Disruptions to healthcare quality and early child health outcomes: Evidence from health-worker strikes in Kenya

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Abstract

This paper measures the effects of disruptions to healthcare quality at birth on early child health outcomes in Kenya. To identify impacts, we exploit variation in the timing and location of health-worker strikes at individual hospitals across the country between 1999 and 2014. Using data from Demographic Health Surveys, we find that children born during strikes are more likely to suffer a neonatal death. We find similar results using separate data collected in two informal settlements in Nairobi located near hospitals with frequent strikes. These results show that interruptions to healthcare quality can have large immediate health impacts, and suggests that status quo hospital care provides positive benefits. We also find suggestive evidence of reductions in later health investments, measured by vaccine take-up, among those who survive. This study provides the first rigorous evidence on the consequences of health-worker strikes, a frequent but understudied phenomenon in Sub-Saharan Africa.

Keywords: child mortality, institutional delivery, health-service provision

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1 Introduction

A large literature documents the many shortcomings of health services in low-income countries. Health-worker absenteeism is common (Chaudhury et al., 2006), and even when workers are present, the quality of care they provide is often low due to lack of knowledge, failure to impart critical information, or low effort (Das and Hammer, 2005; Das et al., 2008; Das and Hammer, 2014). In addition, surveys routinely find that many health facilities, including hospitals, lack an adequate supply of drugs, equipment, and infrastructure.[1] However, there is less evidence on whether this limited quality of care at health facilities affects health outcomes.

This paper measures the effect of hospital-care quality on early child health in Kenya. We identify the effects of healthcare quality on child outcomes by using variation in the timing and location of health-worker strikes in Kenyan hospitals. In data we collected from a broad set of media reports, we identify 23 health-worker strikes spanning 7 counties and 12 different hospitals from 1999 to early 2014.[2] The hospitals where strikes occur are typically large national or district-level referral hospitals. Although these hospitals often fall short of international standards for supplies, personnel, and training, they are generally among the best equipped to treat high-risk patients relative to other hospitals and health facilities in the country (Murphy et al., 2018; English et al., 2004).

Children face the highest risk of mortality in the neonatal period (the first 28 days of life), and they are particularly vulnerable within the first few days of life (Zupan and Aahman, 2005; Lawn et al., 2005). The most common causes of early child mortality include prematurity or low birth weight, severe infection, and asphyxiation (Lawn et al., 2005). While these and other common perinatal and neonatal complications are highly treatable, even very short-term delays in receiving appropriate care can lead to drastically increased chances of death or long-term morbidity (Lawn et al., 2009; Baqui et al., 2009; Simmons et al., 2010). In Kenya, it is estimated that approximately 18 percent of all newborns will develop complications that require inpatient neonatal services (Murphy et al., 2017).[3]

[1] see, e.g., Demographic and Health Surveys Service Provision Assessment, `https://dhsprogram.com/What-We-Do/Survey-Types/SPA.cfm`; World Bank Service Delivery Indicators, `https://www.sdindicators.org/`

[2] There are 47 counties in Kenya. In this time, there were also 6 national strikes, which we effectively control for through the inclusion of time fixed effects.

[3] Murphy et al. (2017) estimate that – with full care – their "findings suggest that of those newborns requiring neonatal care (183 per 1000 live births), 21% currently die within the neonatal period."

Nearly three-quarters of strikes feature either doctors or nurses, but not both, and therefore often result in major staffing shortages but not always a hospital closure. The average strike lasts 7 days. As we document with media reports, vulnerable neonates receive reduced services during strikes for at least three possible reasons: (a) the care provided in a striking hospital that has remained open is worse due to severely limited staff, (b) the child instead receives care in a nearby facility that is either lower quality or whose quality suffers due to overcrowding, or (c) the child cannot access necessary care.

To measure impacts on early child health outcomes, we combine our records of strikes with data from several waves of Demographic and Health Surveys (DHS) from 2003, 2008/09, and 2014. Child records in the DHS are linked to strikes by county, month, and year of birth. Our main empirical strategy relies on county and month-year fixed effects to compare outcomes of children born during a strike with others, conditional on where and when they are born. The key identifying assumption for this analysis is that the timing of strikes is exogenous to the timing of births, and we provide supporting evidence that this assumption holds, including no evidence of differential trends in births or outcomes within strike counties relative to other counties in Kenya, either in the months before or after strikes take place, and no difference in fixed characteristics of the mothers who give birth during strikes relative to those who give birth at other times.

We find that children born in counties during months when strikes occur are more likely to suffer a neonatal death. In particular, we estimate that a strike causes an additional 19 neonatal deaths per 1000 births, which represents a 68 percent increase in neonatal mortality relative to the overall sample mean. Both wild bootstrap and randomization inference permutation test based p-values reject the null hypothesis of no effect beyond the 10 percent significance level ($p = .094$ and .082, respectively). We show with a battery of alternate specifications, which relax assumptions about location effects and time trends or which impose various sample restrictions, that our point estimates are likely not an artifact of model choice.

As further evidence that we are not picking up a spurious correlation, we replicate our estimation of the effects of strikes on neonatal mortality using data from the Nairobi Urban Health and Demographic Surveillance System (NUHDSS). The NUHDSS tracks households in two urban informal settlements located near large hospitals in Nairobi with frequent strikes. In addition to information on exact day of birth and mortality, this data set also includes rich verbal autopsy data regarding cause of death. We again find statistically significant increases in neonatal mortality for children born on days when strikes occurred (p-values of .067 and .091 using cluster robust standard errors and randomization inference permutation tests, respectively), and we see that deaths of those born during strike days are disproportionately attributed to neonatal causes.

The effects we document are likely largely driven by newborns who require the services of striking hospitals, i.e. those that develop neonatal complications in the first few days of life. In a bench-marking exercise, we show that our results are consistent with a tripling in mortality among this subset of children. While large, effects of this size are in line with evidence on the efficacy of providing timely, adequate care for at-risk newborns. Indeed, access to a suite of readily-available interventions in low-income countries (such as clean delivery, antibiotics, and newborn resuscitation and temperature management, among others) has been estimated to reduce neonatal mortality by 55-82 percent (Jones et al., 2003; Darmstadt et al., 2005; Lawn et al., 2009; Bhutta et al., 2014). These estimates imply that when this care is not provided, neonatal mortality can be 122-455 percent higher.

Other studies also show large benefits of institutional births.[4] Godlonton and Okeke (2016) find that a temporary ban on traditional birth attendants in Malawi increased facility deliveries and, among women whose closest facility is "high" quality, reduced 7-day mortality by 1.3-1.4 percentage points and 1-month mortality by 1.6-1.8 percentage points. Okeke and Chari (2018) use a difference-in-differences strategy to show that non-institutional births in Nigeria increase neonatal mortality by approximately 10 deaths per 1000 births overall (a doubling of the mean). In Ghana, Friedman and Keats (2020) find that a policy that made facility births free closed the rural-urban neonatal and infant mortality gap. Large effects have been found in high-income countries as well. Daysal et al. (2015) show hospital births among low-risk women in the Netherlands decrease neonatal mortality relative to home deliveries by 8 to 9 deaths per 1000 live births (compared to a base of 2 per 1000), while Almond et al. (2010) find that the package of services offered in US Neonatal Intensive Care Units (NICUs) reduces neonatal mortality by one percentage point (off of a base of 3.8 percentage points) for the marginal NICU entrant.

Nevertheless, we caution against taking our point estimates of the effect of strikes on neonatal

[4]One exception is Powell-Jackson et al. (2015), who find that a conditional cash transfer program in India designed to encourage institutional births had no effect on neonatal mortality.

mortality too literally. Our estimates, using either the DHS or NUHDSS samples, are imprecisely measured and our study is not powered to detect smaller effects. A growing literature highlights concerns that statistically significant results from low-powered studies may be exaggerated representations of treatment effects (Camerer et al., 2016, 2018; Gelman and Carlin, 2014). Although the statistical evidence suggests a null effect is very unlikely, we cannot rule out a wide range of positive mortality effects that, while still economically significant, may be lower than what our point estimates suggest.

In addition to neonatal mortality, we also report on longer-run outcomes. We find no effects on post-neonatal infant mortality (deaths in the first year of life conditional on surviving the first month), suggesting that strikes cause excess mortality rather than accelerating deaths that would have occurred later in infancy. For children who survive, while we do not find any evidence of reductions in health status, we do find some suggestive evidence that vaccine take-up may be lower. We use child height- and weight-for-age z-scores as summary measures of long- and short-term health, respectively, and find that neither outcome is affected by strikes. However, children born during strikes may be less likely to be fully vaccinated against common illnesses such as polio and measles, although these results are sensitive to the choice of specification. Given that the recommended immunization schedule for most vaccines begins once children are at least 6 weeks old, and that the typical strike lasts only 1 week, a reduction in vaccinations would suggest that strikes may have impacts on these and other important child health inputs that continue long after strikes have ended. While such a finding is consistent with prior literature on long-run impacts of interruptions to care (e.g. Goldstein et al. (2013); Sievertsen and Wüst (2017); Okeke and Chari (2018)), we stress that the results in our study are more speculative.

Overall, our findings contribute to a small but growing literature on the causal links between healthcare quality and health outcomes. Björkman and Svensson (2009) find that a program that encouraged community monitoring in Uganda increased provider effort and had positive effects on under-5 mortality and infant weight. Similarly, Okeke (2019) finds that peer monitors in Nigeria increase doctor effort and patient reports of overall health. In addition, two randomized control trials examining pay-for-performance schemes in Rwanda and the Philippines, respectively, find that incentives increase provider productivity and improve child height and weight and self-reported health (Gertler and Vermeersch, 2013; Peabody et al., 2013). Our paper extends this literature by showing that disruptions to status quo care in top-line hospitals have negative effects on early child mortality, and suggests that when these disruptions do not occur, hospitals are providing immediate, and positive health benefits.

Finally, this paper contributes the first rigorous evidence on the effects of health-worker strikes in Sub-Saharan Africa, an apparently growing but still under-studied phenomenon. In 2014 alone health-worker strikes limited service provision in Kenya, Liberia, Nigeria, South Africa, Sudan, Uganda, and Zimbabwe. In the three years before, Botswana, Burundi, Ghana, Guinea Bissau, Malawi, Mozambique, Namibia, Sierra Leone, Swaziland, Tanzania, Uganda, and Zambia also saw health-worker strikes. Prior evidence on the effects of these strikes is limited to a handful of medical case studies (Gyamfi, 2011; Bhuiyan and Machowski, 2012; Njuguna, 2015; Adam et al., 2018, see), which generally find that health outcomes are negatively correlated with strikes.[5] However, while suggestive, these studies are based on extrapolating from the patients who visit a facility immediately prior to, during, or after a strike, and there may be important selection biases at work.

The remainder of the paper proceeds as follows. Section 2 describes the data on health-worker strikes and outcomes, and provides additional context on the healthcare system in Kenya and on strike hospitals in particular. In Section 3 we outline the estimation strategy, and in Section 4 we present the results. Section 5 concludes.

2 Data and Context

This paper uses panel data, linking information on the timing and location of health-worker strikes with birth and early life inputs and outcomes. Information about strikes comes from data we collected through digital archives of newspapers. For the main analysis, the birth and child data comes from the Demographic and Health Surveys, which we link to the strikes data by county, year, and month. We also supplement this analysis with birth and mortality data from the Nairobi Urban Health and Demographic Surveillance System (NUHDSS). The NUHDSS data is linked to the strikes information by exact date.

[5] There are also a small number of papers measuring the outcomes of health-worker strikes in high-income countries. For example, Kronborg et al. (2016) estimate the effects of a nurse strike in Denmark and find a reduction in breastfeeding duration (a measure of parents' investment in health), and Gruber and Kleiner (2012) find an increase in in-patient mortality during a nurse strike in New York state.

2.1 Strikes data

The database of health-worker strikes was compiled by searching through the digital archives of newspapers from Kenya and sub-Saharan Africa.[6] In total, we recorded 29 strikes from 1999 through April 2014, of which 23 are local. There appears to be an increase of strikes over the last 5 years of this period, although we cannot rule out the possibility that this is driven by an increase in reporting. For each strike, we record the location, the start and end-dates, who is on strike, and the target of the grievance.

Most health-worker strikes in Kenya are local (in our sample, 23 are local and 6 are national). Half of the local strikes occurred in Nairobi, the capital, with the remainder spread across 6 separate counties (there are 47 counties in Kenya total). The complete list of the local strikes used in the analysis is presented in Table 1. Almost all of the local strikes were based in single large public facilities serving many clients, including 8 in Kenyatta National Hospital, 5 in Pumwani Maternity Hospital (Kenya's largest maternity hospital), and 3 in Moi Teaching and Referral Hospital. When an article listing the end-date of the strike was not available, the date of the most recent article that said the strike was ongoing was used as the end-date.[7] For this reason, we are able to know the minimum duration of each strike in our data set, but some may have lasted longer. Local strikes last about one week on average, while national strikes are longer, lasting almost 27 days on average. Only local strikes are used in the analysis in order to utilize both time and place variation.

Strikes may consist of only doctors, only nurses, or all health workers, and therefore, while strikes cause major disruptions to the delivery of care, they do not always result in hospitals completely closing down (just 25 percent of local strikes involve all health workers). In the vast majority of cases, health-worker salaries, general compensation, or working conditions are the main complaints of the striking workers. The most common grievance is low or unpaid salaries or other payments, and this is the main complaint in 62 percent of strikes.[8] This includes both demands for higher

[6]The sources that we searched and where we found articles are the following: The Daily Nation, The Star, The Standard, The New Humanitarian (formerly IRIN news), and All Africa. All Africa aggregated articles from many of these newspapers, and provided very helpful search capabilities, allowing us to find articles that were not otherwise accessible.

[7]In a robustness check described in Section 4.1.1 we find our results are not sensitive to reasonable changes to end dates.

[8]Management of public health services was centralized until 2013, when control was devolved to county governments. This policy change was accompanied by a government agreement to double physician salaries and hire additional health workers. However, failure to follow through with this promise, along with problems implementing the devolution reform led to a surge in strikes beginning in 2014. For example, in December 2016, doctors conducted

wages or stipends and demands for previously agreed upon but unpaid compensation. Another 15 percent included low or unpaid salaries along with another complaint. The remaining grievances mostly deal with working conditions (e.g., the stock of drugs and equipment, workers being allowed to join a union, and the firing of contract employees).

2.2 Context and Characteristics of Strike Hospitals

To place these strikes in context, it can be helpful to understand a bit about the delivery of maternity and neonatal services in Kenya and where the health-worker strikes occur. Public facility maternity and neonatal services are offered at sub-county health centers, and sub-county, county, provincial, and national hospitals. Private clinics are much less frequently used.

Kenyan health services are characterized by relatively poor quality and are roughly comparable to those in other Sub-Sahara African countries (Daniels et al., 2017). A recent World Bank Service Delivery Indicator survey shows that absenteeism, diagnostic skill and clinical practice, facilities, and the numbers of doctors, nurses, and midwives per person in Kenya are all similar to or slightly higher than other countries in Sub-Saharan Africa.[9]

The majority (65 percent) of the hospitals where strikes occurred in our sample are first-referral level county hospitals and national referral hospitals. Another 22 percent of the strikes in our sample occurred in Pumwani Maternity Hospital, the largest maternity hospital in East Africa. These are the top-line hospitals that exist in Kenya and the ones that take the hardest cases. Children born elsewhere are also referred to these hospitals if they develop neonatal complications following birth.

a nationwide strike lasting nearly 4 months, followed by a national nurses strike that ended after 5 months. Both the main union for doctors (the Kenya Medical Practitioners, Pharmacists, and Dentists Union (KMPDU)) and the union for nurses (the Kenyan National Union of Nurses (KNUN)) negotiate with the national government and county governors who are responsible for carrying out national health policies within their constituencies. In some local strikes, smaller groups of health workers organize to negotiate with hospital management.

[9] This survey found 28 percent of healthcare providers (and 38 percent of doctors) were absent from facilities during unannounced visits (World Bank, 2013), an absenteeism rate on par with other SDI countries and many other low-income countries (Chaudhury et al., 2006). While health workers in Kenya perform better than their peers in Uganda, Tanzania, and Senegal at diagnosing common illnesses provided in case studies (72 percent), they are no better at following proper clinical guidelines for those same illnesses, and guidelines are followed less than half the time (World Bank, 2013; Wane and Martin, 2013). Kenyan facilities score similarly well as other SDI countries on infrastructure and equipment availability (57 and 76 percent, respectively), and higher than Uganda on drug availability (67 percent compared to 40 percent). The country has 16.8 doctors, nurses, and midwives per 10,000 people, which compares favorably to the Sub-Saharan Africa regional average of 13.1, but is still well below the recommended level of 23 top level health workers per 10,000 people advocated by the World Health Organization (WHO, 2018), suggesting that overcrowding and congestion is an issue.

First-referral level county hospitals and national referral hospitals in Kenya have many shortcomings, but relative to most other health facilities, they are the best suited to deliver life-saving care for these complications. The World Bank's Service Delivery Indicators (SDI) report (2013) and the DHS Service Provision Assessment (SPA) report (2010) both show that hospitals in general are relatively more likely than other facilities to have essential equipment and supplies (including oxygen, antibiotics, antimalarials, intravenous fluids, and vitamin A) as well as better trained doctors and nurses (in terms of both identification of common illnesses and case management practices). For example, while just 30 percent of all health facilities offer services for normal deliveries, 95 percent of hospitals do. Further, 52 percent of hospitals are equipped to perform cesarean sections, while these services are virtually non-existent at other types of facilities (DHS SPA, 2010). Similarly, Murphy et al. (2018) find that large public hospitals in Nairobi are better equipped than smaller hospitals and are generally better equipped than private hospitals. Within counties, strike hospitals also often represent the only options for this level of care. On average, hospitals where strikes occur account for 55 percent of beds available in top-level facilities, and in about one-third of cases, strike hospitals account for 100 percent of these beds.[10]

2.3 DHS Data

Demographic and Health Surveys are population surveys conducted across low-income countries with the original goal of providing information necessary to estimate future population trends. The surveys ask a nationally representative sample of women ages 15-49 to report extensive details of their fertility histories, including the timing of all births in the last five years, the actions of the mother and services sought both before and at birth, and initial and long-term health outcomes of the children. Some information about children is asked for all previous births. We use the sample of births in the last five years for our analysis, to avoid concerns about recall bias and to maintain a consistent sample across outcomes. The data used in the analysis is from the 2003, 2008/09, and 2014 DHS and includes children born between 1999 and 2014.

Sample characteristics of children, including key health inputs and outcomes, are presented in Panel A of Table 2. The first column shows summary statistics for the full analysis sample of just

[10]Authors' calculation using data from the Kenya Master Health Facility List, which provides details on hospitals by county including information on the level of the hospital and how many beds it has (available at http://kmhfl.health.go.ke).

over 31,000 births. Columns 2-3 split the sample by whether the birth was at home or in any health facility, while Columns 4-5 divide the sample by births in counties that never experienced a strike and those that had at least one strike during the study period.

Mortality rates among children are remarkably similar across locations. There were approximately 28 deaths per 1000 births in the first month of life in our sample, and an additional 22 deaths per 1000 births by 1 year of age. Neonatal mortality rates are identical for children born in health facilities compared to children born at home, and slightly (statistically significantly) higher in counties where strikes occurred.[11] To avoid double-counting deaths, we use as our next outcome post-neonatal infant mortality.[12] Post-neonatal infant mortality is slightly higher for those born at home and slightly higher in counties that did not experience strikes. Both of these differences are statistically significant.

Children in Kenya are smaller than the world-wide reference population for height- and weight-for-age, approximately one standard-deviation below the mean in each.[13] Those born at home are .4 standard deviations shorter than those born in facilities and those born in non-strike counties are .08 standard deviations shorter than those born in strike counties. Similarly, those born at home are .49 standard deviations lighter than those born in facilities and those born in strike counties are .2 standard deviations lighter than those born in strike-counties. These differences are all statistically significantly different.

Half of all births are reported to have taken place in a health facility, rather than in a home. Of the facility births, three-quarters were reported to have been in hospitals. Nearly all facility births, and almost no home births, were attended by either a doctor, nurse, or midwife.[14] Facility

[11] We code as neonatal mortality any births listed by the mother in a list of all live births, in which death occurred within the first month. Neonatal mortality is defined by the World Health Organization as deaths within the first 28 days following live births. Therefore, we may have introduced a small amount of measurement error with deaths that occurred in the last few days of a month. We may also inadvertently include some intrapartum deaths – deaths which occurred in the 12 hours *before* birth – if a mother reports them in a list of *live* births. This variability in how intrapartum deaths are recorded has been demonstrated to be especially large in developing countries, complicating comparisons across different data sources (Blencowe et al., 2016). We choose to use this definition, as this is the definition used by MeasureDHS (`https://dhsprogram.com/data/Guide-to-DHS-Statistics/Early_Childhood_Mortality.htm`). In addition, including intrapartum deaths, which are known to be influenced by the quality of healthcare (McNamara et al., 2018), is consistent with the aim of looking at the effects of a temporary shock to healthcare service quality and birth outcomes.

[12] To construct this variable, we include all births listed in a register of births in which the month of death is more than 1 and less than or equal to 12. Neonatal deaths - in the first month - are not included in this sample.

[13] We use the height-for-age and weight-for-age z-scores based on the CDC Standard Deviation-derived Growth Reference Curves, as reported in the DHS data.

[14] Approximately 68 percent of home births were attended by either a traditional birth attendant or a community health worker.

births, and hospital births in particular, are higher in counties that experienced at least one strike compared to those that never had any strikes.

Beyond location of birth, vaccination rates offer another measure of early infant care included in the DHS. With the exception of the tuberculosis (BCG) vaccine, which is often administered at or near birth, vaccinations for diphtheria, pertussis, and tetanus (DPT), polio, and measles are typically given beginning when children are at least one month old through the first year of life.[15] While most children receive the BCG vaccine (92 percent), 21-29 percent of children are not fully immunized for DPT, polio, or measles. Vaccination rates are higher among children born in health facilities (by 6-13 percentage points) and are higher in counties that had strikes compared to non-strike counties (by 4-6 percentage points).

Characteristics of mothers in the sample are presented in Panel B of Table 2. Summary statistics are again presented for the full sample (Column 1) of 22,483 mothers who reported giving birth at least once in the five years before the survey. Next we compare mothers who reported delivering all births in the last five years at home with those reporting at least one birth in a health facility (Columns 2-3), and mothers residing in counties with and without strikes (Columns 4-5). The average mother in the sample is 29 years old, has 3.5 children, and 7 years of schooling (in Kenya, primary school consists of 8 years). Just over three-quarters of mothers are married, one-third live in an urban area, and 20 percent have access to electricity in their home.

There are large differences between mothers who deliver in facilities and those who do not, as well as between mothers in counties that experienced a strike compared to mothers in non-strike counties. Mothers who delivered at least one child in a facility in the five years preceding the survey have 1.4 one fewer children, have almost four years additional schooling, and are about 50 percent more likely to live in urban areas relative to women who report delivering all births in the last five years at home. Women residing in counties where strikes occurred are also more educated and tend to live in more urban areas than their counterparts in other counties. Thus while mothers residing in counties where strikes occur are not representative of average women in Kenya, they appear to be representative of the average Kenyan woman who typically delivers in a health facility.

[15]According to Kenya's national guidelines on immunizations, the DPT and polio vaccines require 3 doses at 6, 10, and 14 weeks of age, and the measles vaccine requires 1 dose at 9 months of age. The outcome variables we used are based on self-reports from mothers at the time of the survey. For DPT and polio, our vaccinated variable is equal to one if they have had all three doses and zero otherwise.

3 Empirical Strategy

To motivate our empirical strategy, we first present the raw neonatal mortality data from the main analysis sample in a set of plots in Figure 1. The goal of this exercise is to observe how neonatal mortality rates evolve in the course of a year around a health-worker strike, both within counties where strikes take place and across the country. To do this, we regress neonatal mortality on indicators for births occurring in each of the six months before and after a local strike in any location, and indicators for those same periods in locations where the strike specifically occurred:

$$Y_{ict} = \alpha + \sum_{t=-6}^{6} \phi_t I_t + \sum_{t=-6}^{6} \sigma_t Strike_{ct} I_t + \varepsilon_{ict} \tag{1}$$

I_t is the indicator for a birth t months distant from the date of a strike, and $Strike_{ct}$ is the indicator for whether the birth occurred in a strike county t months from the date of the strike.[16] In this specification, α represents average neonatal mortality in Kenya in periods more than 6 months distant from any strikes, the coefficients ϕ_t represent the difference in neonatal mortality between children born t months from a strike occurring anywhere in Kenya and this base rate, and the coefficients σ_t represent the difference in neonatal mortality between children born in counties in which there was a strike, t months distant from that strike, relative to children born in the same periods in non-striking counties.

The coefficients ϕ_t and σ_t are plotted in panels (a) and (b) of Figure 1, respectively. In panel (a) the coefficients ϕ_t are all close to 0, with no apparent trend in neonatal mortality rates across Kenya in the months before or after a strike, suggesting that strikes do not occur during periods of worsening or improving mortality nationwide. In panel (b) the coefficients σ_t are larger in absolute value (possibly because they are less precisely estimated since there are fewer observations within cells), but they similarly show no apparent trend in neonatal mortality in the months preceding or following a strike in counties where strikes occur. The coefficient at time zero in this plot, however, is large and positive, suggesting that there are a greater number of neonatal deaths among children

[16] To clarify, these indicators are not mutually exclusive for all observed births. Depending on the timing of strikes both within the birth county and across the country, it is possible for multiple indicators to be nonzero for a single observation. For example, children born in Nairobi in February 2002 both qualify as being born during a strike month in a strike county and as being born two months prior to a strike occurring in another county (Uasin Gishu, April 2002).

born when strikes occur in their birth county.

Thus, in the main analysis that follows we set out to test directly whether there are differences in outcomes of children born in a given county when there is a strike and when there isn't one, relative to the differences in outcomes of children born in other counties in the same months. We estimate the following equation:

$$Y_{icym} = \beta_1 Strike_{cym} + \gamma_c + \delta_{ym} + \mathbf{X}_{icym} + \varepsilon_{icym} \tag{2}$$

where Y_{icym} is the outcome variable for a birth to mother i, in county c, during year y, and month m. The variable $Strike_{cym}$ is an indicator for whether there was a local strike occurring in the month, year, and county of birth. County and year-month fixed effects, represented by γ_c and δ_{ym} respectively, control for time-invariant variation in unobservables across counties as well as any factors that change outcomes over time similarly across the country. $\mathbf{X}_{icym}$ are individual mother and child controls (described below). Thus, β_1 is the coefficient of interest, which captures the difference in outcomes in a county with strikes relative to periods without strikes in the same county and relative to other counties at the same time.

The specification in equation 2 makes several parametric assumptions about the evolution of outcomes over time and across counties. To address these, we include a set of 10 supplementary specifications that relax or alter constraints on time and location effects, restrict the sample in various ways to change the set of comparison observations (e.g. restrict the data to exclude counties which never experienced strikes), or that allow the months just around a strike to be unique (e.g. by including leads and lags for births occurring in strike counties in the months before and after a strike). These specifications and their motivations are described in more detail in results section 4.1.1.

Identification in this analysis rests (in part) on the assumption that it is plausibly exogenous whether a child is born during a strike month compared with just before or after. The fact that conception occurs 9 months before the birth makes this a reasonable assumption: for the most part, women cannot choose to change the timing of their deliveries with respect to the timing of a strike. There are some exceptions to this. Particularly with the availability of induction and cesarean-sections, it is absolutely possible for women to change the date of delivery, within a small window.[17] However, inductions and cesarean sections are relatively uncommon in Kenya, as in many low-income countries, and therefore these are less likely to be used to alter the date of delivery.[18] Further, while women can change the timing within a few days, it is unlikely that women can, or would want to, change the timing by a larger amount. In this paper, the main analysis relies on the month of birth, which is not likely to be altered frequently in response to a health-worker strike.

We can test this assumption directly using our data on the timing and frequency of births across months. If women were shifting births into the month before a strike, we would expect to see more births reported in the last few days of a month just before a strike and fewer births reported in the first few days of a strike month. We only have information on exact birthday when the child is still alive at the time of survey. Using the reported birth dates for this sub-sample, we do not see evidence of shifting births. Additionally, using data on all births, the last column of Table A1 shows there are no statistically significant differences in the number of births in strike counties in months when strikes occur.

We can also test this assumption directly by comparing the demographic characteristics of women who gave birth during strikes with those who gave birth at other times. If the timing of strikes is exogenous to the timing of births, there should be no differences in observables across women who deliver during strike months and those that deliver at other times. The results of this test are presented in Table A1. Across a range of characteristics, including age at the time of survey, years of schooling, literacy, marital status, urban-rural status, electricity access, sex of the child, subjective size of the newborn, and the mother's age at the time of birth, we see only two statistically significant differences. Mothers are slightly younger at the time of birth and at the time of the survey. As a precaution, in all regressions we control for mothers' age and age-squared, education, urban-rural status, and household wealth quintiles. The vector $\mathbf{X}_{icym}$ includes these controls as well as controls for child's gender, birth order, and an indicator for being a multiple birth.

For our main results we report three tests of statistical significance: cluster robust standard

[17] It has been shown in high-income countries that women do have some control over the timing of deliveries and respond to tax incentives (Gans and Leigh, 2009; Dickert-Conlin and Chandra, 1999; Milligan, 2005).

[18] In our sample, 6 percent of births (13 percent of those in facilities) were delivered through cesarean sections. While we do not have information about inductions, the WHO estimates that 3.9 percent of births in Kenya are induced, and 12.1 percent of these are elective inductions.

errors, wild bootstrap p-values, and p-values based on randomization inference permutation tests. The standard errors are clustered at the county level to adjust for any correlation of errors within each county (Bertrand et al., 2004). There are 47 counties in Kenya – exceeding the 'rule of 42' clusters often cited as sufficient for reliable inference (Angrist and Pischke, 2009). However, the *effective* number of clusters is likely smaller (due to unequal observations across clusters) and as a consequence the cluster robust standard errors may be inappropriately sized and tend to over-reject (Carter et al., 2017; MacKinnon and Webb, 2017).

As a more conservative approach, we therefore also include wild bootstrap p-values. Following MacKinnon and Webb (2017) and MacKinnon and Webb (2018), we present subcluster wild bootstrap p-values, using the ordinary wild bootstrap, rather than the wild cluster bootstrap of Cameron et al. (2008). MacKinnon and Webb (2017) and MacKinnon and Webb (2018) show that when there are few treated clusters (as in our case), the restricted wild cluster bootstrap can severely under-reject, while the unrestricted wild cluster bootstrap can severely over-reject. Our data exhibits this exact pattern of very large differences between restricted and unrestricted p-values. We implement the wild bootstrap using Roodman et al. (2019).

A second option, which we also implement, uses a randomization-inference-based permutation test to create p-values for statistical inference. MacKinnon and Webb (2020) recommend a randomization inference procedure based on t-statistics as an alternative to test statistical significance when the number of treated clusters is small. This t-statistic approach has also been shown to have exact size under the sharp null of no treatment effects for all observations, as well as to be asymptotically valid under the weak null of no *average* treatment effects (Roth and Sant'Anna, 2021; Roth et al., 2022). To estimate the permutation test p-values, we randomly re-assign county-year-months to "strike" or "no-strike" conditions – within counties that ever experienced strikes – and estimate the treatment effects 2000 times, saving the t-statistics.[19] Then we compare the estimated t-statistic from the true treatment status to the permutation distribution to come up with the likelihood of finding an effect with a t-statistic as large as what we observe. The procedure is implemented in STATA using Heß (2017).

Finally, as a last check against conflating spurious sample-driven results with true causal effects, we repeat our analysis using a different data set. These data were collected by the Nairobi Urban

[19] A distribution based on treatment assignment in all counties yields smaller p-values.

Health and Demographic Surveillance System (NUHDSS), and contain records of births and deaths which can be matched to our data on strikes. We describe this data in more detail in results section 4.1.2.

4 Results

4.1 Neonatal mortality

We find that children born in counties during months when strikes occur are more likely to suffer a neonatal death. Specifically, our results suggest that children born during a strike are 1.9 percentage points more likely to die within the first month of life (Table 3 panel A column 1). Both the wild bootstrap and permutation-based p-values reject the null hypothesis of no effect beyond the 10 percent significance level ($p = .094$ and .082, respectively); using the cluster robust standard errors, the null hypothesis is rejected beyond the 1 percent level. The point estimate is large, representing an increase in neonatal mortality of 68 percent relative to the sample mean (an increase of 19 deaths per 1000 births relative to a mean of 28 deaths per 1000 births). However, confidence intervals are also wide, and we cannot rule out a wide range of effects.

In the following subsections, we show that the size of the estimated effect of strikes on neonatal mortality is not sensitive to variations to our main specification or to different sample restrictions (subsection 4.1.1); we show that we obtain comparable estimates using different data collected from two informal settlements in Nairobi located near hospitals with frequent strikes (subsection 4.1.2); we assess the plausibility of large effect sizes by bench-marking our results to effects suggested by secondary sources in the medical literature (subsection 4.1.3); and we discuss the likelihood that, given the relatively limited statistical power in our analysis, our point estimate may nevertheless overstate the true effect of strikes on neonatal mortality (subsection 4.1.4).

4.1.1 Similar estimates with alternate specifications

Our main specification makes several parametric assumptions about the evolution of outcomes over time and across counties. To investigate whether these assumptions influence the size of the estimated effect of strikes on mortality, we include 10 alternate specifications that alter how we model outcomes across locations and over time, or that restrict the sample to particular locations or times when most strikes in our panel occur, or which do both.

The first set of alternate specifications relax constraints on time and location effects. We add county-specific time trends to our main specification to allow outcomes to follow different (linear) paths over time across counties. In another specification, we use county-year fixed effects in place of county fixed effects to allow (non-parametrically) for the possibility that outcomes evolved differently in years just prior to or following strikes in strike counties, and relative to other counties that did not experience strikes. In a third regression, we include DHS survey cluster fixed effects (rather than county fixed effects).[20] DHS survey cluster fixed effects soak up variation at a smaller geographic level and help account for differences in outcomes and effects within counties between locations close to striking hospitals and those farther away.

We next restrict the sample to focus on periods and locations where strikes were more frequent. Because infant mortality (and to a lesser extent, neonatal mortality) declined over the 20 years of the study, and because most strikes are concentrated in the last 5 years of the study period, we restrict our analysis to births that occurred after 2009. This helps avoid extrapolating from years when there were few strikes and also higher mortality. Separately, we restrict the sample to only include counties that ever experienced strikes (7 out of the 42 counties in Kenya), as counties without strikes over the 20 year period are systematically different from counties with strikes and these differences may confound identification of treatment effects. Finally, we combine both of these sample restrictions with the county trends in another specification.

One also might be concerned that changes in healthcare quality in the months just before or after a strike might be driving our results. For example, worsening conditions in the months before a strike might motivate the strike, which would bias our effects toward zero. On the other hand, if conditions improve in the months immediately following a strike, then we would over-estimate the health costs of the strike. We therefore present a set of specifications that (1) include additional fixed effects for births in each of the six months before and after a strike, (2) exclude births from strike counties that occurred within six months before or after a strike, and (3) restrict the sample to include only those births that occurred within six months before or after a strike.

Finally, because there is likely some measurement error in our records of strikes, we repeat our

[20]DHS selects geographic clusters of households as part of their standard sampling design. In rural areas, this corresponds with a small village, and in urban areas, it will be a small part of the city. These are the DHS survey clusters.

main analysis with alternate end-dates of strikes when end-dates are uncertain. We are missing 9 out of 23 end-dates in the full sample of local strikes, and 3 out of 13 for the years 2010-2014.[21] We can use our data to directly address how much this mis-measurement could change our results. Most of the strikes with missing end-dates are coded as quite short (mean 3.2 days, median 2 days). Assuming each of the missing end-dates is off by 2 weeks, and re-coding all births that happened in these county-months as having occurred during a strike, we then re-run the main analysis.[22]

Reassuringly, across the 10 different specifications described above, we obtain nearly identical results for the effect of strikes on neonatal mortality. Figure 2 presents the coefficients on the strike variable for each specification along with wild bootstrap 95 percent confidence intervals and p-values. Although statistical power varies across specifications and sample restrictions, with some p-values larger than others, the point estimates remain large (ranging from .016 to .022), and are consistent with the results from our main specification.

4.1.2 NUHDSS sample also shows a similar estimated neonatal mortality effect

While it seems unlikely that mis-specification error is contributing to the large estimated effect of strikes on neonatal mortality that we observe in the DHS data, these results could still be due to sampling error. We therefore supplement the main analysis by exploiting a separate panel data set with observations collected from two informal settlements in Nairobi (Korogocho and Viwandani) located near two hospitals that experienced strikes during this period (Pumwani Maternity Hospital – the largest maternity hospital in the country, and Kenyatta National Hospital – the country's largest referral hospital).

The longitudinal data set was collected by the Nairobi Urban Health and Demographic Surveillance System (NUHDSS). The sample frame began with a census in 2002, and over time individuals entered and exited the sample through births, migration, and deaths, which are tracked through household visits every four months. During the survey period (between 2003-2014) there were 23,181 births.

Although not nationally representative, the NUHDSS has a number of advantages relative to the DHS data. First, since the informal settlements are located near two large hospitals which

[21] Two of the strikes between 2010 and 2014 with missing dates were mentioned on Twitter as ending the same day as our last written article about them, suggestively corroborating our estimated end-date.

[22] Results are similar if we assume the end dates are off by 1 week or 3 weeks.

experienced strikes, we can be more confident that pregnant women in the NUHDSS data were directly affected by strikes. Second, the NUHDSS records exact day of birth (and death), and so there is likely less measurement error in classifying which births were affected by strikes. Finally, in the case of deaths, the cause of death is also recorded through a verbal autopsy technique, providing some insight into the factors driving the mortality results.

Because the data are restricted to just these two communities within Nairobi, we are unable to separately control for county location fixed effects and therefore estimate the following equation:

$$Y_{iymd} = \beta_1 Strike_{ymd} + \delta_y + \upsilon_m + \phi_d + \varepsilon_{iymd} \tag{3}$$

where Y_{iymd} is the outcome variable for a birth to mother i, during year y, month m, and day of the week d. As with the DHS, we use information on births between 2003-2014 to construct a retrospective panel of births, which we link with the strikes data. For each birth we record whether the child was still alive by the end of the panel, or, if the child had died, the age at death. We present cluster robust standard errors (clustered at the birthday level) as well as p-values based on permutation tests similar to what we used with the DHS data (Heß, 2017). In this case, we randomly re-assign treatment (strikes) to different days 2,000 times, re-estimate the equation above, and compare the t-statistic in our initial estimate to the resulting distribution of t-statistics.

Reassuringly, the results using the NUHDSS data are very similar to those found using the DHS data – health-worker strikes increase neonatal mortality. Panel B of Table 3 presents the results. The point estimate suggests that children born during days when there are health-worker strikes are 1.4 percentage points more likely to suffer a neonatal death. Using the cluster robust standard errors, the null hypothesis of no effect is rejected beyond the 5 percent significance level, while using the permutation-based p-value rejects no effect beyond the 10 percent level.

We next examine the verbal autopsy records to check whether the causes of death for those born during strikes are consistent with receiving limited care around the time of birth. In Figure 3, we present the causes of deaths for all children who were reported to have died in the NUHDSS sample, split by those who were born during strikes and those born at other times. We see a noticeably larger fraction of deaths of those born during strikes attributable to neonatal causes. Notably, we do not see this increase in deaths due to other causes (such as injuries, malnutrition, or AIDS/HIV), which would not likely be affected by a strike.

Finally, in both the NUHDSS and DHS data, we find suggestive evidence that the effect of strikes is more pronounced earlier in the neonatal period, which is also consistent with the hypothesis that strikes disrupt access to timely newborn care. Both data sets include age (in days) at death based on mothers' recall. In the DHS, using mortality within the first 7 days as the dependent variable, we obtain a coefficient for the effect of strikes of .016. Given that the coefficient for the effect of strikes on neonatal mortality was .019, this suggests that the vast majority of the neonatal mortality effect is driven by deaths within the first week of life. In the NUHDSS, 1-week mortality accounts for 36 percent of the neonatal mortality effect (a point estimate of .005 compared to .014), deaths in the first 14 days account for 64 percent of this effect (point estimate of .009), and deaths within the first 21 days account for 86 percent of the total neonatal mortality effect (point estimate of .012). However, we caution against over-interpreting these findings given that mothers' recall may be imprecise over the exact age at death in days, especially for more distant births.[23]

4.1.3 Large neonatal effects are plausible

The large neonatal mortality effects suggested by our point estimates are consistent with other estimates in the medical literature on the benefits of both hospital deliveries and access to neonatal interventions, including after taking into account that only a fraction of the children born during strike months are directly affected by strikes, as we explain in the bench-marking exercise below.

To begin, we assume that the mortality effect is driven mainly by those children who (a) develop neonatal complications that would require the services of a striking hospital, (b) would actually seek hospital care in the absence of a strike (whether they are born in strike hospitals or are referred to them), and (c) are born during or within a few days of a strike. It has been estimated that approximately 18 percent of children in low-income countries will develop complications at birth or shortly after birth that require inpatient neonatal care (Murphy et al., 2017).[24] Of these, it has been estimated in Kenya that 60 percent would access hospital care (in the absence of a strike) (Murphy et al., 2018).[25] Finally, given the average length of strikes, we assume that one-third of

[23]Results available upon request.

[24]Studies from individual hospitals in Kenya estimate this rate to be between 13 and 30 percent (Aluvaala et al., 2015a; Kasirye-Bainda and Musoke, 1992).

[25]This estimate is also consistent with our data, which shows that mothers anticipating high-risk pregnancies disproportionately select hospitals as a place to give birth. For example, births of multiples occur in a facility at a

the at-risk children who would normally receive inpatient neonatal care are directly affected by the strike during a strike month.[26] Applying these scaling factors (fraction needing care, fraction accessing hospitals, and fraction of children born in a strike month affected by a strike) – and assuming that strikes had no spillover effects on anyone else – would imply that neonatal mortality increased among this subgroup by $.019/(.18 \times .6 \times .33) = .527$ percentage points.[27]

Next, to get a sense of the magnitude of this effect, we compare the implied increase in neonatal deaths to the baseline neonatal mortality rate among children admitted to the types of hospitals where strikes occur (i.e. large public hospitals). Because they often take the worst cases, neonatal mortality rates among children who are admitted to these hospitals are substantially higher than in the population. Across 11 studies from Kenyan district, national, and referral hospitals during the study period, inpatient neonatal mortality rates ranged between 143 to 377 per 1000 admissions.[28] Assuming that baseline neonatal mortality among children who require hospital care, and who would seek such care in the absence of a strike, is the median of these studies (250 deaths per 1000 admissions), then the scaled up effect of strikes on neonatal mortality (527 additional deaths per 1000) represents a 211 percent increase in neonatal mortality for this subgroup of at-risk children.[29]

Effects of this size are in line with evidence on the efficacy of gaining access to the suite of readily-available interventions provided by hospitals. According to several large systematic reviews of the medical literature, early-life interventions such as clean delivery, antibiotics, and newborn resuscitation and temperature management, among others, are estimated to reduce neonatal mortality by 55-82 percent (Jones et al., 2003; Darmstadt et al., 2005; Lawn et al., 2009; Bhutta et

rate of 61 percent while singleton births happen in a facility at a rate of 49 percent in our data. Additionally, our data shows that women who were told about pregnancy complications at their last antenatal visit delivered at facilities 67 percent of the time, while women who were not told about complications delivered at facilities only 47 percent of the time. While our data does not provide the specific type of hospital, we see that, conditional on delivering in a facility, 80 percent of multiple births occur in hospitals, and 78% of women told of complications delivered in hospitals.

[26] The average length of strikes (7 days) is a lower bound since we are missing nearly 40 percent of end dates. Further, most neonatal complications arise in the first few days of life, and therefore children who are born a few days prior to the start of a strike, and who develop complications that necessitate inpatient neonatal care, can also be affected. Our results are similar if we assumed only one-fourth of at-risk children are affected by strikes within a month.

[27] Note, this calculation assumes that high-risk births and accessing hospitals around the time of birth are independent events, which we know is not true. Therefore, this implied effect likely overstates the true effect on at-risk children. At the extreme, if all at-risk births visit a hospital then the implied effect size would be $.019/(.18\times.33) = .32$.

[28] The 11 studies include Kasirye-Bainda and Musoke (1992); English et al. (2003); Simiyu (2003); English et al. (2004); Gathara et al. (2011); Aluvaala et al. (2015a,b); Tele et al. (2016); Yego et al. (2013); Murphy et al. (2018); Tank et al. (2019).

[29] Again, to the extent that high-risk births and hospital visits are positively correlated, this is likely an overestimate. In the case that all at-risk births enter hospital care, the implied increase in at-risk neonatal mortality would be 128 percent, which could be considered an extreme lower bound.

al., 2014). These estimates imply that when this care is not provided, neonatal mortality can be 122-455 percent higher.

Other studies in economics also find large treatment effects of facility care on birth outcomes, although direct comparisons are complicated as each study estimates a different local average treatment effect among often distinct populations. For example, Almond et al. (2010) find that the suite of interventions offered in the Neonatal Intensive Care Unit (NICU) in U.S. facilities reduces neonatal mortality by one percentage point (off of a base of 3.8 percentage points) for the marginal NICU entrant, using a regression discontinuity based on a policy with a fixed weight cut-off for eligibility. This effect is estimated from the kids who need the NICU the least (i.e. who only marginally qualify for extra care), and for whom the counterfactual level and quality of health care is already relatively high. Among low-risk pregnancies in the Netherlands, Daysal et al. (2015) show hospital births decrease neonatal mortality relative to home deliveries by 8 to 9 deaths per 1000 live births, compared to a sample mean of 2 deaths per 1000 births. In Malawi, Godlonton and Okeke (2016) find that, among women whose closest facility is "high" quality, a temporary ban on traditional birth attendants reduced 7-day mortality by 1.3-1.4 percentage points and 1-month mortality by 1.6-1.8 percentage points. For this group, the policy increased the use of formal facilities by approximately 15 percentage points.[30] Okeke and Chari (2018) show that births at night in areas without 24-hour care are 12.6 percentage points less likely to deliver in a facility, which is associated with a 1.3 percentage point increase in newborn mortality, relative to a base of .9 percentage point mortality. Similar to our study, in each of these studies, a large fraction of births are unaffected by the source of variation in the quality or type of care.

4.1.4 Point estimate may nevertheless overstate the true effect on neonatal mortality

Although we obtain consistent point estimates for the effect of strikes on neonatal mortality in both the DHS and NUHDSS data, and although these point estimates are consistent with consensus evidence on the efficacy of basic care derived from the medical literature, they are nevertheless measured imprecisely and we cannot rule out a wide range of effect sizes. Further, given the

[30] Godlonton and Okeke (2016) define "high" quality facilities as those meeting at least four of the following criteria: has operating theater, has intensive care unit, has pharmacy, has trained staff available 24 hours per day, offers blood transfusions, offers ambulance services, offers laboratory services, is open 7 days a week. Although we cannot be certain, hospitals in Malawi are the most likely facilities to meet this threshold, and thus this is the relevant comparison. Across all facility types the authors find the ban had no overall effect on newborn mortality.

limited power of our study, it is not unreasonable to believe that the true effect may be on the lower end of confidence intervals.

Camerer et al. (2016) and Camerer et al. (2018) show that studies that are less than fully powered – such as ours – tend to overstate the magnitude of treatment effects when they are there. Using higher powered samples, they replicate more than three dozen social science studies published in Nature, Science, the American Economic Review, and the Quarterly Journal of Economics, and find that while the majority (67 percent) of results are substantiated, effect sizes are about 70 percent as large as those in the original studies. Similar issues have been raised concerning social science and public health research (Gelman and Carlin, 2014).

Therefore we stress that caution should be exercised when interpreting the magnitude of our neonatal mortality results (as well as those from other studies with large confidence intervals). Although we cannot be certain, it is likely that the true effect of strikes on mortality is smaller than our point estimate. At the same time, given that we find similar evidence of adverse effects in both the DHS and NUHDSS data, we think it unlikely our results are a false positive.

4.2 Post-neonatal infant mortality

We test for, and find no evidence of, effects of strikes on post-neonatal infant mortality (deaths in the first year of life conditional on having survived the first month). We present results on this outcome in the second column of Table 3. Using the DHS data (panel A), the point estimate is positive and large, but both the wild bootstrap and permutation-based p-values are also large and we cannot reject a null effect.[31] In the NUHDSS data (panel B), the point estimate is small and also not statistically different from 0 both based on the cluster-robust standard errors and the permutation test p-value.

The evidence of no effects on later mortality suggests that the increase in neonatal mortality we document does not represent simply a shifting of child deaths that would have happened later in the absence of a strike. If there were such shifting, we would expect the effect on post-neonatal infant mortality to be negative. Instead, the null result we obtain is consistent with the hypothesis that health-worker strikes are resulting in new deaths that would not otherwise have occurred.

[31] We obtain qualitatively identical results across the 10 alternate specifications (Figure 2).

4.3 Delivery location

A key assumption in this paper is that strikes affect mortality by disrupting care around the time of birth. We are limited in our ability to test this assumption directly with our data, but we can look at location of birth broadly and we can offer some external evidence that such disruptions exist.

We find little to no evidence that strikes affect the type of location where women choose to deliver as reported in the DHS data. Overall, we see a small and not statistically significant reduction in the likelihood of delivering in any formal health facility rather than at home (column 3 of Table 3).[32] Results are similarly small and statistically insignificant across specifications (Figure 2 and Table A2), as well as across type of facility (e.g. public hospital vs private clinic, Table A4).

Given the nature of strikes in our data, these results are not especially surprising, and they do not on their own rule out that strikes disrupt care substantially at facilities where they occur. First, for the majority of strikes (74 percent), hospitals remained open but with limited trained staff as either doctors or nurses were absent. Therefore women were not necessarily forced to alter location of birth, but their care would have been disrupted by the lack of trained staff. Second, our data does not allow us to distinguish between government hospitals in general and the particular hospitals where strikes take place. Since most striking hospitals are located in more urban areas, and since local strikes are typically confined to individual hospitals, it is possible that some women affected by strikes were still able to find other health facilities to deliver their children. They too could face worse care if quality is diminished as a consequence of excess demand during strikes at neighboring hospitals and/or to the extent that alternate facilities are generally of lower quality (which may often be the case given that, in our data, strikes mainly occur in top-level district or national hospitals).

Anecdotal and empirical evidence from outside our study bear this out. For example, the *Standard* newspaper of Kenya reported that a nurses' strike at Pumwani Maternity Hospital "seriously disrupted normal service delivery that averages about 80 deliveries daily" *Standard*, Aug 10, 2004.[33] In another instance of a nurses' strike at Pumwani, a woman is quoted as saying "I gave birth at 8:25am with the help of trainee nurses; things were really bad for many of us on that day. Whenever

[32]Location of birth is not available in the NUHDSS data.

[33]`https://allafrica.com/stories/200408091258.html`

I called out to a nurse, they would refuse to attend to me. It is only later that I learnt that they were on strike" *IRIN News*, Mar 18, 2011.[34] Finally, during a nurses' strike at Kenyatta National Hospital patients reportedly "had a difficult time getting treatment... and an undercover visit to the wards revealed that they were unattended" *Capital News*, June 20, 2013.[35]

In terms of spillovers to other hospitals, Adam et al. (2018) show that overcrowding led to increased infant mortality in one not-for-profit hospital that remained open during the 2016-2017 doctor strike in Kenya. There is also anecdotal evidence from the Kenyan popular press of overcrowding at non-striking hospitals during strikes. For instance, a health worker strike at Pumwani in which the hospital closed resulted in "...expectant women going to Kenyatta National Hospital, Mbagathi Hospital, and Mama Lucy Hospital in Umoja" *Nation*, Sept 11, 2013.[36] During that same strike, "three of the infants that had been taken to KNH [Kenyatta National Hospital] died in what was argued as the facility's inability to cope with the influx" *The Star*, Sept 13, 2013.[37]

4.4 Other downstream outcomes: long-run health indicators and investments

We next look for effects of strikes on later markers of health and health investments among survivors, as measured by height-for-age and weight-for-age z-scores and use of vaccines. Theoretically, strikes could lead to downstream health consequences through a number of channels, including differences in health endowments and in health investments that come *after* the strike.

4.4.1 Height and weight

Given that the most common causes of early childhood mortality and morbidity – prematurity or low birth weight, and severe infections including sepsis, pneumonia, tetanus, and diarrhea – are also important determinants of stunting and wasting (Black et al., 2008, 2013; Prendergast and Humphrey, 2014), at-risk children who receive substandard care during strikes, and who nevertheless survive, may continue to be more vulnerable to these conditions relative to children born when no strikes occur. If so, these effects could be reflected in differences in height-for-age, which measures the long term effects of malnutrition and chronic illness since birth, or weight-for-age,

[34] https://www.thenewhumanitarian.org/report/92229/kenya-nurses-go-slow-highlights-ills-maternity-hospitals
[35] https://www.capitalfm.co.ke/news/2013/06/life-returns-to-knh-as-strike-ends/
[36] https://nation.africa/kenya/counties/nairobi/Babies+die+as+Pumwani+Maternity+Hospital+strike+bites/1954174-1987978-sfyv6x/index.html
[37] https://allafrica.com/stories/201309131284.html

which can also capture the effects of poor health since birth, though it is more sensitive to recent bouts of illness or poor nutrition.

On the other hand, if the marginal deaths caused by strikes are among the weakest born, then we might expect any measured effects on long-term health to be reduced, because of a change in who survives (Almond and Currie, 2011). If this "culling" effect dominates the "scarring" effect of worse quality care, then this would bias any negative effects of strikes on later-life health in the opposite direction, upward toward zero.

In fact, we find no evidence of effects of strikes on the health of survivors, as measured by height- and weight-for-age. Columns 4 and 5 of Table 3 presents the estimates of the effects of strikes on height-for-age and weight-for-age z-scores. For the DHS sample, we see a small and statistically insignificant effects of strikes both height and weight (see also Figure 2 and Table A3). Coefficients are somewhat larger the NUHDSS sample (panel B), but are also statistically indistinguishable from zero.[38]

4.4.2 Vaccines

Previous research finds that short-term delays, barriers, and reductions in the quality of care can lead to long-term reductions in take-up of formal health services. This could come from a lack of encouragement to return for future care, a reduction in trust of formal health providers, or reinforcing behavior of parents causing lower investments in children with weaker initial endowments.

Previous research has shown reductions in early health investments specifically in response to changes in conditions at birth. For example, Sievertsen and Wüst (2017) show idiosyncratically shorter hospital stays at birth reduce vaccine take-up in Denmark. Okeke and Chari (2018) show a reduction in postnatal checkups for children born outside of facilities in Nigeria, and Friedman and Keats (2020) show increased take-up of vaccines when facility births increase in Ghana. Previous research has also shown strong evidence of parental reinforcing behaviors of allocating more inputs to those with stronger initial endowments (Almond and Mazumder, 2013). If a strike changes initial health of children, or even changes parents' beliefs about their child's health, this may push parents

[38]In Kenya, height- and weight-for-age indicators fall precipitously in the first year of life leaving children aged 1-5 approximately 1 standard deviation behind the norm. This pattern of "growth faltering" is common in low-income countries as documented by Shrimpton et al. (2001), Victora et al. (2010), and Aiyar and Cummins (2021). When we limit our sample to only those at least 1 year old, we see similar null results.

to invest relatively less in health inputs for that child.

More broadly, a reduction in take-up of future health investments is consistent with findings showing that a short-term barrier to getting immediate and quality care can lead to much more than a short delay. For example, Banerjee et al. (2010) find that small incentives at one-time immunization days lead to long-term changes in vaccine take-up, even when the same vaccines are available and free in local health facilities. In another study showing a reduction in take-up of care from a temporary reduction in care quality, Goldstein et al. (2013) find that when a nurse is absent on the *first* day that a woman visits a prenatal clinic, she is significantly less likely to deliver in a hospital, and less likely to ever get tested for HIV during pregnancy, receive treatment to reduce the likelihood of transmission of HIV to the baby, or breastfeed.[39]

In our data, we find some suggestive evidence that children born during strikes, and who survive, receive fewer vaccinations early in life. We check for effects of being born during a strike on the likelihood of being fully immunized against tuberculosis (BCG), diphtheria, pertussis, and tetanus (DPT), polio, and the measles. In order to address concerns about multiple hypothesis testing, we create a vaccine index, which is a mean effect of the four vaccines recorded, following the method of Kling et al. (2007). Results are presented in column 6 of Table 3. In both the DHS (panel A) and the NUHDSS data (panel B), we find a negative and statistically significant effect of strikes on this index (in panel A wild bootstrap and permutation test p-values of .076 and .031, respectively, and in panel B permutation test p-value of .025). Looking at the specific vaccines that make up the index (Table A5 and Table A6), this effect appears to be driven mainly by lower take-up of vaccines given (or completed) several months after birth. We see very little effect on the BCG vaccine, which is given at birth, but larger (and sometimes statistically significant) effects on completed DPT, polio, and measles vaccines, which are scheduled for children 2-6 months of age.

We stress that these results are only suggestive since, at least in the DHS data, they are not particularly robust across specifications (Figure 2, Table A3, and Table A5). However, given that data on vaccines is only available for children who survive until the survey date, and to the extent that early mortality and vaccinations are negatively correlated, our results may nevertheless

[39] Also consistent with a broader idea of worse care leading to reductions in future use of formal healthcare, Alsan et al. (2019) find greater take-up of preventive health services when patients are randomly assigned to a same-race physician. Four studies find that specific incidents of mistreatment by medical professionals led to very long-term reductions in trust in modern medicine (Lowes and Montero, 2021) and take-up of preventive healthcare (Alsan and Wanamaker, 2017; Martinez-Bravo and Stegmann, 2022; Archibong and Annan, 2021).

underestimate the true effect of strikes on vaccines.

4.5 Nairobi vs other counties

We do not find evidence that our results are driven solely by responses to strikes occurring in Nairobi county. About 61 percent of strikes take place in Nairobi hospitals, and our auxiliary data set (the NUHDSS) contains observations only from Nairobi. Depending on the extent to which hospitals in Nairobi – or the types of people who use hospitals in Nairobi – are different from those in the rest of the country, if the results were driven mainly by effects on births occurring in the capital, this could limit their external validity and narrow their policy relevance. We test for this directly by adding to our main specification an interaction term between the indicator for strikes and an indicator for births to mothers who live in Nairobi county (Table A7).[40] For both neonatal mortality and the vaccine index (outcomes for which we find evidence of effects of strikes), the coefficient on the indicator for a strike is almost identical to the coefficient in Table 3, and the coefficient on the interaction term, which measures any differential effect for Nairobi, is close to zero. For the other outcomes, such as facility birth and height- and weight-for-age z-scores, the coefficients are of similar magnitudes but opposite signs, suggesting an absence of any effect for children born to mothers from Nairobi, which also matches our main results.

5 Conclusion

This paper finds that children born during strikes are more likely to suffer a neonatal death. With a set of ten alternative specifications and alternative sample selection criteria, we show that this result is likely not an artifact of specification of choice or sampling error. Although the magnitude of the point estimate we obtain is large, it is also plausibly consistent with existing scientific evidence on the benefits of institutional births. At the same time, we caution that the precise point estimate – while statistically significantly different from zero at the ten-percent level – may still overstate of the true magnitude of the effect of strikes on neonatal mortality. We do not see a statistically significant change in post-neonatal infant mortality (deaths in months 2-12 of life). Notably, we

[40] We do not include wild bootstrap or permutation-based p-values in this table because the cluster robust standard errors (which tend to over-reject) fail to reject for each outcome at standard confidence levels. While we are under-powered to detect effects separately between Nairobi and other counties, the sign and magnitude of the coefficients is still informative.

also do not see a decline for deaths in this period, which suggests that the increase in neonatal deaths do not just reflect a change in the timing of mortality but a change in long-term survival.

We also present some suggestive evidence that children born during strikes, and who survive, receive fewer vaccinations – and potentially other important early child health interventions – in the months after strikes have ended. While the reductions in future health investments are consistent with prior evidence of the long-run consequences of disruptions to care, we stress that this set of results is more speculative. We do not find evidence of long-run effects on child health as measured by height- and weight-for-age z-scores.

Overall, this paper has shown that strikes have immediate impacts on child survival. These results suggest that large public hospitals – where the majority of the strikes documented in this paper occur – likely do in fact provide positive health benefits to newborn children under normal operations. If they did not, we would not be able to see an impact on health when these services are disrupted. This evidence builds on a handful of prior studies that demonstrate a causal link between the quality of healthcare provided in low-income countries and health outcomes (Björkman and Svensson, 2009; Gertler and Vermeersch, 2013; Peabody et al., 2013; Okeke, 2019).

Our approach also opens the door for future research to fill in the gaps in a general understanding of the full consequences of health-worker strikes. In particular, we are unable to measure the effects on other health outcomes or to assess any long-run costs or benefits of the strikes. For example, strikes may also lead to interruptions in HIV treatment that facilitates the development of antiretroviral drug resistance, which can hurt both the direct recipients of the drugs and anybody who is infected with the mutated strain. Similarly, there may be adverse effects for those having heart-attacks or involved in traffic accidents or those who postpone preventive health interventions. These are more difficult to measure because the timing of demonstrated need could be changed by the start of a strike. Finally, our identification strategy does not allow us to clearly estimate the long-run effects of strikes. If strikes lead to the demands of health workers being met, and this in turn increases their motivation and effort, the long-term benefits may well be positive.

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6 Figures and Tables

Table 1 – Kenya Strikes List

Start Date	Min. Days	County	Hospitals Affected	Strike Actor
Local strikes				
April 22, 1999	3	Nyamira	Nyamira District Hospital	health workers
February 18, 2002	2	Nairobi	Pumwani Maternity Hospital, Langata Maternity Ward	nurses
April 15, 2002	7	Uasin Gishu	Uasin Gishu Memorial Hospital	nurses
November 1, 2002	3	Nairobi	Kenyatta National Hospital	doctors
May 31, 2004	2	Nyeri	PCEA Tumutumu Hospital	nursing students
August 9, 2004	2	Nairobi	Pumwani Maternity Hospital	nurses
November 25, 2004	2	Nairobi	Kenyatta National Hospital	health workers
November 30, 2004	2	Nairobi	Kenyatta National Hospital	health workers
May 24, 2005	4	Nairobi	Kenyatta National Hospital	nurses
July 3, 2008	2	Nakuru	Rift Valley Provincial Hospital	health workers
January 15, 2010	2	Tana River	Ngao District Hospital	health workers
March 16, 2011	3	Nairobi	Pumwani Maternity Hospital	nurses
April 27, 2011	1	Nairobi	Pumwani Maternity Hospital	nurses
October 20, 2011	17	Uasin Gishu	Moi Teaching and Referral Hospital	doctors
November 9, 2011	3	Nairobi	Kenyatta National Hospital	health workers
June 9, 2012	37	Nairobi	Gertrude Hospital	doctors
August 13, 2012	6	Uasin Gishu	Moi Teaching and Referral Hospital	doctors
August 27, 2012	39	Nairobi	Kenyatta National Hospital	doctors
September 13, 2012	14	Uasin Gishu	Moi Teaching and Referral Hospital	nurses
January 9, 2013	2	Nairobi	Kenyatta National Hospital	health workers
June 19, 2013	2	Nairobi	Kenyatta National Hospital	nurses
September 9, 2013	5	Nairobi	Pumwani Maternity Hospital	nurses
March 1, 2014	11	Embu	Embu County Hospital	health workers
National strikes				
December 5, 2011	10			health workers
March 1, 2012	4			nurses
March 1, 2012	15			health workers
September 13, 2012	22			doctors
December 3, 2012	71			nurses
December 10, 2013	11			health workers

Note: Strike data compiled by authors as described in Section 2.1.

Table 2 – Summary Statistics

Panel A: Children	All (1)	Home Births (2)	Facility Births (3)	Non-Strike Counties (4)	Strike Counties (5)	*p*-value (2)=(3) (6)	*p*-value (4)=(5) (7)
Neonatal mortality	0.028	0.027	0.027	0.027	0.033	0.82	0.03
Post-neonatal infant mortality	0.022	0.023	0.020	0.023	0.018	0.05	0.09
Height for age: z-score	-1.09	-1.29	-0.89	-1.10	-1.02	0.00	0.00
	(1.42)	(1.45)	(1.36)	(1.43)	(1.38)		
Weight for age: z-score	-0.95	-1.20	-0.71	-0.98	-0.78	0.00	0.00
	(1.22)	(1.18)	(1.21)	(1.21)	(1.23)		
Facility birth	0.50			0.47	0.68		0.00
Hospital birth (if facility birth)	0.75			0.74	0.82		0.00
Doctor, nurse, or midwife present	0.51	0.03	0.99	0.48	0.68	0.00	0.00
BCG vaccine	0.92	0.88	0.97	0.91	0.96	0.00	0.00
DPT vaccine (3 doses)	0.79	0.72	0.85	0.78	0.84	0.00	0.00
Polio vaccine (3 doses)	0.71	0.68	0.74	0.70	0.74	0.00	0.00
Measles vaccine	0.73	0.68	0.79	0.72	0.78	0.00	0.00
Observations	31390	15763	15627	26659	4731		

Panel B: Mothers	All (1)	Facility Non-User (2)	Facility User (3)	Non-Strike Counties (4)	Strike Counties (5)	*p*-value (2)=(3) (6)	*p*-value (4)=(5) (7)
Age	28.73	29.49	28.15	28.77	28.51	0.00	0.03
	(6.84)	(7.21)	(7.21)	(6.91)	(6.46)		
Number of Children	3.52	4.30	2.92	3.64	2.87	0.00	0.00
	(2.36)	(2.53)	(2.53)	(2.40)	(1.99)		
Education	6.85	4.67	8.51	6.49	8.75	0.00	0.00
	(4.31)	(3.87)	(3.87)	(4.25)	(4.14)		
Literate	0.65	0.46	0.79	0.62	0.80	0.00	0.00
Married	0.77	0.78	0.76	0.77	0.77	0.00	0.60
Urban	0.32	0.16	0.44	0.28	0.54	0.00	0.00
Has electricity	0.20	0.05	0.32	0.16	0.42	0.00	0.00
Observations	22483	9681	12802	18842	3641		

Note: Standard deviations in parentheses. Panel A: Computed based on one observation per child. Sample restricted to children born in the 5 years preceding each survey. Panel B: Computed based on one observation per mother. Sample restricted to mothers of children born in the 5 years preceding each survey. Data from the 2003, 2008/09, and 2014 DHS.

Table 3 – Main Results

	Neonatal mortality (1)	Post-neonatal mortality (2)	Facility birth (3)	Height for age z-score (4)	Weight for age z-score (5)	Vaccine index (6)
Panel A. DHS						
Strike	0.019	0.018	-0.028	-0.044	0.009	-0.093
	(0.006)	(0.010)	(0.015)	(0.085)	(0.052)	(0.029)
	[0.094]	[0.253]	[0.232]	[0.685]	[0.905]	[0.076]
	{0.082}	{0.253}	{0.138}	{0.661}	{0.887}	{0.031}
Mean of dep. var.	0.028	0.022	0.498	-1.087	-0.954	0.000
Std. dev. of dep. var.	0.164	0.146	0.500	1.419	1.217	1.000
Observations	31672	25494	31388	27429	27429	30088
Panel B. NUHDSS						
Strike	0.014	0.004		-0.187	-0.157	-0.176
	(0.007)	(0.010)		(0.135)	(0.110)	(0.076)
	{0.091}	{0.727}		{0.178}	{0.163}	{0.025}
Mean of dep. var.	0.019	0.040		-1.582	-0.640	-0.002
Std. dev. of dep. var.	0.135	0.197		1.636	1.351	1.001
Observations	23181	17799		4923	5002	4730

Note: *In Panel (A) each regression includes includes year-month and county fixed effects, and controls for mothers' age, age-squared, and education, families' wealth-quintile fixed effects and urban-rural status, child's gender, birth order, and an indicator for being a multiple birth. The variable, Strike, is an indicator for whether there was a strike in the county and month of birth. Data from the 2003, 2008/09, and 2014 DHS, restricted to children born in the 5 years preceding each survey. Standard errors, clustered at the county level, are in parentheses. Wild bootstrap p-values are in brackets. Permutation test p-values are in braces. In Panel (B) each regression includes year, month, and day-of-the-week fixed effects. The variable, Strike, is an indicator for whether there was a strike in a Nairobi hospital on the day of birth. Mortality data are from the NUHDSS sample, and include births from 2003-2014; height, weight, and vaccine data from supplementary data collection and include children born after 2009. Standard errors, clustered at the birthday level, are in parentheses. Permutation test p-values are in braces.*

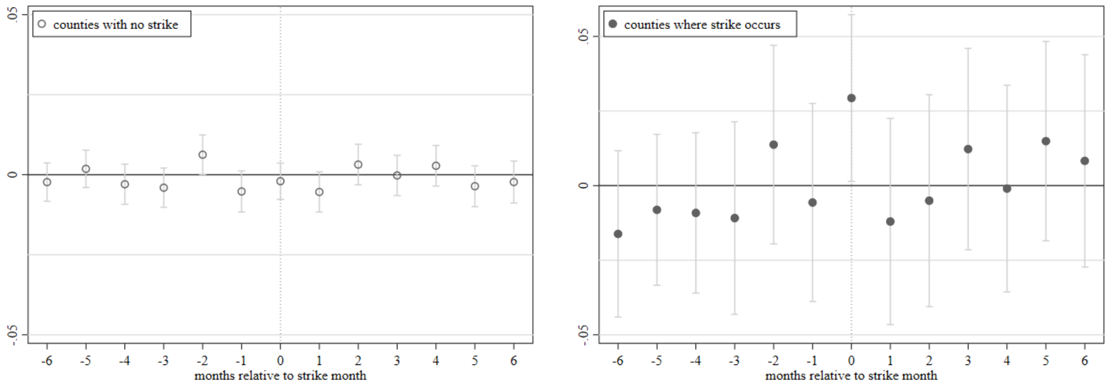

Figure 1 – In Panel (a), each dot represents the difference in neonatal mortality between observations a given number of months from a strike in all counties and the average neonatal mortality in all county-months not within six months of a strike. This is presented to check whether strikes are happening at times with different national patterns of mortality. If they were, these dots will be consistently far from zero. In Panel (b) each dot represents the difference in neonatal mortality between observations a given number of months from a strike in the county where the strike occurred and the neonatal mortality in all other counties that month. This is presented to see whether there are mortality patterns in the months around a strike in the county where the strike occurs. If this were the case, the dots on either side of the y-axis would be consistently different from zero. The dot at 0 months from a strike represents the difference in neonatal mortality in the month of a strike in the county where it occurs and neonatal mortality in all other counties that month. Neonatal mortality reflects the fraction of those births in which the mother reported that the baby died within the first month. Data from the 2003, 2008/09, and 2014 DHS restricted to children born in the 5 years preceding each survey. 95% confidence intervals reported.

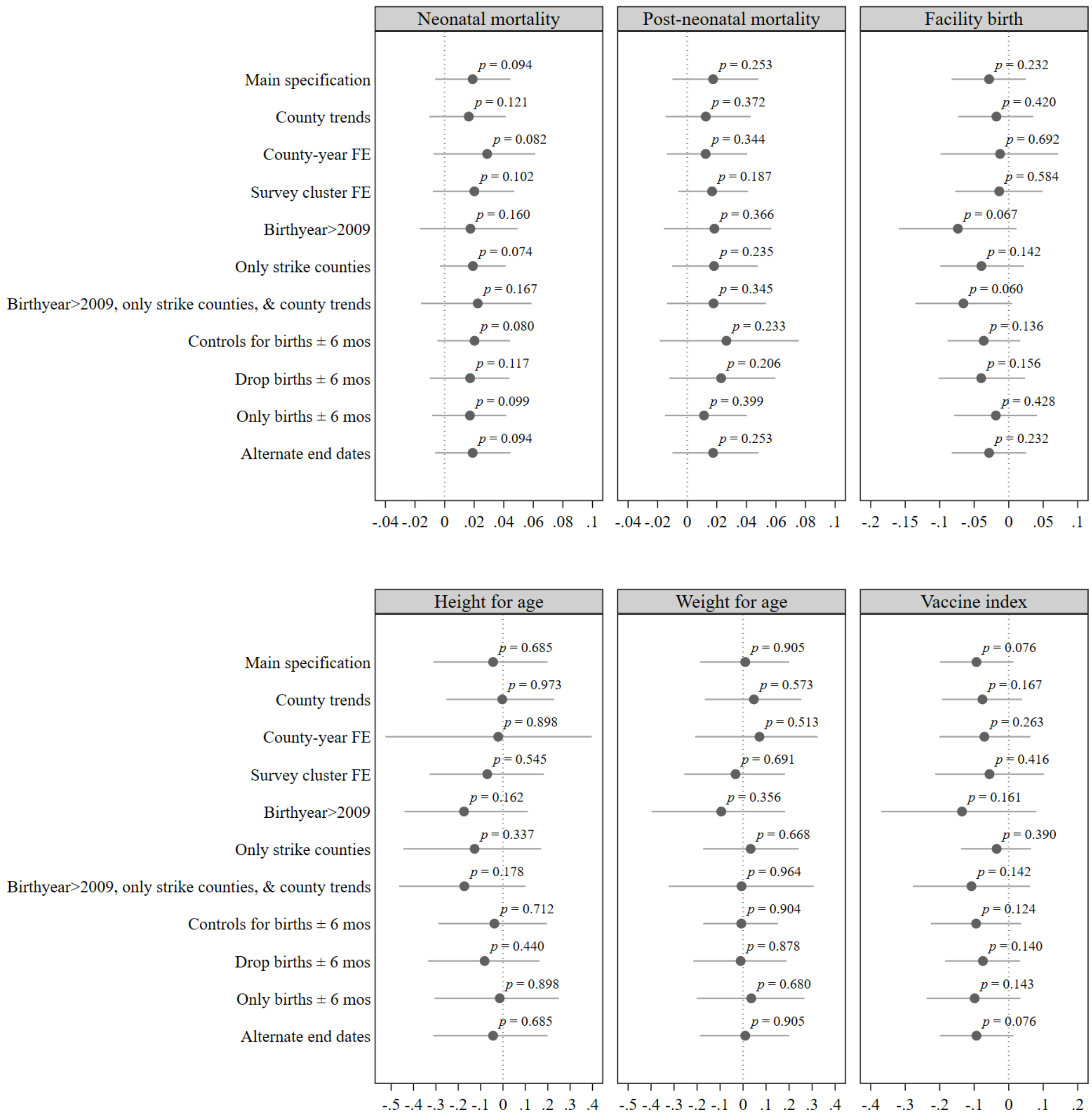

Figure 2 – Effect of strikes on main outcomes across different specifications and data restrictions. See section 4.1.1 for more details on each specification. Data from the 2003, 2008/09, and 2014 DHS restricted to children born in the 5 years preceding each survey. Wild bootstrap 95% confidence intervals and p-values reported.

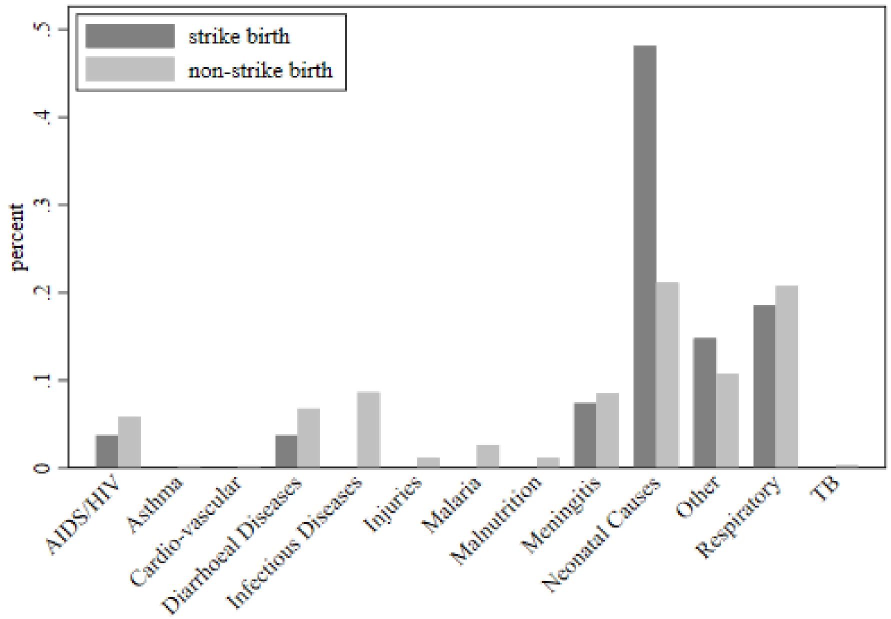

Figure 3 – Reported causes of death from verbal autopsy records. Data are from the NUHDSS sample, and include all births from 2003-2014 that resulted in a death.

7 Appendix

Table A1 – Selection and birth timing

	Mother characteristics						Birth characteristics			
	Age at time of survey (1)	Years of schooling (2)	Can read a sentence (3)	Married (4)	Urban (5)	Has electricity (6)	Age at birth (7)	Female birth (8)	Small birth (9)	No. births in county (10)
Strike	-0.579	0.242	0.033	0.001	0.003	0.028	-0.513	0.009	0.024	-0.639
	(0.279)	(0.184)	(0.023)	(0.029)	(0.037)	(0.017)	(0.286)	(0.016)	(0.016)	(1.408)
Mean of dep. var.	28.600	6.460	0.620	0.790	0.300	0.180	26.520	0.490	0.170	8.360
Std. Dev. of dep. var.	6.600	4.320	0.490	0.410	0.460	0.380	6.480	0.500	0.370	4.230
Observations	31810	31808	31778	31810	31810	31810	31810	31810	21209	3746

Note: *Each regression includes year-month and county fixed effects. The variable, Strike, represents an indicator for whether there was a strike in the county and month of birth. Sample restricted to mothers of children born in the 5 years preceding each survey. Column 10 calculated using only one observation per county-year-month. Data from the 2003, 2008/09, and 2014 DHS. Standard errors, clustered at the county level are in parentheses.*

Table A2 – Main Results Across All Specifications (1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Neonatal mortality											
Strike	0.019	0.016	0.029	0.020	0.017	0.019	0.022	0.020	0.017	0.017	0.019
	(0.006)	(0.006)	(0.007)	(0.006)	(0.008)	(0.006)	(0.011)	(0.006)	(0.007)	(0.006)	(0.006)
	[0.094]	[0.121]	[0.082]	[0.102]	[0.160]	[0.074]	[0.167]	[0.080]	[0.117]	[0.099]	[0.094]
Mean of dep. var.	0.028	0.028	0.028	0.028	0.024	0.033	0.031	0.028	0.028	0.025	0.028
Std. dev. of dep. var.	0.164	0.164	0.164	0.164	0.152	0.177	0.173	0.164	0.164	0.157	0.164
Observations	31672	31672	31672	31672	18520	4765	2637	31672	30641	25013	31672
Post-neonatal infant mortality											
Strike	0.018	0.013	0.012	0.017	0.018	0.018	0.018	0.026	0.023	0.011	0.018
	(0.010)	(0.010)	(0.009)	(0.009)	(0.014)	(0.010)	(0.013)	(0.015)	(0.012)	(0.009)	(0.010)
	[0.253]	[0.372]	[0.344]	[0.187]	[0.366]	[0.235]	[0.345]	[0.233]	[0.206]	[0.399]	[0.253]
Mean of dep. var.	0.022	0.022	0.022	0.022	0.015	0.018	0.015	0.022	0.022	0.018	0.022
Std. dev. of dep. var.	0.146	0.146	0.146	0.146	0.122	0.134	0.121	0.146	0.146	0.133	0.146
Observations	25494	25494	25494	25494	15115	3836	2155	25494	24794	19236	25494
Any facility birth											
Strike	-0.028	-0.018	-0.013	-0.014	-0.074	-0.040	-0.066	-0.036	-0.040	-0.019	-0.028
	(0.015)	(0.015)	(0.023)	(0.019)	(0.020)	(0.020)	(0.020)	(0.014)	(0.017)	(0.016)	(0.015)
	[0.232]	[0.420]	[0.692]	[0.584]	[0.067]	[0.142]	[0.060]	[0.136]	[0.156]	[0.428]	[0.232]
Mean of dep. var.	0.498	0.498	0.498	0.498	0.546	0.681	0.671	0.498	0.490	0.516	0.498
Std. dev. of dep. var.	0.500	0.500	0.500	0.500	0.498	0.466	0.470	0.500	0.500	0.500	0.500
Observations	31388	31388	31388	31388	18256	4731	2607	31388	30368	24760	31388

Note: Each column represents a different specification, as presented in Figure 2. Column (1) presents the main specification, which includes year-month and county fixed effects, and controls for mothers' age, age-squared, and education, families' wealth-quintile fixed effects and urban-rural status, child's gender, birth order, and an indicator for being a multiple birth. The variable, Strike, is an indicator for whether there was a strike in the county and month of birth. Standard errors, clustered at the county level, are in parentheses and Wild bootstrap p-values are in brackets. All other columns are variations on this main specification. Column (2) also includes county trends, while column (3) includes county-year FEs and column (4) includes survey cluster FEs. Column (5) limits the sample to only births after 2009, while column (6) limits the sample to only observations in counties which ever had a strike, and column (7) includes only births after 2009 in counties which ever had a strike, including county trends as a control. Then Column (8) controls for indicators for having been born in each of the six months before and six months after a strike in a strike county. Column (9) drops all observations within six months of a strike, and column (10) includes only observations within six months of a strike. Finally, Column (11) uses a set of alternate end-dates for strikes, assuming strikes lasted longer. See section 4.1.1 for more details on each specification.

Table A3 – Main Results Across All Specifications (2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Height for age											
Strike	-0.044	-0.003	-0.021	-0.070	-0.174	-0.127	-0.173	-0.039	-0.082	-0.015	-0.044
	(0.085)	(0.081)	(0.140)	(0.094)	(0.079)	(0.106)	(0.099)	(0.082)	(0.079)	(0.091)	(0.085)
	[0.685]	[0.973]	[0.898]	[0.545]	[0.162]	[0.337]	[0.178]	[0.712]	[0.440]	[0.898]	[0.685]
Mean of dep. var.	-1.087	-1.087	-1.087	-1.087	-1.035	-1.022	-1.027	-1.087	-1.097	-1.022	-1.087
Std. dev. of dep. var.	1.419	1.419	1.419	1.419	1.346	1.384	1.309	1.419	1.420	1.390	1.419
Observations	27429	27429	27429	27429	16559	4046	2331	27429	26533	22030	27429
Weight for age											
Strike	0.009	0.047	0.071	-0.033	-0.095	0.033	-0.006	-0.008	-0.011	0.036	0.009
	(0.052)	(0.056)	(0.071)	(0.069)	(0.068)	(0.064)	(0.088)	(0.043)	(0.053)	(0.056)	(0.052)
	[0.905]	[0.573]	[0.513]	[0.691]	[0.356]	[0.668]	[0.964]	[0.904]	[0.878]	[0.680]	[0.905]
Mean of dep. var.	-0.954	-0.954	-0.954	-0.954	-0.955	-0.783	-0.836	-0.954	-0.969	-0.913	-0.954
Std. dev. of dep. var.	1.217	1.217	1.217	1.217	1.184	1.230	1.199	1.217	1.210	1.216	1.217
Observations	27429	27429	27429	27429	16559	4046	2331	27429	26533	22030	27429
Vaccine index											
Strike	-0.093	-0.076	-0.071	-0.056	-0.135	-0.035	-0.108	-0.094	-0.075	-0.099	-0.093
	(0.029)	(0.033)	(0.043)	(0.049)	(0.058)	(0.033)	(0.048)	(0.036)	(0.031)	(0.038)	(0.029)
	[0.076]	[0.167]	[0.263]	[0.416]	[0.161]	[0.390]	[0.142]	[0.124]	[0.140]	[0.143]	[0.076]
Mean of dep. var.	0.000	0.000	0.000	0.000	0.146	0.156	0.297	0.000	-0.001	0.015	0.000
Std. dev. of dep. var.	1.000	1.000	1.000	1.000	0.856	0.834	0.692	1.000	1.005	0.977	1.000
Observations	30088	30088	30088	30088	17736	4522	2506	30088	29104	23937	30088

Note: Each column represents a different specification, as presented in Figure 2. Column (1) presents the main specification, which includes year-month and county fixed effects, and controls for mothers' age, age-squared, and education, families' wealth-quintile fixed effects and urban-rural status, child's gender, birth order, and an indicator for being a multiple birth. The variable, Strike, is an indicator for whether there was a strike in the county and month of birth. Standard errors, clustered at the county level, are in parentheses and Wild bootstrap p-values are in brackets. All other columns are variations on this main specification. See Table A2 for a complete list and section 4.1.1 for complete descriptions.

Table A4 – Other Delivery Outcomes Across All Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Public hospital birth											
Strike	-0.005	-0.003	-0.031	0.023	-0.068	-0.025	-0.085	-0.004	0.005	-0.008	-0.005
	(0.025)	(0.022)	(0.024)	(0.024)	(0.029)	(0.019)	(0.030)	(0.028)	(0.026)	(0.025)	(0.025)
	[0.891]	[0.911]	[0.459]	[0.544]	[0.215]	[0.353]	[0.093]	[0.927]	[0.905]	[0.837]	[0.891]
Mean of dep. var.	0.260	0.260	0.260	0.260	0.288	0.350	0.359	0.260	0.257	0.272	0.260
Std. dev. of dep. var.	0.439	0.439	0.439	0.439	0.453	0.477	0.480	0.439	0.437	0.445	0.439
Observations	31388	31388	31388	31388	18256	4731	2607	31388	30368	24760	31388
Public clinic birth											
Strike	-0.016	0.001	0.017	0.000	-0.014	-0.009	0.011	-0.019	-0.036	0.003	-0.016
	(0.020)	(0.021)	(0.018)	(0.035)	(0.025)	(0.022)	(0.032)	(0.020)	(0.018)	(0.024)	(0.020)
	[0.594]	[0.975]	[0.567]	[0.998]	[0.715]	[0.734]	[0.811]	[0.560]	[0.264]	[0.928]	[0.594]
Mean of dep. var.	0.121	0.121	0.121	0.121	0.146	0.119	0.139	0.121	0.122	0.131	0.121
Std. dev. of dep. var.	0.326	0.326	0.326	0.326	0.353	0.324	0.346	0.326	0.327	0.337	0.326
Observations	31388	31388	31388	31388	18256	4731	2607	31388	30368	24760	31388
Private facility birth											
Strike	-0.007	-0.016	0.001	-0.036	0.009	-0.006	0.009	-0.014	-0.009	-0.014	-0.007
	(0.029)	(0.028)	(0.017)	(0.032)	(0.035)	(0.030)	(0.039)	(0.026)	(0.031)	(0.030)	(0.029)
	[0.914]	[0.769]	[0.984]	[0.573]	[0.887]	[0.905]	[0.894]	[0.798]	[0.862]	[0.813]	[0.914]
Mean of dep. var.	0.117	0.117	0.117	0.117	0.112	0.211	0.173	0.117	0.111	0.113	0.117
Std. dev. of dep. var.	0.321	0.321	0.321	0.321	0.316	0.408	0.378	0.321	0.315	0.317	0.321
Observations	31388	31388	31388	31388	18256	4731	2607	31388	30368	24760	31388
Doctor or nurse present											
Strike	-0.014	-0.002	0.000	0.003	-0.056	-0.027	-0.045	-0.021	-0.025	-0.004	-0.014
	(0.015)	(0.015)	(0.023)	(0.023)	(0.020)	(0.021)	(0.022)	(0.016)	(0.018)	(0.017)	(0.015)
	[0.530]	[0.953]	[0.998]	[0.930]	[0.134]	[0.355]	[0.184]	[0.397]	[0.384]	[0.881]	[0.530]
Mean of dep. var.	0.507	0.507	0.507	0.507	0.551	0.682	0.669	0.507	0.499	0.523	0.507
Std. dev. of dep. var.	0.500	0.500	0.500	0.500	0.497	0.466	0.471	0.500	0.500	0.499	0.500
Observations	31678	31678	31678	31678	18433	4768	2627	31678	30648	25001	31678

Note: Each column represents a different specification, as presented in Figure 2. Column (1) presents the main specification, which includes year-month and county fixed effects, and controls for mothers' age, age-squared, and education, families' wealth-quintile fixed effects and urban-rural status, child's gender, birth order, and an indicator for being a multiple birth. The variable, Strike, is an indicator for whether there was a strike in the county and month of birth. Standard errors, clustered at the county level, are in parentheses and Wild bootstrap p-values are in brackets. All other columns are variations on this main specification. See Table A2 for a complete list and section 4.1.1 for complete descriptions.

Table A5 – Vaccine Results Across All Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
BCG											
Strike	-0.021	-0.008	0.000	0.005	-0.012	0.001	0.000	-0.024	-0.023	-0.019	-0.021
	(0.013)	(0.013)	(0.014)	(0.014)	(0.020)	(0.006)	(0.010)	(0.015)	(0.012)	(0.015)	(0.013)
	[0.386]	[0.742]	[0.992]	[0.851]	[0.825]	[0.946]	[0.992]	[0.346]	[0.273]	[0.519]	[0.386]
Mean of dep. var.	0.922	0.922	0.922	0.922	0.948	0.963	0.977	0.922	0.921	0.930	0.922
Std. dev. of dep. var.	0.268	0.268	0.268	0.268	0.222	0.188	0.149	0.268	0.270	0.255	0.268
Observations	30055	30055	30055	30055	17729	4517	2504	30055	29072	23920	30055
DPT											
Strike	0.001	-0.001	0.001	0.004	-0.043	0.017	-0.033	0.004	0.017	-0.009	0.001
	(0.031)	(0.024)	(0.032)	(0.024)	(0.024)	(0.023)	(0.025)	(0.031)	(0.039)	(0.022)	(0.031)
	[0.977]	[0.978]	[0.981]	[0.884]	[0.207]	[0.584]	[0.319]	[0.919]	[0.753]	[0.776]	[0.977]
Mean of dep. var.	0.787	0.787	0.787	0.787	0.849	0.838	0.900	0.787	0.787	0.799	0.787
Std. dev. of dep. var.	0.409	0.409	0.409	0.409	0.358	0.369	0.299	0.409	0.409	0.401	0.409
Observations	30032	30032	30032	30032	17705	4512	2501	30032	29048	23891	30032
Polio											
Strike	-0.051	-0.055	-0.056	-0.061	-0.065	-0.044	-0.078	-0.049	-0.035	-0.058	-0.051
	(0.025)	(0.028)	(0.026)	(0.045)	(0.036)	(0.043)	(0.043)	(0.021)	(0.026)	(0.030)	(0.025)
	[0.311]	[0.294]	[0.241]	[0.427]	[0.286]	[0.459]	[0.250]	[0.205]	[0.453]	[0.268]	[0.311]
Mean of dep. var.	0.708	0.708	0.708	0.708	0.767	0.742	0.829	0.708	0.708	0.720	0.708
Std. dev. of dep. var.	0.455	0.455	0.455	0.455	0.423	0.437	0.377	0.455	0.455	0.449	0.455
Observations	30027	30027	30027	30027	17696	4512	2502	30027	29046	23885	30027
Measles											
Strike	-0.043	-0.035	-0.041	-0.027	-0.051	-0.024	-0.033	-0.043	-0.047	-0.036	-0.043
	(0.013)	(0.014)	(0.024)	(0.017)	(0.022)	(0.009)	(0.016)	(0.013)	(0.013)	(0.014)	(0.013)
	[0.087]	[0.167]	[0.295]	[0.323]	[0.202]	[0.100]	[0.149]	[0.089]	[0.056]	[0.185]	[0.087]
Mean of dep. var.	0.733	0.733	0.733	0.733	0.759	0.785	0.797	0.733	0.735	0.715	0.733
Std. dev. of dep. var.	0.442	0.442	0.442	0.442	0.428	0.411	0.402	0.442	0.442	0.451	0.442
Observations	29998	29998	29998	29998	17690	4512	2500	29998	29016	23869	29998

Note: Each column represents a different specification, as presented in Figure 2. Column (1) presents the main specification, which includes year-month and county fixed effects, and controls for mothers' age, age-squared, and education, families' wealth-quintile fixed effects and urban-rural status, child's gender, birth order, and an indicator for being a multiple birth. The variable, Strike, is an indicator for whether there was a strike in the county and month of birth. Standard errors, clustered at the county level, are in parentheses and Wild bootstrap p-values are in brackets. All other columns are variations on this main specification. See Table A2 for a complete list and section 4.1.1 for complete descriptions.

Table A6 – Vaccines in NUHDSS

	BCG (1)	DPT (2)	Polio (3)	Measles (4)
Strike	-0.011	-0.052	-0.081	-0.099
	(0.019)	(0.032)	(0.037)	(0.039)
	{0.576}	{0.112}	{0.037}	{0.019}
Mean of dep. var.	0.899	0.776	0.724	0.604
Std. dev. of dep. var.	0.301	0.417	0.447	0.489
Observations	4729	4694	4723	4456

Note: The variable, Strike, is an indicator for whether there was a strike in the county and month of birth. Standard errors, clustered at the county level, are in parentheses. Permutation test p-values are in braces. Data are from the NUHDSS supplementary data collection and include children born after 2009.

Table A7 – Main Results (with Nairobi interaction)

	Neonatal mortality (1)	Post-neonatal mortality (2)	Facility birth (3)	Height for age z-score (4)	Weight for age z-score (5)	Vaccine index (6)
Strike	0.018	0.013	-0.056	-0.251	-0.120	-0.097
	(0.019)	(0.028)	(0.043)	(0.212)	(0.107)	(0.067)
Strike × Nairobi	0.002	0.006	0.039	0.306	0.192	0.005
	(0.019)	(0.028)	(0.045)	(0.208)	(0.108)	(0.072)
Mean of dep. var.	0.028	0.022	0.498	-1.087	-0.954	0.000
Std. dev. of dep. var.	0.164	0.146	0.500	1.419	1.217	1.000
Observations	31672	25494	31388	27429	27429	30088

Note: Post-neonatal infant mortality is conditional on the child having survived the first month. Each regression includes year-month and county fixed effects, and controls for age, age-squared, education, wealth quintiles, and urban-rural status. The variable, Strike, is an indicator for whether there was a strike in the county and month of birth. Standard errors, clustered at the county level, are in parentheses.