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More than an Urban Legend: The long-term socioeconomic effects of unplanned fertility shocks

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Abstract

This paper exploits a nearly year-long period of power rationing that took place in Colombia in 1992, to shed light on three interrelated questions. First, we show that power shortages can lead to higher fertility, causing mini baby booms. Second, we show that the increase in fertility had not been offset by having fewer children over the following 12 years. Third, we show that the fertility shock caused mothers worse socioeconomic outcomes 12 years later. Taken together, the results suggest that there are significant indirect social costs to poor public infrastructure.

Keywords: fertility, infrastructure, blackouts, unplanned parenthood.

JEL Codes: J13, J16, O18, H41

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1 Introduction

The idea of baby booms following a blackout has been a subject of contention for a long time. It first came to prominence in popular culture after the great New York blackout of 1965, which left over 30 million people without electricity for 13 hours. However, the seminal work by Udry (1970) concluded that birthrates did not increase significantly nine months after the great New York blackout. Since then, the theory has been termed an "urban legend" by the president of the Population Association of America.

Unlike power outages in the developed world, blackouts are commonplace in developing countries. Many of them experience rolling blackouts that last weeks, if not months, and for several hours a day. However, the existing literature has almost exclusively focused on the costs on firms and investment from unreliable public infrastructure (see Fisher-Vanden et al., 2015; Reinikka and Svensson, 2002). This paper is among the first to highlight the large indirect social costs of unreliable power infrastructure through its effect on household behavior.

We shed light on three interrelated issues. First, we provide evidence that, in the short-run, power rationing does, in fact, create baby booms. Second, we answer: how persistent is this shock on fertility? We check the claim that, after having an unplanned baby, women still maintain the size of family they ultimately want by having fewer children in the future (Ward and Butz, 1980; Barmby and Cigno, 1990). Third, we check the long-run consequences of this fertility shock on some mothers' socioeconomic outcomes like educational attainment (Ribar, 1999; Geronimus and Korenman, 1993; Ashcraft et al., 2013) and participation in the labor market (Angrist and Evans, 1998; Jacobsen et al., 1999).

To shed light on these questions, we exploit a unique natural experiment set in Colombia. In 1992, the El-Niño climate phenomena caused a drought, which lead to such low water levels in rivers and reservoirs that power generation plummeted, resulting in daily electricity rationing over a period of almost 12 months. This constitutes a natural experiment with quasi-random treatment appealing at two levels. First, the year-long power rationing gives us sufficient statistical power to detect even minor effect sizes. Second, the power rationing was heterogeneous across Colombia. We exploit this spatial variation in the intensity of power rationing to study whether and how short- and long-term fertility changed.

We do so by constructing a retrospective mother-level birth history using the micro sample of the 2005 population census of Colombia. This provides us with the population of birth events. We combine this with municipality level variation in the intensity of the power rationing. This measure of treatment intensity is constructed from night lights satellite imagery. We use this dataset to study the impact of power rationing, both in in the short run (the first year following the blackout period) and long run (13 years after the event).

For the short-run analysis, we adopt a fixed effects model. By looking at the impact of the blackout on the fertility of individual mothers, we are able to address concerns of time invariant omitted variables and, in particular, apprehensions related to targeting treatment to specific areas for political reasons. We show that women, on average, experienced a 4 percent increase in the probability of giving birth in 1993, the year following the blackout. This implies that more babies were conceived during 1992, the year in which power was rationed. A back-of-the-envelope calculation suggests that the blackout led to an additional 1.9% of births -representing around 6,800 babies- in 1993.

Next, we show that women do not adjust this short-term fertility boost by having fewer children later. Using the 2005 census cross section, we estimate the long-run effect of the blackout on fertility using a difference-in-difference approach. We take advantage of the fact that women who gave birth during 1992 were physically exposed to the power-rationing period, but were biologically much less likely to conceive because they had just delivered a baby. Therefore, the blackout should not have had an effect on their fertility. We take this set of women as a control group and compare how the total number of children they have had up to 2005 differs between blackout and non-blackout areas. Then we take women who did not give birth in 1992 as a treatment group and compare how the same outcome differs between the same areas. Any difference between treatment and control groups corresponds to the long-run effect of the blackout on fertility. We estimate that women who had a child in 1993 due to the blackout had on average 0.07 more children in 2005 than women who had a child in 1993 due to other reasons. This estimate implies that in 2005 there were approximately 14,100 more children as a consequence of the blackout. Therefore, the fertility shock had not yet been offset 12 years later. Rather, we find that the number of children born as a consequence of the blackout more than doubled, highlighting the long-run impacts of a temporary fertility shock. In addition, we find that, for women who had already given birth at least once prior to the power outage, the blackout reduced the time between births by one to two months. This further supports our interpretation of the power-outage induced shock to fertility as being unplanned.

Finally, we show how babies conceived due to the outage can affect mothers' socioeconomic outcomes. We use the same approach as in our estimation of the long-run effect of the blackout on fertility but change the dependent variable. We find that mothers who gave birth due to the blackout have worse modern-day socioeconomic outcomes. We are able to rule out some of potential confounders such as the impact of the blackout on job loss or other income shocks, since both treatment and control were exposed to the power outage (and a possible income shock), but only the treatment group could physically receive the treatment. In order to deal with selection, we show that the mothers in treatment and control groups are not different on many observables that are fixed or likely to be fixed prior to treatment, most importantly fertility prior to the outage. To our knowledge, this is the first paper suggesting public infrastructure failures can affect long-run household welfare through fertility shocks.

This paper contributes to several strands of literature. First, it contributes to a limited but growing empirical literature on examining the impact of electricity infrastructure in developing countries (see Dinkelman, 2011; Rud, 2012). In particular, it provides evidence on the influence of infrastructure on fertility. On this strand, the work most closely related to ours is Burlando (2014a), who looks at the impact of a month-long power outage in Zanzibar on village-level fertility outcomes. He finds a 20 percent increase in village-level births nine months after the blackout. Second, this paper also contributes to understanding fertility response to other aggregate shocks. Evans et al. (2008) and Pörtner (2008) study the effect of natural disasters and hurricanes in particular. They find a negative relationship between hurricane advisories and births nine months after the event. Other temporary shocks, by creating in-utero exposure to stress, can affect birth outcomes. For example Burlando (2014b) finds that mothers exposure to power outages during pregnancy resulted in lower birth weights.[1] Bozzoli and Quintana-Domeque (2014) study the impact of the economic crisis in Argentina on birth weights, while Quintana-Domeque and Ródenas-Serrano (2014) explores the impact of in utero-exposure to stress caused by terror attacks in Spain.[2] Our study also speaks to the literature understanding the role of culture, media and leisure on fertility (Ferrara et al., 2012; Jensen and Oster, 2009; Kearney and Levine, 2015). These studies have found a link between television programming and fertility behavior, including smaller family sizes. Finally, we also make significant contributions on the methodological front by combining aggregate night light data to micro data sets.

The rest of the paper proceeds as follows. Section 2 provides a brief background and context of the 1992 black out in Colombia. In Section 3, we describe the data and how we constructed our main dependent variables. Section 4 provides the empirical strategy, while section 5 presents the key results. The conclusion follows in Section 6.

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[1] Studies have found that low birth weight correlates with worse life time outcomes. See for example Bozzoli et al. (2009).

[2] Unfortunately, we are unable to document the impact of the power rationing on the socio-economic outcomes of the children born. This is due to a lack of data: first, the babies born due to the outage in 1992 are at most 13 years old in 2005, which is the most recent census year. Many outcomes, such as educational attainment, have yet to be realized at this age. Further, the 2005 census contains little information on health status or other outcomes that could be explored just 12 years after birth.

2 Context

Colombia obtains most of its electricity supply from hydroelectric sources. Installed capacity of electricity in 1991 was 78% hydraulic and 22% thermal.[3] In 1992, about 40% of hydroelectric power was produced in fourteen hydroelectric power plants that were located mainly in the Caldas and Antioquia departments of central Colombia.[4] A large share of the power generating capacity are so-called run-of-the-river power plants, sited along a river or between two rivers, with water for power generation turbines being supplied by the rivers natural flow. This makes this type of power plants particularly vulnerable to reductions in water flows, as shown by the tight correlation between power production and rainfall deficiency in Figure 1.

By the end of 1991, the meteorological phenomenon of El Niño reappeared in the pacific (see Appendix Figure A4). In Colombia, this led to a dramatic depletion of river flows and a depletion of the reservoirs that feed most major power plants. Between 1991 and 1993, water availability for power generation was, on average 14.2% lower compared to the long term average; in the peak months of 1992, water availability was 42.8% lower than the long term average.[5] As a consequence, some power stations needed to cut back power production dramatically because there was simply a lot less water available to produce electricity. One of the biggest energy firms estimated that throughout the year there was a shortfall equivalent to roughly 20% of the annual production of 1991.[6]

On the 28th of February of 1992, the government announced nationwide power rationing, starting the 2nd of March. It was announced as a rationing "for a couple of hours [per day], [lasting] no more than three months." On the 5th of March, the government acknowledged that rationing might be necessary for up to a year. On the 14th of March, rationing was extended to industrial

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[3] UPME, Boletin Estadistico de Minas y Energia 1990-2010, `http://www.simco.gov.co/LinkClick.aspx?fileticket=ABaDJv5Q1Jo=`.

[4] Department of Energy, An Energy Overview of Colombia, `http://goo.gl/nnhWBN`.

[5] UPME, Boletin Estadistico de Minas y Energia 1990-2010, `http://www.simco.gov.co/LinkClick.aspx?fileticket=ABaDJv5Q1Jo=`.

[6] See `http://www.tebsa.com.co/history.htm`, accessed on 20.06.2013.

areas that had previously been excluded. Eventually, the rationing lasted up to nine hours per day in the capital Bogota, but could last for up to 12 hours per day in other cities. Over the year, rationing waxed and waned depending on the rainfall levels, ultimately rendering the continued extent of rationing an exogenous factor. The rationing finally ended on the 1st of April of 1993.[7]

Colombians throughout the country felt the impacts of the extended and long-lasting rationing, which is why the period from 1992 to 1993 is referred to by Colombians simply as the Black Out. It forced Colombians to change their habits. Daylight Savings Time (DST) was introduced to take advantage of daylight. Unable to function without electricity, factories and other workplaces sent workers home earlier. TV business suffered because prime viewing times coincided with rationing hours. Pointing out a supposed negative relationship between time spent watching TV and fertility rates, a commentator wrote at the time: "With these blackouts, family romps will increase the number of Colombians produced in the dark."[8]

The shortfall, though, was not evenly spread across the country. In the northeast, thermal power generation from coal has been historically available. In the south, some regions were barely connected to the national electricity grid. Further, electricity losses along the transmission lines generate a natural gradient. All these contribute to creating spatial variation in the intensity of the power rationing.[9]

The electricity shortfall was also unexpected, at least for the vast majority of the population. Our contention is based on the passage of nearly a decade since Colombias previous drought-driven blackouts (McRae, 2010), the random nature of weather shocks, the strong response of the government to the shortfall and key newspaper articles we surveyed from the yearlong period.

Among the policies implemented as an immediate response to the power

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[7] Appendix Table A2 summarizes the key events.

[8] See `http://www.eltiempo.com/archivo/documento/MAM-123186`, accessed on 08.08.2015.

[9] This is akin to Gerard and Costa (2015), who study the long run effects of power rationing in the south of Brazil due to power production shortages following droughts in 2001. The lack of integration in the power network created distinct spatial variation in the extent of power rationing in their context.

outages of 1992-1993, the government expedited contracts and loans to increase power generation and transmission, reduced import tariffs on inputs related to the supply of electricity, and issued contracts to ensure investors that the electricity produced would be bought for no less than 15 years at a fixed price. During the course of the decade following the blackout year, the government heavily invested in infrastructure, deregulated the energy sector and reformed the public institutions related to the sector. In particular, substantial investment to boost access to on-demand thermal power plant capacity reduced the nations reliance on hydroelectric power. By 2009, two-thirds of power generation capacity stemmed from hydroelectric sources and one-third from thermal sources.[10] El Niño returned in 1997-1998, but by the time the reforms and investments allowed to evade another period of power rationing.

3 Data

3.1 Detecting Power Outages from Remote Sensing

To the best of our knowledge, this is the first paper to use night-lights data to study the exposure to power outages at the regional level. Satellite-derived night lights data offer a potential proxy for standard economic measures, such as GDP. They are particularly valuable in situations in which traditional measures are lacking or unreliable.[11]

Figure 2 highlights our approach to measure power rationing indirectly, using nighttime luminosity data available from US-run Defence Meteorological Satellite Program (DMSP). The figure depicts the luminosity variable around three main urban centres in Colombia in 1992 (left) and 1993 (right). The northern area is the Medellin metropolitan area, while in the south is Colombia's third largest city Cali. The light-blob to the right is the metropolitan

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[10] According to `https://www.cia.gov/library/publications/the-world-factbook/geos/co.html`.

[11] Economists have used this data source to map economic activity (Doll, 2008), economic growth (Henderson et al., 2012), agglomeration clusters (Storeygard, 2015; Fetzer and Shanghavi, 2014) and favoritism in the provision of public goods (Hodler and Raschky, 2014).

area of Bogota. The differences in the pictures are dramatic. Especially around Bogota, the broader geographic area appeared to be completely dark in 1992, while it was lit in 1993.

Note that the night-light data series is only available from 1992 onward. Hence, we can not compare the light intensity in the year 1992 (the year of the outage) with preceding years, as these data simply do not exist. However, we may be able to compare the 1992 lighting intensity with the intensity in 1993 or 1994. For this purpose, we construct the average population-weighted municipality luminosity for the years 1992, 1993 and 1994.[12] Then we construct a power outage intensity variable at municipality level, defined as the ratio of the average population-weighted municipality luminosity in 1992 over that measure for 1993, i.e. it is constructed as:

$$O_m = 100 \times \left(1 - \frac{Lights_{1992}}{Lights_{1993}}\right)$$

With this measure, total luminosity was 30% lower in 1992 compared to 1993.[13]

Though the 1993 luminosity is an outcome variable in itself, it is hard to believe that the micro mother-level variation we exploit has a direct effect on 1993 luminosity. Nevertheless, this measure of outage seems to be subject to a lot of noise.[14] This is illustrated in Figure 3, where we plot kernel densities of the outage distribution across our sample. Though the median is positive, there is a significant fraction of observations where the outage measure suggests *increased* luminosity in 1992 (the year of the blackout) as compared to 1993. The presence of noise is confirmed in the same figure by looking at the distribution of the placebo outage intensity, defined as the average population-weighted municipality luminosity in 1993 over that measure for 1994. Though the mass concentrates around 0, as it should be given that in 1993 and 1994

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[12] See Appendix A.2 for more details on the data and the construction.

[13] This dovetails with estimates from Tebsa, a big power generator at the time, showing a shortfall of around 20% relative to the annual production in 1991, see `http://www.tebsa.com.co/history.htm`, accessed on 20.06.2013.

[14] Most of the sources of this measurement error are purely mechanic due to the remote measurement and the nature of the satellite, these are discussed in detail in Appendix A.2.

there were no shocks on power generation, the dispersion suggest that the measure O_m for a non-outage year contains a lot of noise.

In order to clear some noise from the luminosity data, we create a dummy variable which is equal to one in case the power rationing was above median in our sample. In particular, for every municipality m we define D_m as:

$$D_m = \mathbf{I}(O_m > \text{Median})$$

where **I** is the indicator function. Since the densities in Figure 3 indicate that it is very difficult to judge whether below median rationing in 1992/1993 is actually rationing or simply due to noise, the dummification helps us to increase the signal relative to the noise in the data.[15]

An additional concern regarding our outage measure concerns its use in rural areas, many which were still undergoing the process of electrification in the blackout year. Thus, in our 1992-1993 luminosity comparisons of rural areas, what appears to be a blackout in 1992 is actually electrification in 1993. As a result, we believe our measure is more appropriate for urban areas and hence, we restrict the analysis to individuals living in urban areas.[16] Restricting the analysis to the urban areas is also supported by the nature of the night-light data, which aim to capture stable light emissions from human agglomerations such as cities.

We now proceed to discuss the census data that are used throughout the paper to identify the fertility shocks and the socioeconomic outcomes.

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[15] All results in this paper hold regardless of whether we use the 75th percentile or the median as the cutoff for the dummy. Results with continuous measure of intensity are, subject to some loss in precision on some point estimates, very similar throughout and are presented in Appendix Tables B. Figure 7 motivates the choice of the median as a cut-off, presenting results from a regression studying the heterogeneous effect of power rationing intensity by decile on the probability of a mother giving birth in 1993, while controlling for mother and year fixed effects. It is clear that municipalities with power rationing above the median are driving the effect.

[16] This is supported by data from the World Bank, suggesting that urban electrification was already very high in the 1990s, with 96% of the urban population having access to electricity in their homes in 1990, compared to only 51%of the rural population; hence, our measure is most likely to be capturing rationing in urban areas.

3.2 Census Data

An analysis that studies fertility effects at the aggregate level may fail to discover any statistical effect due to low power conjoint with small effect sizes. In addition, there may be compositional effects as the cohorts of women who have babies in a municipality changes over time. In order to address this, we construct individual level birth histories using the 2005 micro sample census of Colombia provided by IPUMS. The micro-data sample covered 10% of the population at the time and was hailed to be the most successfully conducted population census.[17] For the short run outcomes, we construct a retrospective panel of mothers using the matched mother to children data for the period 1989 to 1996.[18]

We restrict our analysis to women born between 1948 and 1978, resulting in a cohort aged between 15 and 45 in 1993. The choice for the birth-year cutoff is based on ensuring that we only include women considered to be of reproductive age in the year of treatment, consistent with the definition used by the well-known Demographic Health Surveys. Further, any other age cutoff does not affect our results once we consider mother fixed effects, because the variation comes from within a mother rather than across the mothers. The panel structure of the data allows us to identify the effect of the blackout by exploiting within-mother variation in the timing of birth of babies, instead of solely relying on spatial cross-region or within-region variation. This gives us a total number of 457,312 women with 288,600 children born in this period. Due the exogenous timing of the shock, focusing on the years immediately before

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[17] See `http://unstats.un.org/unsd/censuskb20/KnowledgebaseArticle10236.aspx`, accessed 20.06.2013.

[18] More details about the census data and possible alternative data sources can be found in Appendix A.1. The choice of this time period is twofold. First, by considering a tight window around the year of the blackout, we are able to minimize any omitted time varying unobservables. Second, children often leave their family households at the age of 18. Since the census asks question on the number of children living within the household, we would not be able to account for retrospective births prior to 1988, as these children would be older than 18 in 2006 (the census carried on into early 2006 for some municipalities) and may have moved out. To ensure we do not have selection of mothers based on children moving out earlier, we start our analysis in 1989, whereby the eldest child in 2006 would be 17 years old.

and after the blackout allows us to mitigate some of the concerns related to parallel trends across the different municipalities, which would be needed to study fertility dynamics over a longer horizon.

Figure 4 presents the timeline of the power-rationing period over 1992 and 1993. The power rationing started in March 1992 and ended one year later in the beginning of April 1993. For the short-run analysis, our outcome variable are birth events. We define the treatment as a power-rationig measure times being in the year 1993. This generates a small overlap, since some of the births in December 1992 could have been due to the power rationing commencing nine months earlier. Nevertheless, we stick with 1993 being the treatment year, since the bulk of the effect should be captured in 1993; in fact, we will confirm this in the data.[19]

For the long run analysis, we construct a total births variable using the matched mother and child data based on the 2005 census. Our outcome variable is a lower bound, since all births, particularly for older women may not be captured if children have already moved out of the household. This creates a problem of under-reporting total births for older women. In order to address this, we introduce flexible age fixed effects in our analysis. Further, the decision for using the matched data arises from the fact that the self-reported total number of births data are very noisy. For example, one of the women reported having 15 children by the age of 26. We restrict our analysis to comparing women who had delivered during the first nine months of the rationing period (hence unable to get pregnant during the blackout) with women who gave birth nine months after any month of the rationing period (see Figure 4). This leaves us with 22,347 and 40,256 women in the control and treatment groups, respectively.

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[19] The alternative would be to construct a monthly level panel. The resulting data set is very unwieldy with around 43.9 million observations for the 457,312 mothers in our sample. We confirm that we obtain similar, albeit less precise point estimates using the exact treatment period definition based on 25% sample of mothers.

4 Empirical Strategy

We separate the empirical analysis into three steps. First, we look at the short-run implications of the power outages on mother-level fertility. Second, we show that these effects persisted - i.e. that the power outage is associated with a life-time increase in fertility, and a reduction in the time between births. Third, we ask how this long-run effect correlates with economic outcomes for the mothers, thus highlighting the welfare consequences.

4.1 Short-Run Fertility Effects

Our dependent variable is a dummy variable $B_{imt} = 1$, if mother i from municipality m gave birth in year t. We estimate the following linear probability model:

$$B_{imt} = a_i + b_t + \gamma \times D_m \times Z_t + \epsilon_{imt} \tag{1}$$

where we include mother fixed effects a_i and time fixed effects b_t. We add the sub-index m for municipality, since the treatment intensity is fixed at municipality level. The treatment assignment is $Z_t = 1$ for $t = 1993$, i.e. the year in which babies conceived in 1992 are being born.[20] Note that Z_t is perfectly collinear with the time-fixed effects b_t and the power outage dummy D_m is invariant at municipality level, thus perfectly collinear with the mother fixed effects a_i.

The coefficient of interest is γ, which measures the difference in the probability of giving birth in 1993 for women in municipalities that experienced an above the median rationing. The interaction term exploits variation across municipalities by comparing mothers who experienced a blackout to mothers who did not as measured by D_m. The coefficient γ represents the causal effect of power-rationing on the probability of giving birth under the following identifying assumption: After controlling for mother fixed effects and exogenous covariates, the changes in probability of birth for mothers living in municipal-

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[20] It is clear that this is an Intention to Treat design, as we do not actually observe fertility and mating behavior around the time, i.e. we can not rule out that some women who were assigned treatment did not actually receive treatment.

ities that experienced below the median outages provide a counterfactual for mothers living in municipalities that experienced above the median outages.

Municipality unobserved characteristics could be a source of violation of the identifying assumption in equation (1). However, we address this by exploiting within-mother variation over time. This is an improvement on previous work (i.e. Burlando, 2014a) because, by including mother fixed effects, we control for time invariant characteristics both at the mother level (such as education and family background) and at the aggregate level (such as geography, history and local culture). We further add various other demanding time effects interacted with baseline characteristics and add municipality-level trends to alleviate concerns about omitted time-varying factors driving the result. Further, we show that fertility in 1993 clearly spikes in municipalities that experience rationing, compared to municipalities that did not, addressing concerns about the common trends assumption inherent to this design.

4.2 Long-run Fertility Outcomes

A significant contribution of this paper is to study the impact of a short-term fertility shock on long-run fertility outcomes. By studying the effect of a temporary shock on fertility 13 years after the power rationing, we can check if fertility is smoothed over time (Ward and Butz (1980); Barmby and Cigno (1990)). In particular, the total number of children in the lifetime of a woman may be not affected by the power outage because increased fertility during the blackout might be compensated with less fertility in later years.[21]

We define control women as those who gave birth during the first nine months of the blackout and treated women as those who gave birth 9 months after any month of the blackout period, as illustrated in Figure 4. Therefore, our estimating sample is such that both treatment and control were ex- posed to the power outage, but only the treatment group could *physically respond to the treatment*. Control women were, if anything, only partially treated. Even if they were no longer pregnant at the end of the blackout, these women are

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[21] Nevertheless, one may still find an effect of unexpected children on the mother's socio-economic outcomes.

biologically less likely to be responsive to treatment in form of changing fertility behavior. Post-delivery, the likelihood of having another child immediately is very low because post-natal care takes up a large chunk of the mothers time. As per the sample, only 6 per cent of the mothers gave birth in both the treatment and control period. [22]

Our estimation procedure can basically be described as follows: First, we find the difference in the total number of births for treated women in municipalities with intense blackouts versus treated women in municipalities with mild blackouts. Second, we find the same difference for control women. Our estimate is the difference between the later and former differences.

In short, to find the long-run effect of the blackout on fertility, we exploit the variation in nightlight in the municipality a woman lives interacted with her feasibility of conceiving during the blackout. In particular, we estimate:

$$tch_{mic} = b_{mc} + \beta_1 \times T_i + \beta_2 \times T_i \times D_m + \epsilon_{mic} \tag{2}$$

where tch_{mic} is the total number of children born to mother i from age cohort c in municipality m. The variable b_{mc} is a set of municipality-cohort fixed effects. These control for common shocks to women of the same year of birth cohort within a municipality. These fixed-effects are very demanding, but they help us take out a lot of age-specific heterogeneity that could be due to age or time-specific events at the municipal level. Note that in this setup, we cannot control for mother-fixed effects, as there is only cross-sectional variation in the dependent variable. Treatment T_i equals zero if mother i gave birth during March 1992 to November 1992, while equals 1 if mother i gave birth between December 1992 to December 1993. The dummy D_m measures, as before, the power outage in 1992. Note that the level effect of the outage measure D_m is captured by the municipality-birth year fixed effect b_{mc}. The coefficient of interest is β_2, which measures the change in long-run fertility of mothers who gave birth because of the blackout.

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[22] Since the treatment period covers more than a year, and the control period was for only 9 months, it is possible for a mother to have given birth during both periods. We exclude this subset of mothers from the analysis for ease of interpretation.

We take our hypothesis further by looking at birth spacing among mothers in the treatment group, and compare this to the pattern among mothers in the control group. If indeed these births are unintended, then one would expect there to be a fall in the birth spacing among children of treated mothers. This also allows us "confirm" the exogeniety of the blackout as a smaller birth spacing would indicate a truly unexpected shock. Using the same specification as (2), we test this hypothesis by looking at the time gap in months since the last birth, thus restricting the analysis to the set of mothers in the treatment and control group who had already had a child prior to treatment. As noted earlier, we include a hoard of age-specific fixed effects to control for any differences in age profiles that may exist.

Finally, we present two placebo checks for our results. First, we shift the timing of the treatment by one year, comparing mothers who gave birth in March 1991 to November 1991 to mothers who gave birth in December 1991 to December 1992. Second, we maintain the timing of the treatment, but replace the intensity of treatment by the nightlight emissions from 1993 and 1994 as placebo power-outage measure.

4.3 Long-Run Impacts on the Mother

We can now turn to the third pillar of the analysis. Namely, we want to shed light onto whether the fertility shock had some long-lasting effects on the lives of the mother and her family environment. This is particularly interesting because it allows us to look at the impact of unplanned babies on the life path of mothers, children and the family itself.

In order to estimate effect of blackout babies on the long-run socioeconomic outcomes, we take the framework given by Equation 2 and compare the change in socioeconomic outcomes of mothers in the treatment and control groups across blackout and non-blackout municipalities. Hence we run a difference-in-difference model given by

$$y_{mic} = b_{mc} + \eta_1 \times T_i + \eta_2 \times T_i \times D_m + \nu_{mic} \tag{3}$$

where y_{mic} denotes one of the following socioeconomic outcomes: whether the mother is a high school dropout; whether the mother is single or separated; whether the mother owns certain assets (cars, household electric equipment) that serve as household wealth proxies; and whether the mother is active or inactive in the labor market. As before, T_i is a dummy indicating whether the mother belongs to the treatment group (as defined in section 4.2) and D_m is a dummy indicating whether a municipality had an above-the-median power-rationing. The coefficient η_2 measures the change in a mothers' socio-economic outcome for having a child due to the blackout.

We now present the key results from each of the tree steps of the analysis.

5 Results

5.1 Short Term Fertility Effect

The first set of results pertains to the short-term fertility increases due to the power outages. These are presented in Table 1. The estimated coefficients on the interaction term between power outage intensity and treatment are positive and significantly different from zero. In column (2) we add municipality fixed effects, in column (3) we add year fixed effects and in column (4) we replace municipality fixed effects by mother fixed effects. Surprisingly, the coefficient remains very stable and does not change when adding the mother fixed effects, suggesting the treatment was indeed quasi random. In column (5) we replace the year effect by region-year fixed effect. Finally we control for municipality-level linear trends in column (6). Even in the most demanding specification, our estimate remains stable and precisely estimated. According to our estimates, the 1992 blackout increased the probability of a woman giving birth next year by around 0.3 percentage points. Since the probability of of giving birth in any given year is 7.8%, the blackout boost the probability of giving birth in 1993 by almost 4%. These estimates suggests that the number of children born due to the power-outage was between 6,608 and 8,209.[23] Since roughly 360,000 babies were born every year, our estimates suggest that between 1.8% and 2.3% of the babies born in 1993 were due to the power outage. The same figures for our prefered specfication in column (5) are 6,791 of blackout-induced babies or 1.9% of the babies born.

We consider a few robustness checks to ensure the validity of our results. Column (1) of Table 2 presents the preferred specification from Table 1. Since the census sample was conducted in 2005, a major concern with our results is that mothers may have moved across municipalities since 1993, thus biasing our power-outage assignment. If this is due to pure randomness, we would expect our measure to be noise and thus to lead to attenuation bias. On the other hand if the relocation choice of the mother is correlated with some unobserved characteristic of the municipality in 1993, we would have biased estimates for the effect of power outage on probability of birth. In order to address this, we restrict our sample in column (2) of Table 2 to women who were born in the same municipality where they live in 2005 and hence have -most likely-lived there all their lives. In column (3), we restrict the sample to mothers who report to have had their last birth prior to 1993 in the same municipality they report as their current residence. The sample shrinks considerably in both cases, rendering the estimates less precise; however, the point estimate does not change much. Column (4) controls for time varying measures of conflict capturing guerrilla or paramilitary presence in a municipality. This is important, since the period saw significant turmoil with conflict between the guerrilla, paramilitary groups and the central government. The results stay unaffected. Column (5) controls for whether a municipality is the head of a departmental government, interacted with region by year fixed effects. This flexibly controls for the extent to which politically more important municipalities may have been affected by the power rationing differentially. Again, the

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[23] We arrive at this figure by multiplying our lower and upper estimates of 0.289% and 0.359% by the total number of mothers in the sample, 457,312. Next we multiply this result by 0.5 to obtain the number of mothers who experienced a blackout above the median intensity. Next we multiply by 10, since the census micro-data pertains to only 10% of the population.

result stays unaffected. Column (6) controls for municipal tax revenues as a proxy for income; a set of dummies for capturing whether a municipality is an oil, coca or opium producer; land inequality as of 1985; and year fixed effects. These economic covariates rule out some potentially time-varying shocks or trends which may be proxied by our outage measure or may independently affect fertility. Since we exploit some spatial variation, in column (7) we control for geographic covariates capturing altitude, distance to capital, soil erosion, agricultural suitability and year fixed effects. Column (8) combines all the control variables discussed in columns (4) - (7). Reassuringly, our point estimate remains stable in all of these specifications.

In order to test the common trends assumption inherent to our approach, we explore how our estimated coefficient varies when the blackout measure D_m interacts with each year separately. The estimated coefficients and their respective confidence intervals are presented in Figure 5. As it can be seen, our outage measure is only statistically significant in 1993, the year immediately following the blackout. There is a weak, but insignificant uptick in fertility in 1992, which is not surprising since some of the babies conceived during the blackout were born in the last month of 1992. These results give us confidence that 1993 was indeed an exceptional year in terms of fertility due to the power rationing during 1992.[24]

Next, we explore the mechanisms through which the blackout increased fertility by looking at heterogeneous effects by 5 year age groups. The coefficients and the confidence intervals for each group are presented in Figure 6. The only statically significant effects are for women aged between 17 and 21 and between 37 to 41. Therefore, the blackout seems to have affected the fertility behavior of women either at the start or at the end of their reproductive age. The socioeconomic consequences of having a baby as a teenager are further explored in section 5.3.

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[24] An alternative way to study this is by studying the effect of luminosity levels in 1992 on the probability that a mother gives birth in the years 1989-1996. We find no effect of lights on probability of birth other than for the year 1993, where the probability of birth is decreasing with how lit was the municipality of the mother in 1992. The regressions estimates are presented in appendix figure A6.

Instead of using the dummy variable D_m to measure the blackout, we could have used the noisier continuous measure O_m. In appendix B, we show that our results are are robust to this alternative measure, albeit at the cost of losing some precision.

All of the above evidence strongly suggests that there was an abnormally higher rate of fertility in 1993, consistent with our hypothesis. We now turn to the study of the persistent effects of the power outage on total fertility.

5.2 Long-Term Fertility Effect

Having fewer children in the future may dynamically offset the temporary fertility effect demonstrated in the previous section. The results of regression (2), presented in Panel A of Table 3, show that this is not the case.

Column (1) is a simple difference in difference regression without any controls. The coefficient on the interaction term is positive and highly significant. This means that the mothers who gave birth due to the power outage had an overall number of children higher than nothers who gave birth due to other reasons. This simple specification might be biased by location-specific factors that affect life-time fertility. To avoid this bias, column (2) adds fixed effects at the municipality level. As the coefficient remains stable and significant, we rule out this bias. One natural concern is that women in the control group are older, compared to women in the treatment group; this would naturally underestimate the effect, as we understate the lifetime fertility of women in the treatment group. We add age fixed effects (Column (3)) and municipality-age fixed effects (Column (4)) to make sure that our effect is estimated solely off variation between mothers across the same age group. The estimated effect stays virtually the same, even if we remove some spatial variation by controlling for region by treatment status fixed effects in column (5).

Overall, our estimation suggests that women whose pregnancy was a consequence of the blackout had, on average, 0.07 more births over the subsequent 12 years, compared to women whose pregnancies in the same period were for reasons unrelated to the blackout. Henceforth, 7 percent of women were not able to dynamically adjust their overall lifetime fertility. Based on this estimate, we can approximate the long-run fertility effect of the power outage on the aggregate of the population. The result is that by 2005, around 14,090 additional children had been born because of the blackout.[25] When compared to the short-run effect, the long-run estimate may suggest that the fertility shock expands rather than abates: women are likely to have more children after having given birth to the first child. However, since this result comes from back-of-the envelope calculations based on point estimates from two different samples, we interpret this result cautiously.

We can provide further evidence suggesting that procreation was affected by the outage by studying the time gap between births. We check this hypothesis by comparing the time between births measured in the number of months for the treatment and control group since their last birth. Naturally, this is only possible for the women who had already given birth once before the power rationing. This greatly reduces the sample size. The results are presented in Panel B of Table 3. After controlling for fixed effects, we see that the time between births was shorter by, on average, two months for women who gave birth nine months after any point of the power rationing period. This provides further evidence that the power rationing affected procreation patterns. In particular, it provides suggestive evidence that some births following the outage were unplanned.

In the last step of our analysis, we explore the effect of having a blackout baby on other socioeconomic outcomes.

5.3 Long Term Effects on Mothers' Socioeconomic Outcomes

Bringing up a child is costly because it requires time spent away from working or from obtaining a school degree. In addition, women who have an unplanned

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[25] Half the 40,256 pregnant women in our treatment group were in blackout municipalities. Seven percent of this group of women had not yet offset the blackout-linked pregnancy by having fewer subsequent pregnancies in the ensuing 12 years. Again we scale up by 10 because of the micro census data is for a 10 percent sample.

child may find themselves in more unstable relationships. We try to shed light on these questions by checking the effects of the power outage on the socioeconomic outcomes of mothers 13 years after the blackout. For this purpose, we use our most demanding specification from the previous section.

The results from this exercise are presented in panel A of Table 4. Columns (1) and (2) present the preferred specifications for the total number of children and the birth spacing of Table 3. From column (3) we see that women who became pregnant as a consequence of the blackout are 4.1 percentage points more likely to be high school dropouts (when considering the subsample of women who finished primary school). By extrapolating our point estimate from the short-run analysis to the whole population, we estimate that the power outage lead to an increase in high school dropouts by around 0.1%.[26]

Further socio-economic outcomes are explored in columns (4) - (8). From columns (4) and (5) we see that women who had a blackout baby are more likely to be separated or single mothers. They are less likely to own assets like home appliances (Column (6)) and cars (Column (7)). Finally, they are more likely to be inactive in the labor market (Column (8)).

There are two potential concerns. First, we perform a sequence of placebo tests to rule out that our results are driven by the selection of our treatment and control groups. Second, since we exploit variation across women, we need to assess to what extent selection into treatment may be driving the estimated effects.

In panels B and C, we run the regressions of Panel A of Table 4 using two different placebos. To ensure there is no spurious relationship across time, in panel B, we move the treatment and control group one year earlier, comparing mothers who gave birth in 1991 and 1992. Next, to address concerns regarding the nightlight measure capturing something different than the blackout, we construct a placebo measure of the blackout. We compare the nightlights of 1993 to nightlights of 1994, two years without any reported disruption in

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[26] Between 6,608 and 8,208 women had a blackout baby, resulting in an increase in dropouts between 0.041*6,608 = 270 and 8,208 * 0.041 = 337 women. Relative to the total number of 268,145 women who at least completed primary school, this amounts to an increase in dropouts between 0.10% and 0.13%.

electricity in panel C. We find no statistically significant effect from the placebo exercise, reassuring our identification.

Another concern is that mothers with distinct socioeconomic background are more likely to be in the treatment group. This self-selection does not introduce a bias, but naturally implies that we may be capturing a very specific local average treatment effect, which may make it difficult to extrapolate our findings. We can alleviate this concern by checking whether our treatment somehow affects characteristics which are likely to be predetermined before the blackout. For instance, one may believe sexually reproductive women are more likely to be in the treatment group. Using the birth history prior to the blackout, we are able to show that there is no significant difference across the treatment and control groups in terms of total children born prior to 1992. Other predetermined characteristics we have access are whether a women belongs to any ethnic minority, is illiterate, disabled, migrant or has faced the death of one of their children. The results, reported in Table 6, suggests that none of the predetermined characteristics vary systematically between the treatment and the control groups.

6 Conclusion

This paper set out to analyze the impact of power rationing in Colombia in the early 1990s on fertility. In particular, it is the first attempt to evaluate the impact of power rationing on population dynamics, going beyond the question of whether power outages may cause “mini baby booms”. Such research was not possible because of lack of good data on electricity consumption. However, we show that satellite-based night-light measures offer a way to identify the extent of power rationing in different areas.

We show that women who live in areas in which power rationing was more severe were more likely to give birth in the year following the blackout. This suggests that the blackouts did indeed create “mini baby booms”.

Looking at fertility dynamics over time, we find that women who had a baby as a consequence of the blackout did not balance out their overall fertility in the 12 years that followed. Finally, we show that the arrival of these "blackout babies" had long-run consequences on the socioeconomic situation of their mothers, who are about more likely to be single or separated, more likely to drop out of high school, less likely to be active in the labor market, and less likely to have the certain assets. This suggests that low-quality infrastructure contains significant but often hidden costs for society.

In order to equip policy makers in developing countries that face periods of severe power rationing, further research needs to be carried out to understand the timing of load shedding and its effect on fertility, so as to minimize the hidden social cost of blackouts.

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Figures for the Main Text

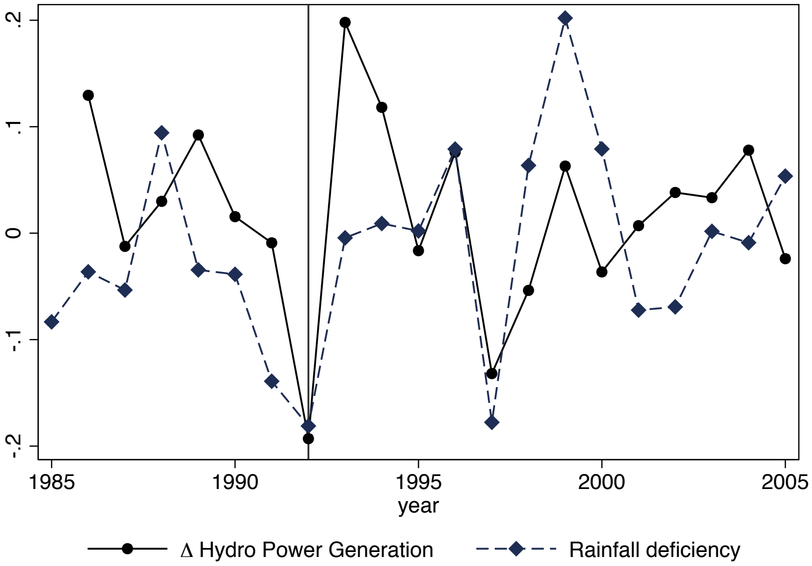

Figure 1: Figure plots the year on year proportional change in estimated hydro power electricity generation for Colombia (solid line) and the proportional rainfall deficiency compared to long term means (dashed) line. Data on hydro power generation from the World Bank.

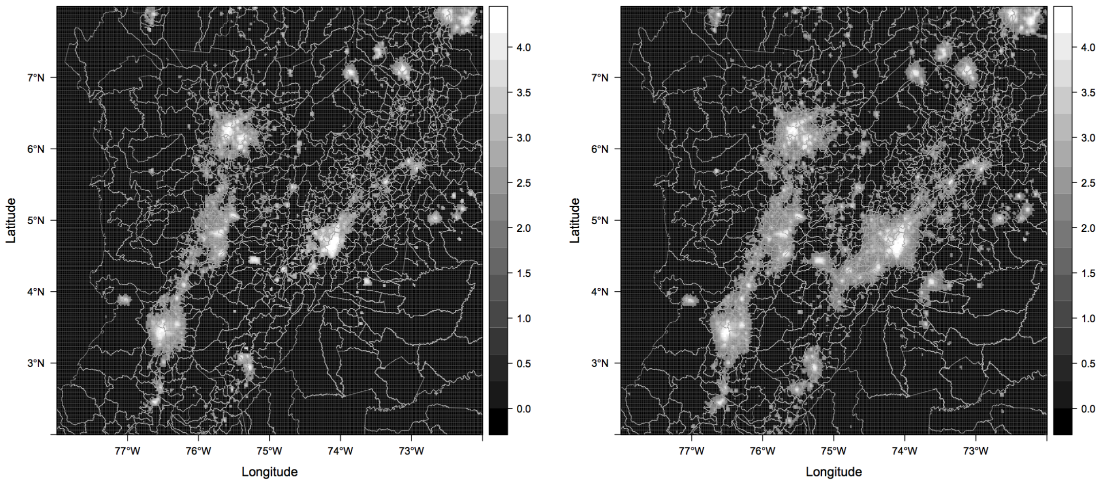

Figure 2: Light intensity in Colombia, 1992 (left) and 1993 (right) on identical log-scales along with municipality borders.

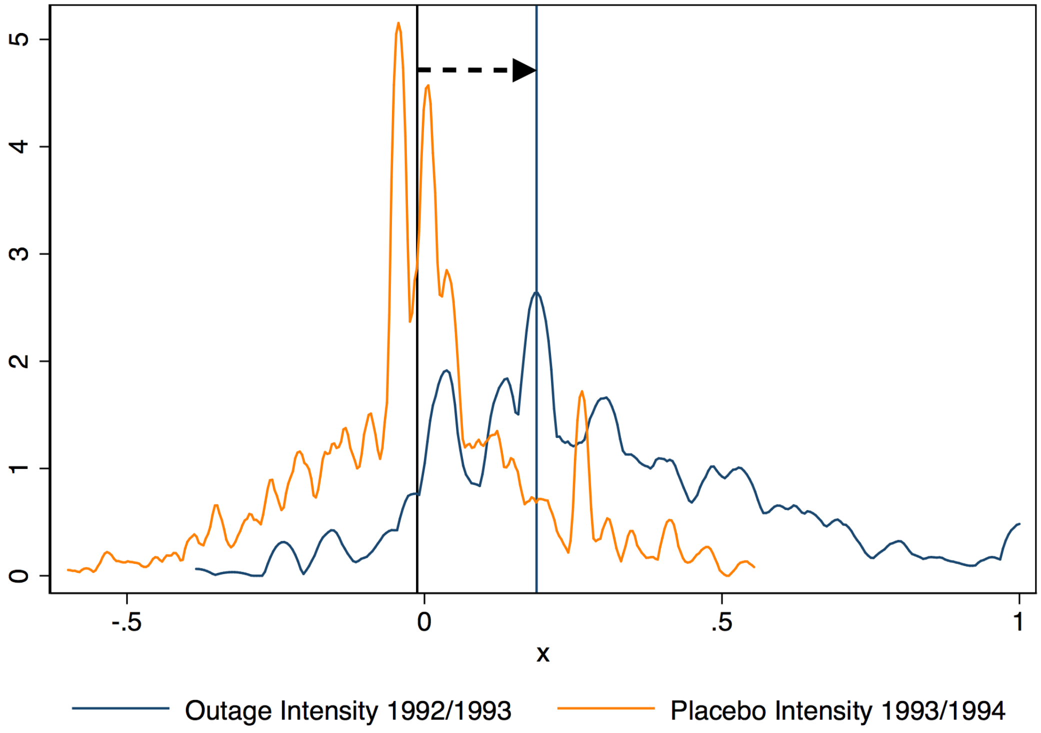

Figure 3: This figure plots the distribution of the power rationing intensity measure in the balanced sample of mothers. The orange kernel density is a placebo outage measure computed for the years 1993 to 1994, in which there was only very limited power rationing. The distribution is effectively normally distributed and centered around zero; the spread indicates that there is significant measurement error. The distribution of the outage measure between 1992 and 1993 is clearly shifted to the right, indicating that there was significantly lower levels of night lights emissions in 1992 relative to 1993. We use this to proxy for the extent of power rationing. The vertical line indicates the median power outage intensity.

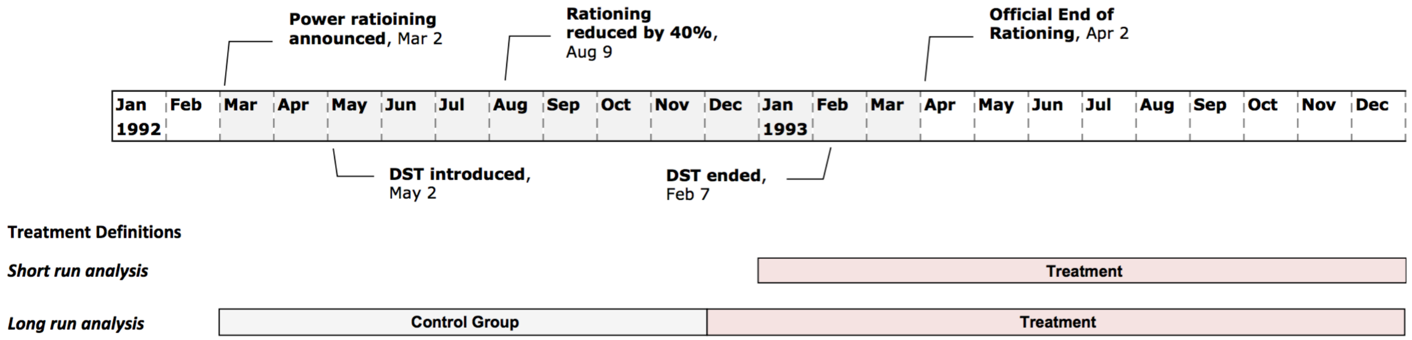

Figure 4: Timeline of major events of the period of power rationing in 1992/1993 in Colombia, along with the treatment and control definitions for the short and long run analysis.

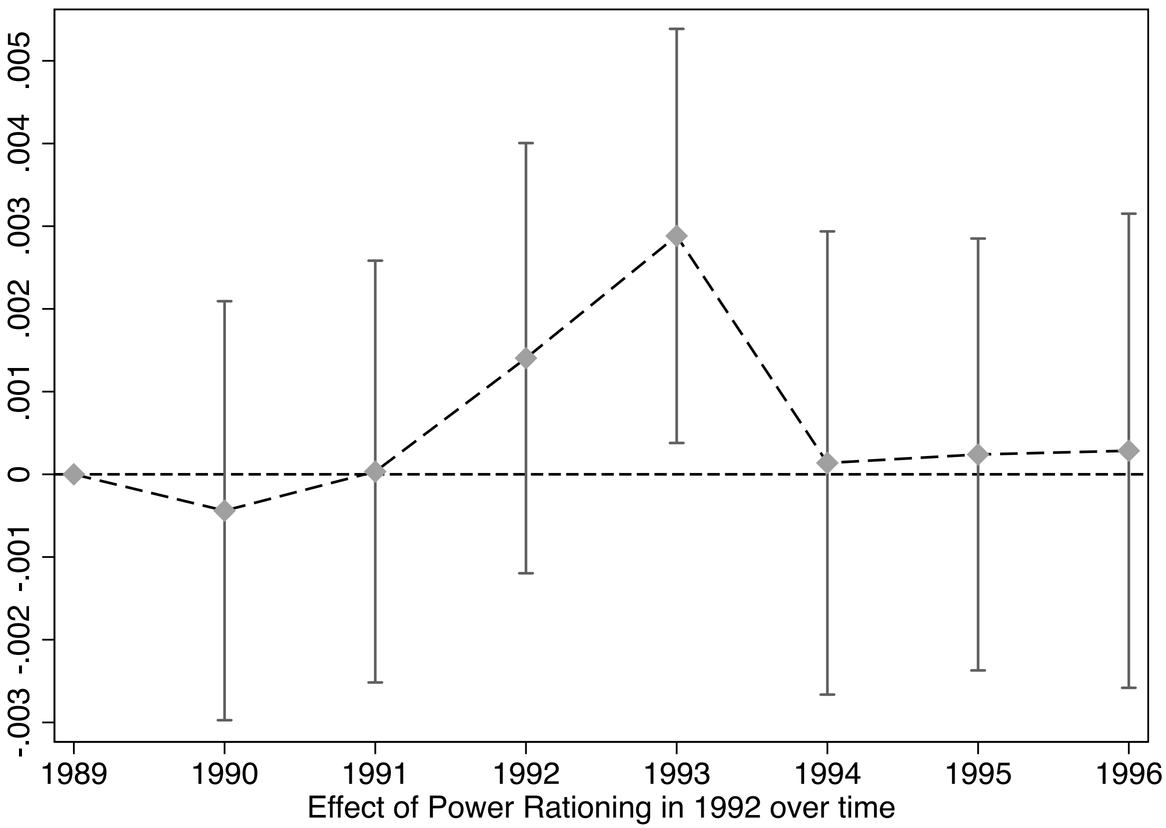

Figure 5: Figure presents results from a regression studying the effect of power rationing on the probability of a mother giving birth, conditioning on mother fixed effects and region by year fixed effects. 95% confidence intervals obtained from clustering standard errors at the municipality level are indicated. The results clearly indicate that 1993 was an exceptional year in terms of fertility. There is a weak, but insignificant uptick in fertility in 1992, which is not surprising since the last two months of 1992 were already treated.

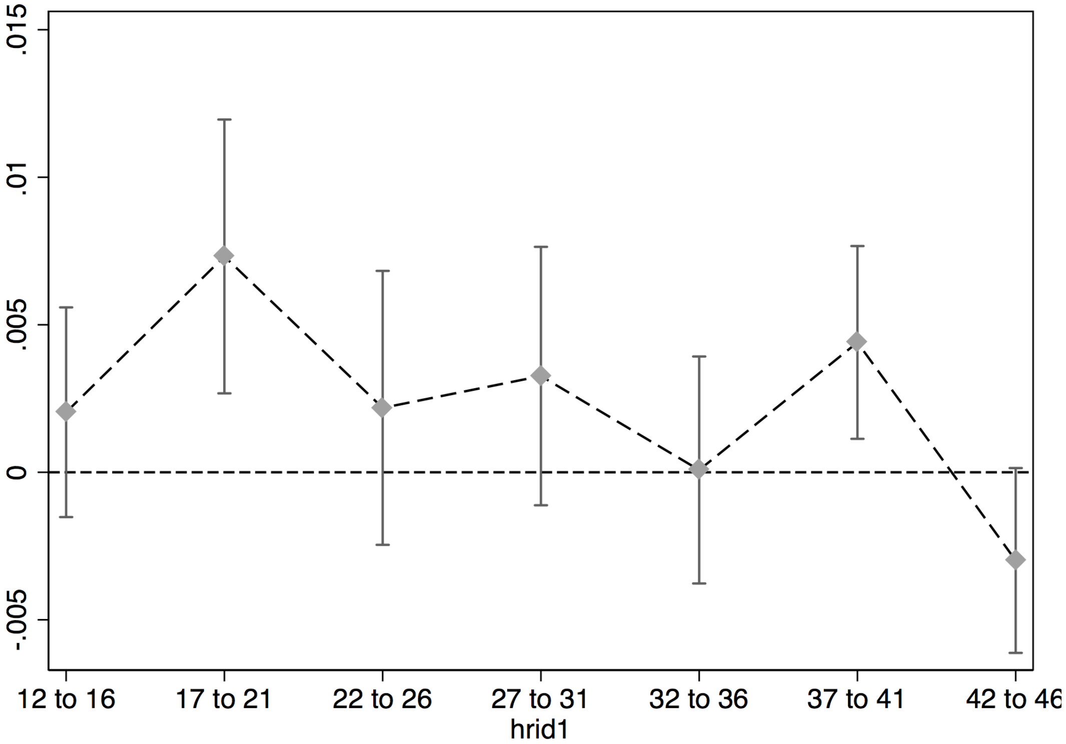

Figure 6: Figure presents results from a regression studying the effect of power rationing intensity on the probability of a mother giving birth in 1993 by the age of mothers in 1992, while controlling for mother and region by age group year fixed effects. 95% confidence bands obtained form clustering at the municipality level are indicated. The positive fertility effect is driven by adolescents between the ages of 17 - 21 and by older women, between the ages of 37- 41.

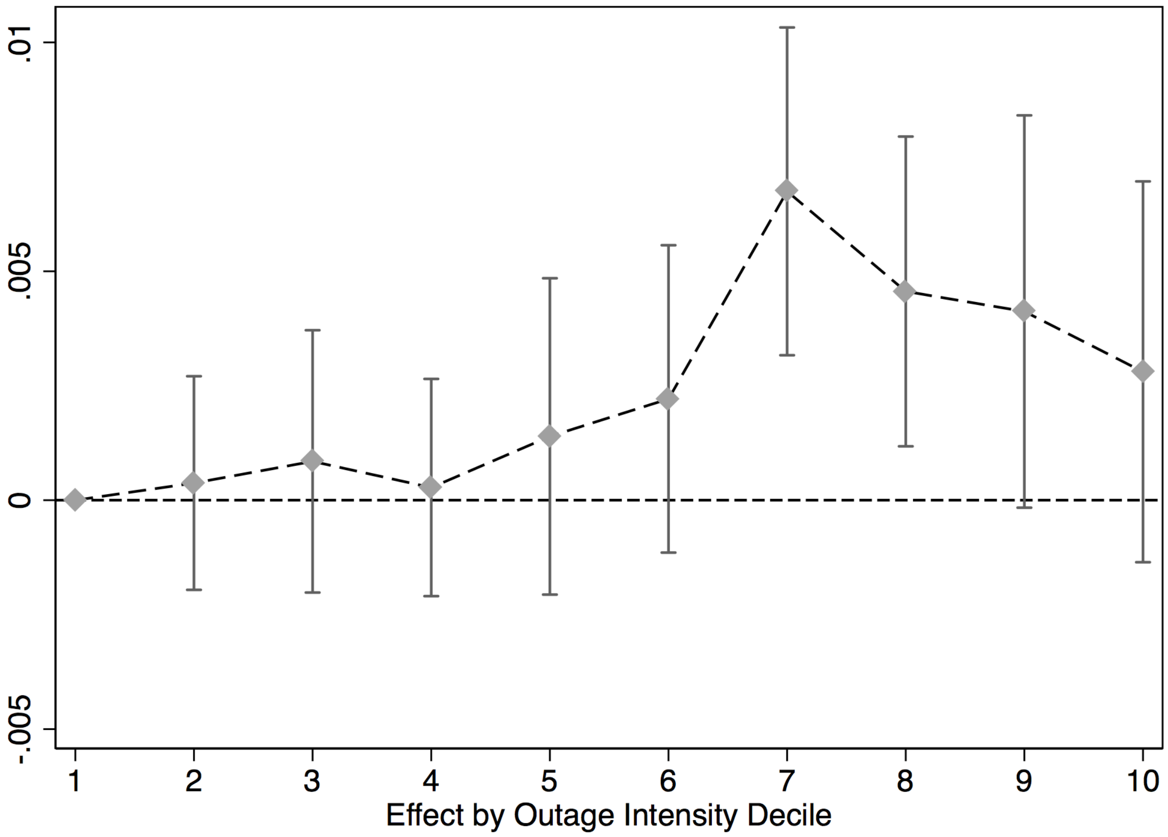

Figure 7: Figure presents results from a regression studying the heterogenous effect of power rationing intensity by decile on the probability of a mother giving birth in 1993, while controlling for mother and year fixed effects. The effect is driven by municipalities with power rationing above median. 95% confidence bands obtained form clustering at the municipality level are indicated.

Tables for the Main Text

Table 1: The Impact of Power Outage Intensity on Birth Probability

	Different Fixed Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Power Outage	0.359***	0.359***	0.359***	0.359***	0.297***	.289***
	(0.086)	(0.086)	(0.086)	(0.086)	(0.099)	(.099)
Municipality FE		Yes	Yes	Yes		Yes
Year FE			Yes			
Mother FE				Yes	Yes	Yes
Region x Year FE					Yes	
Municipality Trends						Yes
Mean Birthrate	.0789	.0789	.0789	.0789	.0789	.0789
Women	457312	457312	457312	457312	457312	457312
Observations	3658496	3658496	3658496	3658496	3658496	3658496
Clusters	515	515	515	515	515	515

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Standard errors in the parentheses are clustered at the municipality level. The dependent variable is an indicator variable equal to one, in case the mother experiences a birth in a given year. Power Outage is a dummy variable that is equal to 1 if a municipality experienced above median power rationing between 1992/1993. Treated is an indicator that is equal to 1 for the year 1993. The coefficients are multiplied by 100 for better exposition .

Table 2: Robustness of Short Run Fertility Effect

	Base	Migration		Controls				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Power Outage	0.297***	0.252*	0.232*	0.303***	0.265**	0.297***	0.227**	0.236**
	(0.099)	(0.135)	(0.133)	(0.099)	(0.106)	(0.100)	(0.101)	(0.108)
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls				Conflict	Political	Economic	Geographic	All
Mean Birthrate	.0789	.078	.081	.0789	.0789	.0789	.0789	.0789
Women	457312	237671	244449	457312	457312	454900	454900	454900
Observations	3658496	1901368	1955592	3658496	3658496	3639200	3639200	3639200
Clusters	515	515	515	515	515	512	512	512

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Standard errors in the parentheses are clustered at the municipality level. The dependent variable is an indicator variable equal to one, in case the mother experiences a birth in a given year. Power Outage is a dummy variable that is equal to 1 if a municipality experienced above median power rationing between 1992/1993. Treated is an indicator that is equal to 1 for the year 1993. The coefficients are multiplied by 100. Column (1) presents the baseline results. Columns (2) restrict the analysis to women whose location of residence in 2005 is the same as the location of birth. Column (3) restricts the sample to mothers who report to have had their last birth prior to 1993 in the same municipality as they report to be living in. Columns (4) - (8) add different municipality level control variables. Column (4) controls for time varying measures of conflict capturing guerilla or paramilitary presence in a municipality. Column (5) controls for whether a municipality is the head of a departmental government and ads region by year fixed effects. Column (6) controls for municipal tax revenues as a proxy for income, a set of dummies for capturing whether a municipality is an oil, coca and opium producer, land inequality as of 1985 and year fixed effects. Column (7) controls for geographic covariates capturing altitude, distance to capital, soil erosion, agricultural suitability and year fixed effects. Column (8) combines all the control variables discussed in columns (4) - (7).

Table 3: Long Run Fertility and Birth Spacing Effects of Power Rationing

Panel A: Long run fertility effect

	(1)	(2)	(3)	(4)	(5)
Treat x Power Outage	0.070***	0.070***	0.070***	0.067***	0.072**
	(0.027)	(0.025)	(0.025)	(0.026)	(0.028)
Municipality FE		Yes	Yes		
Birth Year Cohort FE			Yes		
Municipality x Birth Year FE				Yes	Yes
Region x Treatment					Yes
Mean Number of Births post 1992	2.81	2.81	2.81	2.81	2.81
Women	62603	62603	62603	62603	62603
Clusters	515	515	515	515	515

Panel B: Birth Spacing Effect

	(1)	(2)	(3)	(4)	(5)
Treat x Power Outage	-1.226	-1.404*	-1.740**	-1.820**	-2.094**
	(0.821)	(0.827)	(0.707)	(0.774)	(0.857)
Municipality FE		Yes	Yes		
Birth Year Cohort FE			Yes		
Municipality x Birth Year FE				Yes	Yes
Region x Treatment					Yes
Average Months Between Births	48.1	48.1	48.1	48.1	48.1
Women	32453	32453	32453	32453	32453
Clusters	515	515	515	515	515

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Standard errors in the parentheses are clustered at the municipality level. Power Outage is a dummy variable that is equal to 1 if a municipality experienced above median power rationing between 1992/1993. Treated is an indicator that is equal to 1 for births occurring in the treatment time window as defined in Figure 4. The dependent variable in Panel A is the total number of births since 1990. The dependent variable in Panel B is the time gap in months since the last birth, thus restricting the analysis to the set of mothers in the treatment and control group who had already a child prior to treatment.

Table 4: Long Run Socioeconomic Outcomes for the Mother

	Fertility		Socio Economic Outcomes					
	(1) Births	(2) Spacing	(3) Dropout	(4) Separated	(5) Single Mother	(6) Assets	(7) Car	(8) Inactive
Panel A: Main results								
Treat x Power Outage	0.072**	-2.094**	0.041***	0.011*	0.020**	-0.049**	-0.010*	0.018*
	(0.028)	(0.857)	(0.014)	(0.007)	(0.008)	(0.023)	(0.006)	(0.009)
Municipality x Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region x Treatment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dependent Variable	2.81	48.1	.421	.099	.287	1.97	.112	.597
Women	62603	32453	34256	62168	62603	61399	61670	62603
Clusters	515	515	515	515	515	515	515	515
Panel B: Placebo Treatment assignment								
Placebo x Power Outage	-0.032	-0.619	-0.018	0.001	-0.003	0.006	0.000	0.001
	(0.027)	(0.937)	(0.015)	(0.006)	(0.008)	(0.020)	(0.006)	(0.010)
Municipality x Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region x Treatment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dependent Variable	2.81	47.3	.432	.103	.289	1.98	.113	.596
Women	58947	29053	31548	58518	58947	57511	57736	58947
Clusters	515	515	515	515	515	515	515	515
Panel C: Placebo Treatment intensity								
Treated x Placebo Power Outage	0.017	0.485	0.004	0.004	0.016*	-0.029	0.003	-0.005
	(0.031)	(0.909)	(0.014)	(0.008)	(0.008)	(0.024)	(0.007)	(0.010)
Municipality x Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region x Treatment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dependent Variable	2.8	48.1	.421	.0992	.287	1.97	.113	.596
Women	62409	32330	34183	61993	62409	61212	61477	62409
Clusters	512	512	512	512	512	512	512	512

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Standard errors in the parentheses are clustered at the municipality level. Panel A presents the main results how power rationing affects socio economic outcomes for the mothers who conceived a child during the outage, compared to the set of mothers who delivered a baby during the outage. Panel B is a placebo treatment, moving the treatment and control group one year ahead of time. Panel C uses the treatment definition as in Panel A, but uses as placebo power outage measure the above median year on year change in light emissions between 1993/1994, when there was no outage. The dependent variables are indicated in the column heads.

Table 5: Long Run Sample Balance Table

	Control		Treatment		P-val
	Mean	N	Mean	N	p
Age in 1992	39.32	22347.00	38.40	40256.00	<0.01
Power Outage Intensity	0.26	22347.00	0.27	40256.00	0.29
Lights in 1992	6.79	22347.00	6.67	40256.00	0.13
Placebo Outage Intensity	-0.00	22280.00	-0.01	40129.00	0.06
Births prior to 1992	1.56	22008.00	1.48	39766.00	<0.01
Disabled?	0.05	22347.00	0.05	40256.00	0.92
Literate?	0.94	22347.00	0.94	40256.00	0.84
Ethnic Minority	0.13	22347.00	0.13	40256.00	0.13
Internal Migrant?	0.45	22347.00	0.46	40256.00	0.41
Any Child Died?	0.08	22347.00	0.08	40256.00	0.56
Total Number of Children Born	2.79	22347.00	2.82	40256.00	0.06
Primary school completed	0.75	22347.00	0.75	40256.00	0.62
Secondary school dropout	0.42	12135.00	0.42	22121.00	0.82
Some University?	0.22	7039.00	0.23	12801.00	0.40
Divorced / separated	0.10	22195.00	0.10	39973.00	0.05
Single Mother Household	0.15	22347.00	0.15	40256.00	0.37
hhasset	1.98	21818.00	1.96	39581.00	0.03
Inactive in Labor Market	0.59	22347.00	0.60	40256.00	0.22

Source: Table presents the simple averages and the p-values of the difference in means of the variables between the treatment and control group as defined in Figure 4.

Table 6: Selection into Treatment

	Predetermined Mother Characteristics					
	(1) Prior births	(2) Minority	(3) Literate	(4) Disabled	(5) Migrant	(6) Any Child Mortality
Treat x Power Outage	0.009	-0.006	-0.001	-0.002	-0.014	0.003
	(0.031)	(0.005)	(0.005)	(0.005)	(0.010)	(0.006)
Municipality x Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region x Treatment	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dependent Variable	1.51	.129	.94	.0493	.456	.0776
Women	61774	62603	62603	62603	62603	62603
Clusters	515	515	515	515	515	515

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Standard errors in the parentheses are clustered at the municipality level. The Table tests for selection into treatment, studying mother level characteristics who are (likely) to be predetermined at the time of treatment. The results suggest that none of the predetermined characteristics vary systematically with the power outage measure between the set of treatment and control mothers.

A Online Appendix

The online appendix for “More than an Urban Legend: The long-term socio-economic effects of unplanned fertility shocks” provides additional information on the construction of variables and data sets, further results and figures.

A.1 Census Data and Birth History

This section describes how we construct the data set used in the main body of the paper. Throughout, we work with the IPUMS public use census file from the General Census 2005 (XVII of Population and Dwelling and VI of Housing), collected by the Departmento Administrativo Nacional de Estadstica (DANE). The sample population was the entire population of the country, including all households and dwellings. The data was collected between May 2005 and February 2006, with interruptions for vacation periods between June and July 2005 and between December 2005 and January 2006. IPUMS provides a micro-data sample covering approximately 10% of the overall population. The respondents in the survey were persons over twelve years old who are habitual residents. In the case of the person not being present and not being able to be interviewed, the information should be given by the head of household (male or female), or his/her spouse or a resident person who is over fifteen years old and who knows the information about the person.

The geographic resolution is a municipality with 20,000+ population. Smaller municipalities were merged with larger ones for confidentiality protection. A household is defined as a person or group of people, related or not, who occupy all or part of a dwelling; attend to basic needs charged to a common budget, and they generally share food.

For each household member inside a dwelling, the data contains a list of household members. Children living in the household in 2005 are linked to their parents, in case their parents are part of the households. This allows us to reconstruct the birth histories of a mother, who still lives with their children in the same households. The link does not distinguish between biological parents or non biological parents. The relationships within a household of individual members are defined by the household members relationship to the head of a household, which, technically, could be any individual.

The whole dataset contains information on 2,327,228 individuals living in urban areas. The following sequence of steps describes how we arrive at a mother level panel data set used throughout.[27]

1. We create a set of 457,312 potential mothers, which includes all women born between 1948 and 1978, which results in women being between 15 and 45 years of age in 1993, when the bulk of children conceived in 1992 would be born. Each women is identified by a household identifier as well as a person number, within the household. In the same way, partners within a household are linked to one another, through their person number within the household. Lastly, each child is linked to its mother within a household through the person identifier of the mother.

2. In the second step, we map each potential mother to their cohabiting partner or spouse, in case one is present in a household. For the 469,855 potential mothers, 277,973 are linked to a partner within a household. The unmatched set of 191,882 women contains single mother/ single person households (58,434) or women, who have no partner in 2005 and live in an extended family (82,030).

3. In the third step, we merge the full remainder census dataset of 1,579,400 individuals to the set of 457,312 potential mothers to obtain matches between mothers and children. This results in 745,691 children being matched to 346,507 birth mothers. We can use this, to compare the extent to which we are able to map out a mothers' entire birth history. The raw data provides, for every female, the total number of children, she has ever given birth to up to the census date. We can compare this number, to the number of children that the mother gave birth too, which still live in the same household in 2005. For 55.7% (275,188) of the 346,507 matched birth mothers, we have reconstructed their entire

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[27] For more detailed information about the census, see `https://international.ipums.org/international/sample_designs/sample_designs_co.shtml`.

birth history. Naturally, women, for which we are unable to map out their birth history are, on average, older.

4. Since, for almost every person, we know their birth year and birth month, in the fourth step, we construct an additional measures used in the analysis. We compute, within mother, for each birth the (approximate) number of months since the most recent previous birth. This measure is bound to be a noisy measure and is only precise for women, where we observe the entire birth history in our sample (i.e. women, who in 2005 still live with all their children in the same household).

5. Using the set of 457,312 candidate mothers, we construct a balanced mother level panel covering the period from 1989 to 1996. This time frame is chosen, since children born in 1989 were, were between 16/17 in the census years 2005/2006. Since children typically live with their mothers and 18 years is a common age at which children leave a household, this sample time frame ensures that our reconstructed birth histories based on children living with their mothers, is most likely, the complete birth history for that sample time period.

The resulting dataset is a balanced panel with 3,658,496 observations.

A.2 Night Time Lights Power Outage Measures

We construct a measure of power rationing from outer space, using satellite measured night time light emissions. We use night light emission data collected from the United States Air Force Defense Meteorological Satellite Program (DMSP). These satellites have been carrying an Operational Linescan System (OLS) sensor, which can be used to detect natural light emissions from the earth. The primary aim of the sensors is to observe low intensity light emissions stemming from lunar light reflectance. The satellites were not designed to map human light emissions, yet in particular the processed stable lights product have been shown to correlate extremely well with measures of economic development, incomes, electrification rates and urbanization (see

Michalopoulos and Papaioannou, 2013; Henderson et al., 2012; Jiang et al., 2014; Deichmann et al., 2014. Night lights data is appealing, because it provides consistent data over a long time period on human activity in contexts, where primary data is not widely available. The satellites have been carrying the OLS sensors since the 1970s, a digital archive of the pictures is only available from 1992 onwards. The DMSP satellites are orbiting the earth 14 times per day. This ensures that for each location on the globe there exists a daily picture taken between 8:30 and 10:00 pm local time. The satellites are regularly replaced every three to four years; the sensors on older satellite deteriorate, which makes it challenging to compare new with old satellites. Throughout, we use the images derived from readings from the F12 satellite, providing data for the years 1992- 1994.

The raw data is processed at the Earth Observatory Group at the National Oceanic and Atmospheric Administration. provides four different products. An unfiltered image, containing the average digital number value per pixel; a stable lights layer, which contains lights from cities, towns and other places with persistent light emissions; ephemeral lights have been filtered. An additional layer provides, per pixel, the number of cloud-free images taken for each pixel. Lastly, there is a third layer that multiplies the average unfiltered digital number with a percentage frequency of light detection. Throughout, we work with the stable lights layer, as it is the cleanest in providing stable light emissions. In particular, forest fires or gas flares and systematic distortions due to the varying lunar intensity as well as late sun-sets during summer or winter for the northern- and southern hemispheres are removed from the stable lights product. The result is supposed to capture light emissions from human settlements; this is measured on a digital scale between 0 and 63, where 0 stands for no light emissions and 63 is the maximal value, which is top-coded. The pixel resolution is 30 arc-seconds or about 0.86 square kilometers at the equator.

Since the nightlight data series is only available from 1992 onward, we can not compare the light intensity in the year 1992 (the year with most extensive rationing) with previous years. Nevertheless, we can measure the changes between the years 1992 and 1993, comparing this change to subsequent years. If 1992 was a year with very low night light emissions, then the year on year change between 1992 and 1993 is a good proxy for the extent of rationing.

Hence, we construct a simple measure that, with several refinements is computed using the following formula:

$$O_m = 100 \times \left(1 - \frac{Lights_{1992}}{Lights_{1993}}\right)$$

To illustrate, if a pixel was unlit in 1992 and lit with any positive value in 1993, this measure would have a value of 100%, capturing the observed lack of any light emissions. Alternatively, if the pixel was lit with a digital number 10 in 1992, while it was lit with a digital number of 20 in 1993, the measure would have a value of 50%, capturing the fact that a pixel emitted only half the amount of light. A look at aggregate figures is indicative of the extent of power rationing across Colombia in 1992.

For the main measure, we weight the data by initial 1990 municipality level population. The weighting by population becomes necessary as the IPUMS data merges several municipalities that have population sizes less than 20,000 to ensure that users of the data are not able to reverse-engineer who the individuals in the sample were. This makes the treatment intensity construction more tedious and less clean, but does not represent a significant issue. In total we are left with 515 municipalities that have population above 20,000.

Unfortunately, the night light data is suffering from measurement error. This measurement error has multiple sources.

1. Satellite images are not taken at the same point in time every night, but only in roughly the same time window in the evening. Clearly, dawn hours may have different light emission patterns compared to later evenings.

2. Images are taken at different angles depending on the position of the satellite to the earth, which results in different over-glow patterns.

3. While ephermeral lights are supposed to be removed, there is still a

chance that non stable light sources (like fires, gas flaring) contaminates the images.

4. Cloud cover distorts or renders images useless, resulting in few data points for some locations.

Due to significant noise in the year on year changes in light emissions, throughout the main part of the paper, we present the results based on a dummified outage measure, which captures whether power rationing is above median across the estimating sample. The median power rationing is around 20%.

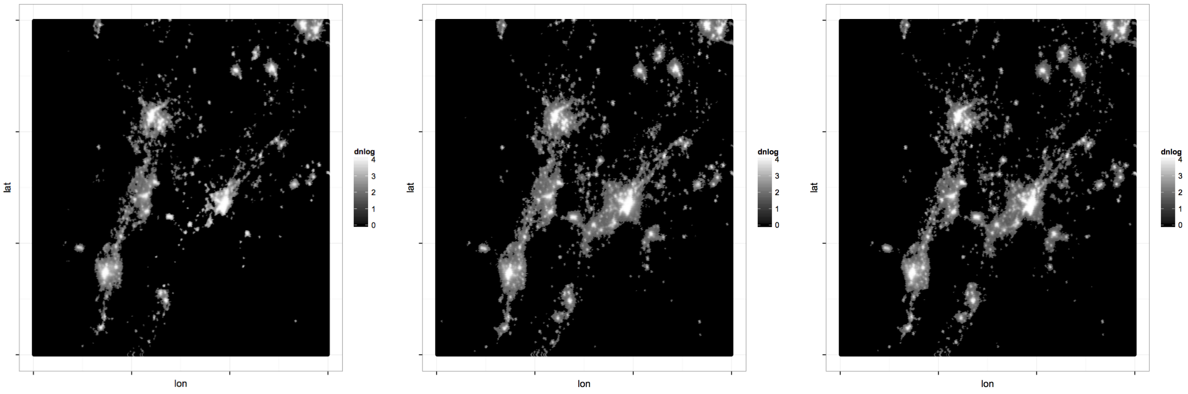


Figure A1: Light Intensity in Central Colombia, 1992 (left), 1993 (center) and 1994 (right)

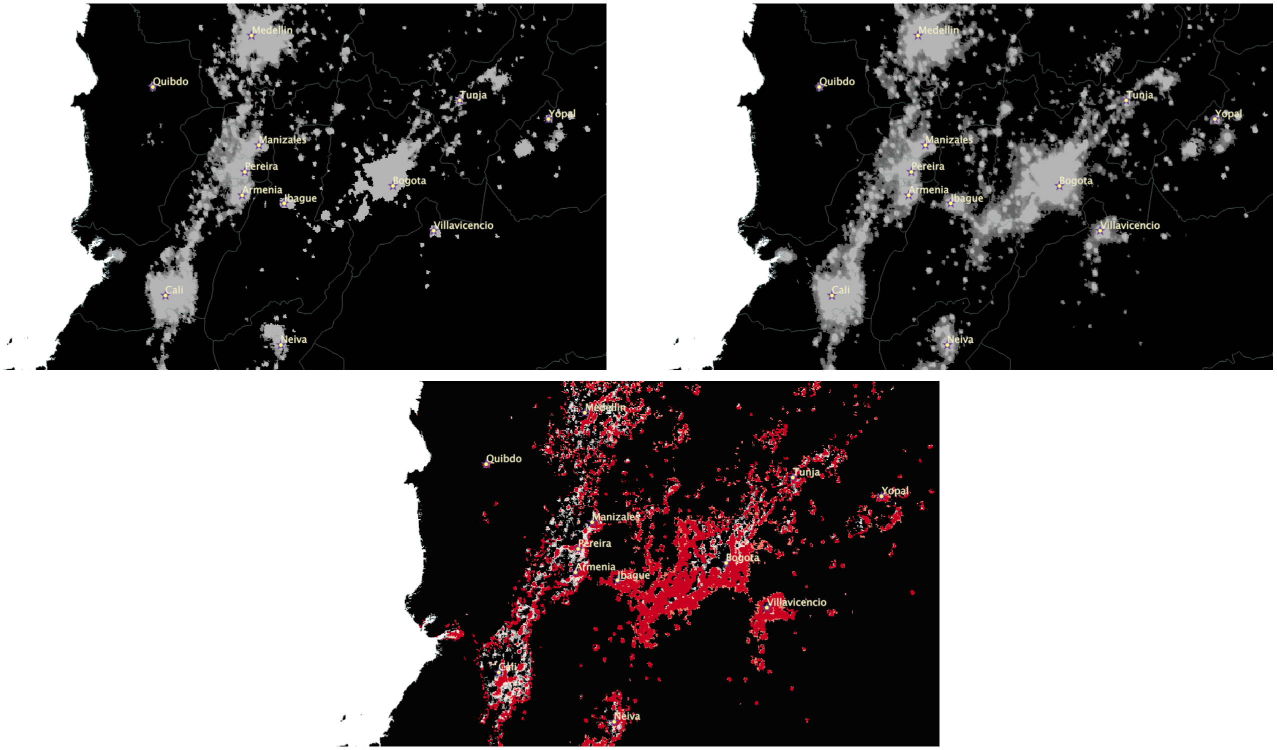


Figure A2: The images above display the light intensity around Bogota in 1992 (left) versus light intensity around Bogota in 1993 (right). The image in the center displays pixels that went from having some light in 1992 to no light in 1993 (i.e completely dark). This is marked by the color red.

A.3 Additional Figures

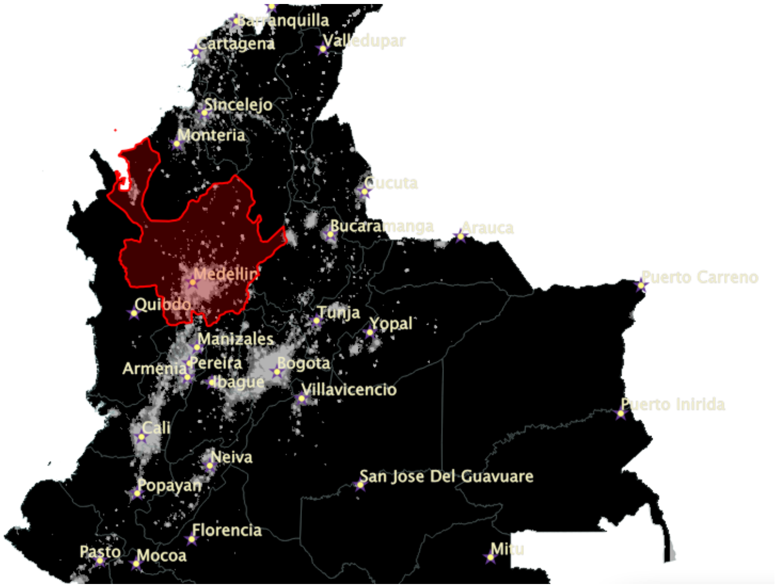
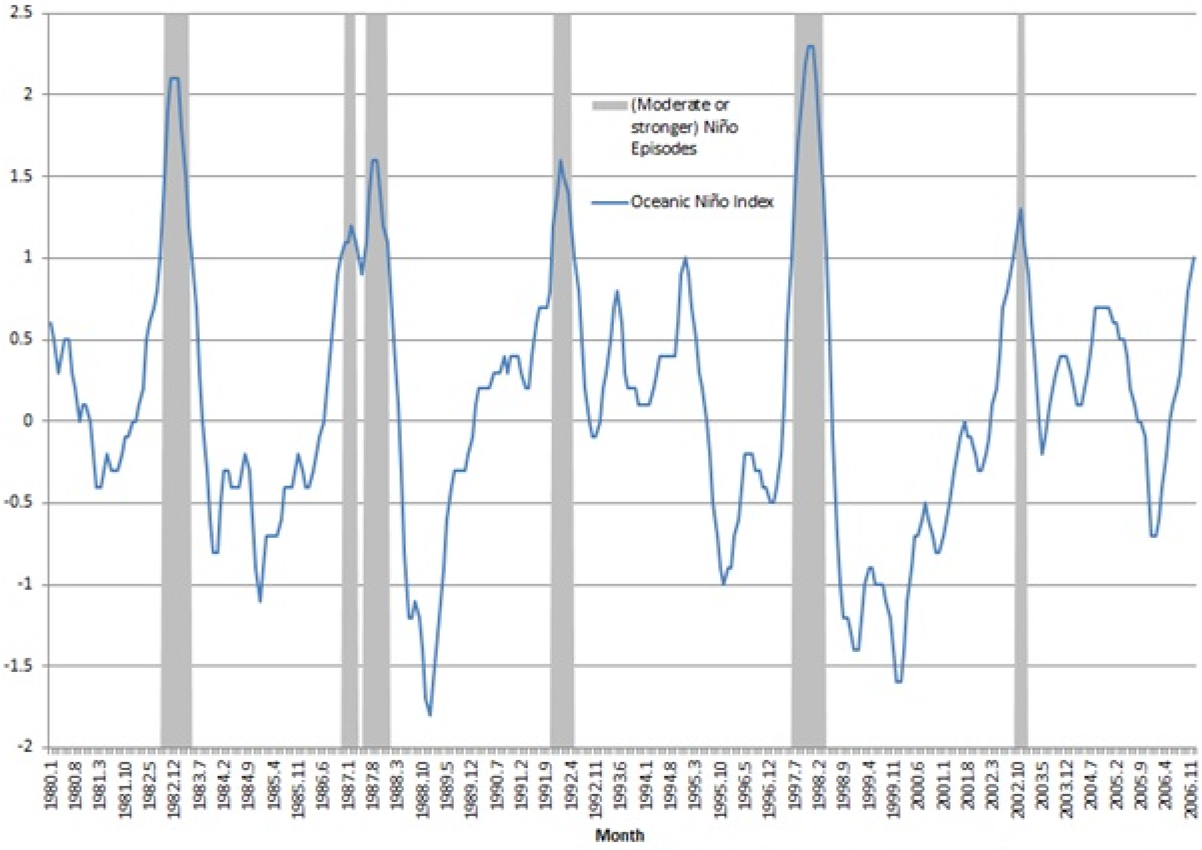


Figure A3: Colombia Administrative Regions, Night Lights Emissions in 1992 and Provincial Capital Cities. Antioquia department, where around 25% of the hydro electric power generation capacity is located is highlighted.



Source: Golden Gate Weather Services http://ggweather.com/enso/oni.htm

Figure A4: Oceanic Niño Index

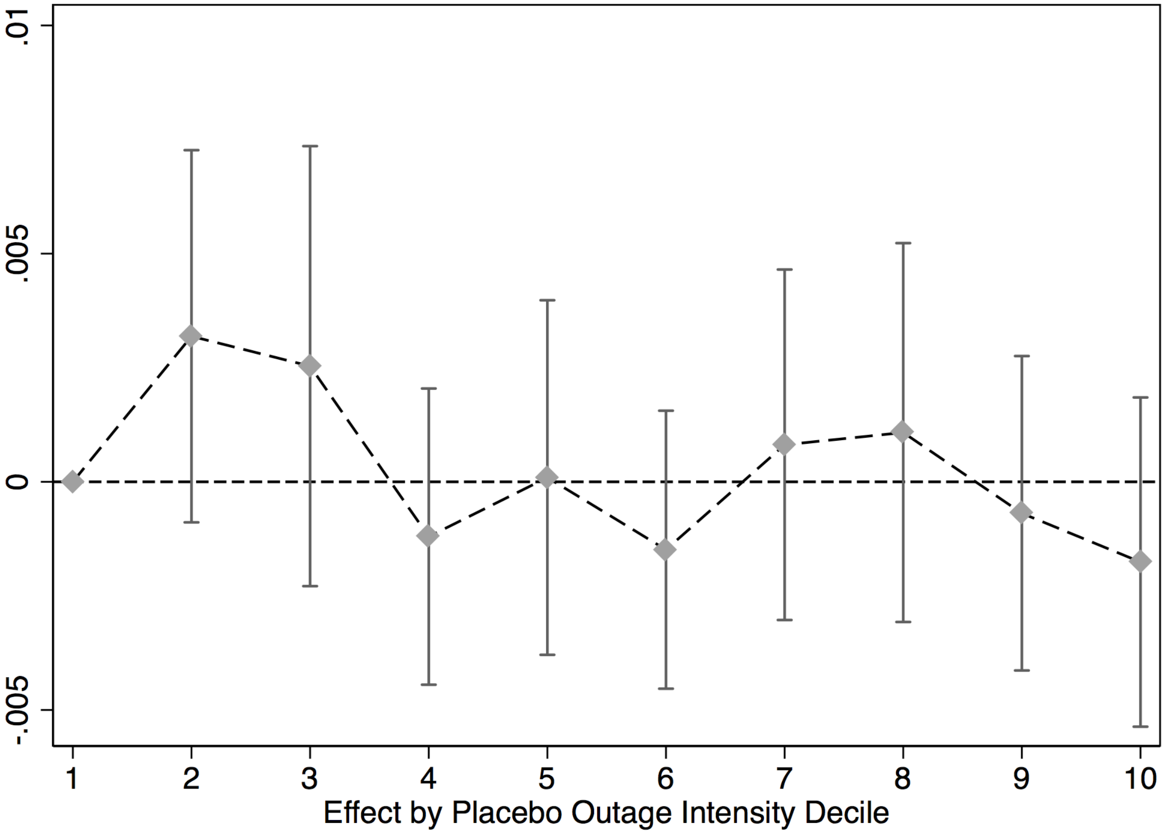


Figure A5: Figure presents results from a regression studying the effect of the placebo power rationing intensity by decile on the probability of a mother giving birth in 1993, while controlling for mother and year fixed effects. The effect is driven by municipalities with power rationing above median. 95% confidence bands obtained form clustering at the municipality level are indicated.

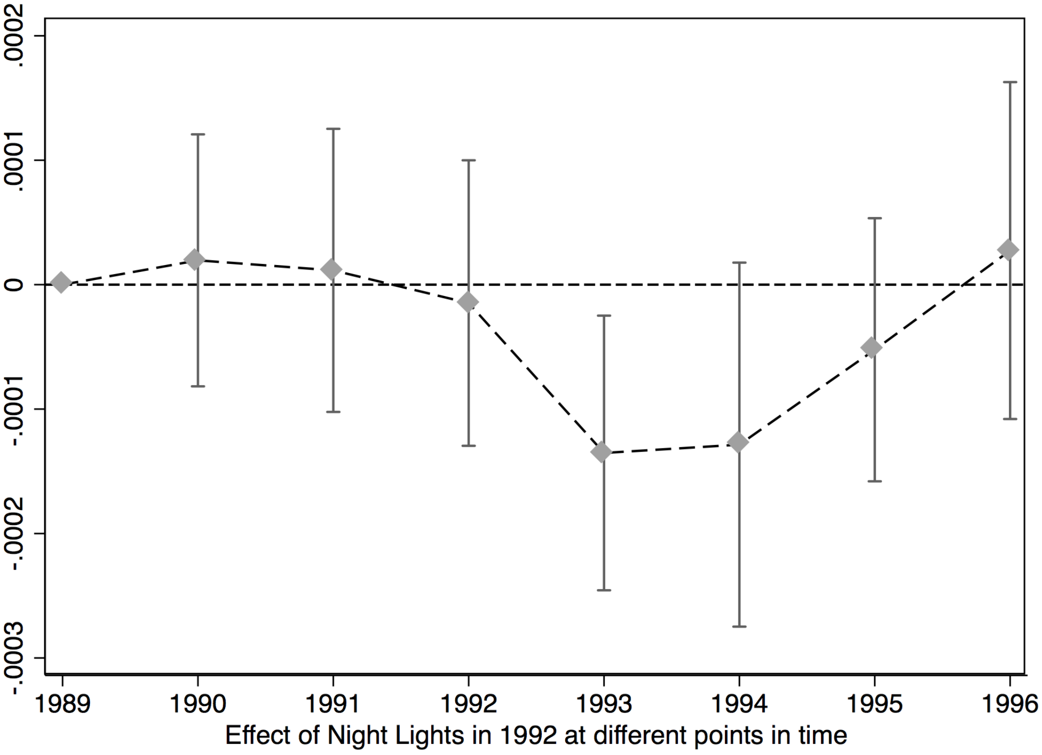


Figure A6: Figure presents results from a regression studying the effect of night light emissions in 1992 flexibly affecting the probability of a mother giving birth in the years from 1989 to 1996, controlling for mother and region by year fixed effects. The result indicate that 1993 saw a drop in fertility rates in places that were relatively more lit in 1992, compared to places that were unlit.

A.4 Additional Tables

Source: Table presents the simple averages and the p-values of the difference in means of the variables between the treatment and control group as defined in Figure 4.

Table A2: Summary of key events

Date	Event
Jan 92	The *Oceanic Niño Index* peaks. Water sources for hydroelectric power depleting.
28th Feb 92	Power rationing is announced.
2nd Mar 92	Power rationing starts.
Apr 92	The government starts to implement reforms to stabilize long-run electricity supply.
1st Apr 93	Power rationing ends.
Oct 97	El Niño returns, but this time there is no need for power rationing.

B Additional Tables

Table A3: Continuous Power Outage Measure: Impact of Power Outage Intensity on Birth Probability

	Different Fixed Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Power Outage Intensity	0.586***	0.586***	0.586***	0.586***	0.468**	.448**
	(0.172)	(0.172)	(0.172)	(0.172)	(0.197)	(0.196)
Municipality FE		Yes	Yes	Yes		Yes
Year FE			Yes			
Mother FE				Yes	Yes	Yes
Region x Year FE					Yes	
Municipality Trends						Yes
Mean Birthrate	.0789	.0789	.0789	.0789	.0789	.0789
Women	457312	457312	457312	457312	457312	457312
Observations	3658496	3658496	3658496	3658496	3658496	3658496
Clusters	515	515	515	515	515	515

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Standard errors in the parentheses are clustered at the municipality level. Outage x Intensity measures the proportional change in municipality-level luminosity between 1992 and 1993. The dependent variable is an indicator variable equal to one, in case the mother experiences a birth in a given year. Note that the municipality fixed effects are perfectly collinear with the mother fixed effects in specifications (4) - (6). The coefficients are multiplied by 100.

Table A4: Continuous Power Outage Measure: Robustness of Short Run Fertility Effect

	Base	Migration		Controls			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated x Power Outage Intensity	0.468**		0.301	0.473**	0.462**	0.288	0.266
	(0.197)		(0.250)	(0.196)	(0.201)	(0.210)	(0.214)
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls				Conflict	Economic	Geographic	All
Mean Birthrate	.0789	.078	.081	.0789	.0789	.0789	.0789
Women		237671	244449	457312	454900	454900	454900
Observations	3658496	1901368	1955592	3658496	3639200	3639200	3639200
Clusters	515	515	515	515	512	512	512

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Standard errors in the parentheses are clustered at the municipality level. Outage x Intensity measures the proportional change in municipality-level luminosity between 1992 and 1993. The dependent variable is an indicator variable equal to one, in case the mother experiences a birth in a given year. Note that the municipality fixed effects are perfectly collinear with the mother fixed effects in specifications (4) - (6). The coefficients are multiplied by 100.

Table A5: Continuous Power Outage Measure: Long Run Fertility and Birth Spacing Effects of Power Rationing

Panel A: Long run fertility effect	(1)	(2)	(3)	(4)	(5)
Treat x Power Outage Intensity	0.140**	0.137***	0.139***	0.116**	0.137**
	(0.055)	(0.052)	(0.051)	(0.051)	(0.057)
Municipality FE		Yes	Yes		
Birth Year Cohort FE			Yes		
Municipality x Birth Year FE				Yes	Yes
Region x Treatment					Yes
Mean Number of Births	2.81	2.81	2.81	2.81	2.81
Women	62603	62603	62603	62603	62603
Clusters	515	515	515	515	515

Panel B: Birth Spacing Effect	(1)	(2)	(3)	(4)	(5)
Treat x Power Outage Intensity	-1.479	-1.384	-1.967	-1.170	-1.687
	(1.618)	(1.609)	(1.367)	(1.610)	(1.785)
Municipality FE		Yes	Yes		
Birth Year Cohort FE			Yes		
Municipality x Birth Year FE				Yes	Yes
Region x Treatment					Yes
Average Months Between Births	48.1	48.1	48.1	48.1	48.1
Women	32453	32453	32453	32453	32453
Clusters	515	515	515	515	515

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Standard errors in the parentheses are clustered at the municipality level. Outage x Intensity measures the proportional change in municipality-level luminosity between 1992 and 1993. The dependent variable is an indicator variable equal to one, in case the mother experiences a birth in a given year. Note that the municipality fixed effects are perfectly collinear with the mother fixed effects in specifications (4) - (6). The coefficients are multiplied by 100.

Table A6: Continuous Power Outage Measure: The Impact of Power Outage Intensity on Birth Probability

	Fertility		Socio Economic Outcomes					
	(1) Births	(2) Spacing	(3) Dropout	(4) Separated	(5) Single Mother	(6) Assets	(7) Car	(8) Inactive
Panel A: Main results								
Treat x Power Outage	0.137**	-1.687	0.072**	0.026*	0.034**	-0.083*	-0.010	0.036**
	(0.057)	(1.785)	(0.033)	(0.014)	(0.017)	(0.049)	(0.011)	(0.018)
Municipality x Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region x Treatment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dependent Variable	2.81	48.1	.421	.099	.287	1.97	.112	.597
Women	62603	32453	34256	62168	62603	61399	61670	62603
Clusters	515	515	515	515	515	515	515	515
Panel B: Placebo Treatment assignment								
Placebo x Power Outage	0.043	-0.709	-0.038	0.001	-0.006	-0.030	0.001	-0.002
	(0.060)	(1.936)	(0.035)	(0.011)	(0.019)	(0.042)	(0.011)	(0.020)
Municipality x Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region x Treatment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dependent Variable	2.81	47.3	.432	.103	.289	1.98	.113	.596
Women	58947	29053	31548	58518	58947	57511	57736	58947
Clusters	515	515	515	515	515	515	515	515
Panel C: Placebo Treatment intensity								
Treated x Placebo Power Outage	0.016	-2.299	-0.008	-0.003	0.008	-0.027	0.020	0.000
	(0.085)	(2.200)	(0.042)	(0.015)	(0.021)	(0.068)	(0.013)	(0.024)
Municipality x Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region x Treatment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dependent Variable	2.8	48.1	.421	.0992	.287	1.97	.113	.596
Women	62409	32330	34183	61993	62409	61212	61477	62409
Clusters	512	512	512	512	512	512	512	512

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Standard errors in the parentheses are clustered at the municipality level. Outage x Intensity measures the proportional change in municipality-level luminosity between 1992 and 1993. The dependent variable is an indicator variable equal to one, in case the mother experiences a birth in a given year. Note that the municipality fixed effects are perfectly collinear with the mother fixed effects in specifications (4) - (6). The coefficients are multiplied by 100.

Table A7: Census Measure of Total Children Born: Long Run Fertility and Birth Spacing Effects of Power Rationing

Panel A: Long run fertility effect

	(1)	(2)	(3)	(4)	(5)
Treat x Power Outage	0.056	0.053	0.043	0.041	0.046
	(0.035)	(0.032)	(0.031)	(0.033)	(0.036)
Municipality FE		Yes	Yes		
Birth Year Cohort FE			Yes		
Municipality x Birth Year FE				Yes	Yes
Region x Treatment					Yes
Mean Number of Births	3.49	3.49	3.49	3.49	3.49
Women	61774	61774	61774	61774	61774
Clusters	515	515	515	515	515

Panel B: Birth Spacing Effect

	(1)	(2)	(3)	(4)	(5)
Treat x Power Outage	-1.226	-1.404*	-1.740**	-1.820**	-2.094**
	(0.821)	(0.827)	(0.707)	(0.774)	(0.857)
Municipality FE		Yes	Yes		
Birth Year Cohort FE			Yes		
Municipality x Birth Year FE				Yes	Yes
Region x Treatment					Yes
Average Months Between Births	48.1	48.1	48.1	48.1	48.1
Women	32453	32453	32453	32453	32453
Clusters	515	515	515	515	515

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Standard errors in the parentheses are clustered at the municipality level. Outage x Intensity measures the proportional change in municipality-level luminosity between 1992 and 1993. The dependent variable is an indicator variable equal to one, in case the mother experiences a birth in a given year. Note that the municipality fixed effects are perfectly collinear with the mother fixed effects in specifications (4) - (6). The coefficients are multiplied by 100.