Transitory Shocks and Birth Weights: Evidence from a Blackout in Zanzibar*

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Abstract

Do transitory economic shocks affect neonatal outcomes? I show that an unexpected, month-long blackout in Tanzania caused a sharp but temporary drop in work hours and earnings for workers in electricity-dependent jobs. Using records from a maternity ward, I document a reduction in birth weights for children exposed *in utero* to the blackout, and an increase in the probability of low birth weight. The reduction is correlated with measures of maternal exposure to the blackout. The blackout also increased fertility for teenage and first time mothers, but selection into pregnancy cannot fully explain the drop in weights.

JEL Classification: O15, O14, J29, I12

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1 Introduction

For children in the developing world, income shocks suffered *in utero* have important effects on future health, education, and socioeconomic status (Van den Berg et al [2006], Maccini and Yang [2009], Banerjee et al [2010] among others).[1] One reason for these results is that a shock suffered during gestation can lead to lower birth weights, which are linked to adult health problems (Barker, 1995; Berhman and Rosenzweig, 2004). If this holds true, then it is important to understand which types of shock lead to low birth weights, and why. Past research has focused on large, permanent

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[1]Other papers show the effects of *in utero* and early childhood exposure to disease (Case and Paxson, 2009; Almond, 2006; Bleakley, 2010; Barreca, 2010)

economic shocks. We still do not know whether short-lived, transitory economic shocks typical of daily life in developing countries have a similar impact.[2]

This paper presents evidence that fully transitory shocks affect birth weights of children exposed *in utero*. The evidence comes from the study of a month-long electric power outage on the island of Zanzibar, in Tanzania. Because the blackout was caused by an unforeseen equipment malfunction, it was completely unexpected, and its timing uncorrelated with the economic conditions then present on the island.[3] I first use a unique household survey conducted shortly after the event to study the economic effects of such a disruption. I find evidence that, for some workers, the blackout created a negative income shock. In particular, I find that users of electricity at work reported a steep decrease in earnings and hours worked. I also find some evidence that for at least one category of indirect users of electricity–fishermen–the loss of income was significant. While sharp, these declines were temporary, and earnings went back to normal soon after power resumed. The power outage had modest impacts on other aspects of daily life; most notably, electrified households increased their idle time spent inside the home.

I document the effects of this shock on births by analyzing administrative birth records from the largest maternity ward of the island, which includes information on birth outcomes as well as some demographic and maternal characteristics. I also link birth records to the ward of residence of the mother, and utilize the 2007 Zanzibar Labor Force survey to construct ward-level proxies of household exposure to the effects of the blackout. While the study does not cover children born at home (or indeed, in any of the other maternity wards), the 20,000 births observed represent approximately 25% of all births in the island.

The blackout is associated with several outcomes. First, it increased short-run fertility, as measured by the number of births that occurred in the facility nine months later. Births increased for teenage women and women with no prior pregnancies, with much lower or nonexistent changes in the frequency of births for all other women. Second, it reduced the average birth weight by 107-165 grams in children conceived around the time of the blackout. Third, women whose

[2] For instance, recessions considered by Van den Berg et al. cause unemployment, which in turn changes permanent income; Maccini and Yang look at changes in the monsoon rains, which affect an entire year's worth of agricultural yields and can lead to several months or even years of higher or lower income.

[3] Many blackouts, especially in Africa, are categorized as "rolling blackouts" and originate from demand for power outstripping supply. Other economic conditions, such as increases in industrial activity or rainfall, are correlated with the timing of those power outages.

pregnancy was not associated with the fertility change had the largest birth weight reductions. In contrast, birth weight losses were not as large or statistically significant for first time mothers and teenagers, possibly because there was *positive* selection into pregnancy. This is consistent with a situation where fertility increased more for a "middle class"–those who have televisions, electric lights, and work in occupations affected by the blackout–that, other things equal, give birth to heavier babies. Fourth, birth weight reductions were generally greater in communities that plausibly had a larger share of workers affected by the income shock. Finally, in addition to average birth weight reductions, there was a relatively large increase in the proportion of children born with a low birth weight (less than 2.5 kilograms)–a medical condition associated with cognitive and health problems later in life.

In terms of magnitude, the effects found here are larger than, but consistent with, the existing literature on the impact of economic shocks during early gestation periods on birth weights. Bozzoli and Quintana-Domeque (2013) found that exposure to the Peso crisis in Argentina led to up to a 30 gram reduction in birth weights, Eccleston (2011) found a 12-14 gram reduction in New York City post 9/11, Almond and Mazumder (2011) estimate an average loss of 35-45 grams for children in the first trimester during Ramadan in Michigan, and Brown (2014) who found a 75 gram reduction in birth weights following an escalation of violence in Mexico.[4] In terms of impact, my estimates are closer to those found for women in the US with high cortisol levels or who smoke (Lien and Evans 2005, Aizer et al. 2012). Of course, the context in Zanzibar is very different from those discussed above–there are significant differences in economic conditions and maternal fitness between low and high-income countries. Moreover, the channel of transmission in Zanzibar is unclear. Households suffered a shock to earnings, time use, and living conditions; consequently, maternal stress, changes in nutrition, and even reductions in the spacing of births could have been contributing factors to the sizable weight declines observed.

This paper makes contributions to three literatures. By using an unexpected economic shock with precise start and end dates, it links birth weight changes to fully transitory shocks. It suggests that a widely observed characteristic of households in developing countries—their inability to fully insure consumption against transitory shocks—also involves a similar inability to insure

[4]See also Mansour and Rees (2010) and Camacho (2008), who found that maternal stress in the first trimester of gestation was associated with lower birth weights.

birth weights. This is a contribution to a literature that emphasizes longer-lasting household or economic shocks,[5] or research that shows the effects of fetal exposure to health (as opposed to economic) insults (Almond 2006, Case and Paxson 2009). It also establishes that these birth weight responses are large in magnitude, and involve increases in the proportion of children born with low birth weight. In addition, the setup allows me to provide some interesting results on the short run effects of electricity blackouts on fertility. Indeed, the idea that providing electricity as a form of birth control (or, conversely, of avoiding blackouts) is common in some policy circles. While to my knowledge this has never been proven correct, a byproduct of electricity –television viewing– has been linked to lower fertility levels (Chong et al, 2012, Jensen and Oster 2010). This paper suggests that power interruptions increase procreation, at least in the short run. Finally, to my knowledge this is one of the few papers that can measure, with some degree of precision, what are the effects of blackouts on labor, earnings, and leisure in developing countries where it is not clear *a priori* how large of a shock is caused by power outages. As such, it complements work on the effects on firm production and profitability (Adenikinju 2003).

The remainder of the paper is structured as follows. Section 2 provides background information on blackouts and the nature of the Zanzibar event. Section 3 uses the post-blackout survey to measure the size of the shock to the labor force. Section 4 and 5 introduces the birth outcomes data sets used in this study and the estimation strategy respectively. The impact on fertility and neonatal fitness is discussed in section 6. Finally, section 7 concludes.

2 Background information

2.1 Blackouts in Africa

Although there are no existing statistics on the phenomenon, many countries in Africa suffer from tremendous power instability. Large cities like Lagos, Nigeria are renown for constant power cuts. Other places where service has historically been considered reliable have been in the news for blackouts, including Addis Ababa, Nairobi, Dar es Salaam, and Johannesburg–all of which have

[5] For instance, Lindo (2010) finds that birth weights decline after the loss of a parent's job, indicating that neonatal health responds to *permanent* income. Dehejia and Lleras-Muney (2004) do find that birth weights are counter-cyclical, but cannot say whether recessions affect health through transitory income, permanent income, or adverse selection. Like this paper, the work by Dehejia and Lleras-Muney indicates that positive selection is an important mechanism of transmission from economic shock to birth weights.

suffered power outages lasting weeks if not months during the past few years. When they occur, outages often take the form of rolling blackouts, in which access to power is rationed but available for a few hours during the day or the week. These types of electricity outages are qualitatively different from the type considered in this paper, which involves a protracted and continuous power cut. Such cases of complete, unexpected, multi-week blackouts are, however, not uncommon. For instance, in the summer of 2008 local Tanzanian newspapers reported a 3 week-long blackout in the Mtwara region on Tanzania. In Zanzibar itself, in addition to the 2008 blackout, there was a longer power outage between December 2009 and March 2010. Rural areas in Africa are particularly prone to these types of accidents because they are often served by a single power line (as opposed to a grid of several lines connected to each other), for which any accident or theft of material can result in a prolonged blackout. It is also possible to find accounts of protracted electricity outages in areas that are at the margin of big cities suffering from rolling blackouts (BBC, 2010).

2.2 The 2008 Zanzibar Blackout

The Zanzibar blackout under consideration in this paper started on May 21, 2008 at approximately 10 p.m. and lasted until June 18, 2008. The cause was the rupture of the undersea cable that connects the Zanzibar island substation with the electricity generators on mainland Tanzania. This cable was built in 1980 to import electricity from mainland Tanzania and replace an inefficient coal-powered plant, and it is the sole source of large-scale electricity provision of Zanzibar. Why the cable broke at that time is the subject of speculation, although it happened a few minutes before halftime during an important international soccer match–the Champion's League final that pitted Chelsea against Manchester United. It has been suggested, perhaps mischievously, that the staff at the utility company were among those watching the game. Even if staff negligence was a factor, interviews with Zanzibar Electricity Corporation (ZECO) officials clearly point to underinvestment in maintenance as the ultimate culprit.

Within a few days, it was clear that the problem was serious, and that the blackout was likely to be long (BBC, 2008). On June 3—two weeks into the power cut—a Norwegian technician arrived to assess the damage, propose a solution, and indicate a possible resumption date. The technician's assessment was the cause of much confusion: the morning after, one newspaper reported an estimated resumption of power in July (The Guardian, 2008), whereas another reported the

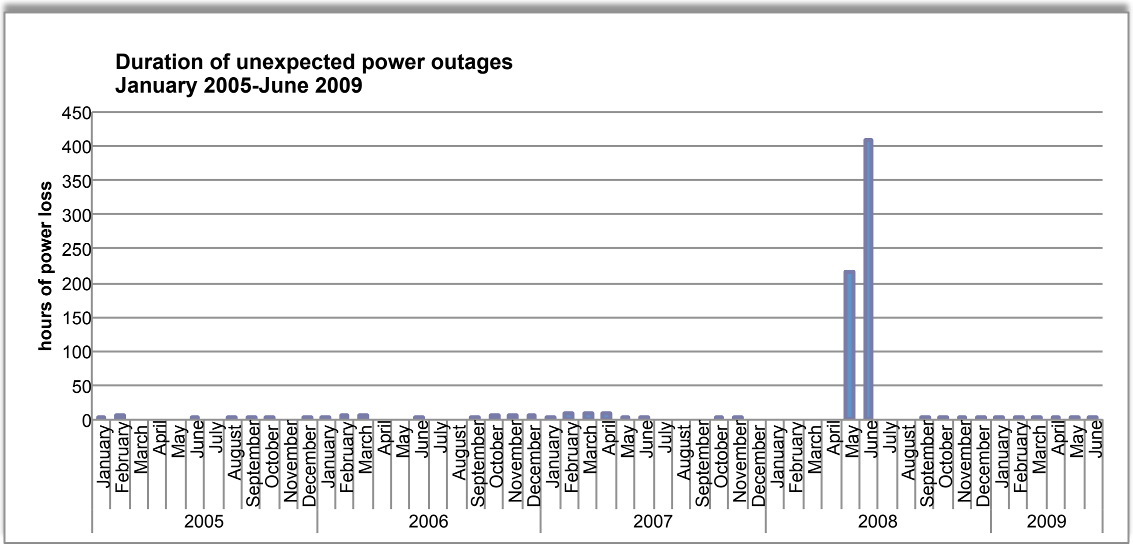

Figure 1: Source: Zanzibar Electric Company (ZECO)

date to be September (Citizen, 2008). In a radio address, the President of Zanzibar encouraged citizens to get used to candlelit dinners, which he admitted he found quite romantic. Disillusioned Zanzibaris believed that the situation would not improve before Ramadan in September.

On June 17th, the government announced the imminent restoration of power. The following day, electricity was flowing.[6] The restoration took many people by surprise since the government had been careful to play down expectations of a quick solution. The event was the longest recorded time without power in Zanzibar's recent history, although as figure 1 shows, shorter unexpected blackouts were not unusual.

3 Labor market effects

The sudden lack of electricity was responsible for a sharp, sudden, and short-lived economic shock to the labor force in Zanzibar. To quantify the size of the shock, I use recall data from a household survey collected five months after the blackout and specifically designed to study this event. The sample covers 767 randomly selected individuals across 19 rural, peri-urban, and urban communities called *shehias*. Details of the data collection and construction of variables is provided in the appendix. The data includes estimates of labor hours and earnings for all those who reported

[6] A limited number of rural areas reported a continuation of the blackout for a number of days after restoration, affecting a small proportion of the rural population.

working the month before, the month during, or five months after the blackout period, and can be used to estimate of the size and distribution of the shock.

The first two panels of Table 1 report summary statistics of this sample. Panel A shows that 20% of workers report using electricity at work, and over 80% report only one income generating activity. Workers spent an average of 37 hours a week working, with a slight dip during the blackout. Panel B breaks down the sample by primary occupation. Most respondents were self employed, either as farmers or as micro-entrepreneurs. Usage rates of electricity are very low or non-existent in farming and fishing, while almost a third of micro-entrepreneurs use electricity. For comparison, I also include employment statistics from the 2007 Zanzibar Labor Force Survey (LFS), which is representative of the island population. The employment profile of in my sample follows quite closely that found in the LFS, which is an encouraging sign that the post-blackout survey is representative of the Zanzibari labor force.

Table 2 reports the estimated labor market effects of the blackout on the sample of workers. For each worker i in two periods t, before the blackout (April 24-May 20) and during the blackout (May 21-June 18), we observe the outcome variables y_{it}, the log of total labor hours and earnings. Assuming that the blackout had a more significant effect on those who make use of electricity at work ($WorkEle$) or home ($DomesticEle$), the differential impact of the blackout is estimated in the following difference-in-difference model:

$$\begin{aligned} y_{it} = \alpha_1 POutage_t + \alpha_2 WorkEle_i + \alpha_3 DomesticEle_i + \alpha_4 POutage_t * WorkEle_i + \\ + \alpha_5 POutage_t * DomesticEle_i + X_i \beta + \mu_{it}, \end{aligned} \tag{1}$$

where $POutage$ is a dummy that identifies the blackout period and X_i is a set of time-invariant covariates. In this specification, the impact of the blackout on users of electricity at work and home is given by coefficient estimates α_4 and α_5 respectively. The coefficient α_1 is the average effect of the blackout period on the outcome variable for the remaining types of workers. The estimates of this regression are presented in the first column in table 2. Hours worked fell by an average of 25% for commercial or industrial users of electricity, but did not change for anyone else (including those with electricity at home).

Next, I estimate an alternative specification where $POutage$ is now interacted with the primary employment sector. The estimated coefficients (reported in column 2) provide a sector-by-sector view of the changes in hours worked between the two periods, relative to farming. The blackout is associated with a small but statistically significant decline in hours across most activities, excluding fishing and the public sector. Column 3 includes all coefficients from the two estimation methods. The coefficient on the interaction between electricity use and the blackout period is again strongly significant and negative, with a magnitude similar to the one observed in column 1. Controlling for electricity use, no sector shows a significant decline in hours, suggesting that the effect of the blackout on hours was generally limited to users of electricity across occupations. An exception comes from the public and NGO sector: the positive coefficient observed for this category of workers is explained by the fact that hours remained quite unchanged for both users and non-users of electricity.

Columns 4-6 reports coefficients from earnings regressions. In the first specification, earnings declines are observed for both electricity users and non-users alike, with steeper declines for users. In particular, non-users experienced a 4,000 shilling average decline in earnings,[7] corresponding to 7.8% of pre-blackout earnings for this particular group. For users, the decline is approximately 13,300 shillings, or 14.2% of pre-blackout earnings. As before, the use of electricity at home plays no role in earnings differences. Column 5 reports the sectorial results; all sectors reported significant declines in earnings relative to the pre-blackout period. Some sectorial differences are now evident, with statistically larger drops for fishermen and micro business owners relative to farmers. In column 6 I add back work and domestic electricity. Controlling for sector of employment, the coefficient on work electricity is again very negative and statistically significant. On the other hand, F-tests indicate that, once use of electricity is taken into account, earnings declines across sectors disappear and only the farming and fishing sector have statistically significant declines. For fishermen in particular the reported drop in earnings is quite sizable: 11,464 shillings, or 15% of monthly income. While typically fishermen are not direct users of electricity, they could have suffered due to the lack of refrigeration in fish markets.[8] Lack of refrigeration is a lesser factor in animal husbandry and agricultural production, where items are not as perishable; this may explain

[7] Exchange rate in June 2008 was 1,190 Tanzanian shillings to the dollar.

[8] Qualitative interviews with fishmongers suggest that fish markets were disrupted by the blackout. In normal times, unsold fish is moved to freezers until the following working day. During the blackout the freezers were out of order. As a consequence, fish continued to be traded during the day, but market prices experienced significant volatility, as they would decline steeply by the end of each working day to avoid wastage.

the smaller declines observed in those sectors.

Taken together, these results suggest that the blackout effect on earnings was diffused across occupations, but was significantly larger for fishermen and commercial and industrial users of electricity. These results are robust to alternative specifications or standard errors estimates, and the coefficients remain largely unchanged whether controls or worker fixed effects are included or excluded.[9] However, since the model is estimated over two periods only, it is important to highlight that seasonal variations cannot be controlled for. In particular, the "during blackout" coefficient and the coefficients on the interaction between $POutage$ and the sector of employment could be upwardly biased if hours or work or earnings are cyclically lower in the the month of May/June relative to the month of April/May. To the extent that seasonality is uncorrelated to the use of electricity, this should have no effect on the parameters α_4 and α_5. It is also unlikely that this element alone is able to explain the relative decline observed in the fishing sector.[10]

3.1 Other responses to the blackout

Workers had some ways of mitigating the effects of the blackout, and table 3 explores some of these strategies by regressing (1) on a variety of response variables collected in the survey. First, workers could turn to generators. In column 1, the response variable is a dummy variable for whether a worker used a generator during the blackout. 26.6% of those who use electricity at work utilized a generator; this behavior did not depend on any other variable such as pre-blackout earnings, age, or wealth.[11] Second, workers could have reallocated time across income-generating activities. If so, we would expect that the overall change in the labor supply of workers with multiple income generating activities was smaller than those with only one activity. I do not find evidence of this kind of behavior, meaning that workers do not substitute across work activities. However, they do appear to substitute labor for leisure. This is shown in column 2, which reports the results

[9] There is some heterogeneity in the estimated impact, with larger reductions in hours and earnings among the self employed and users of electric tools. I do not find any evidence that those with higher schooling levels were less affected by the blackout. See appendix table 1 for more details.

[10] A second problem with the interpretation of the estimates is that the data on the two time periods was collected at one point in time, and non-random recall bias is possible. It is unclear which direction the bias could go. Standard measurement error would lead to underestimation of the reported coefficients in table 2, while the same would be overestimated if affected workers reported worse than actual outcomes.

[11] Unfortunately, the survey did not ask how much generator time each person had access to. Access time could vary since many people shared or rented generator time, and intensity of use could be plausibly related to income. As reported in the appendix table 1, total labor hours and earning declines were not lower among generator users.

of a regression on log leisure hours spent at home for the subsample of workers who were asked about their non-work time use during the blackout. Those with electrified homes spent on average 16% more time (50 minutes) per day at home relative to those without electricity. This can be thought of as an increase in "idle" time.[12] Since many of those connected to the electric grid have televisions, which were inoperable due to the blackout, it may seem counterintuitive that leisure hours spent at home increased rather than decreased. However, this is consistent with the fact that the blackout heightened fears of theft, and households with electric appliances would have spent more time at home to protect those valuable assets.

Another response to the outage was to move to a different location while the problem persisted. Column 3 uses an indicator variable for temporarily leaving the island as the outcome variable. The coefficients of interest are mostly negative and statistically significant, indicating that people were, if anything, *less* likely to leave the island during the blackout. This is not surprising if one considers that travel is costly, and particularly so at a time of economic hardship. Alternatively, the blackout could have caused workers to permanently migrate. We do not observe permanent leavers it in the data, since only those who *eventually* returned were interviewed. We also would not observe these permanent leavers in the birth database. To the extent that emigrants left because they were more impacted by the blackout, the results presented here and in the rest of the paper underestimate the true impact of the blackout on the entire population. Since there are no obvious reasons to believe that there were sizable migratory outflows during the blackout, it is reasonable to presume that this bias is likely small or negligible.

Women's work In light of the results on birth weight which will be discussed in section 6, the impact of the blackout on women's time allocation is of particular interest. If the blackout caused large-scale reallocations of labor within affected households, market or home production for women might have increased significantly, to the point of affecting fetal development (Kramer, 1981).[13] To check for evidence of this, I run regression (1) on the total amount of time spent on market and domestic work for the subsample of women workers who responded to the time use questionnaire, and report the relevant estimates in column 4. Women working with electricity reported lower

[12] There is no additional effect on leisure time for those with work electricity, although working with electricity is associated with an increase in leisure time spent *outside* of the house. Results available from the author upon request.

[13] Miller and Urdiola (2010) in particular link higher child mortality and morbidity to higher maternal work among Colombian children.

hours, whereas other women saw no difference in the amount of work they took on. Overall, then, women spent fewer hours working during the blackout.[14]

Other effects of the blackout Because electricity use remains very limited under normal circumstances, many aspects of daily life were in fact unchanged.[15] The blackout had a limited impact on food availability, prices, health and health provision, and cooking behavior (which rarely involves no refrigeration or electric stoves). Fish and meat markets remained open; however, the lack of refrigeration limited the shelf life of fish to a single day and caused significant price volatility. The lack of indoor and outdoor lighting reportedly increased the fear of theft and insecurity.

Work after the blackout The data on workers suggests that once power was restored labor hours returned to normal within five months. When asked about their work hours in the most recent month (i.e., November 2008), 97% of the surveyed workers reported working the same amount of hours as those worked during the pre-blackout period. This proportion does not depend on whether workers used electricity at home or work. In addition, the few who reported different hours were equally likely to have increased or reduced their hours. The data is thus suggestive that hours of work returned to normal levels within five months, and is consistent with informal interviews with affected respondents that suggest a quick return to normalcy. This shock of earnings, time use, and living conditions was therefore fully transitory in nature.

4 Maternity ward data

I now turn to the main results of the paper, which uses birth records from Mnazi Mmoja Hospital, the main maternity ward on the island located in Zanzibar Town. The ward has relatively modern equipment and qualified staff, and delivers 500-900 children per month, representing 48% of all children born in health facilities (according to facilities data from the Ministry of Health). Since it is estimated that 61% of all children in Zanzibar are born at a health facility (NBS 2011), the data presented here represent approximately 25% of the total monthly births on the island. The

[14] In prior versions of this paper, I also showed that hours worked for women weakly declined when other household members worked with electricity, and that this was mostly driven by the fact that time spent doing housework fell. This is consistent with a situation in which other workers substituted domestic help for market work.

[15] An online web appendix provides a narrative of responses that are possibly of interest to the reader.

use of maternity wards records is necessary since Tanzania does not have a reliable vital statistics office. In addition, each *shehia* is supposed to keep a record of local births, but direct observation indicates that many domestic births are not registered.

Maternity records at Mnazi Mmoja are well kept and include records listing the name, home town, number of prior pregnancies, age and admission date of all expectant mothers. The book also includes some basic characteristics of the newborn, such as gender, weight, and any delivery complications. I do not observe gestational age, and assume throughout that a child born at a certain date was conceived 38 weeks prior.[16] All available delivery books from January 2007 until the end of May 2009, were photocopied and entered into a database, thus covering facility births prior to, during, and after the blackout. No record was available prior to 2007. In total, I transcribed 20,027 births from this two and a half year period, out of which 19,636 were complete, usable records. Next, I identified the village of residence of the mother, and linked them to the respective administrative ward (*shehia*). The identification of administrative areas was not always successful: some birth records were left blank, others had misspellings or used ambiguous physical markers. I used the administrative ward to link birth records with average *shehia* characteristics as described by the Labor Force Survey (LFS) of 2007. The nationally-representative survey inquired about labor habits of Zanzibaris, including sector of employment and type of employer. The surveys were conducted in 137 *shehias*, out of which 76 were successfully matched with the birth records. Thus, the matched birth records-labor force survey includes 11,973 observations.

Panel C of table 1 shows summary statistics for the full and the matched LFS sample. There are minimal differences between the two, and none are statistically significant. Mothers are, on average, 26 years old and have had two and a half pregnancies. The sex ratio is skewed in favor of boys, who represent 54% of all births. Birth weights average about 3 kg (6.8 lb.).

5 Estimation strategy

I use the birth records from Mnazi Mmoja to measure the impact of the blackout on pregnancy rates and child health at birth. The basic specification is a regression on outcome y_{itv} for child i

[16]The lack of actual gestation data is not particularly limiting. For instance, Almond and Mazumder (2010) find effects of Ramadhan exposure in samples with and without gestation age. Gestation age is also often collected with considerable measurement error in developing countries.

from *shehia* v born in day t using a set of regressors that measure the timing of *in utero* exposure to the blackout:

$$y_{itv} = \alpha_0 + X_i\beta + \sum_{j=-1}^{9} \gamma_j month_{jit} + T_t + \omega_v + \epsilon_{itv}, \tag{2}$$

where outcomes are birth weights in grams or an indicator variable for being born with low birth weight. In this regression, I assume that children who were in different stages of gestation during the blackout had different outcomes. These differences are captured by the set of exposure dummies $month_{jit}$, which indicate whether the child was exposed to the blackout during month j of gestation. Since we do not observe gestation directly, each dummy $month_{jit}$ indicates the predicted gestation age during the blackout.[17] The specification allows for exposure to take place up to one month prior to conception. This accommodates the fact that the effects of the blackout might have continued some time after the resumption of electricity. The regression includes a set of variables X_i which controls for the information available in the birth records: mothers age, age squared, and number of prior pregnancies; and child's gender and twin status. Since there is seasonal variation in birth weights, regressions also include a variety of time controls such as year of birth dummies and quarter or month of birth dummies, and *shehia* fixed effects ω_v.

A possible problem with estimated coefficients γ_j is that they could possibly include unobserved seasonal variation not controlled by T_t. To the extent that this unobserved variation is common to all *shehias*, a blackout effect net of seasonality can be obtained through a difference in difference strategy that compares affected with unaffected *shehias*. This requires being able to measure the degree of exposure to the effects of the blackout for each location, $Exposure_v$, and then estimating the difference in difference model,

$$\begin{aligned} y_{itv} = \alpha_0 + \alpha_1 Exposure_v + X_i\beta + \sum_{j=-1}^{9} \delta_j month_{jit} + \\ \sum_{j=-1}^{9} \gamma_j month_{jit} \times Exposure_v + T_t + \omega_v + \epsilon_{itv}. \end{aligned} \tag{3}$$

If the shock to electricity reduces birth weight in month j of gestation, then the average birth weight of children in gestation month j during the blackout should be lower in villages that had a

[17] More precisely, I assume that conception took place 266 days prior to birth. The dummy for month j then takes the value of 1 if the conception date falls j months before the start date of the blackout.

higher exposure to the blackout: γ_j should be negative.

How should local exposure to the blackout be measured? One approach would be to compare locations by their pre-blackout electrification rate (measured as the share of households with an electricity connection), something easily obtained from the LFS. However, residents in villages with a low or zero electrification rate could have been affected by the blackout at work. An alternative is to measure the fraction of the local population likely to have suffered an economic shock due the lack of electricity. While a direct, *shehia*-level measure of the economic shock is not available, I make use of the results from the post-blackout survey to construct a proxy measure. Based on the results of section 3, it is clear that the effects of the blackout were somewhat diffused across most sectors; nonetheless, a natural measure of $Exposure$ would be the share of the labor force composed of fishermen and other commercial users of electricity, as these were the more seriously affected categories of workers. Unfortunately, no data in Zanzibar separately identifies these categories of workers. In particular, the LFS does not separate fishermen from agricultural workers, who are much less affected by the blackout, nor collects information on electricity use in work sites.

Given these challenges, two indirect exposure measures are thus used. The first is a dummy variable for coastal, non-urban *shehias*. The estimated γ_j's identify differences in the birth weight responses between communities more reliant on the fishing sectors relative to all other communities.[18] Since non-coastal towns and cities could have also been affected by the blackout through other economic sectors, this strategy only highlights a differential impact, and is weakened by the possible presence of unobserved fishing sector-specific seasonality shocks. Second, I use a pre-blackout estimate of share of *shehia* employment in the private, non-primary sector calculated from the LFS. This category is the sums of the share of two LFS employment categories: self employed micro-business owners without hired labor and employees working in the private sector. These two categories closely match the employment categories "employee, private sector" and "owns small business" categories used in the blackout survey that were found to have suffered reductions in earnings due to their relatively high use of electricity.[19]

The strategies presented here present some challenges. Since the two adopted measures

[18] In the post-blackout survey, fishermen constituted 20% of surveyed workers in coastal towns, but only 2% in inland or urban *shehias*.

[19] To be informative, the probability that a participant in the primary sector uses electricity should be non-decreasing in the share of employment in the private sector.

of exposure are noisy, we should expect a downward bias in the estimated difference in difference coefficients. Second, one might worry that other factors might have affected the composition of maternity ward clients, thus affecting the estimates of γ_j in both (2) and (3). This is somewhat unlikely. After the event discussed in this paper, there were no more blackouts, no obvious policy changes in the way hospitals were run, and no other major upheavals, at Mnazi Mmoja or at other facilities. Third, the blackout itself might have led to changes in the composition of ward clients. This is certainly a concern for those cohorts born during and immediately after the blackout, but unlikely to be a factor for later periods. Of particular note is the possible disruption of ante-natal care services (ANC), which are widely attended by pregnant women (NBS, 2005) and provide a service which in itself could affect child health and birth weights. A Ministry of Health report on the impact of the blackout did not mention any problems in this area (Straheler-Pol and Haji, 2008). This is unsurprising, as ANC clinics are very low-tech, visits generally take place early in the morning when sunlight is plentiful, and neither the medical visit, nor the standard tests (weight, blood, anemia, malaria), nor the basic treatments require electricity. In the event that ANC was disrupted, this could create a confounding effect on birth weights of children exposed in the second or third trimester, as visits to the ANC clinic generally happen at a late stage of pregnancy. Since only 12.4% of pregnant women visiting before their fourth month (NBS, 2005), it very doubtful that an "ANC effect" would impact children exposed in their first trimester. Fourth, given that birth records from children born at home are not present in the analysis, it is important to interpret the estimated γ coefficients as the effect of the blackout on birth outcomes of children delivered in hospitals. In particular, it is conceivable that the results of the paper overestimate the average effect of the blackout on the entire population, since the blackout disproportionally affected users of electricity (who are conceivably more likely to use the maternity ward). Fifth, births may be correlated within *shehias*. In the preferred estimates using the matched LFS sample, I report errors clustered at the *shehia* level. I also used the Cameron-Gelbach-Miller multi-way clustering procedure to cluster at the shehia-month-year level. I chose to report the simple *shehia*-clustered standard errors because they are more conservative.

6 Results

6.1 Fertility responses

The first question that needs to be addressed is whether the blackout caused more births. It certainly reduced the instantaneous opportunity cost of procreation: televisions were out of order, public meeting places became deserted in the evenings, lights were shut off, and people spent more time at home. It is possible that this led to more sexual activity.[20] One way to determine whether this was the case is to show that the number of deliveries in Mnazi Mmoja increased approximately 9 months later. To show this, I run Poisson regressions of the number of births in the facility per week on the month of exposure dummies and the month and year fixed effects. Controlling for seasonality, we expect that the number of births is unchanged for all month exposures with the exception for those conceived during the blackout (i.e., born 9 months later). If the number of pre-term babies increased significantly, we should also expect a positive coefficient on those whose expected conception date was the prior month. A positive coefficient on those conceived after is expected if children conceived during the blackout were born late, or if the aftermath of the blackout also led to an increase in conceptions.

Table 4, column 1 reports the results of the Poisson regressions on the number of births per week for the sample of children born in *shehias* matched to the LFS survey. (Regressions on the full sample lead to very similar results). As conjectured, the cohort conceived during the blackout was 17.7% larger than expected. Coefficients on conceived one month before or after are, on the other hand, within the standard of error. There was also a significant increase in deliveries during the blackout, which is perhaps indicative of the fact that the blackout caused some problems to other maternity centers and forced some women to deliver at Mnazi Mmoja. On the other hand, there were fewer deliveries two months after the blackout (due to a 10-day closing of the hospital) and five-six months after the blackout, for reasons that are not clear.

Heterogeneity I next explore the compositional effects associated with the fertility increase in the remaining columns by running the regression on specific categories of women. Column 2 shows that births from teenage mothers 8 and 9 months after the blackout saw a 28% and 43.3% increase,

[20]See Burlando (2014) for more details on the fertility effect of the blackout.

suggesting that there was significant selection into pregnancy for this group and a high proportion of premature children. Among women 20-35 years old (column 3), a much lower fertility increase is observed, with most of the gains concentrated 9 to 10 months after the blackout, and almost no gain for those born sooner. Women over the age of 35 (column 4) had a statistically insignificant change in the number of births nine months after the blackout. Column 5 and 6 repeat the exercise by separating first pregnancies from births after past pregnancies. It is clear that fertility increased almost exclusively among first time mothers. Overall, these columns support the idea that conception rates increased among those women who are at a higher risk of delivering low birth weight babies–teenagers and first time mothers.

Discussion While the fertility effect was real, it is quite possible that it will not translate into a permanent population increase. For many women, the blackout provided an opportunity to anticipate a planned pregnancy. Indeed, the increase in fertility affected almost exclusively women with excess fertility. Nonetheless, to the extent that women use birth spacing as a contraceptive method (as common in many parts of sub-Saharan Africa including Tanzania), this "harvesting effect" of the blackout could eventually translate into an increase in total fertility.

Regardless of the long-run effects on population, the short -run spike in births is interesting because it would represent an unusual fertility response to an economic shock.[21] However, the blackout had no impact on long-run income, so the additional births are likely the result of the *leisure* shock. At the very least, it leads some credence to the belief that blackouts can increase fertility rates by decreasing the opportunity cost of procreation.[22]

6.2 Birth weights

Aside from the fertility shift, the blackout is associated with a reduction in birth weights. This can be visually seen in figure 2, which plots average birth weights for children born at Mnazi Mmoja starting three months prior to the blackout until the end of the panel. While there is significant

[21]Prior work suggests that fertility is counter-cyclical (Dehejia and Lleras-Muney, 2004) and that children are normal goods (Lindo, 2011). Pörtner (2008) showed that hurricanes reduce fertility in the short run, but not in the long run.

[22]The popular press and the general public are particularly fascinated by the idea. An interesting example came from the Planning Minister of Uganda who affirmed that "power blackouts were fueling a baby boom" in his country (BBC, 2009). Using the same Zanzibar data, Burlando (2014) also shows that the fertility increase was similar across electrification levels in Zanzibar. Jensen and Oster (2010) and Chong et al. (2012) find other evidence that changes in leisure–brought by television programs– reduce fertility rates.

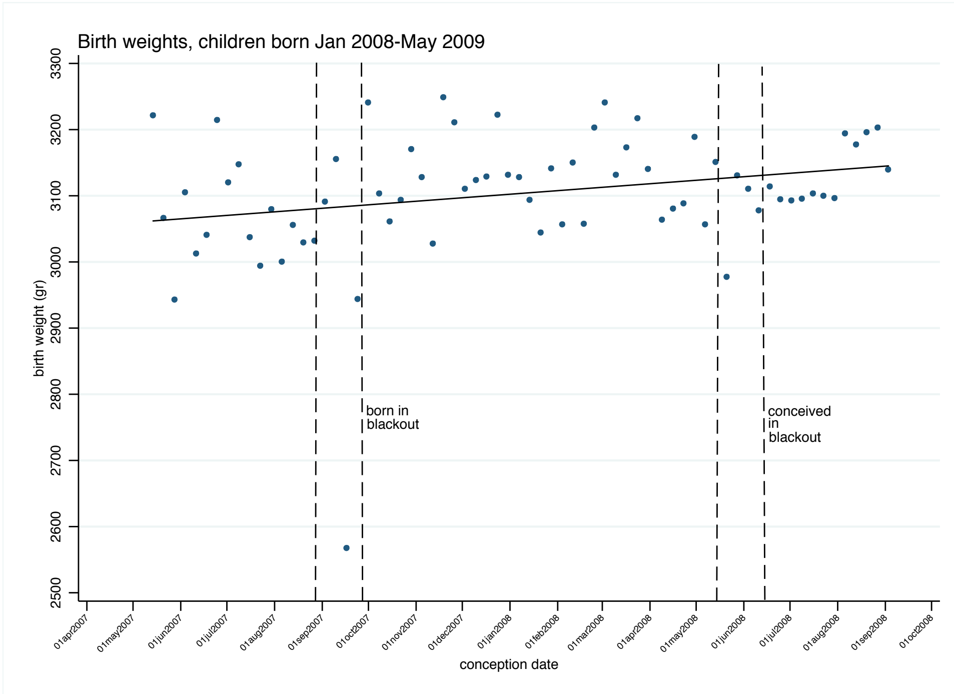

Figure 2: Average birth weights by week of conception

week-on-week variation, birth weights are increasing over time.[23] There are two outlier weeks for children born during the blackout, which again could be explained by the different mix of women who attended Mnazi Mmoja during this period. Less obvious, but perhaps more striking, is the period of below trend birth weight for children conceived around the period of the blackout. This period covers children born 9-10 months later, but also, with a couple of exceptions, births from the period immediately preceding it.

Table 5 provides parametric confirmation of this pattern. The first three columns provide the estimates from several specifications of regression (2), starting from the full sample of births and controlling for time fixed effects only (column 1), full controls and village fixed effects (column 2), the reduced sample matched with the LFS data (column 3). Across most specifications, being born approximately 8 or 10 months after the blackout is strongly associated with lower average birth weights, while the coefficient on "conceived during" is also negative, but statistically significant only in the first specification. This finding that birth weights are impacted when exposure happens

[23]The upward trend in birth weights exists for children born in 2007 too. The reason for the secular gain in birth weights is unclear: the gain could be a real improvement in child development in this period, but I cannot exclude some selection bias. For instance, it is possible (although somewhat unlikely) that the hospital admits fewer out-of-town women due to the increase in the quality of services in rural health facilities.

early in the pregnancy fits a pattern established elsewhere (Bozzoli and Quintana-Domeque 2013, Camacho 2008, Mansour and Rees 2010, Brown 2014). On the other hand, the negative and significant coefficients on children born during or the month after the blackout are perhaps more surprising. Aside from capturing the impact of the outage on post-term births among women who were in their first month of pregnancy during the blackout, they could also be explained by a somewhat slower recovery from the blackout following the return of electricity. Many other exposure coefficients are negative in the second and third specification, with some (such as month 7 and month 5 after the blackout) being quite large but statistically not significant. This is suggestive that exposure to the blackout might have had a negative impact across gestational ages, and it is consistent with evidence that fetal development in the third trimester is responsive to nutrition shocks (Stein 1975, Almond, Hoynes and Shanzenback 2011, Bozzoli and Quintana-Domeque 2013). As noted, however, this cohort was born during a moment of clear upheaval in other health care units, and there might have been significant compositional changes around the time of the blackout due to the closure and reduced functioning at other maternity wards. The conservative conclusion is that the blackout had clear impacts on birth weights of children exposed early in the first trimester of gestation, and unclear but possibly negative effects on those exposed in later stages of gestation.

Heterogeneity of impacts The remaining columns of table 5 run regression (2) on several subsamples. In column 4, I consider women above the age of 20. Children born 8 to 10 months after the blackout had significantly lower birth weights, with estimates ranging from 75 to 115 grams for different cohorts. Also, those conceived two months prior had significantly lower birth weights. While those conceived during or after the blackout might have been the result of selection (see table 4), this is not a factor among those conceived one to two months before. Focusing on these two "selection free" cohorts, the estimated impact of the blackout net of selection is approximately 91-95 grams. In column 5, I limit the sample to women who had a previous pregnancy. A similar pattern emerges: lower birth weights for children conceived two months before to one month after the blackout, with statistical significance for three out of the four coefficients. The reduction in birth weights is higher for this group than for the average, in the range of 60-160 grams per child.[24]

[24] As percentage of birth weight, the difference is not as large because children born after one pregnancy are heavier. For instance, a 107 gram reduction in birth weight across all births (from column 4 of table 5) is 3.5% of the average birth weight (3.08 kilograms), whereas 164 gram reduction for children born after another pregnancy is 5.2% of the

Again, this is a "selection free" birth weight effect, given that selection is not observed among women with prior pregnancies.

I next consider birth weights from cohorts of women whose fertility was significantly affected by the blackout: teenage mothers (column 6) and first time mothers (column 7). Among teenage mothers, none of the coefficients of interest were statistically significant. Among the first time mothers sample, only children born 10 months later had statistically significant and large drops. The inconclusiveness of these results points to muddled compositional effects of the blackout: women at higher risk of lower birth weight children increased their pregnancy rate, but the nature of the shock might have caused this selection to occur among women with higher socioeconomic background, whose babies are less likely to have lower birth weights.

Robustness checks Table 6 provides some robustness tests and additional results, based on the LFS sample. For clarity, I report only the coefficients on those conceived within one month from the blackout; across specifications, the other coefficients are rarely significant. In column 1, I include a "Conceived 10 months before" for children born before the start of the blackout, and a "conceived 2 months after" for children born 11 months later. Neither coefficient is large or significant, and the other coefficients remain within their confidence interval. This reassures us that regression (2) is correctly specified. In column 2, I add quadratic time trends (and keep the month and year of birth fixed effects); in column 3, I exclude twins. These do not change results significantly. In column 4, I add the log price of the main staple rice during the first, second, and third trimester of pregnancy. While prices are correlated with birth weights, they do not change the estimated coefficients. Columns 5 and 6 runs regressions separately for boys and girls. Lower birth weights are found for both groups, although the estimated coefficients are somewhat worse for boys. Negative environmental conditions *in utero* may result in skewed sex ratios (Almond and Mazumder, 2011). Column 7 shows that that the observed sex ratio, measured as the fraction of total births coming from girls, was unchanged. Finally, a concern with the estimation strategy is that the results are driven by residual seasonality that is not captured by the fixed effects. Column 8 runs a regression on a placebo regression that took place one year before the one described here (May 18 to June 21, 2007). The coefficients in the placebo regression are not statistically different

average birth weight for that group (3.15 kilograms).

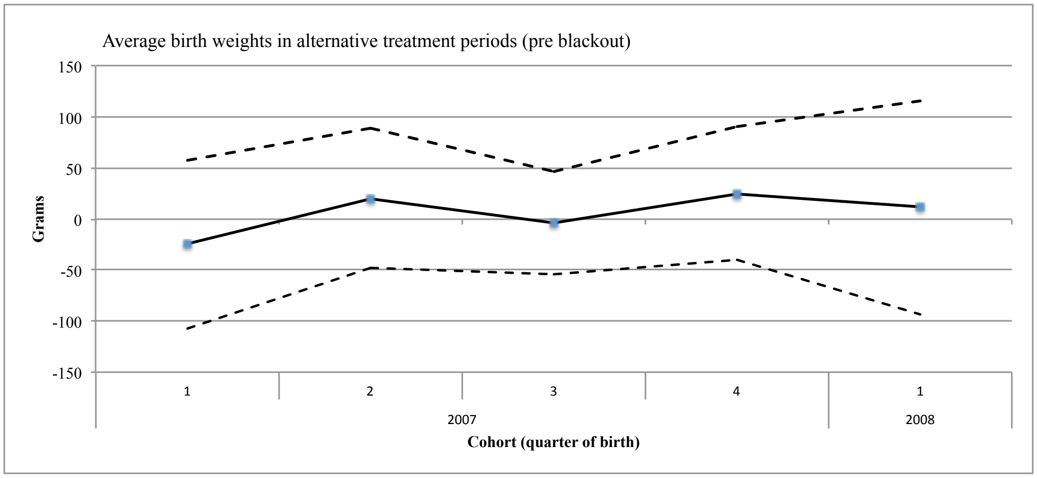

Figure 3: Plot of estimated coefficients on pre-blackout cohort dummies

from zero, and have the opposite sign. As a further check, I regress birth weights on a dummy that identifies counterfactual "affected" cohorts of births preceding the onset of the blackout, and report the plot of estimated coefficients in figure 3.[25] If residual seasonality is a concern, we should expect some unpredicted variation in birth weights. However, none of the counterfactual cohorts have significantly lower (or higher) birth weights.

Low birth weights Average declines in birth weights are not notable by themselves (Almond and Currie, 2011). What really matters is the distribution of those declines, and the incidence of low birth weight (Almond, Chay, and Lee, 2005). In that respect, the estimated birth weight losses are very notable here because they disproportionately affected the bottom of the birth weight distribution. Figure 4 shows coefficient estimates of quantile regressions at the 8, 16, 33, 50, 66 and 83 percentiles of weight for the three cohorts conceived around the blackout. Lower birth weights are registered throughout the weight distribution with the largest drop registered at the 8th percentile, where birth weights average around 2 kg.

To show that the blackout was associated with a higher probability of low birth weight, table 7 estimates a logit model of a child having low birth weight across the same specifications as table 5. In general, the estimates mirror the birth weight results from table 5, albeit with generally

[25]More precisely, the plotted coefficients are on dummies identifying cohorts of children born in a twelve-week period starting in January 2007. Thus, the first coefficient relates to the birth weight of children born between 1 and 12 weeks from the start of 2007, the second identifies those born between 13 and 24 weeks, and so on. The model includes the set of X_i controls and fixed effects as in equation(2), and includes births from 2007 and 2008 only. Results are robust to birth cohorts constructed using alternative start and end dates.

lower significance levels. That is, low birth weights are more prevalent among the same cohorts of children and categories of women who reported lower birth weights. The magnitude of effects is large: a 5% increase in probability for children conceived the month before (from column 4) means an increase of 44% over the mean incidence level.

6.3 Exposure to the blackout and birth weights

Results so far indicate a significant decline in birth weights in children born seven to ten months after the blackout. Since unobserved seasonal factors may have contributed to these results, I show in table 8 that the declines in birth weights were larger in those communities more severely affected by the blackout by presenting results from regression (3). In column 1, I report estimates of the difference in difference model where being a coastal community dummy is interacted with the month of exposure. For simplicity, I report only the estimated coefficients from those born six to ten months after the blackout. It can be seen that there is no differential impact of the blackout for those born eight to ten months later: coastal communities saw as large of a decline as non-coastal communities among those cohorts. However, the difference in difference specification does pick up a significant difference among those born six and seven months after the blackout. Taken together, it is evident that birth weights in coastal towns saw a large and sustained (relative) decline in birth weights among the cohort born six to ten months later.

Column 2 interacts the month of exposure with the share of the population employed in the private sector. The coefficient on the month indicates the predicted birth weight change in areas with no private sector workers, while the coefficient on the interaction indicates the predicted weight change when the share increases by 100%. All coefficients are negative, as we would expect, although they are also statistically insignificant. Given that the private sector proxy measures exposure to the blackout with considerable error, this is not too surprising. One way to reduce this measurement error is to exclude coastal villages, where the presence of fishermen (who are excluded from the private sector category) could bias the results. Indeed, when focusing on inland and urban *shehias* only (column 3), the relationship between the exposure proxy and birth weights improves in magnitude. In particular, there is a statistically significant negative relationship between birth weights and private sector share among those born 10 months after the blackout. These results provide some (noisy) evidence that the fitness of children born in harder-hit communities was indeed

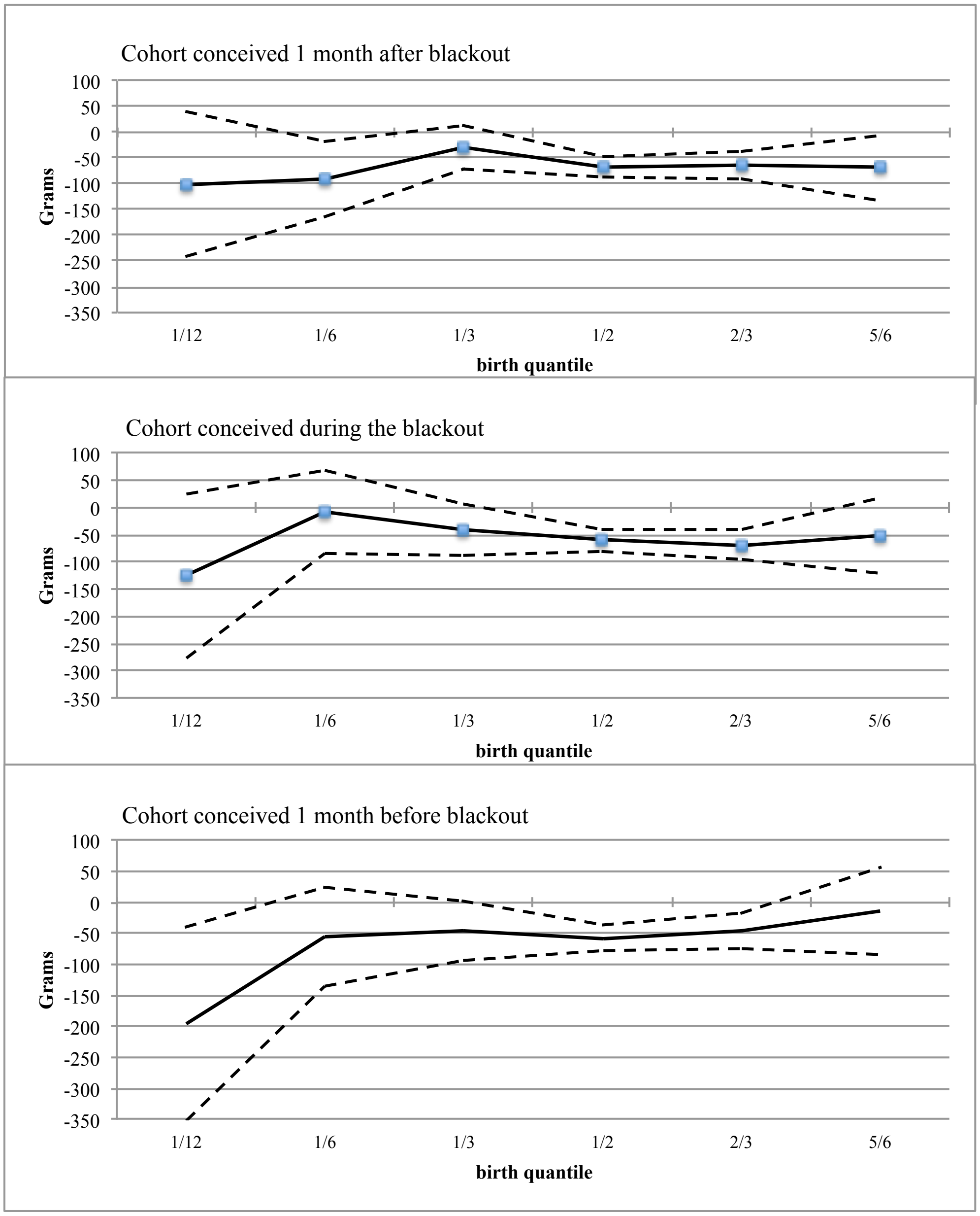

Figure 4: Estimated coefficients from quantile regressions on conceived around the blackout

compromised.

7 Conclusion

I use a month-long blackout that unexpectedly hit the Indian Ocean island of Zanzibar, Tanzania, in May 2008 to measure the effects of electricity on earnings, short-run fertility, and birth weights. Using a household survey collected during field work, I find that the blackout caused significant income losses among those households who use electricity at work and fishermen, but had little effect on other households' earnings. Moreover, the effect of the shock was short-lived, with labor hours and earnings returning to normal within five months. I also use records from a government hospital to show that the number of children born 9 months later increased, and that those children who were conceived one month before to one month after the blackout had lower birth weights on average than expected. Moreover, there was a marked increase in probability of low birth weight. The effects suggest that, at least in developing countries, pregnant women are unable to insure their pregnancy from short transitory shocks.

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8 Appendix: Post-blackout survey

The post-blackout survey sample consists of 366 randomly selected households in 19 local administration areas (called *shehias*). 12 survey locations are rural or semi-rural villages from the North, East, and South of the island, and have electricity coverage varying from 0 to 40% of the households. The remaining seven areas are urban and peri-urban neighborhoods of the main town, where between 70% and 100% of households are connected to the grid. The data was collected over a one month period, beginning in November 2008. For each household, enumerators identified all adults (aged 15 and over) who did any work in the prior month, or in the period April to June (immediately before to immediately after the blackout). It therefore captures labor market entry and exit during the selected pre to post-blackout period. No worker was found transitioning from no work to work or viceversa. Workers were interviewed separately.

Hours of work and earnings To capture the range of activities carried out, we collected descriptions of each type of income-generating activity separately, and a personal assessment of the number of weekly hours spent doing each activity within each time period. Workers were asked about their hours of work during three periods: the month before, the month of, and five months after the blackout. For each economic activity, workers were also asked the total monthly profits or wages from the activity for the period before the blackout. Due to the difficulty of obtaining truthful reports of earnings, workers were given the choice over 6 earnings bins (less than 20,000 shillings, 20-50,000 shillings, and so on). This strategy was found to be effective during piloting. For each activity, respondents were also asked to provide an estimate of the change in earnings relative to the month before. The earnings variable used in the regressions of the paper are thus constructed in the following way. For each worker, the pre-blackout earning estimate was obtained by summing over the mid-point of earnings in each bin. For the last bin (over 200,000 shillings) the value of 250,000 was chosen. This affects only 2% of the sample, and the estimates are robust to changes to this value. The blackout period earnings estimate was constructed by adding the change in earnings to those calculated for the pre-blackout period.

Time use In addition, household heads and spouses were also asked a number of additional questions intended to quantify the amount of time they devoted daily to certain activities, including doing domestic chores, spending rest time at home, sleeping, or spending time outside in the community. Other variables collected include family structure, asset ownership, income levels, education, and religious practices. Crucially, the data includes a dummy for electricity use at home and one for electricity use at work.

Occupations Each economic activity was categorized into one of eight occupations or sectors. Sectors of work include: farming and livestock, fishing, store clerks, factory work, private sector employee, NGO and government sector, self employed with employees, self employed in micro enterprise. From this, I created six sector dummies (store clerks were combined with micro-entrepreneurs and factory workers with self employed with employees). Summary statistics are shown in the second column of table 1B. As a robustness test, I also created an alternative set of employment dummies, equal to one if the respondent identified being employed in that sector at all; results are essentially unchanged.

The six employment dummies are similar, but not identical, to the employment categories found in the LFS survey. In particular, LFS module 2 question 5 asks "what is the economic activity in which person spent most of the time". The answers are central government, local government, parastatal organization, political party, cooperative, NGO, international organization, religious organization, private sector, self employed in business with employees, self employed in business without employees, working in the farm and unpaid work in family business. In order to compare it to the occupations in the post-blackout survey, I combined the first 8 into one "government/NGO sector" category. Summary statistics, excluding the "unpaid work" category, are shown in the third column of table 1B. The variable "privatesector", which is used in the difference in difference specification, is built as the percentage of the shehia employed in the private sector or self employed in business without employees.

Appendix table A1: Change in labor hours and earnings: heterogeneous effects of the blackout

Interacted dummy variable:	(1) self employed	(2) female	(3) domestic elect.	(4) television	(5) generator	(6) electric tools	(7) wealth
Panel A: Difference in log labor hours during relative to before blackout							
work electricity	-0.150*	-0.144*	-0.254	-0.207	-0.309***	-0.081**	-0.232***
	(0.080)	(0.074)	(0.180)	(0.129)	(0.106)	(0.039)	(0.089)
characteristic	-0.016	-0.010	0.016	-0.060	0.189	-0.353**	0.000
dummy	(0.023)	(0.021)	(0.060)	(0.066)	(0.131)	(0.142)	(0.004)
characteristic	-0.202	-0.265	0.003	-0.059			-0.010
dummy× work ele	(0.142)	(0.173)	(0.196)	(0.157)			(0.025)
R-squared	0.069	0.071	0.058	0.060	0.065	0.089	0.058
F test dummy	1.168	1.487	0.0447	0.701			0.167
Prob>F	0.312	0.227	0.956	0.497			0.846
Panel B: Difference in earnings, during relative to before blackout							
work electricity	-644	-8,040**	-12,185*	-9,659*	-9,281***	-1,925	-5,977
	(4,460)	(4,0569)	(7,346)	(5,524)	(3,233)	(2,999)	(4,769)
characteristic	-383	2,350*	9,330	5,637	6,449	-11,163**	-716
dummy	(3,639)	(1,3673)	(5,779)	(6,258)	(5,807)	(5,285)	-510
characteristic	-13,073**	1,865	5,012	2,987			-705
dummy× work ele	(5,901)	(5,543)	(8,268)	(7,023)			(2,418)
R-squared	0.060	0.044	0.059	0.049	0.047	0.055	0.044
F test dummy	2.96	1.717	2.414	1.191			1.05
Prob>F	0.053	0.181	0.0910	0.305			0.351
Observations	782	782	782	782	782	781	782

Title of each column indicates the characteristic dummy included in the dependent variable set. Controls include age and age squared, education, and size of household. Robust standard errors in parenthesis, clustered at the household level. F statistics report the probability of joint significance for dummy and dummy*work electricity coefficients.
Self employed is defined as person who works in farming, fishing, or has own business/microbusiness. Those who report using electricity at work but without using electric tools use electricity for lighting purposes only.
*** p<0.01, ** p<0.05, * p<0.1

Table 1: Summary statistics

Panel A: Post-blackout worker survey

	Mean	St. Dev.
proportion using electricity	0.19	0.39
number of jobs	1.21	0.43
earnings (Tanzanian Shillings)	61,518	58,942
eworkers in hhld	2.43	1.19
education (years)	7.57	4.47
age	38.23	13.75
size of hhld	6.14	2.56
weekly hours worked:		
before blackout	36.69	20.30
month of blackout	35.37	19.30
five months after blackout	36.81	19.80
number of workers	790	
number of working age adults	1,164	
number of households	366	

Panel B: Sector of employment, post-blackout survey vs. LFS

Main employment sector	Uses electricity	Share employment	
	Survey	Survey	LFS
Farming	0.010	0.404	0.463
Fishing	0.008	0.074	
Government/NGO sector	0.452	0.120	0.128
Employed, private company	0.280	0.138	0.071
Self employed, with employees	0.171	0.045	0.022
Self employed, without employees	0.318	0.219	0.315

LFS means are population weighted. Fishing is included in farming in the LFS.

Panel C: Summary statistics of birth records from Mnazi Mmoja Hospital

	Matched LFS sample		Full sample	
	Mean	St. Dev	Mean	St. Dev.
birth weight (grams)	3080.7	687.7	3084.2	675.8
proportion LBW	0.114	0.318	0.112	0.315
age of mother	26.640	6.567	26.57	6.62
number of pregnancies	2.836	2.346	2.841	2.352
proportion first pregnancy	0.442	0.497	0.445	0.497
proportion female baby	0.476	0.499	0.477	0.499
proportion twin	0.036	0.186	0.039	0.193
number of weekly births	114.16	23.65	166.89	42.34
number of monthly births	470.46	112.54	715.25	174.7
proportion working in electrified sectors	0.109	0.099		
proportion with domestic electricity	0.287	0.337		
number of weeks in sample	107		107	
number of births in sample	11,973		19,636	

Table 2: Effect of blackout on labor hours and earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variables:	Log (weekly work hours)			Monthly earnings (shillings)		
during blackout	-0.022	-0.043**	-0.032	-3,910***	-3,029***	-2,605***
	(0.015)	(0.020)	(0.020)	(1,298)	(957)	(938)
works with electricity	0.199***		0.213***	1,473		1,901
	(0.054)		(0.054)	(5,293)		(5,307)
during blackout ×:	-0.254***		-0.282***	-9,407***		-10,263***
works with electricity	(0.078)		(0.080)	(3,097)		(3,238)
electricity at home	0.212**		0.225***	-11,752		-11,306
	(0.084)		(0.084)	(7,388)		(7,460)
during blackout ×:	0.004		-0.022	46		-847
electricity at home	(0.030)		(0.035)	(1,992)		(2,453)
during blackout ×:						
fishing		0.017	0.007		-8,435*	-8,781**
		(0.027)	(0.027)		(4,419)	(4,392)
employee, private sector		-0.056	0.028		-5,737	-2,657
		(0.058)	(0.055)		(3,589)	(4,096)
public and NGO sector		0.031	0.170***		-917	4,189
		(0.021)	(0.048)		(1,734)	(3,200)
owns business		-0.054	0.004		-4,428	-2,291
		(0.071)	(0.069)		(3,662)	(3,789)
owns small business (retail)		-0.089	0.002		-4,170*	-817
		(0.059)	(0.042)		(2,344)	(2,343)
Controls	YES	YES	YES	YES	YES	YES
Sector controls	YES	YES	YES	YES	YES	YES
Observations	1,564	1,564	1,564	1,564	1,564	1,564
R-squared	0.186	0.224	0.235	0.318	0.360	0.365

Hours and earnings summed over all occupations of all individuals who reported working during any period of time. Controls include main employment sector worked, worker gender, quadratic age, education, size of household, and index of asset holdings. Farming is the excluded sector.
Errors clustered at the household level in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: Other responses to the blackout

Dependent variable:	(1) Used generator during blackout	(2) Log (daily hours of domestic leasure)	(3) Temporarily left island	(4) Work + domestic chores (women)
during blackout		0.017	-0.003	0.002
		(0.018)	(0.004)	(0.008)
during blackout ×	0.268***	-0.053	-0.049	-0.176***
works with electricity	(0.042)	(0.080)	(0.032)	(0.063)
during blackout ×	-0.009	0.162***	-0.005	-0.007
electricity at home	(0.043)	(0.054)	(0.017)	(0.033)
Controls	YES	YES	YES	YES
Sector controls	YES	YES	YES	YES
Observations	767	1,141	1,146	522
R-squared	0.312	0.051	0.031	0.189
Mean dependent variable	0.07	4.1 hours	0.031	32.5 hours

Regression 1 estimated during blackout period only. Non-work and domestic chores hours reported for the subsample of workers who were administered the survey on leisure activities. Controls include sector worked, worker gender, quadratic age, education, size of household, and an index of asset holdings. Errors clustered at the household level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 4 : Exposure to blackout on the number of births per week

Dep var: weekly births from group	(1) Total births	(2) Mother <20	(3) Mother 20-34	(4) Mother 35+	(5) First pregnancy	(6) Past pregnancies
Sample	LFS	LFS	LFS	LFS	LFS	LFS
Date of predicted conception:						
1 month after	0.100	0.160	0.135*	-0.183	0.227*	-0.015
(June 19-July 18, 2008)	(0.091)	(0.222)	(0.080)	(0.184)	(0.126)	(0.094)
during blackout	0.177**	0.433***	0.111	0.294	0.265***	0.103
(May 21-June 18, 2008)	(0.089)	(0.149)	(0.096)	(0.180)	(0.093)	(0.113)
1 month before	0.068	0.281*	0.054	-0.044	0.188	-0.026
(April 21-May 20, 2008)	(0.114)	(0.162)	(0.123)	(0.170)	(0.120)	(0.140)
2 months before	0.017	0.275	0.005	-0.108	0.088	-0.035
(March 21-April 20, 2008)	(0.121)	(0.191)	(0.128)	(0.205)	(0.151)	(0.138)
3 months before	-0.201*	-0.035	-0.189**	-0.348	-0.256	-0.159
(Feb 21-March 20, 2008)	(0.113)	(0.137)	(0.096)	(0.249)	(0.158)	(0.108)
4 months before	-0.190*	-0.393**	-0.137	-0.288	-0.323**	-0.077
(Jan 21-Feb 20, 2008)	(0.108)	(0.179)	(0.089)	(0.243)	(0.127)	(0.114)
5 months before	-0.020	-0.199	-0.027	0.122	-0.195**	0.109
(Dec 21-Jan 20, 2008)	(0.061)	(0.197)	(0.059)	(0.178)	(0.088)	(0.096)
6 months before	0.064	0.387**	-0.015	0.163	0.016	0.098
(Nov 21-Dec 20, 2007)	(0.093)	(0.158)	(0.091)	(0.203)	(0.107)	(0.107)
7 months before	-0.366**	-0.393*	-0.379**	-0.299	-0.508***	-0.265*
(Oct 21-Nov 20, 2007)	(0.155)	(0.207)	(0.153)	(0.207)	(0.177)	(0.159)
8 months before	0.075	0.065	0.078	0.058	-0.005	0.134
(Sept 21-Oct 20, 2007)	(0.081)	(0.144)	(0.074)	(0.148)	(0.081)	(0.102)
9 months before/during	0.118*	0.103	0.115*	0.151	0.128*	0.108
blackout	(0.068)	(0.145)	(0.063)	(0.130)	(0.066)	(0.087)
(Aug 21-Sept 20, 2007)						
Number of weekly births	110.7	13.1	80.4	17.2	49.3	61.4
Observations	119	119	119	119	119	119

Column titles explain sample restrictions. All regressions include month and year of birth fixed effects. Errors are clustered at the shehia level.
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Exposure to blackout on child birth weight, by month of predicted conception

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep var: weight (gr)	All births	All births	All births		Past		First
Sample restricted to:	Full sample	Full sample	LFS sample	Mother>20	pregnancies	Mother<20	pregnancy
Predicted conception:							
1 month after	-77.4**	-79.6***	-106.5***	-114.3***	-106.8**	-56.5	-98.5**
	(30.040)	(26.611)	(29.686)	(31.966)	(45.050)	(77.506)	(44.632)
during blackout	-66.7**	-54.4	-51.2	-75.9*	-59.2	114.0	-29.5
	(32.777)	(36.139)	(41.190)	(43.052)	(62.461)	(78.570)	(57.979)
1 month before	-50.5	-72.3*	-107.2**	-95.5*	-164.3***	-106.7	-42.7
	(38.121)	(38.701)	(43.266)	(48.458)	(52.576)	(96.555)	(86.117)
2 months before	-24.8	-22.2	-70.1	-91.1*	-145.9**	100.8	21.6
	(43.547)	(40.559)	(45.004)	(47.779)	(63.900)	(128.414)	(67.449)
3 months before	70.8*	22.0	-2.2	-25.9	3.7	175.8	-18.9
	(41.014)	(46.255)	(53.103)	(56.663)	(79.334)	(137.805)	(80.770)
4 months before	-13.5	-35.3	-58.7	-60.2	-56.7	-77.5	-45.4
	(34.436)	(32.697)	(42.104)	(44.170)	(60.810)	(97.085)	(62.116)
5 months before	-1.5	-22.2	-38.5	-29.4	-28.4	-147.5	-66.3
	(34.474)	(35.698)	(44.674)	(51.141)	(60.096)	(113.030)	(63.562)
6 months before	33.3	14.3	15.7	9.4	15.3	85.6	15.1
	(36.427)	(32.977)	(38.968)	(42.418)	(59.451)	(125.361)	(72.367)
7 months before	8.3	-6.3	-14.3	-21.3	3.5	55.6	-52.5
	(38.979)	(40.589)	(45.787)	(52.512)	(64.396)	(87.662)	(67.000)
8 months before	-31.1	-20.4	-72.7*	-81.7*	-90.8	50.3	-46.0
	(37.751)	(35.485)	(40.848)	(43.074)	(66.409)	(103.169)	(59.356)
9 months before/	-27.5	-53.1*	-54.7*	-18.9	-28.3	-302.1***	-87.8*
during blackout	(32.348)	(29.218)	(30.964)	(32.843)	(53.374)	(89.794)	(49.444)
Birth controls	NO	YES	YES	YES	YES	YES	YES
Shehia f.e.	NO	YES	YES	YES	YES	YES	YES
Observations	18,238	16,959	12,004	10,576	6,690	1,428	5,314
R-squared	0.01	0.07	0.07	0.07	0.07	0.08	0.05

All regressions include month and year fixed effects. Birth controls include age and age squared of mother, sex of child, gravida, prima gravida dummy and twin dummy. Robust standard errors in column 1, and clustered at the *shehia* level in column 2-7.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: Robustness tests

	(1) Additional exposure cohorts	(2) Adding quadratic time trends	(3) Singletons only	(4) Adding world rice price	(5) Boys only	(6) Girls only	(7) Outcome is gender=girl (Logit)	(8) Placebo months
Date of predicted conception:								
2 months after	26.6							
(July 19-August 18, 2008)	(45.650)							
1 month after	-87.9*	-108.9***	-92.7***	-103.3***	-113.4**	-99.7**	0.008	54.9
	(44.983)	(29.822)	(30.494)	(32.884)	(54.840)	(49.129)	(0.033)	(33.235)
during blackout	-32.7	-57.2	-27.0	-66.3	-42.7	-57.2	-0.046	-23.1
	(43.891)	(40.711)	(38.950)	(47.631)	(67.846)	(52.066)	(0.032)	(46.137)
1 month before	-90.7*	-113.0**	-97.7**	-127.1**	-140.7*	-83.1	-0.020	83.8
	(51.773)	(43.051)	(41.169)	(62.128)	(75.436)	(58.850)	(0.029)	(51.067)
10 months before	-8.5							
(July 21-August 20, 2007)	(39.163)							
Birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Shehia f.e.	YES	YES	YES	YES	YES	YES	YES	YES
Observations	12,004	11,973	11,576	12,004	6,285	5,719	12,004	12,004

Specification used is from column 3, table 5. Column title reports specification. Dependent variable is birth weight; for column 6, it is whether child is a girl. World rice price is log average price of rice during first trimester, second trimester, and third trimester of gestation. Marginal effects reported in column 6. Placebo months consider month of exposure to a "placebo blackout" that took place 365 days earlier. Errors clustered at shehia level in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p< 0.1$

Table 7: Exposure to blackout on the probability of low birth weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep var: LBW	All births	All births	All births		Past		First
Sample restricted to:	Full sample	Full sample	LFS sample	Mother>20	pregnancies	Mother<20	pregnancy
Predicted conception							
1 month after	0.005	0.014	0.031	0.039**	0.048*	-0.043	0.016
	(0.016)	(0.016)	(0.019)	(0.019)	(0.024)	(0.071)	(0.031)
during blackout	0.021	0.023	0.026	0.032	0.024	-0.008	0.033
	(0.016)	(0.019)	(0.024)	(0.025)	(0.028)	(0.074)	(0.034)
1 month before	0.031*	0.049***	0.052**	0.055***	0.071***	0.022	0.037
	(0.017)	(0.017)	(0.021)	(0.020)	(0.023)	(0.060)	(0.037)
2 months before	0.027	0.022	0.027	0.045*	0.065**	-0.165**	-0.022
	(0.021)	(0.022)	(0.023)	(0.023)	(0.030)	(0.080)	(0.040)
3 months before	-0.003	0.022	0.026	0.041	0.041	-0.140**	0.015
	(0.020)	(0.020)	(0.023)	(0.025)	(0.037)	(0.062)	(0.039)
4 months before	-0.001	0.007	0.030	0.030	0.041	0.044	0.017
	(0.017)	(0.018)	(0.021)	(0.021)	(0.030)	(0.058)	(0.028)
5 months before	-0.006	0.001	-0.002	-0.005	-0.002	0.022	0.007
	(0.017)	(0.019)	(0.024)	(0.028)	(0.036)	(0.064)	(0.030)
6 months before	0.001	0.008	-0.009	0.001	-0.009	-0.085	-0.013
	(0.017)	(0.017)	(0.021)	(0.024)	(0.029)	(0.074)	(0.033)
7 months before	0.008	0.010	0.010	0.022	0.003	-0.123	0.024
	(0.019)	(0.021)	(0.026)	(0.028)	(0.031)	(0.102)	(0.042)
8 months before	0.020	0.016	0.042**	0.050**	0.041	-0.038	0.046
	(0.018)	(0.018)	(0.021)	(0.022)	(0.032)	(0.082)	(0.034)
9 months before/	0.013	0.024	0.023	0.013	-0.017	0.097**	0.057**
during blackout	(0.015)	(0.015)	(0.017)	(0.020)	(0.030)	(0.040)	(0.024)
Birth controls	NO	YES	YES	YES	YES	YES	YES
Shehia f.e.	NO	YES	YES	YES	YES	YES	YES
Observations	18,238	16,835	11,976	10,559	6,644	1,343	5,287

All regressions include month and year fixed effects. Birth controls include age and age squared of mother, sex of child, gravida, prima gravida dummy and twin dummy. Robust standard errors in column 1, and clustered at the *shehia* level in column 2-7. Errors clustered at the household level in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Severity of exposure and birth weights

	(1) Full sample	(2) LFS sample	(3) LFS excluding coastal *shehias*
1 month after	-82.7***	-5.9	114.5
	(29.474)	(107.661)	(99.205)
× coastal town	10.3		
	(51.254)		
× privatesector		-171.5	-334.9*
		(171.407)	(167.267)
during blackout	-61.3	-6.9	-7.5
	(39.884)	(107.191)	(121.917)
× coastal town	8.0		
	(41.391)		
× privatesector		-77.5	-66.2
		(169.582)	(204.004)
1 month before	-80.7*	-85.7	-102.6
	(42.862)	(95.655)	(114.269)
× coastal town	-12.3		
	(51.627)		
× privatesector		-38.8	-43.5
		(151.951)	(193.297)
2 months before	14.8	-27.2	169.0
	(44.519)	(171.594)	(173.060)
× coastal town	-157.1**		
	(65.816)		
× privatesector		-72.2	-352.5
		(247.813)	(255.857)
3 months before	60.6	139.0	42.1
	(50.750)	(115.880)	(119.395)
× coastal town	-123.5**		
	(60.913)		
× privatesector		-238.6	-5.2
		(184.669)	(171.096)
Birth controls	YES	YES	YES
Shehia f.e.	YES	YES	YES
Observations	16,755	12,004	9,026
R-squared	0.073	0.073	0.074

Column 1 includes full birth sample of shehias identified as costal or not coastal (i.e., inland or urban). "Coastal town" is a dummy. Regression 1 includes all exposure months interacted with coastal dummy. Privatesector is a continuous variable (see appendix for an explanation). Regressions 2 and 3 include all exposure months interacted with the privatesector variable.
Errors clustered at the household level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1