standardized average of food security outcomes including number of days in the past week meals were skipped, gone entire days without food, and gone to bed hungry. The construction of both indices was pre-specified. The sample is restricted to eligible households.

Table A.15: Association Between Treatment Status and Baseline Characteristics For Eligible Households with 2015-17 Births

	(1)	(2)	(3)	(4)	(5)	(6)
	Education	Age	Marital Status	Income	Assets	Household Size
Total Effect IV	0.05 (0.33)	-5.08 (4.18)	0.06 (0.04)	-1649.02 (2452.02)	-7425.11 (4574.36)	0.09 (0.28)
Control Mean	8.50	26.71	0.72	12342.35	33897.07	4.35
Observations	1353	1353	1353	1353	1353	1353

*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.16: Unconditional Cash Transfers and Actual versus Predicted 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	0.28 (0.25)	0.79** (0.34)	0.02 (0.27)	0.70 (0.86)	-0.15 (0.60)
Control Mean	7.19	7.54	8.31	8.86	9.16
Observations	23597	23597	23597	1353	1353

*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. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table A.17: 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	0.70 (0.86)	0.11 (0.10)
Control Mean	8.86	7.11
Observations	1353	1353

*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.18: Impacts on Infant Mortality in Household Survey Sample

	(1) Pre-Specified Controls	(2) Socioeconomic Controls	(3) PDS LASSO- Selected Controls
<b><i>Panel A: Spatial IV Estimates</i></b>			
Total Effect IV	-36.16* (18.81)	-35.41* (19.16)	-37.83** (18.70)
<b><i>Panel B: Reduced-Form Estimates</i></b>			
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.11, 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.11 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.6). 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 major 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.5 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).<sup>44</sup> In 2014, the year in which cash transfers in our study region commenced,

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<sup>44</sup>We 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.5, 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.9.

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.9 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  $\theta$ , is uncertain and follows a distribution  $\theta \sim F(\theta)$ . We use the estimated posterior distribution of  $\theta$  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  $\theta$ , 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  $\theta$  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  $\theta$ :

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

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.