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

Willa Friedman* Anthony Keats†

July 5, 2019

Abstract

This paper measures the effects of health care 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 1995 and 2014. We find that disruptions to care quality during strikes increases both neonatal and infant mortality. In addition, children born during strikes who survive have lower height- and weight-for age z-scores and receive fewer vaccinations in the first year of life. Overall these results show that health care quality can have large immediate health impacts and suggests that these effects persist over time. This study also provides the first rigorous evidence on the consequences of health worker strikes, a growing but understudied phenomenon in Sub-Saharan Africa.

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

JEL Codes: I15, I18, O15

*University of Houston, email: willa.friedman@gmail.com
†Wesleyan University, email: akeats@wesleyan.edu

We thank Iman Nourhussein and Sharon Xuejing Zuo for excellent research assistance. We are also grateful to Mary Ann Bronson, Aimee Chin, Yoonjoung Choi, Shoshana Grossbard, Rema Hanna, Giulia La Mattina, Muthoni Ngatia, Lelei Shen, Alessandro Tarozzi, Nicholas Wilson, various seminar and conference participants, and, especially, two anonymous referees for generous feedback and comments.

1 Introduction

A large literature documents the many shortcomings of health facilities in developing 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 the quality of care at health facilities (or lack thereof) affects health outcomes.

This paper estimates the impact of health care quality on early child health by using disruptions to care around the time of birth caused by localized health worker strikes at major (mostly public) hospitals in Kenya. We combine data from a retrospective panel of births (from Demographic and Health Surveys) with a record of the timing and location of health worker strikes that we collected from a broad set of media reports. Our sample frame covers the years 1995-2014, during which time there were 32 local strikes across 11 counties (out of 47 total) and 16 separate hospitals.

The majority of strikes occur at top-level referral hospitals, which treat the most at-risk children and which provide some of the highest levels of care in Kenya. Nearly three-quarters of strikes are partial, featuring either doctors or nurses but not both. Thus, during strikes vulnerable children may receive substandard care for two reasons: (a) the striking hospital remains open but with severely limited staff, or (b) the child is delivered in, or brought to, a non-striking hospital that is either lower quality (by definition) and/or offers worse care due to overcrowding as a consequence of the strike. Since the timing of strikes is plausibly exogenous to women's decisions to conceive, and hence the timing of births, these strikes provide an exogenous shock to health care quality at birth for children who would normally seek care at strike hospitals.

We find large and statistically significant increases in both neonatal and infant mortality for children born in counties experiencing a strike at a major hospital during the month of birth. Strikes cause an additional 15 deaths per 1000 live births at both 1 week and 1 month, representing a 68 and 54 percent increase in overall mortality at the mean, respectively. Children born during strikes are also more likely to die before reaching 6 months and 12 months of age. Conditional on

[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/`

surviving the first month of life, there are another 15 deaths per 1000 births by 6 months of age; and, conditional on surviving to 6 months of life, there are an additional 9 deaths per 1000 births by 1 year. These results suggest that short-lived disturbances to the quality of care at delivery can have lasting effects.

For children who survive, we also find reductions in health. We use child height- and weight-for-age z-scores as summary measures of long- and short-term health, respectively, and find that both outcomes are negatively affected by strikes. Conditional on surviving the first year of life, for example, children are statistically significantly shorter (-0.12 s.d.) and lighter (-0.06 s.d.). Moreover, these estimates may understate the true effect of strikes on health to the extent that smaller children are less likely to survive to be measured. As with the findings on mortality, these results also indicate that for children born during strikes there may be impacts on health, or use of health care, that persist as children age.

Finally, we also find that strikes have impacts on essential child health inputs that continue after strikes have ended. Specifically, children born during strikes are less likely to be fully vaccinated against common illnesses such as polio and the measles. These effects are remarkable 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. However, our results are consistent with evidence from Nigeria showing that women who otherwise would use facilities, but who randomly deliver at home, are less likely to get their child postnatal care (Okeke and Chari, 2018), and evidence from Ghana that finds that women who switch from home births to facility births also increase child vaccination rates (Friedman and Keats, 2019).

Our results are robust to various sample restrictions (e.g. limiting the sample frame to 2009-2014 when two-thirds of strikes occur, or to only counties that ever experienced strikes) and a number of alternative specifications that also relax assumptions about location effects and time trends. Further, because about half of strikes occur in the capital, Nairobi, we also test whether those strikes are driving the results. When we exclude Nairobi from the analysis, we obtain nearly identical point estimates from the remaining counties showing that strikes increase mortality and reduce health and vaccines, although these are measured with less precision. Finally, we test whether seasonal patterns in the timing of births are correlated with the timing of strikes. This could lead to, for example, excess births during strike months which may bias estimates upward, particularly if overcrowding is a concern. Reassuringly, we find no correlation between strikes and the timing of births, indicating that seasonal patterns are not likely influencing the results.

As further evidence that health worker strikes resulted in increased mortality and we are not picking up a spurious correlation, we replicate each of the three main results using longitudinal data from the Nairobi Urban Health and Demographic Surveillance System (NUHDSS). The NUHDSS tracks households in two urban slums located near large hospitals in Nairobi with frequent strikes. In addition to an indicator for mortality linked with exact day of birth, this data set also includes rich verbal autopsy data regarding cause of death. We find statistically significant early child mortality results of a similar magnitude in this data for children born on days when strikes occurred, and we see that the increase in early deaths is largely driven by neonatal causes. Among children who survive, we again find that they are smaller for their age and are less likely to receive vaccines.

Taken together, our findings contribute to a small but growing literature on the causal links between health care 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 (Unpublished manuscript, 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 results extend this literature by showing that hospitals, under normal operations, provide positive health benefits to newborn children. If they did not, we would not see an impact on health when these services are disrupted. This suggests that efforts to increase access and use of public health facilities, without commensurate efforts to improve the robustness of these facilities to cope with increased demand, may have limited impacts on health outcomes. Our results also build on evidence that shows that health systems have limited ability to respond to shocks ex post or to adequately ensure that patients are returned to the chain of care after such interruptions (Goldstein et al., 2013). For example, our results suggest that one important secondary cost of the Ebola crisis may have been through the reduction in health services provided by health-workers who were unable to provide care because they were sick themselves or busy treating those who were stricken by Ebola.

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. 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 health care 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 along with a discussion of how to interpret our estimated effect sizes. 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, month, and year. We also supplement this analysis with birth and mortality data from the Nairobi Urban Health and Demographic Surveillance System (NUHDSS). This data is linked to the strikes information by exact date.

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. In total, we recorded 38 strikes from 1998 through the end of 2014. 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 (32 are local and 6 are national). Half of the local strikes occurred in Nairobi, the capital, with the remainder spread across 10 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 10 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. 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.

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 28 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. This includes both demands for higher 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 services (and health services in general) in Kenya. Public facility maternity services are offered at sub-county health centers, sub-county and county hospitals, and provincial and national hospitals. Private clinics and nursing homes also provide child delivery services, but these are much less frequently used. 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 and continuing to the present.[2] The main union for doctors is the Kenya Medical Practitioners, Pharmacists, and Dentists Union (KMPDU), while the Kenyan National Union of Nurses (KNUN) organizes nurses. Both 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.

In general, Kenyan health services are characterized by relatively poor quality and are roughly comparable to those in other Sub-Sahara African countries, with a few notable exceptions. A recent World Bank Service Delivery Indicator (SDI) survey found 28 percent of health care 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 developing 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 slightly 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.

The majority (66 percent) of the hospitals where strikes occurred in our sample are first-referral level county (formerly district) hospitals and national referral hospitals. These are the top-line public hospitals that exist in Kenya and the ones that take the hardest cases. For example, in

[2] In December 2016, doctors conducted a nationwide strike lasting nearly 4 months, followed by a national nurses strike that ended after 5 months.

our data, women who were told during an antenatal visit about pregnancy complications were 45 percent more likely to deliver in a government hospital (DHS data does not distinguish between sub-county, county, or national public hospitals). Children born elsewhere can also be referred to these hospitals if they develop complications following birth. Another 16 percent of the strikes in our sample occurred in Pumwani Maternity Hospital. Pumwani is the largest maternity hospital in East Africa, and predominantly serves the poor who are also at greater risk of early child mortality (Lawn et al., 2005).

Indeed, neonatal mortality rates at the types of hospitals where strikes occur are typically several times larger than national rates. A survey of 14 district hospitals (not including Nairobi) found neonatal mortality was 51 per 1000 births, a rate nearly double the national average (English et al., 2004). Seven-day mortality in Moi Teaching and Referral Hospital ranged from 60 to 80 deaths per 1000 births between 2004 and 2011 (Yego et al., 2013). In Nairobi hospitals, neonatal mortality was 135 per 1000 births overall (Murphy et al., 2018), while in Pumwani Maternity Hospital and Kenyatta National Hospital rates were 128 and 315 deaths per 1000 births, respectively, or 4 to 11 times the national average (Tele et al., 2017; Simiyu, 2003).

This suggests that a disproportionate number of the children directly affected by strikes either were high-risk pregnancies or had major health complications within the first weeks of life. In developing countries, nearly all neonatal mortality (86 percent) is due to either preterm birth/low birth weight, severe infections (sepsis, pneumonia, tetanus, or diarrhoea), or asphyxia (Lawn et al., 2005). Three quarters of neonatal deaths occur within the first week of life, with the highest risk of mortality in the first day (Zupan and Aahman, 2005).

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 Banks Service Delivery Indicators (SDI) report (2013) and the DHS Service Provision Assessment (SPA) report (2010) both show that hospitals in general are more likely 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 caesarian 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.

2.3 DHS Data

Demographic and Health Surveys are population surveys conducted all over the developing world with the original goal of providing information necessary to estimate future population trends. The surveys ask respondents to report extensive details of their fertility histories, including the timing of all previous births, the actions of the mother and services sought both before and at birth, and initial and long-term health outcomes of the children. This information is collected for all children born in the previous 5 years to the DHS's nationally representative sample of women ages 15-49. A few questions are asked of all previous births (beyond just those born in the past 5 years), including whether the child is still alive. The data used in the analysis is from the 2003, 2008/09, and 2014 DHS and includes children born between 1995 and 2014.

Sample characteristics of children, including key health inputs and outcomes, are presented in Table 2. The first column shows summary statistics for the full sample, 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. Half of all births took place in a health facility, rather than in a home. Of the facility births, three-quarters were in hospitals. All facility births, and no home births, were attended by either a doctor, nurse, or midwife.[3] Facility 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

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

at or near birth, vaccinations for diphtheria, pertussis, and tetanus (DPT), polio, and the measles are typically given beginning when children are at least one month old through the first year of life.[4] While most children receive the BCG vaccine (94 percent), 23-31 percent of children are not fully immunized for DPT, polio, or measles. Vaccination rates are higher among children born in health facilities (by 5-12 percentage points), and are higher in counties that had strikes compared to non-strike counties (by 1-4 percentage points).

Despite differences in location of birth and early care, mortality rates among children are remarkably similar. There were approximately 22 deaths per 1000 births in the first week of life in our sample, 28 deaths per 1000 births by the first month, and 54 deaths per 1000 births by 1 year of age. Mortality rates are somewhat lower for children born in health facilities compared to children born at home, and slightly higher in counties where strikes occurred.

Characteristics of mothers in the sample are presented in Table 3. Summary statistics are again presented for the full sample (column 1), for mothers who reported delivering all births in the last five years at home against those reporting at least one birth in a health facility (columns 2-3), and for mothers residing in counties with and without strikes (columns 4-5). The average mother in the sample is 31 years old, has 3.6 children, and 7 years of schooling (in Kenya, primary school consists of 8 years). Nearly three-quarters of mothers are married, one-third live in an urban area, and 23 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 approximately one fewer children, have more than two years additional schooling, and are nearly twice as 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 similarly selected on observables relative to women from counties with no strikes. Even when excluding the capital Nairobi, women in strike counties are 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 are representative of the average

[4]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.

Kenyan woman who typically delivers in health facilities.

3 Empirical Strategy

In the main analysis that follows, we use variation in strike occurrences across counties and over time to identify the causal impacts of health worker strikes. We compare the differences in outcomes of babies born in a given county when there is a strike and when there isn't one relative to the differences in outcomes of babies born in other counties in the same months. We estimate the following equation:

$$Y_{icym} = \beta_1 Strike_{cym} + \gamma_c + \delta_{ym} + \mathbf{X}_{icym} + \varepsilon_{icym} \tag{1}$$

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. 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. Standard errors are clustered at the county level.

The identification rests on the assumption that it is plausibly exogenous whether a woman gives birth during a strike 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. It has been shown in developed 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). However, 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. The main analysis relies on the month of birth, and thus this is not likely to be altered frequently in response to a health worker strike. In addition, inductions and cesarean sections are relatively uncommon in Kenya, as in much of the developing world, and thus these are less likely to be used to alter the date of delivery. In our sample, 6 percent of births (13 percent of those in facilities) were delivered through cesarean sections.

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, and electricity access, we see only one statistically significant difference, which we attribute to chance. However, as a precaution we control in all subsequent specifications for age, age-squared, education, urban-rural status, and wealth quintiles. Finally, the last column of Table A1 shows there are no differences in the number of births in strike counties when strikes occur.

The specification in equation 1 also makes some parametric assumptions about the evolution of outcomes over time and across counties. To address these, we include results from restricted samples (in the main analysis tables) and results from alternate specifications that relax constraints on time and location effects (included in the Online Appendix). 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 the data to these years to avoid extrapolating from years when there were few strikes and high mortality. We also restrict the sample to only include counties that ever experienced strikes (10 out of the 42 counties in Kenya), as counties without strikes over the 20 year period may be systematically different from counties with strikes and may confound identification of treatment effects. Finally, as additional robustness checks we include, separately, DHS survey cluster FE (rather than county FE) and county-year FE. DHS survey cluster FE may be more appropriate given that strikes typically affected a single hospital and, depending on proximity, there may be important heterogeneity of impacts across different locations within counties. The county-year FE relax the constraint that all counties follow the same trends over time, and in particular allow for the possibility that outcomes in strike counties evolved differently in years prior to or following strikes relative to counties that did not experience strikes.

4 Results

4.1 Main Results

We first present the effects health worker strikes on child mortality. We find that children born in counties during months when strikes are occurring are less likely to survive the neonatal period. Columns 2 and 3 of Panel A in Table 4 show statistically significant increases (of 1.5 percentage points) in the likelihood that a child born during a strike died within the first 7 days and first month of life. These are large effects and represent an increase in mortality of 68 and 54 percent, respectively, at the mean. The pattern of results is largely unchanged when we restrict the sample to births between 2010 and 2014, when the majority of strikes occurred, or to only include counties that ever experienced a strike during the study period (Panels B and C).

In addition, we find the effects on mortality from strikes persist even among children who survive the first month of life. If the impact of strikes was only to increase neonatal mortality, we would expect the difference in the fraction of children who died at later ages, conditional on surviving the first month, to be zero. Instead we find a widening mortality gap between children born during strikes and those born when no strikes occurred. Column 3 shows an additional 1.5 percentage point increase in the likelihood of mortality within the first 6 months (conditional on surviving past 1 month), while column 4 shows another .9 percentage point increase in mortality within the first year (conditional on surviving past 6 months). We obtain qualitatively identical results in Panels B and C.

Among children who do survive, there is suggestive evidence their health is worse as well. Table 5 presents estimates of the effects of strikes on height-for-age and weight-for-age z-scores. Height-for-age measures the long term effects of malnutrition and chronic illness since birth; weight-for-age also measures the effects poor health since birth but may also reflect more recent bouts of illness or poor nutrition. The average child in Kenya is born with average (or slightly above average) height and weight relative to international norms. However, both indicators fall precipitously in the first year of life leaving children aged 1-5 approximately 1 standard deviation behind the norm.

For the full sample, strikes appear to further reduce height (column 1), although not statistically significantly, and have no effect on weight (column 3). However, when we restrict the sample to children who survived the first year (columns 2 and 4), we see negative and often statistically significant effects across both outcomes. This pattern of results is consistent with a story of selection effects dominating scarring (non-fatal adverse effects of health shocks) at younger ages, and scarring dominating selection effects for children who survive past age 1 (Bozzoli et al., 2009). Alternatively, the fact that these impacts do not materialize until children are older could also suggest that one effect of strikes is to reduce early investments in child health (a point which we take up further below).

Table 6 begins to investigate mechanisms by looking at variation in the place of delivery as a function of health worker strikes. Overall, we do not see statistically significant differences in the likelihood of delivering in any formal health facility (rather than at home), whether the delivery was in a government or private hospital, or whether a doctor or nurse was present at birth. The point estimates consistently go in the direction that we would expect from strikes that discouraged the use of public facilities, e.g. fewer public facility births, but these differences are only statistically significant in Panel B, which limits the sample to births between 2010 and 2014 when the majority of strikes occurred.

Although there is no apparent change in the use of facilities, it is likely that many women with at-risk pregnancies, or with children who developed complications shortly after being born, nevertheless experienced lower quality care during strikes. First, for the majority of strikes (72 percent) hospitals remained open but with limited trained staff as either doctors or nurses were absent. In addition, since most hospitals are located in more urban areas, and since local strikes are typically confined to individual hospitals, it is also possible that some women affected by strikes were still able to find other health facilities to deliver their children. However, 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. 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, with reporting in one case that Kenyatta National Hospital (which remained open in this instance) provided delivery services for three times as many mothers than it had accommodations to serve.[5] Even in the absence of a demand shock, switching to deliver in a non-striking facility could also involve a reduction in the quality of care if the new facility is second-best – which may often be the case given that, in our data, strikes mainly occur in top-level district or national hospitals.

Table 7 looks at whether strikes affect access to early childhood health inputs. Despite little to no measurable change in hospital births during strikes, we do find that children born during strikes are less likely to be fully immunized against tuberculosis (BCG), diphtheria, pertussis, and tetanus (DPT), polio, and the measles. Effects are largest, and statistically significant, for both polio and the measles and indicate a 5.5-6.7 percent reduction in coverage of these illnesses. 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 underestimate the true effect of strikes on vaccines. Restricting the data to periods when most strikes occur (Panel B) or to counties that ever experienced strikes (Panel C) has little effect on the pattern of outcomes.

The reduction in vaccination rates is particularly striking given that the average strike lasts only one week and that (except for the BCG vaccine) children are recommended to be at least 6 weeks old before their first dosing (in the case of measles, children are recommended to be 9 months old). Women who deliver during strikes should still be able to get their child vaccinated after the strike has ended and, indeed, vaccinations are available at other locations besides hospitals such as health clinics. The fact that they don't suggests that interruptions to standard care can have long-lasting impacts on health seeking behaviors among affected women. This could potentially help to explain both the reduction in vaccination rates, the mortality increases, and the worsening health status among survivors seen for children born during health worker strikes.[6] While this link is somewhat speculative, it does match other evidence from Nigeria and Ghana showing that

[5]see, e.g., "Eight unborn babies die in strike tragedy" *Daily Nation*, Dec. 17, 2013; "How ongoing nurses strike is killing babies in Kenya" *Daily Nation*, Aug. 15, 2017.

[6]In the context of Kenya, one important intervention received by some children at the time of birth is Prevention of Mother to Child Transmission of HIV (PMTCT). Unfortunately, the DHS survey does not include information about whether the mother received PMTCT, but its absence would have a dramatic effect on mortality among children born to HIV positive mothers. Mothers' HIV status was collected for the 2003 and 2008/2009 DHS waves. If we look at mothers who are HIV positive at the time of the survey (8 percent of the sample), 18 percent of their children have died. Among those who are currently HIV negative, 8 percent have died. In this sample, only two children were born to HIV positive mothers during strikes, and so we cannot separately estimate the effect of a strike on them.

facility births increase neonatal and and infant care (Okeke and Chari, 2018; Friedman and Keats, 2019).

In Online Appendix Table A2 we replicate the main results tables using two alternative specifications. First, we use DHS survey cluster fixed effects in place of county fixed effects. 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. Second, we use county-year fixed effects to allow for different trends over time between counties that experience strikes in particular years and those that do not. We also test whether the mortality effects found in this paper are driven solely by strikes occurring in Nairobi county, given that half of the strikes took place there. Across these alternate specifications the pattern of results is largely unchanged, and in particular, the mortality results mirror those in the main analysis in both size and statistical significance.[7]

4.2 Nairobi Urban Health and Demographic Surveillance System Data

Nine strikes took place in the Pumwani Maternity Hospital (the largest maternity hospital in the country) and the Kenyatta National Hospital (the largest referral hospital) between 2003 and 2014. We add to the main analysis by exploiting a panel data set from two informal settlements (Korogocho and Viwandani) located near these two hospitals. 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 enter and exit the sample through births, migration, and deaths, which are tracked through household visits every four months. During the survey period 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 the two large hospitals 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.

[7] We find similar results for the other outcomes as well, available upon request.

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

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

where Y_{iymd} is the outcome variable for a birth to mother i, during year y, month m, and day 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. Table 8 presents the results on mortality. Reassuringly, the results are nearly identical to those found using the DHS data – health worker strikes increase neonatal and infant mortality.

We can also use the verbal autopsy to check whether the causes of death for those born during strikes are consistent with limited neonatal care. In Figure 1, we present the causes of deaths for 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.

Finally, the NUHDSS also contains a supplementary data collection effort to measure health outcomes and vaccinations for all children born after 2009. Table 9 presents results from this data on the effects of strikes on height- and weight-for-age z-scores and vaccines. Point estimates for both height- and weight-for-age are negative and of similar magnitudes to those found in the DHS data (Table 6), although none are statistically significant. In addition, in columns 4 and 5 we see that children born during strikes are less likely to receive both the polio and measles vaccines.

4.3 Discussion of Effect Sizes

The point estimates for the impacts on mortality that we measure in this paper imply large effect sizes. We first note that our estimates are measured with some imprecision such that we cannot reject a wide range of effects. We then present additional evidence that, nevertheless, large effects are indeed plausible (even when taking into account that strikes are often short-lived and that only a minority of the sample requires services provided by striking hospitals).

Our point estimates are measured with considerable imprecision. Standard errors are large, and though we can rule out a zero effect, we cannot rule out a wide range of effect sizes. For example, in the main specification (Table 4 Panel A), we estimate that infant mortality increases by 1.5 percentage points. However, the 95 percent confidence interval for this estimate ranges from 0.2 to 2.8 percentage points, and we cannot reject any of these potential effect sizes.

However, there is some reason to believe the true treatment effect may lie in the lower end of this interval. Camerer et al. (2016) and Camerer et al. (2018) show that studies that are less than fully powered 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. For our neonatal mortality results, the ex-post MDE ($2.8 \times .006 = 0.0168$) is slightly higher than our point estimate, which likewise suggests we are only powered to detect relatively large effects. At the same time, the fact that we find similar estimates for neonatal mortality in the NUHDSS data also suggests that our results are not a false positive.

Despite the above caveats, large child mortality effects are indeed plausible in this setting. Even very short-term delays in receiving appropriate care for common perinatal and neonatal complications can lead to drastically increased chances of death or long-term morbidity (Lawn et al., 2009; Baqui et al., 2009; Simmons et al., 2010). Access to readily-available interventions in developing 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). Similarly, in Kenya, about 70 percent of the decline in neonatal deaths between 2003 and 2014 has been attributed to expanded access to effective early-life interventions (Keats et al., 2017). Together, these estimates imply that when care is not provided, or is unavailable, neonatal mortality can increase by as much as 122-455 percent.

Our point estimates are consistent with this range of treatment effects. A back of the envelope calculation is useful to show this. Taken at face value, the point estimate in Table 4 column 2 says that strikes increase neonatal mortality by 1.5 percentage points from a base of 2.8, or a 54 percent increase. Of course, not all children are directly affected by strikes. Assume that 16 percent of the children born in a given month would be delivered or seek care in a strike hospital in the absence of a strike. Sixteen percent is reasonable given that 26 percent of children in the sample are born in a government hospital and that strikes affect 55 percent of government hospital beds within a county on average. Sixteen percent is also the fraction of children who were born at either Pumwani Maternity Hosptial or Kenyatta National Hospital (the two main strike hospitals in Nairobi) in the NUHDSS data. Additionally, some children born elsewhere would be referred to strike hospitals after developing complications shortly after birth, and these children need to be accounted for as well.

Scaling the point estimate of 1.5 by 1/.16, this implies neonatal mortality increased by 9 percentage points. Importantly, however, this is an increase among those who would have used a strike hospital in the absence of a strike. As described earlier, baseline neonatal mortality rates for these children are much higher than in the general population. Assuming that neonatal mortality rates for children who would seek care in strike hospitals is 90 per 1000 births (which is consistent with the evidence presented above), the scaled effect represents a 100 percent increase in neonatal mortality for this sub-population. Note that this is on the lower end of the 122-455 percent range implied by estimates of the effects of access to early-life medical interventions.

However, we also need to adjust our results to take into account the fact that most strikes did not last an entire month. On average, strikes last 7 days within a month. Given that most neonatal mortality occurs within the first few days after birth (Zupan and Aahman, 2005; Lawn et al., 2005), it seems reasonable to also include as affected children born a few days prior to the onset of a strike as well. Thus, if we assume that strikes impacted births occurring over a 10-11 day period, this suggests an additional scaling factor of approximately 3. Together with the scaling factor described above to account for the affected sub-population of children, this implies that neonatal mortality increased by nearly 300 percent among high-risk children who were directly affected by strikes. This scaled effect size is roughly midway in the 122-455 percent range implied by evidence on the efficacy of gaining access to live-saving care.

The effect sizes we document are also consistent with the large variation in mortality rates across countries in Sub-Saharan Africa that appears related to health care quality. Compared to other countries in Sub-Saharan Africa, Kenyan child mortality rates are lower than average. Over the period 1995-2014 neonatal (infant) mortality in Kenya was approximately 27 (53) per 1000 births on average, while neonatal (infant) mortality across Sub-Saharan Africa during the same period was 37 (80) per 1000 births. At the same time, health facilities in Kenya are of somewhat higher quality relative to those in other African countries and, across some dimensions, compare favorably to those in India and rural China (Daniels et al., 2017).[8] The effect sizes estimated in this paper – derived from a reduction in the quality of care at birth – are equivalent to increasing neonatal and infant mortality rates in Kenya to regional averages.

Finally, our results are also in line with a number of other studies within a related literature on the efficacy of institutional births.[9] 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 both 7-day and 1-month mortality by magnitudes nearly identical to those presented in this paper.[10] Okeke and Chari (2018) estimate the differences-in-differences in outcomes for children born during the day compared to at night in areas with and without 24-hour facility care in Nigeria and find that non-institutional births increase neonatal mortality by approximately 10 deaths per 1000 births (a doubling of the mean). In Ghana, the removal of user fees for facility births increased facility usage and, while there was no measurable effect on neonatal mortality, led to large reductions in 1-year and 3-year mortality (Friedman and Keats, 2019). Finally, Daysal et al. (2015) show hospital births decrease neonatal mortality relative to home deliveries by 8 to 9 deaths per 1000 live births among low-risk women in the Netherlands.

5 Conclusion

This paper has shown that strikes have immediate and persistent impacts on child health and survival. This appears to be driven by lower quality of care caused by either reduced staffing at striking hospitals or overwhelmed facilities at nearby non-striking hospitals, which themselves may be of lower quality. In the short-run, we see that children born during strikes are less likely to survive. This disruption of care also has longer-run consequences as evidenced by the fact that

[8]ibid.

[9]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.

[10]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.

children born during strikes are less likely to receive vaccinations – and potentially other important early child health interventions – in the months after strikes have ended.

Beyond demonstrating the immediate effects of strikes themselves, these results offer a few broader policy implications. First, they suggest that large public hospitals (where the majority of the strikes documented in this paper occur) 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. Our results also build on evidence that shows that health facilities have only limited abilities to respond to shocks ex post and, moreover, to adequately ensure that patients are returned to the chain of care after such interruptions. Taken together this suggests that efforts to increase access and use of public health facilities, without commensurate efforts to improve the robustness of these facilities to cope with increased demand, may have limited impacts on health outcomes.

Our approach 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.

References

Adam, Mary Beth, Sarah Muma, Jecinter Achieng Modi, Mardi Steere, Nate Cook, Wayne Ellis, Catherine T Chen, Arianna Shirk, John K Muma Nyagetuba, and Erik N Hansen, "Paediatric and obstetric outcomes at a faith-based hospital during the 100-day public sector physician strike in Kenya," *BMJ Global Health*, 2018, *3* (2).

Baqui, Abdullah H, Saifuddin Ahmed, Shams El Arifeen, Gary L Darmstadt, Amanda M Rosecrans, Ishtiaq Mannan, Syed M Rahman, Nazma Begum, Arif BA Mahmud, Habibur R Seraji et al., "Effect of timing of first postnatal care home visit on neonatal mortality in Bangladesh: a observational cohort study," *Bmj*, 2009, *339*, b2826.

Bhuiyan, MMZU and A Machowski, "Impact of 20-day strike in Polokwane hospital (18 August-6 September 2010)," *SAMJ: South African Medical Journal*, 2012, *102* (9), 755–756.

Björkman, Martina and Jakob Svensson, "Power to the people: evidence from a randomized field experiment on community-based monitoring in Uganda," *The Quarterly Journal of Economics*, 2009, *124* (2), 735–769.

Bozzoli, Carlos, Angus Deaton, and Climent Quintana-Domeque, "Adult height and childhood disease," *Demography*, 2009, *46* (4), 647–669.

Camerer, Colin F, Anna Dreber, Eskil Forsell, Teck-Hua Ho, Jürgen Huber, Magnus Johannesson, Michael Kirchler, Johan Almenberg, Adam Altmejd, Taizan Chan et al., "Evaluating replicability of laboratory experiments in economics," *Science*, 2016, *351* (6280), 1433–1436.

_ , _ , Felix Holzmeister, Teck-Hua Ho, Jürgen Huber, Magnus Johannesson, Michael Kirchler, Gideon Nave, Brian A Nosek, Thomas Pfeiffer et al., "Evaluating the replicability of social science experiments in Nature and Science between 2010 and 2015," *Nature Human Behaviour*, 2018, *2* (9), 637.

Chaudhury, Nazmul, Jeffrey Hammer, Michael Kremer, Karthik Muralidharan, and F Halsey Rogers, "Missing in action: teacher and health worker absence in developing countries," *The Journal of Economic Perspectives*, 2006, *20* (1), 91–116.

Daniels, Benjamin, Amy Dolinger, Guadalupe Bedoya, Khama Rogo, Ana Goicoechea, Jorge Coarasa, Francis Wafula, Njeri Mwaura, Redemptar Kimeu, and Jishnu Das, "Use of standardised patients to assess quality of healthcare in Nairobi, Kenya: a pilot, cross-sectional study with international comparisons," *BMJ global health*, 2017, *2* (2), e000333.

Darmstadt, Gary L, Zulfiqar A Bhutta, Simon Cousens, Taghreed Adam, Neff Walker, Luc De Bernis, Lancet Neonatal Survival Steering Team et al., "Evidence-based, cost-effective interventions: how many newborn babies can we save?," *The Lancet*, 2005, *365* (9463), 977–988.

Das, Jishnu and Jeffrey Hammer, "Which doctor? Combining vignettes and item response to measure clinical competence," *Journal of Development Economics*, 2005, *78* (2), 348–383.

_ and _ , "Quality of primary care in low-income countries: Facts and economics," *Annu. Rev. Econ.*, 2014, *6* (1), 525–553.

_ , _ , and Kenneth Leonard, "The quality of medical advice in low-income countries," *The Journal of Economic Perspectives*, 2008, *22* (2), 93–114.

Daysal, N Meltem, Mircea Trandafir, and Reyn Van Ewijk, "Saving lives at birth: The impact of home births on infant outcomes," *American Economic Journal: Applied Economics*, 2015, *7* (3), 28–50.

Dickert-Conlin, Stacy and Amitabh Chandra, "Taxes and the Timing of Births," *Journal of Political Economy*, 1999, *107* (1), 161–177.

English, Mike, Fabian Esamai, Aggrey Wasunna, Fred Were, Bernhards Ogutu, Annah Wamae, Robert W Snow, and Norbert Peshu, "Assessment of inpatient paediatric care in first referral level hospitals in 13 districts in Kenya," *The Lancet*, 2004, *363* (9425), 1948–1953.

Friedman, Willa and Anthony Keats, "Institutional Births and Early Child Health: Evidence from Ghana's Free Delivery Policy," 2019. Working paper.

Gans, Joshua S and Andrew Leigh, "Born on the first of July: An (un) natural experiment in birth timing," *Journal of Public Economics*, 2009, *93* (1), 246–263.

Gertler, Paul and Christel Vermeersch, "Using performance incentives to improve medical care productivity and health outcomes," Technical Report, National Bureau of Economic Research 2013.

Godlonton, Susan and Edward N Okeke, "Does a ban on informal health providers save lives? Evidence from Malawi," *Journal of Development Economics*, 2016, *118*, 112–132.

Goldstein, Markus, Joshua Graff Zivin, James Habyarimana, Cristian Pop-Eleches, and Harsha Thirumurthy, "The Effect of Absenteeism and Clinic Protocol on Health Outcomes: The Case of Mother-to-Child Transmission of HIV in Kenya," *American Economic Journal: Applied Economics*, 2013, *5*, 58–85.

Gyamfi, Gerald Dapaah, "Assessing the effects of industrial unrest on Ghana health service: A case study of nurses at Korle-Bu teaching hospital," *International Journal of Nursing and Midwifery*, 2011, *3* (1), 1–5.

Jones, Gareth, Richard W Steketee, Robert E Black, Zulfiqar A Bhutta, Saul S Morris, Bellagio Child Survival Study Group et al., "How many child deaths can we prevent this year?," *The Lancet*, 2003, *362* (9377), 65–71.

Keats, Emily C, Anthony Ngugi, William Macharia, Nadia Akseer, Emma Nelima Khaemba, Zaid Bhatti, Arjumand Rizvi, John Tole, and Zulfiqar A Bhutta, "Progress and priorities for reproductive, maternal, newborn, and child health in Kenya: a Countdown to 2015 country case study," *The Lancet Global Health*, 2017, *5* (8), e782–e795.

Lawn, JE, Kate Kerber, Christabel Enweronu-Laryea, and O Massee Bateman, "Newborn survival in low resource settingsare we delivering?," *BJOG: An International Journal of Obstetrics & Gynaecology*, 2009, *116*, 49–59.

Lawn, Joy E, Simon Cousens, Jelka Zupan, Lancet Neonatal Survival Steering Team et al., "4 million neonatal deaths: when? Where? Why?," *The lancet*, 2005, *365* (9462), 891–900.

Milligan, Kevin, "Subsidizing the stork: New evidence on tax incentives and fertility," *Review of Economics and Statistics*, 2005, *87* (3), 539–555.

Murphy, Georgina AV, David Gathara, Nancy Abuya, Jacintah Mwachiro, Sam Ochola, Robert Ayisi, Mike English et al., "What capacity exists to provide essential inpatient care to small and sick newborns in a high mortality urban setting?-A cross-sectional study in Nairobi City County, Kenya," *PLoS One*, 2018, *13* (4), e0196585.

Njuguna, John, "Impact of Health Workers' Strike in August 2014 on Health Services in Mombasa County Referral Hospital, Kenya," *Journal of Health Care for the Poor and Underserved*, 2015, *26* (4), 1200–1206.

Okeke, Edward N, "Working Hard or Hardly Working: Health Worker Effort and Health Outcomes," Unpublished manuscript, 2019.

_ **and AV Chari**, "Health care at birth and infant mortality: Evidence from nighttime deliveries in Nigeria," *Social Science & Medicine*, 2018, *196*, 86–95.

Peabody, John W, Riti Shimkhada, Stella Quimbo, Orville Solon, Xylee Javier, and Charles McCulloch, "The impact of performance incentives on child health outcomes: results from a cluster randomized controlled trial in the Philippines," *Health Policy and Planning*, 2013, *29* (5), 615–621.

Powell-Jackson, Timothy, Sumit Mazumdar, and Anne Mills, "Financial incentives in health: New evidence from India's Janani Suraksha Yojana," *Journal of Health Economics*, 2015, *43*, 154–169.

Simiyu, DE, "Morbidity and mortality of neonates admitted in general paediatric wards at Kenyatta National Hospital," *East African medical journal*, 2003, *80* (12), 611–616.

Simmons, LaVone E, Craig E Rubens, Gary L Darmstadt, and Michael G Gravett, "Preventing preterm birth and neonatal mortality: exploring the epidemiology, causes, and interventions," in "Seminars in perinatology," Vol. 34 Elsevier 2010, pp. 408–415.

Tele, Albert, David Nyamu, Rashid Juma, and Gitonga Isaiah, "Determinants of Neonatal Mortality in a Public Urban Maternity Hospital in Kenya," *Reproductive health*, 2017, *23*, 9–13.

Wane, Waly and Gayle Martin, "Education and health services in Uganda: Data for results and accountability," 2013.

World Bank, "Kenya Service Delivery Indicators," Technical Report, World Bank Group 2013.
World Health Organization

World Health Organization, "World health statistics 2018: monitoring health for the SDGs, sustainable development goals," 2018.

Yego, Faith, Jennifer Stewart Williams, Julie Byles, Paul Nyongesa, Wilson Aruasa, and Catherine D'Este, "A retrospective analysis of maternal and neonatal mortality at a teaching and referral hospital in Kenya," *Reproductive health*, 2013, *10* (1), 13.

Zupan, Jelka and Elizabeth Aahman, "Perinatal mortality for the year 2000: estimates developed by WHO," *Geneva: World Health Organization*, 2005, pp. 129–33.

6 Figures and Tables

Table 1 – Kenya Strikes List

Start Date	Min. Days	County	Hospitals Affected	Strike Actor
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
August 4, 2014	32	Mombasa	Coast General Hospital	nurses
August 6, 2014	14	Kitui	Kitui County Referral Hospital	doctors
August 11, 2014	2	Kisumu	Jaramogi Oginga Odinga Teaching and Referral Hospital	nurses
August 19, 2014	1	Uasin Gishu	Uasin Gishu Hospital	nurses
August 21, 2014	1	Kisumu	Naivasha Sub-County Hospital	intern doctors
August 21, 2014	1	Nairobi	Kenyatta National Hospital	intern doctors
August 25, 2014	3	Homa Bay	Homa Bay Referral Hospital	health workers
September 5, 2014	21	Uasin Gishu	Uasin Gishu District Hospital	nurses
September 11, 2014	13	Nairobi	Kenyatta National Hospital	doctors

Table 2 – Child Characteristics

	All (1)	Home Births (2)	Facility Births (3)	Non-Strike Counties (4)	Strike Counties (5)
Facility birth	0.50			0.46	0.63
	(0.50)			(0.50)	(0.48)
Hospital birth (if facility birth)	0.75			0.73	0.79
	(0.43)			(0.44)	(0.41)
Doctor, nurse, or midwife present	0.51	0.03	0.99	0.47	0.63
	(0.50)	(0.17)	(0.08)	(0.50)	(0.48)
Number antenatal visits	3.77	3.10	4.33	3.62	4.18
	(2.15)	(2.10)	(2.02)	(2.11)	(2.19)
BCG vaccine	0.92	0.87	0.97	0.91	0.95
	(0.27)	(0.34)	(0.18)	(0.29)	(0.22)
DPT vaccine (3 doses)	0.77	0.71	0.83	0.76	0.78
	(0.42)	(0.46)	(0.38)	(0.43)	(0.41)
Polio vaccine (3 doses)	0.69	0.66	0.71	0.69	0.70
	(0.46)	(0.47)	(0.45)	(0.46)	(0.46)
Measles vaccine	0.71	0.66	0.76	0.70	0.74
	(0.45)	(0.47)	(0.43)	(0.46)	(0.44)
Has a health card	0.95	0.90	0.99	0.94	0.98
	(0.22)	(0.29)	(0.10)	(0.24)	(0.15)
Height for age: z-score	-1.06	-1.26	-0.86	-1.08	-1.00
	(1.42)	(1.46)	(1.36)	(1.42)	(1.43)
Weight for age: z-score	-0.91	-1.16	-0.66	-0.97	-0.73
	(1.24)	(1.20)	(1.23)	(1.23)	(1.26)
Number of observations	32361	16164	16197	24310	8051
Child died, first 7 days	0.022	0.019	0.022	0.022	0.023
	(0.147)	(0.138)	(0.146)	(0.147)	(0.149)
Child died, first month	0.028	0.027	0.026	0.028	0.029
	(0.166)	(0.162)	(0.160)	(0.165)	(0.168)
Child died, first 6 months	0.041	0.039	0.034	0.040	0.043
	(0.198)	(0.194)	(0.182)	(0.196)	(0.203)
Child died, first year	0.054	0.050	0.045	0.053	0.059
	(0.226)	(0.217)	(0.207)	(0.224)	(0.235)
Number of observations	96327	16164	16197	73264	23063

Note: Standard deviations in parentheses. Computed based on one observation per child. Mortality information is collected for all children born to surveyed women. Other variables are only asked about children born in the last 5 years.

Table 3 – Mother Characteristics

	All (1)	Facility Non-User (2)	Facility User (3)	Non-Strike Counties (4)	Strike Counties (5)
Age	31.38	33.56	28.08	31.48	31.11
	(8.05)	(8.23)	(8.23)	(8.10)	(7.91)
Number of Children	3.63	4.11	2.91	3.79	3.22
	(2.37)	(2.46)	(2.46)	(2.42)	(2.18)
Education	7.09	6.16	8.51	6.56	8.54
	(4.28)	(4.28)	(4.28)	(4.32)	(3.80)
Literate	0.67	0.58	0.79	0.62	0.79
	(0.47)	(0.49)	(0.49)	(0.49)	(0.41)
Married	0.73	0.72	0.76	0.74	0.73
	(0.44)	(0.45)	(0.45)	(0.44)	(0.44)
Urban	0.34	0.27	0.44	0.28	0.51
	(0.47)	(0.45)	(0.45)	(0.45)	(0.50)
Has electricity	0.23	0.17	0.32	0.17	0.38
	(0.42)	(0.38)	(0.38)	(0.38)	(0.48)
Number of observations	33099	19932	13167	24132	8967

Note: Standard deviations in parentheses. Computed based on one observation per mother.

Table 4 – Child Mortality

	Child died first 7 days (1)	Child died first 1 month (2)	Child died first 6 months (3)	Child died first year (4)
Panel A. Full sample				
Strike	0.015 *	0.015 ***	0.015 *	0.009 **
	(0.008)	(0.006)	(0.008)	(0.004)
Mean of dep. var.	0.022	0.028	0.013	0.014
Std. Dev. of dep. var.	0.147	0.166	0.113	0.116
Observations	96324	96018	90560	86058
Panel B. Only births 2010-2014				
Strike	0.023 *	0.029 ***	0.006	0.009
	(0.012)	(0.011)	(0.010)	(0.007)
Mean of dep. var.	0.018	0.024	0.008	0.007
Std. Dev. of dep. var.	0.134	0.152	0.088	0.086
Observations	19509	19339	17204	14996
Panel C. Only counties with local strikes				
Strike	0.007	0.009	0.013	0.014 ***
	(0.008)	(0.007)	(0.010)	(0.006)
Mean of dep. var.	0.023	0.029	0.015	0.016
Std. Dev. of dep. var.	0.149	0.168	0.122	0.125
Observations	23063	22986	21645	20504

Note: 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, represents an indicator for whether there was a strike in the county and month of birth. Panel B restricts the sample to only births in 2010-2013, while the sample to 2010-2013 and only the counties that ever had local strikes. Standard errors, clustered at the county level are in parentheses.

Table 5 – Child Health Measures

	Height for age (1)	Height for age (age>1) (2)	Weight for age (3)	Weight for age (age>1) (4)
Panel A. Full sample				
Strike	-0.073	-0.125 *	0.014	-0.059 ***
	(0.088)	(0.064)	(0.061)	(0.023)
Mean of dep. var.	-1.060	-1.230	-0.910	-1.111
Std. Dev. of dep. var.	1.422	1.393	1.241	1.136
Observations	28241	22518	28241	22518
Panel B. Only births 2010-2014				
Strike	-0.222 ***	-0.177 ***	-0.085	-0.106 ***
	(0.072)	(0.041)	(0.085)	(0.031)
Mean of dep. var.	-0.995	-1.153	-0.884	-1.092
Std. Dev. of dep. var.	1.352	1.337	1.226	1.117
Observations	17371	13725	17371	13725
Panel C. Only counties with local strikes				
Strike	-0.067	-0.072	0.079 **	0.042
	(0.085)	(0.082)	(0.037)	(0.052)
Mean of dep. var.	-0.995	-1.158	-0.735	-0.922
Std. Dev. of dep. var.	1.429	1.394	1.256	1.166
Observations	6856	5462	6856	5462

Note: 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, represents an indicator for whether there was a strike in the county and month of birth. Panel B restricts the sample to only births in 2010-2013, while Panel C restricts the sample to only the counties that ever had local strikes. Standard errors, clustered at the county level are in parentheses.

Table 6 – Delivery Outcomes

	Any facility birth (1)	Public hospital birth (2)	Public clinic birth (3)	Private facility birth (4)	Doctor or Nurse present (5)
Panel A. Full sample					
Strike	-0.019	-0.003	-0.021	0.005	-0.004
	(0.014)	(0.021)	(0.020)	(0.026)	(0.015)
Mean of dep. var.	0.501	0.262	0.123	0.116	0.509
Std. Dev. of dep. var.	0.500	0.440	0.328	0.320	0.500
Observations	32359	32359	32359	32359	32664
Panel B. Only births 2010-2014					
Strike	-0.057 ***	-0.064 **	-0.019	0.026	-0.039 **
	(0.018)	(0.028)	(0.026)	(0.027)	(0.018)
Mean of dep. var.	0.548	0.290	0.148	0.111	0.553
Std. Dev. of dep. var.	0.498	0.454	0.355	0.314	0.497
Observations	19227	19227	19227	19227	19419
Panel C. Only counties with local strikes					
Strike	-0.017	0.000	-0.008	-0.010	-0.004
	(0.027)	(0.023)	(0.019)	(0.031)	(0.026)
Mean of dep. var.	0.627	0.327	0.125	0.175	0.628
Std. Dev. of dep. var.	0.484	0.469	0.331	0.380	0.483
Observations	8051	8051	8051	8051	8131

Note: 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, represents an indicator for whether there was a strike in the county and month of birth. Panel B restricts the sample to only births in 2010-2013, while Panel C restricts the sample to only the counties that ever had local strikes. Standard errors, clustered at the county level are in parentheses.

Table 7 – Vaccines

	BCG (1)	DPT (2)	Polio (3)	Measles (4)
Panel A. Full sample				
Strike	-0.018	-0.002	-0.046 *	-0.039 ***
	(0.014)	(0.032)	(0.024)	(0.011)
Mean of dep. var.	0.918	0.766	0.688	0.710
Std. Dev. of dep. var.	0.274	0.424	0.463	0.454
Observations	31021	30998	30993	30964
Panel B. Only births 2010-2014				
Strike	-0.007	-0.032	-0.043	-0.042 ***
	(0.021)	(0.024)	(0.030)	(0.018)
Mean of dep. var.	0.940	0.809	0.732	0.720
Std. Dev. of dep. var.	0.238	0.393	0.443	0.449
Observations	18695	18671	18662	18656
Panel C. Only counties with local strikes				
Strike	-0.012	0.003	-0.054 *	-0.026 ***
	(0.014)	(0.028)	(0.030)	(0.010)
Mean of dep. var.	0.949	0.782	0.697	0.740
Std. Dev. of dep. var.	0.220	0.413	0.460	0.439
Observations	7639	7628	7627	7626

Note: 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, represents an indicator for whether there was a strike in the county and month of birth. Panel B restricts the sample to only births in 2010-2013, while Panel C restricts the sample to only the counties that ever had local strikes. Standard errors, clustered at the county level are in parentheses.

Table 8 – Child Mortality in NUHDSS

	Child died first 7 days (1)	Child died first 1 month (2)	Child died first 6 months (3)	Child died first year (4)
Strike	0.007	0.015 ***	0.024 ***	0.017
	(0.005)	(0.006)	(0.009)	(0.012)
Mean of dep. var.	0.013	0.019	0.036	0.062
Std. dev. of dep. var.	0.114	0.135	0.186	0.242
Number of observations	23361	23181	21050	18314

*Note: Each regression includes month, year, and day of week fixed effects. The variable, Strike, represents an indicator for whether there was a strike on the day of birth. Standard errors are in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.*

Table 9 – Child Health and Vaccines in NUHDSS

	Height for age (1)	Weight for age (2)	BCG (3)	Polio (4)	Measles (5)
Strike	-0.124	-0.060	-0.021	-0.103 ***	-0.092 ***
	(0.117)	(0.096)	(0.021)	(0.030)	(0.034)
Mean of dep. var.	-2.077	-0.987	0.900	0.737	0.619
Std. dev. of dep. var.	1.646	1.372	0.299	0.440	0.486
Number of observations	4790	4981	4750	4744	4464

*Note: Each regression includes month, year, and day of week fixed effects. The variable, Strike, represents an indicator for whether there was a strike on the day of birth. Standard errors are in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.*

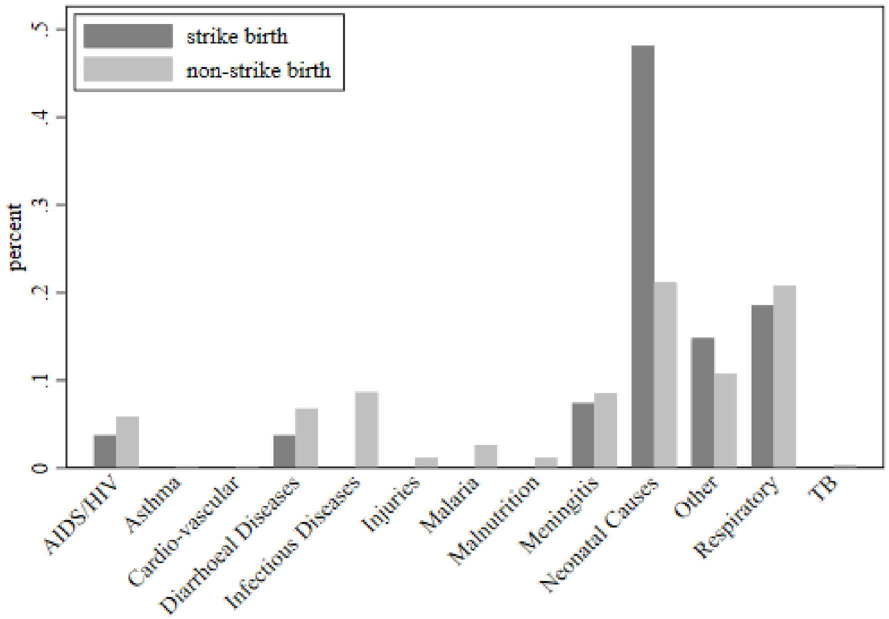

Figure 1 – NUHDSS reported causes of death

7 Online Appendix

Table A1 – Selection and birth timing

	Age at time of survey (1)	Years of schooling (2)	Can read a sentence (3)	Married (4)	Urban (5)	Has electricity (6)	County births (7)
Strike	-0.121	0.222 *	0.006	-0.004	0.008	0.013	-1.025
	(0.283)	(0.126)	(0.024)	(0.014)	(0.038)	(0.015)	(0.965)
Mean of dep. var.	33.078	6.218	0.591	0.784	0.287	0.171	8.683
Std. Dev. of dep. var.	7.422	4.256	0.492	0.412	0.453	0.377	4.511
Observations	96327	96324	96217	96327	96327	96327	11094

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. Standard errors, clustered at the county level are in parentheses.

Table A2 – Child Mortality with Survey Cluster and County-Year Fixed Effects

	Child died first 7 days (1)	Child died first 1 month (2)	Child died first 6 months (3)	Child died first year (4)
Panel A. Survey cluster fixed effects				
Strike	0.016 **	0.016 ***	0.014	0.006
	(0.007)	(0.006)	(0.009)	(0.004)
Panel B. County-year fixed effects				
Strike	0.024 ***	0.024 ***	0.012	0.004
	(0.010)	(0.008)	(0.010)	(0.005)
Panel C. Excluding Nairobi				
Strike	0.005	0.009	0.031	0.010
	(0.016)	(0.015)	(0.025)	(0.012)
Mean of dep. var.	0.022	0.028	0.013	0.014
Std. Dev. of dep. var.	0.147	0.166	0.113	0.116
Observations	96324	96018	90560	86058

Note: Each regression includes controls for age, age-squared, education, wealth quintiles, and urban-rural status. Panel A also includes survey cluster fixed effects, and Panel B includes county-year fixed effects. The variable, Strike, represents 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.