Agricultural Fires and Health at Birth*

MARCOS A. RANGEL
Duke University
and BREAD

TOM S. VOGL
Princeton University
BREAD and NBER

June 2017

Abstract

Fire has long served as a tool in agriculture, but the practice's inherent link with economic activity has made its human capital consequences difficult to study. Drawing on data from satellites, air monitors, and vital records, we study how *in utero* exposure to smoke from sugarcane harvest fires affects health at birth in the Brazilian state that produces one-fifth of the world's sugarcane. We exploit daily changes in both fire location and prevailing wind direction for identification, finding that late-pregnancy exposure to upwind fires decreases birth weight, gestational length, and *in utero* survival, but not early neonatal survival. Other fires positively predict health, highlighting the importance of disentangling pollution from the economic activity that drives it.

*We thank seminar and conference participants at CPC/DuPRI, Duke Economics, Duke's Sanford School of Public Policy, Fordham, Georgia State, IFPRI, Princeton, PAA for helpful comments. Rangel gratefully acknowledges pilot funding from the Duke Population Research Center under NIH award number 2P2CHD065563, as well as Princeton University's Research Program in Development Studies and Program in Latin American Studies for hospitality in the early phases of this research. Vogl gratefully acknowledges funding from the Health Grand Challenge at Princeton University. The content is solely the responsibility of the authors and does not necessarily represent the official views of the National Institutes of Health.

1 Introduction

Human use of fire predates the Neolithic Revolution, and controlled burns have played an important role in agriculture throughout its history (Pyne 1997; Scott et al. 2013). To this day, a key part of the economic activity associated with farming is the use of fire to clear forest and brush land, control weeds, regenerate soil nutrients, and dispose of agricultural waste (Andreae 1991), generating externalities across their own and neighboring communities in the form of smoke (Crutzen and Andreae 1990). Yet the health consequences of this pre-industrial source of air pollution have proved difficult to study. Given the widespread historical and current use of these techniques, an understanding of their effects on health offers a lens onto the determinants of human capital accumulation in the past, the public health dimensions of present-day rural economic development, and future sustainability. This article contributes on these fronts by estimating the effects of pollution from agricultural field fires on health at birth.

The tradeoff between economic development and environmental sustainability is a subject of longstanding concern (World Bank 1992; Grossman and Krueger 1995; Brock and Taylor 2010), and a growing literature examines its underlying mechanisms; its climate and welfare implications; and the effectiveness of mitigation policies (Greenstone and Jack 2015). Environmental degradation is often seen as a problem of industry, but some forms of anthropogenic pollution precede industrialization by thousands of years, as in the case of agricultural burns. Whether impacts intensify or subside with economic development is unclear. On the one hand, global agricultural land use is expanding with the demand for food (Johnson et al. 2013); on the other, the marginal willingness to pay for environmental quality may increase with income (Greenstone and Jack 2015). Furthermore, the adoption of new agricultural technologies may either increase or decrease negative production externalities like pollution. For example, combine harvesters increase field burning on rice farms in India (Gupta 2012) but decrease it on sugarcane farms in Brazil (Capaz et al. 2013). These issues also make the health costs of agricultural fires difficult to estimate. Burning is associated with economic activity across space and time, and technologies or policies that reduce it may have economic side effects. Because health has economic determinants, a central goal of this paper is to overcome this identification problem.[1]

[1] For the infant health outcomes we study, see Hoynes et al. (2015) and Amarante et al. (2016) on the effects of household income, and see Dehejia and Lleras-Muney (2004); Miller and Urdinola (2010); Baird et al. (2011); and

Controlled agricultural burns are an important contributor to fire activity globally. Using satellite remote sensing data on fires, Korontzi et al. (2006) estimates that areas that are at least 80% covered by cropland (8% of the global landmass, according to Friedl et al. [2010]) account for up to 11% of world's fires in terms of burnt square kilometer.[2] To illustrate, in Figure 1, we map fires and croplands around the world at the turn of the 21^{st} century, using Ramankutty et al.'s (2008) cropland estimates for the year 2000 and our own fire estimates based on the same satellite Korontzi et al. use for the closest available time period (Nov. 2000-Oct. 2001).[3] Three observations emerge. First, fires are widely distributed across the globe and are often found in areas with significant cropland coverage. Second, fires tend to be located outside major forests. Of those fires in the vicinity of forests, many are located on the outskirts, where they are related to land clearance for agriculture and grazing. Thus, although catastrophic forest fires garner much attention, other types of fires—occurring in different settings at vastly different scales—pose a separate and more frequent challenge. Third, fires are most commonly located in savannas and other grasslands, where they play a key role in the ecosystem and have both natural and man-made causes. Given that croplands are interspersed throughout the African and Latin American savannas, some of these fires are agricultural, but many others are ignited by pastoralists, hunters, or lightening, and most are uncontrolled (Cahoon et al. 1992). Controlled agricultural burns thus coexist with (and sometimes turn into) wildfires in areas with significant savanna, grassland, or forest cover. This overlap makes it difficult to isolate the health effects of controlled burns in these settings.

We thus focus on a setting where the spatial and temporal distribution of fires has a clear link with agriculture: the sugar-growing region of the Brazilian state of São Paulo, which is engulfed each year by widespread field burning in the leadup to the sugarcane harvest. São Paulo is a major producer of sugarcane, accounting for roughly one fifth of the tonnage produced annually across the globe (FAO 2016; UNICA 2016). In Brazil as elsewhere, traditional sugarcane cultivators burn their fields before harvest, eliminating debris and creating plumes of smoke that potentially elevate nearby air pollutant concentrations. Controlled fires are an integral part of the traditional harvesting process because they increase the productivity of labor (cane cutters) with minimal loss

Bozzoli and Quintana-Domeque (2014) on the effects of local or aggregate economic conditions.

[2]The actual share of fires stemming from agricultural activity is likely to be larger, given that croplands are not all so dense. Pastoralists also use fire to clear land for grazing, an activity closely related to agriculture that is omitted because pastures are not part of the cropland land class.

[3]We describe the available satellite data on fires in Section 3.

in the produce's glucose content. Field burning is continuously paced to optimize processing capacity utilization in sugar mills, with the harvest/milling period lasting up to six months. Though potentially harmful to health, the pollution from these fires does not reach levels considered dangerous by present-day industrial standards. Hence, the setting provides an opportunity to study the health burden of repeated exposure to moderate-scale pollution from traditional sources. Figure 1 shows that the study site is a regional hotspot in cropland density and fire frequency.

Our empirical strategy exploits wind direction on the day of each fire to resolve the identification challenges posed by the seasonal and economic correlates of field burning. The analysis is made possible by São Paulo's sophisticated, high-frequency monitoring systems for pollution, climate, and wind, which we link with satellite remote sensing data on fires and vital registration data on infant health. The richness and completeness of São Paulo's birth records allow us to consider outcomes less extreme than death, which is important given the setting's low mortality rates among the very young.[4] In using these data, we join a growing literature in development economics that studies early-life health using vital statistics microdata from Latin America, where national vital registration systems have recently taken great strides towards representativeness (Bharadwaj et al. 2013; Arceo et al. 2016; Amarante et al. 2016). We relate smoke exposure *in utero* to broad measures of health at birth (birth weight and prematurity), perinatal morbidity (hospital admission and APGAR scores), and perinatal mortality (stillbirth and death just after birth).[5]

Using daily measurements of fires, prevailing wind direction, and air pollution, we first show that fires upwind from a pollution monitor (that is to say, fires with smoke that should blow towards the monitor) raise pollution significantly more than fires at other angles to the wind or fires taking place during periods of calm. Particulate matter (PM_{10}) concentrations increase 5% during weeks in which an additional upwind fire occurs within 50 kilometers, holding constant the total number of fires within that radius. Ozone (O_3) also rises moderately but significantly, although we find no effect on nitrogen oxides (NO_x). The fires captured in our satellite images produce smoke which moves in a way consistent with expected atmospheric dispersion. These air pollution patterns allow us to use fires that are not upwind from a mother's place of residence to

[4] The state of São Paulo has an infant mortality rate of 11 per 1000 live births, while the municipalities in the study area range from 7 to 12 (SEADE 2016). The US rate is 6 per 1000 live births (Kochanek 2016).

[5] We focus on pre-birth exposure and perinatal outcomes rather than post-birth exposure and infant death mainly for statistical power. Low birth weight and prematurity are ten times more common than infant death in our setting.

control for confounders, under the assumption that upwind and other fires are equally correlated with other determinants of health: in particular the business cycle, which we document bears a close relation with fires. In essence, we use the differential association of upwind fires with health, relative to the association of other fires with health, to identify the effect of smoke exposure net of economic impacts. Our models include location and time fixed effects, so the empirical approach amounts to asking whether, within a locality, relocation of fires to upwind positions is negatively associated with infant health. Given that the outcomes of interest reflect health at birth, we count fires over relevant exposure periods during gestation.

We find that increased *in utero* exposure to smoke from sugarcane fires reduces birth weight, both on average and at the lower tail of the distribution (low and very low birth weight). During the last three months of gestation, an additional upwind fire per week (within 50 kilometers of the population center) raises the prevalence of low birth weight by 8 per 1000. Upwind fires also significantly reduce gestational age at birth (with increases in very preterm birth) and birth cohort size (with increases in reported stillbirths), but we do not detect effects on early-life mortality or measured morbidity outside the womb. Nor do we find effects of exposure in earlier gestational periods, although estimates for these periods may suffer from selection bias due to our retrospective dating of exposure from the date of birth, rather than prospective dating from the (unknown) date of conception. Using auxiliary data, we also document marginally significant increases in hospitalizations during periods with more upwind fires for two relevant groups of women—women of childbearing age (15-45) and prenatal care patients in the public health care system. Strikingly, counts of fires close to but not upwind from the mother's municipality of residence have a mild but significant *positive* relationship with health at birth, consistent with the potential confounding effects of the agricultural business cycle. Therefore, without considering the differential impact of upwind fires, we would have missed the detrimental health effects of pollution associated with this agricultural activity.

Our identification strategy is novel for a developing country, but we join a growing literature that exploits pollution variation from wind direction in the United States (Schlenker and Walker 2016; Anderson 2016; Deryugina et al. 2016).[6] Our context and strategy are set apart, however,

[6] In a related approach, Knittel et al. (2016) use pollution variation stemming from the interaction of automobile congestion with weather conditions.

because *both* wind and pollution source locations frequently change within an area, which proves to be particularly useful for clearly distinguishing pollution effects from economic effects. Cross-sectional and time-series analyses in Brazil have tended to find negative relationships between sugarcane fires and respiratory health, but the effect of fires *per se* has proved elusive (Arbex et al. 2000 Arbex et al. 2004; Cançado et al. 2006; Arbex et al. 2007; Ribeiro 2008; Uriarte et al. 2009; Chagas et al. 2014). With these empirical approaches, causality would be even more difficult to assess for more general measures of health as human capital like the ones we study.

Concerns about confounding from economic correlates of pollution may also apply to prominent research on pollution and child health in industrialized (or industrializing) contexts, such as Chay and Greenstone's (2003) seminal study of recession-induced pollution variation in the United States; Greenstone and Hanna's (2014) study of environmental regulations in India; and Cesur et al.'s (2017) study of natural gas infrastructure expansion in Turkey. These studies are part of a large literature, spanning both epidemiology and economics, that estimates the health burden of pollution from industry, motorized travel, and other modern sources, mostly in developed countries (Lacasana et al. 2005; Olmo et al. 2011; Bernstein et al. 2004; Currie et al. 2014; Graff Zivin and Neidell 2013). More recent studies focus on similar air pollution sources in developing countries (Bharadwaj et al. 2014; Hanna and Oliva 2015; Arceo et al. 2016), finding broadly negative effects on health, but with unclear implications for the consequences of agricultural burns because emissions from biomass burning are different in chemical composition from those generated by fossil fuel burning. A separate line of research does consider biomass specifically but concentrates either on massive pollution shocks, as in the case of major forest fires (Sastry 2002; Jayachandran 2009; Frankenberg et al. 2005; Tan-Soo and Pattanayak 2016), or on settings with extremely low air quality, as in the case of indoor air pollution from traditional cookstoves (Smith 2000; Ezzati and Kammen 2001; Pitt et al. 2010; Smith et al. 2011; Mobarak et al. 2012; Jeuland et al. 2015; Duflo et al. 2016). Whether the results of these studies generalize to a context of repeated exposure to moderate air pollution clearly is an open question. Moreover, atmospheric scientists treat the burning of agricultural residues as its own category of biomass burning, with different emission factors from forest or charcoal burning (Andreae and Merlet 2001).

Our findings suggest that exposure to this common but moderate form of pollution *in utero* influences overall measures of health in early life. Given the strong connection between these measures and long-term outcomes, our results suggest possible lasting benefits from the adoption of mechanized harvesting methods, which obviate the need for fire in our context. Our estimates are relevant to policy efforts aimed at finding renewable and environment-friendly alternative energy sources (Chu and Majumdar 2012). Based on proximate measures of economic costs, energy balances, and carbon savings, sugarcane-based ethanol stands out as an attractive renewable energy source (Goldemberg 2007). Sugarcane demand has surged both inside Brazil, with the government requiring automobile fuel to be at least one-fifth ethanol, and globally, leading to rising production in São Paulo. But negative production externalities pose distributional questions, with non-producing areas disproportionately benefiting from cheaper fuel or reduced emissions even as producing areas may suffer. To combat this pollution, the state government and the sugarcane industry have been collaborating on policies to encourage adoption of mechanized harvesting methods. While fires have not increased as rapidly as the recent expansion of sugarcane production in São Paulo, neither have they declined in number (Aguiar et al. 2011). Our results suggest infant health gains from redoubling efforts to reduce reliance on burning, so long as their effects on local economic activity are relatively limited.

2 Background: Harvesting and Health

2.1 Sugarcane Harvesting in Brazil

Brazil is the world's leader in sugarcane production (FAO 2016), and the state of São Paulo accounts for more than two-thirds of the country's harvest (UNICA 2016). In recent years, rising demand for biofuels and sugar has led to rapid growth in production (McConnell et al. 2010). The share of Sao Paulo's land area devoted to sugarcane more than doubled from 2000 to 2014, from 10% to 23%, displacing other crops and cattle production. Sugarcane cultivation expanded over the plateau northwest of the *Serra do Mar* mountain range, with more municipalities planting sugarcane and more area devoted to the crop in municipalities already producing it. These trends are confirmed in Appendix Figure A1, which maps the spatial distribution of municipality-level sugarcane land shares in the 1990s and our study period, 2009-2014. Sugarcane was initially concentrated around the city of Ribeirão Preto, roughly 200 miles north of the city of São Paulo, but has expanded 300 miles westward to cover the northern swath of the state.

From April to November, sugarcane farms and plantations in São Paulo harvest their crop using either traditional manual methods or modern mechanical methods. To carry out a manual harvest, cultivators burn the field before cutting the stalk of the cane with a cane knife or machete. The fire burns off straw and other extraneous materials, leaving only the harvestable part of the cane and its roots (which will later rattoon, or sprout new harvestable stems). The use of fires is believed to increase the productivity of laborers by a factor of 10 relative to the manual harvesting of unburned fields (Fernandes 1988; Marinho and Kirchhoff 1991). While the fire by itself does not lead to significant glucose loss, the crop starts degrading once cut, due to evaporation and bacterial growth (Salassi et al. 2004; Saska et al. 2009; Saxena et al. 2010). As a result, fires are set throughout the harvesting season, depending on transport logistics and the operational capacity of the mills processing the cane. In fact, Lamsal et al. (2013) report that a conservative estimate of cut-to-crush time is approximately three hours, with harvest being conducted 24 hours a day during peak season.

With modern methods, a mechanical combine harvester or sugarcane harvester cuts the stalk at the base, removes the straw, and then chops the cane into pieces. No fire is necessary. São Paulo's sugarcane industry is undergoing widespread mechanization, due in part to economic trends but also to laws, state-industry agreements, credit access to purchase machinery, and favorable exchange rates for importing machinery. By state law, pre-harvest burning will become illegal in most parts of the state in 2021; until then, an agreement between the industry and the state government set non-binding, interim goals for reductions in burning starting in 2007 (the so-called Green Ethanol Protocol). While the agreement may have curtailed the growth of fires, the area undergoing burn harvesting has not appreciably declined (Aguiar et al. 2011).

2.2 Potential Environment and Health Impacts

Despite increased attention to sugarcane fires in the environmental literature, specific information about emission factors remains relatively scarce.[7] In the laboratory, França et al. (2012) find that burning sugarcane straw raises particulate matter concentrations thought to be central to respiratory disease risk, as well as unburned hydrocarbons and trace gases such as carbon dioxide,

[7]Andreae et al. (2001) review the literature on emission from biomass fires, finding few published studies about sugarcane emissions. Our own review includes Marinho and Kirchhoff (1991), Le Canut (1996), Yokelson et al. (2008), Lopes de Carvalho (2009), and França et al. (2012).

carbon monoxide, and nitrogen oxides. Particulate matter emissions may be higher outside the laboratory, where objects larger than straw also burn. Ozone (O_3) concentrations are also likely to increase from open-area burning, since it results from the exposure of NO_x to sunlight and hydrocarbons. This process is more rapid under stagnant high pressure, which commonly accompanies the weather conditions that prevail in São Paulo during harvest: low humidity, moderate to high temperature, and sunshine (Logan 1989; Vukovich 1995). Based on these findings, one would expect increased pollution during sugarcane harvests that employ fire. Observational studies do document associations between harvest fires and pollution over time and across space (Marinho and Kirchhoff 1996; Cançado et al. 2006; Martinelli and Filoso 2008; Tsao et al. 2012).

Existing research on the health impact of this pollution focuses on respiratory health (Arbex et al. 2000; Arbex et al. 2004; Cançado et al. 2006; Arbex et al. 2007; Ribeiro 2008; Uriarte et al. 2009, Chagas et al. 2014). We depart from this tradition by using birth outcomes, which measure health (at the start of life) more comprehensively. Low birth weight results from two intertwined elements: preterm birth and intrauterine growth restriction. Both are associated with infant mortality, myriad morbidities both early and late in life, and poor socioeconomic outcomes in adulthood (Black et al. 2007; Currie 2009; Currie 2011; Currie and Almond 2011; Currie and Vogl, 2013; Bharadwaj et al. 2014). Perinatal conditions are influenced by multiple factors, from genetics to nutrition, physiological stressors, and environmental conditions.

Epidemiological evidence indicates that particulate matter concentrations correlate with several perinatal outcomes, and economic research suggests these correlations in part reflect causal effects of pollution.[8] The mechanism biologically relating particulate matter to perinatal outcomes is not precisely known, but the respiratory and cardiovascular health of pregnant women likely plays an important role. Particulate matter exposure could contribute to systemic oxidative stress, affecting the embryo in the earliest phases of growth, although this channel is more likely in urban areas, due to the transition-metal composition of air pollution in those locations. Both particulate matter and ozone could also decrease fetal-placental exchange of oxygen and nutrients, which is key to fetal growth. These impacts of exposure would operate via pulmonary and placental (allergic) inflammation, and would result from maternal infection, especially during the last trimester

[8] Reviews of the epidemiology literature can be found in Glinianaia et al. (2004); Sram et al. (2005), Kannan et al. (2006), Ritz and Wilhelm (2008), and Stieb et al. (2012). Examples of economic research include Currie and Neidell (2005), Currie and Walker (2011), Currie et al. (2014), and Knittel et al. (2016).

of pregnancy, potentially resulting in premature contractions and rupture of membranes. It is also possible that exposure to air pollution alters blood viscosity and coagulability, which could change hemoglobin, platelets, and white blood cells in ways that contribute to adverse fetal growth or increase the risk of maternal cardiovascular events, leading to pre-eclampsia and preterm deliveries.

3 Fires: Understanding Variation over Time And Space

To motivate our study design, we provide a descriptive analysis of fire variation in São Paulo. We first introduce the satellite-based dataset that we use to track fires, both in this section and in the main analysis. With these data, we describe the spatial and temporal distributions of fire and sugarcane harvesting. We then document longitudinal relationships among sugarcane harvesting, local economic activity, and fires.

3.1 Satellite Remote-Sensing Data

Satellite remote sensing technology allows researchers and policymakers to study patterns of fires over time and across space. To this end, we assemble panel data on fires from a Brazilian space agency (*Instituto Nacional the Pesquisas Espaciais* - INPE) database based on pictures of the Brazilian territory by US-operated satellites. Data from 3 satellites, NOAA-15 (orbiting at 800km and launched by the US National Oceanic and Atmospheric Research Agency), TERRA, and AQUA (both orbiting at 730 km and launched by the US space agency NASA), are employed.[9] Each satellites passes over the Brazilian territory twice per day, covering mornings, afternoons, evenings, and late nights.

Detection algorithms are applied to each picture, with AVHRR detection implemented by INPE and MODIS implemented by researchers at the University of Maryland. The resulting data consist of binary information about the existence of a potential fire in a given time and location as a function of pixels within specific thermal ranges (luminosity). The two sensors/algorithms can detect fires as small as 30m $\times$ 1m, but data output is at the pixel level, representing a 1km

[9] NOAA satellites use Advanced Very High Resolution Radiometer (AVHRR) sensors, while TERRA and AQUA use Moderate Resolution Imaging Spectroradiometer (MODIS) sensors. For descriptive analyses, we extend the data back further in time using NOAA-12, the predecessor of NOAA-15.

$\times$ 1km area.[10] Each potential fire is assigned a confidence index reflecting the probability that a fire occurred, varying from 0% to 100%, based on meteorological conditions and vegetation at the time of potential fire detection (Setzer and Sismanoglu 2007). Most of our empirical investigation, both in this section and after, weights fires by their confidence. Since pre-harvest burns take place at all times of day, we average the three data series to reduce noise in the daily count of fires.

3.2 Time Series and Spatial Patterns

We report temporal and seasonal patterns of fires and production in Figure 2. Panel A presents the rolling cumulative count of fires for every fortnight between January 2004 and December 2014, revealing no obvious time trend in the occurrence of fires but a clear seasonal pattern. Panel B details this seasonal pattern further by plotting the log of average monthly counts over our main study period, which runs from 2009 to 2014. Starting from their trough in January, fires climb through the year until peaking in August or September. The peak-to-trough difference in monthly fires is approximately 2.5 log-points, corresponding to a 35% increase. This pattern is even more dramatic if we focus on the confidence-weighted counts, yielding a 73% increase in fire activity between January and September. The seasonality of fires also matches the agricultural cycle, further suggesting a link to sugarcane harvesting. To illustrate this link, Panel B of Figure 2 also reports seasonal patterns in the tonnage of cane processed monthly by São Paulo mills from the National Union of Sugarcane Producers (UNICA) during our main study period, 2009-2014. Milling activity is lowest in the summer months, from December to March, and highest in the winter months, peaking in August.

For added confirmation of the production-fires link, Appendix Figure A2 maps the spatial distribution of yearly confidence-weighted fires in São Paulo between 2009 and 2014. For each of the state's municipalities, we sum confidence-weighted counts of fires over the year, normalizing by the number of pixels in the municipality, and then map each municipality's percentile in the distribution of average fire intensity. The spatial distribution is similar to that of sugarcane production in Appendix Figure A1. Fires are concentrated in the sugarcane belt that spans the north of the state. In the city of São Paulo, located in the southeast, sugarcane is absent and fires rare.

[10] We geocode the centroid of each pixel. In validation exercises, INPE reports location errors of 400m on average, with a 3km standard deviation; 80% of the fires are correctly placed within the pixel.

3.3 Sugarcane, Fires, and the Local Economy

The prevalence of field fires depends on both the extent of sugarcane production and the choice of harvesting method, both of which change over time in ways that may be closely linked to the local economy. This potential link poses a concern for identifying the health effects of field fires, given that some health-relevant aspects of the local economy may go unmeasured. For motivating evidence on this point, we assemble an annual panel of all 645 São Paulo municipalities covering 2004-2014, with data on fires, sugarcane production, and municipal GDP per capita. We use these data to regress confidence-weighted fire intensity on measures of sugarcane harvesting and the local economy. To measure fires for this analysis, we again combine data from three satellites, which are intended to provide continuous tracking over the entire period; we sum confidence-weighted fires (at the pixel-day level) within a municipality over the year and divide by the municipality's land area. Agricultural and economic data are from the Brazilian statistical agency IBGE (*Instituto Brasileiro de Geografía e Estatística*). We fit linear models with municipality and year fixed effects; the results reflect sugarcane-fire associations within a municipality over time.

Table 1 presents the results, showing clear links among harvest area, GDP per capita, and fires. In columns (1) and (2), which use the full 2004-2014 period, area harvested is significantly and powerfully associated with fire. Having any harvest is associated with an increase of 0.4 annual fires per 100km^2, more than 10% of mean fire intensity. The more area harvested, the stronger the association. An expansion from no harvest to having at least three-quarters of the municipal territory under harvest is associated with 2 more annual fires per 100km^2, more than two-thirds of mean fire intensity. When we restrict the sample to period with available GDP data (2004-2012) in column (3), these associations remain the same. Controlling for log GDP per capita in column (4) negligibly alters the coefficients on harvest area. But GDP per capita is independently associated with fires; a doubling of GDP per capita predicts one extra fire per 100km^2. Fires thus also reflect broader economic activity.[11]

Columns (1)-(4) of Table 1 provide evidence of annual relationships, but are fires related to economic activity within the year as well? The final column of Table 1 considers intra-year variation, which requires a new dataset and a new frequency. We use data from the Brazilian Ministry of

[11] If we omit harvest shares from column (4), the coefficient on log GDP per capita rises to 1.28, implying that area harvested accounts for one-quarter of the relationship between GDP per capita and fires.

Labor CAGED database on the creation and destruction of formal sector jobs at the municipality-month level for 2009-2014. After merging with summed confidence-weighted fires at the same level, we regress the change in fires from the previous month on jobs created and destroyed, controlling for month-year fixed effects. This first difference approach accommodates the explanatory variables, which are in changes rather than levels, while remaining analogous to the annual fixed effect specification we use in the first four columns of Table 1. Here again, the data show a clear link between fires and economic activity. One job created per 1000 people is associated with an increase of .9 fires per 100km^2; one job destroyed per 1000 people is associated with a decrease of 1.5 fires per 100km^2.

Expansions in sugarcane cultivation are significantly related to fires, as are per-capita income and the strength of the labor market.[12] These findings are relevant for the analysis of fires and child health in at least two ways. On the one hand, they confirm a strong link between sugarcane cultivation and the fires identified by satellite remote sensing, which we use in our main analysis. On the other, they demonstrate that the drivers of fire variation are also related to other potential determinants of health. In a naïve panel analysis of fires and infant health, even detailed controls for seasonality and weather would not fully eliminate concerns about omitted variables.

4 Pollution and Health: Data and Methods

This concern motivates our strategy of leveraging the wind in order to generate exogenous variation in smoke exposure. We compare how infant health relates to fires upwind from a population center, over and above its relation to nearby fires occurring at other angles to the prevailing wind or anywhere on days that have no prevailing wind. Air pollution in the population center should be positively associated with the number of upwind fires but unrelated or only weakly related to the number of other fires. Our identifying assumption is that the association of fires with other determinants of child health, such as economic activity, does not depend on the wind. In other words, we assume that fire locations interact with the wind to influence the intensity of pollution exposure but not economic activity.

[12] These associations suggest that in this context, tracking fires using satellite data is akin to tracking nighttime lights for proxying economic growth (Henderson et al. 2012).

4.1 Data

Our research design depends on geographically granular and high-frequency data. In addition to the satellite remote-sensing data introduced in Section 3, two other datasets satisfying this requirement form the core of our analysis: one containing pollution and weather data from state-run air quality monitoring stations, the other containing birth and death certificate information from the national vital statistics system.

4.1.1 Air Monitoring Station Data

Data on pollution concentrations and atmospheric conditions come from air monitoring stations operated by the São Paulo's environmental agency CETESB (*Companhia Ambiental do Estado de São Paulo*). Although CETESB has been collecting data in metropolitan São Paulo for more than two decades, monitoring in agricultural areas began only after 2008. Thirteen stations started collecting data in sugar-growing areas between January 2008 and April 2009, with all but one in operation at least through the end of 2014.[13] Our study period thus runs from 2009 though 2014, the years for which wind data are available for the prenatal exposure period in most locations.

Data from CETESB stations are organized in hourly observations of pollution concentrations and atmospheric conditions. All stations measure temperature, relative humidity, wind direction, particulate matter (under 10 μm, PM_{10}), and ozone (O_3) concentrations, while twelve also measure the concentration of nitrogen oxides (NO_x). Pollution concentrations serve as outcomes, while atmospheric conditions serve as covariates. For all variables except wind direction (discussed below), we convert these hourly observations into a daily mean for every day with at least 8 hours of raw data. To further smooth the variability in the data, we compute rolling week averages, so that for each day t, we take the average daily reading between $t-6$ and t if daily means were available for at least three days. Depending on the outcome, our data contain approximately 26,000 location-date cells. Appendix Table A1 reports the share of rolling weeks with missing data for each reading, while Appendix Table A2 reports their weekly averages and standard deviations. Missing data are rare for most stations, and average pollution readings are far below

[13]To provide information on their spatial distribution, all maps in the Appendix are overlaid with the locations of the thirteen stations. Despite including only a subset of producing areas, they nonetheless cover varied levels of production intensity and traditional harvesting techniques.

current regulatory limits in the United States and São Paulo.[14]

For winds, we similarly aggregate hourly measurements into a daily summary—the daily prevailing wind—because we lack precise information on the start and end of each fire to build an hourly model of pollution dispersion. Measurements are coded as angles in degrees, such that 0 corresponds to wind from due North, and 180 corresponds to wind from due South. To find the daily prevailing wind, we search for the sector of the wind rose that contains the most hourly measurements, starting from the sector centered at due north and then increasing in intervals of 10 degrees. In our analysis of the pollution effects of fires, which is akin to a first stage relationship, we experiment with different central angles for this sector—30, 45, and 90 degrees—searching for the angle that best trades off the benefits of wider angles (more fires) with those of narrower angles (more precision in wind direction). We settle on the octant (45 degrees) as the sector that best captures pollution dispersion, so we use octants throughout our analysis of health. To minimize noise, we require that the wind blow from the modal sector for at least eight hours of the day; if this condition is not met, then we assign no prevailing wind for the day. This stringent requirement allows us to avoid measurement error from days with extremely variable winds or from calm days; in these cases, winds could blow from the modal direction for as little as a single hour.[15] Summarizing wind patterns across stations, Appendix Table A3 reports the share of hours with wind blowing from each octant on the wind rose, as well as the share of hours with no wind direction recorded. Winds are widely dispersed, with no octant ever accounting for more than half of a station's hourly measurements, and most accounting for between 5% and 20%. Stations also record no wind in 5% to 20% of hours. Given this dispersion and prevalence of calm, each station is coded as having no prevailing wind on one-quarter to one-half of all days.

The interaction of wind direction with fire location is key to our research design. We organize our working data file by first counting the daily number of 1km-by-1km grid cells containing a fire within a 50km radius of a municipality's population centroid (averaged across the three satellites), omitting those within 5km because they are likely to be within the urbanized areas, not related to agriculture and generating smoke that affects the population independently of the

[14]See http://www.epa.gov/criteria-air-pollutants and http://ar.cetesb.sp.gov.br/padroes-de-qualidade-do-ar.

[15]In cases of ties, we choose the tied octant with lowest angle. Depending on the station, ties occur on 5-12% of days, mostly coming from octants that overlap as we increase the base angle 10 degrees at a time. In cases where winds blow from the opposing quadrant for more than two hours, we assign no daily prevailing wind. Results are not sensitive to either choice.

wind's prevailing direction. Fires occurring within the modal wind octant in a given day (and between 5 and 50kms of distance) are defined as upwind; those in the opposite octant are defined as downwind; and all those outside the modal octant are defined as non-upwind. From these daily measures we compute rolling week counts, capturing the total number of upwind, downwind, and non-upwind fires occurring between day $t-6$ and day t. The rolling week measures serve as the basis for all analyses. In most analyses, we weight fires by the INPE confidence measure, with the weights varying from 0 for no confidence to 1 for certainty. Appendix Table A4 reports means and standard deviations for these fire measures. On average, 2.77 fires are detected near each monitoring station per week; weighted by confidence, this count falls to 1.75. Of these 1.75 confidence-weighted fires, 0.12 occur upwind from the municipal population centroid on days with a prevailing wind.

4.1.2 Vital and Hospital Records

Data on birth and perinatal outcomes are drawn from individual-level vital records and hospitalization records in DATASUS, the Brazilian Ministry of Health's Usage Information System. Although vital registration systems in developing countries often have incomplete coverage, Brazil has made great strides in this area, minimizing concerns about sample selectivity. According to estimates from 2010, at least 99% of births and infant deaths were registered in the administrative areas containing the study sample (IBGE 2011). All datasets include information on municipality of residence, which is key to our research design. To guarantee correct wind measurement, we restrict attention to mothers residing in the 13 sugar-growing municipalities with air monitoring stations, leading to a sample of 287,506 live singleton births.

Our primary outcomes are birth weight and gestational age at birth, drawn from the birth registry. Birth weight is measured continuously in grams or as an indicator for low (< 2,500 grams) or very low (< 1,500 grams) birth weight. Brazilian birth records code gestational age at birth in coarse categories, limiting our ability to analyze it as a continuous dependent variable and to estimate the date of conception. We define indicators for preterm (< 37 weeks) and very preterm (< 32 weeks) births, and we also impute weeks of gestation, using the weekly gestational age distribution in the US to estimate the average gestational age at birth within each category.[16] Secondary

[16]The categories are 17-21, 22-27, 28-31, 32-36, 37-41, and 42-47 weeks. Births are distributed similarly across these

outcomes from the natality data include 1- and 5-minute APGAR scores; reported stillbirths; the number of births; hospital admission charged to the public health care system in the first full day of life (the day following the birthdate); and mortality in the first full day of life.[17] To reduce computational demands and to link the birth, death, and hospitalization data without individual identifiers, we collapse individual records by location and date of birth, leading to a dataset of average birth outcomes in 26,100 location-date cells. We weight all analyses by cell size.

Table 2 presents summary statistics for the births dataset, detailing how we reduce the individual-level dataset to location-date cells. Column (1) presents the mean, column (2) the individual-level standard deviation, and column (3) the cell-level standard deviation. The table highlights São Paulo's favorable performance on health and social metrics. Birth weight averages 3.2kg, with 80 per 1000 infants born low birth weight and 12 per 1000 born very low birth weight. Imputed gestational age averages 39 weeks, with 97 infants per 1000 born preterm and 12 per 1000 born very preterm. Maternal education is high, with 25% having at least completed high school.

4.2 Estimation

We carry out two estimations, one linking fires to air pollution, the other to health outcomes. In all estimations, we cluster standard errors at the monitor (or municipality) level. Because our data have few clusters, we compute two *p*-values for each estimate following recommendations by Cameron et al. (2008). The first is based on the analytic *t*-statistic, using the critical value from a *t*-distribution with degrees of freedom set to the number of clusters minus one. The second is based on a wild cluster-bootstrap percentile-*t* procedure, imposing the null hypothesis. The stations are dispersed across a large area, minimizing concerns about spatial dependence.

To test our research design's assumption that upwind fires raise pollution more than downwind fires, we first analyze air monitoring station data on pollutant concentrations. Our primary focus is particular matter, a byproduct of sugarcane burning with well-known health effects, but we also examine the other pollutants (NO_x and O_3) tracked by the stations to shed light on the composition of pollution from sugarcane burning. For the air monitoring station in municipality

categories in São Paulo and the US, motivating our imputation. In US natality data for 2007-2014, gestational age within each category averages to the midpoint of the category for term births, the next highest week from the midpoint for pre-term births, and the next-lowest week for post-term births.

[17] A newborn is admitted to the hospital separately from the mother in cases of pronounced vulnerability, such as admission to a neonatal intensive care unit.

j on date t, we run:

$$y_{jt} = \alpha^U upfires_{jt} + \alpha^N nonupfires_{jt} + X'_{jt}\beta + \mu_j + \tau_t + \varepsilon_{jt} \quad (1)$$

μ_j and τ_t are station and date fixed effects, respectively, so we only leverage within-station, within-time (rolling-week) variation. The dependent variable y_{jt} is the average pollution concentration from date $t-6$ to t. The central covariates for our research design are $upfires_{jt}$, the count of upwind fires, and $nonupfires_{jt}$, the count of other fires, both during the same week-long period. For completeness, we also report results from estimations that look at the effect of the total count of fires $(upfires_{jt} + nonupfires_{jt})$, as well as others that decompose non-upwind fires into downwind fires (in the octant opposing the prevailing wind) and other fires. The vector of covariates X_{jt} includes weekly average temperature; weekly average relative humidity; the share of hours with no wind; the share of hours with winds blowing from each octant of the wind rose; and the share of days with no prevailing wind. We include temperature and humidity because they are associated with both fire incidence and pollutant concentrations, but we note that they are not differentially associated with fires by wind direction, so our research design does not depend on them. Similarly, we control for wind direction and calm to assuage concerns about a long-term relationship between fire placement and wind, although these covariates too are unnecessary for identification. In the Appendix, we assess robustness to municipality-specific time and season effects, and we also present results for alternative radii and wind angles to help justify our analytic choices. In this setup, $\alpha^U - \alpha^N$ captures the differential impact of upwind fires, relative to the overall association between fires and pollution. Our identification comes from the comparison of impacts associated with two-different types of fires within the same area around a populated center, and not from comparing exposed and non-exposed populations.

Our infant health regression specification is similar to equation (1), except that the analysis of *in utero* exposure requires many lags. We set the number of lags to 38 weeks, allowing for unrestricted week-specific effects of fires over the course of the potential pregnancy. However, backdating exposure in this way presents an estimation hurdle because the timing and length of pregnancy are endogenous. Some premature infants were not yet conceived, and those exposed to fires long before birth are positively selected precisely because they were not born prematurely.

Ideally, one could include counts of fires during every week of pregnancy, but because Brazilian birth certificates record gestational age at birth coarsely, we cannot backdate conception. Because the selection mechanism biases estimates of the effects of exposure to fires occurring long before birth, we emphasize estimates pertaining to the last three months of pregnancy, although we do report results for all 39 weeks.

We thus employ a distributed lag model on grouped data, with births aggregated into municipality-birthdate cells. For births in municipality j on date t, we run:

$$\bar{y}_{jt} = \sum_{s=0}^{38} \alpha_s^U upfires_{j,t-7s} + \sum_{s=0}^{38} \alpha_s^N nonupfires_{j,t-7s} + \sum_{s=0}^{38} X'_{j,t-7s}\beta_s + \mu_j + \tau_t + \varepsilon_{jt} \tag{2}$$

where $\bar{y}_{jt}$ is an average birth outcome; μ_j and τ_t are municipality and date fixed effects; and α_s^U, α_s^N, and β_s are the distributed-lag versions of the coefficients from equation (1). Each independent variable is an average or count for the week leading up to date $t - 7s$, where s is multiplied by 7 because t is measured in days, while s is measured in weeks. In robustness checks, we control for average mother and infant characteristics in the cell, as well as for municipality-specific time and season effects. We also conduct a number of falsification exercises, employing either in-sample randomization of location and event timing or out-of-sample birth outcomes.

The differential impact of an upwind fire occurring s weeks before birth is $\alpha_s^U - \alpha_s^N$. Equation (2) yields many week-specific estimates. Yet due to the unrestricted distributed lag structure and the strong autocorrelation of fire occurrences, individual coefficients are noisy (Almon 1965; Sargan 1980). We thus follow the literature on *in utero* exposure by reporting on three periods of gestation, akin to trimesters, but retaining the unrestricted estimation procedure.[18] Specifically, we report 13-week sums of the coefficients, corresponding to the last, second-to-last, and third-to-last periods before birth:.

$$\alpha_{\{\underline{T},\overline{T}\}} \equiv \sum_{s=\underline{T}}^{\overline{T}} \alpha_s^U - \alpha_s^N$$

where the periods $\{\underline{T}, \overline{T}\}$ are $\{0, 12\}$, $\{13, 25\}$, and $\{26, 38\}$. These sums represent the impact of increasing fires by one occurrence per week during periods of approximately three months. Despite the similarity between these periods and pregnancy trimesters, we refrain from referring

[18] Existing research typically uses trimester-averaged covariates, which in our model would correspond to imposing that the weekly coefficients are identical within a trimester.

to them as such because we count backward from the date of birth, rather than forward from the date of conception. Given the aforementioned selection biases due to backdating, we view our estimate of $\alpha_{\{0,12\}}$, relative to the period closest to birth, as being more interpretable than our estimates of $\alpha_{\{13,25\}}$ and $\alpha_{\{26,38\}}$, which would reflect earlier stages of the pregnancy.

By interpreting the differential association of upwind fires with health as a causal effect, we assume that the interaction of fires and wind is exogenous to the causal system, conditional on the covariates and fixed effects in our model. The nuance of this assumption merits further discussion. If we were comparing the associations of upwind and other fires with health in the cross section, then sorting—in which mothers living downwind from high-burn areas were selected on characteristics relevant to infant health—would pose an important concern. However, the municipality fixed effect in equation (2) allows us to eliminate this concern by focusing on differences in exposure within the same municipality. One could also worry that some mothers time their pregnancies to avoid fetal smoke exposure, so that comparisons of infants born at different times of the year are invalid. However, we show that our results are robust to the inclusion of municipality-by-week-of-year fixed effects, which leads to a comparison of children born at the same time of year in the same municipality, just in years with different fire intensity. A final matter is the dependence of burn activity on wind. Sugar harvesters do pay attention to the wind, avoiding field burning on days with high wind due to the risk of a fire becoming uncontained. But given our flexible covariates for the presence and direction of wind, this relationship between wind and fire poses no threat to identifying how their interaction affects health.

5 Pollution and Health: Results

5.1 Effects of Fires on Air Pollution

Table 3 reports estimations of equation (1), revealing that upwind fires differentially increase PM_{10} and O_3 but not NO_x. For now, we focus on our main choices for the central angle of the upwind sector, 45 degrees, and the radius of the catchment area, 50km. We then discuss appendix results on alternative angles and radii, which help explain our main choices.

To build intuition, we start with an unsophisticated specification for PM_{10} and then gradually refine it. Columns (1)-(2) report results for unweighted counts of fires, disregarding their orientation to the wind. In column (1), which does not control for atmospheric conditions, one additional fire in the past week raises PM_{10} by 0.5 units in the same time frame. Controlling for atmospheric conditions in column (2) does little to change this estimated effect. Because the fire counts are not weighted by confidence, however, these estimates may understate the true impact of fires due to errors in fire detection. To address this concern, column (3) uses confidence-weighted fire counts. The coefficient grows, reaching 0.6 units of PM_{10} per confidence-weighted fire.

In column (4), we decompose confidence-weighted fire counts into upwind fires, which are located in the prevailing wind octant; downwind fires, which are located in the diametrically opposing quadrant; and other fires, which include those at other angles to the prevailing wind and those taking place on days without a prevailing wind. The effect of upwind fires on particulate matter concentrations is 4-5 times larger than the effects of downwind or other fires, although all three effects are statistically significant. An upwind fire raises PM_{10} by 2.5, or 0.16 standard deviations, compared with effects of 0.6 and 0.5 for downwind and other fires, respectively.

Because downwind and other fires have similar effects, our main regression specification, equation (1), groups them together into non-upwind fires. We implement this specification in column (5) using confidence-weighted fire counts, with similar results: coefficients of 2.5 for upwind fires and 0.5 for non-upwind fires. Our identifying variation for the rest of the analysis comes from the difference between these effects, which at 2.0 is highly statistically significant based on both asymptotic and bootstrap inference. Relative to non-upwind fires, upwind fires raise PM_{10} concentrations by 0.12 standard deviations. We apply the same model to measures of NO_x and O_3 in columns (6) and (7), respectively. Both pollutants are byproducts of biomass burning but may appear at different stages because sunlight interacts with some nitrogen oxides to produce ozone. Column (7) reveals a significant differential upwind-fire impact on O_3 of 0.7 units, or 0.05 standard deviations. Meanwhile, column (6) shows no evidence of a differential impact on NO_x by wind direction. These results are internally consistent because the exposure of NO_x to sunlight leads to a chemical reaction that produces O_3 in a period shorter than one week.

Appendix Table A5 further explores the results for the affected pollutants, PM_{10} and O_3, in four ways. First, we consider alternative central angles of 30 and 90 degrees for the upwind sector. Wider angles may either reduce noise by capturing more fires or increase noise by less precisely capturing the wind. Although both alternative angles lead to coefficients of the same sign, the coefficients and t-statistics are smaller than those in Table 3. Because the 45 degree definition appears to best capture pollution dispersion, we use it henceforth. Second, we assess alternatives to the 50km radius we use for our catchment area. At smaller radii, standard errors increase substantially, but coefficients do not change in any substantial way. Third, we investigate robustness to alternative specifications of time effects and local-level seasonality by (1) replacing date fixed effects with year and day-of-year fixed effects; (2) including station-by-year fixed effects; and (3) including station-by-week-of-year fixed effects. Even these more demanding specifications, which account for station-specific annual and seasonal trends, do not alter the magnitudes or significance levels of the estimated pollution effects. Fourth, we re-estimate the models employing log-transformations of the dependent variables in columns. Our results remain statistically significant and are similar in magnitude to the level estimates, relative to the outcome means, with implied upwind fire impacts of 4.2% for PM_{10} and 1.0% for O_3.

5.2 Effects of Fires on Infant Health

Tables 4-7 present the results for infant health. We begin with the outcomes most typically used to reflect the cumulative impact of the *in utero* environment: birth weight and gestational age at birth. Next, we consider perinatal mortality—including reported stillbirths, the daily number or births (which we use as a proxy for fetal survival), and death in the first day and week of life—and more acute measures of perinatal morbidity—including 1- and 5-minute APGAR scores, as well as hospitalization admissions in the first full day of life. To conclude the section, we carry out a number of robustness checks and falsification exercises for our main results relating to birth weight and gestational age at birth. Throughout, we use confidence-weighted fire counts and outcome data collapsed to the municipality-birthdate level.

5.2.1 Birth Weight and Gestational Age at Birth

Table 4 reports full-sample estimates of the 13-week sums of coefficients for our primary birth outcomes, birth weight and gestational age at birth. Columns (1)-(5) build up the full model for birth weight across three regressions, first disregarding wind and leaving out the atmospheric condition covariates X_{jt}, then adding X_{jt}, and finally differentiating by wind direction. Columns

(6)-(10) then apply the full model to indicators for low and very low birth weight, the imputed measure of gestational age, and indicators for preterm and very preterm.

In columns (1)-(2), the results for all fires within 5-50 kilometers is positive and, in some cases, even significant. For example, an extra fire per week occurring in the last 13 weeks of gestations is associated with a 1.3-1.9 gram increase in birth weight, with p-values of roughly 0.1. Similar associations are evident for 13-25 weeks before birth but not for 26-38 weeks before birth. A naive panel data analysis with location and time fixed effects therefore leads to the conclusion that *in utero* exposure to fires has marginally significant, positive effects on birth outcomes. Moreover, this result appears to be robust to controlling for atmospheric conditions that covary with fire exposure.

However, these positive associations appear to mostly reflect the child health benefits of increased economic activity, which can be seen by contrasting the sums of coefficients for upwind (column 3) and non-upwind fires (column 4), estimated in the a single regression. Here, an extra upwind fire per week during the last 13 weeks of gestation is associated with a significant 20 gram decrease in birth weight, consistent with a negative effect of fire-related air pollution. Meanwhile, all other fires during this final period of gestation are associated with a significant 3 gram increase in birth weight. When fire activity do not put mothers at risk for pollution exposure, its correlates improve the *in utero* environment. But when wind carries the smoke toward mothers, the detrimental effects of pollution exposure greatly outweigh these benefits. Column (5) reports the difference between the upwind and non-upwind coefficients, revealing that in the last gestational period, the pollution differentially generated by an additional upwind fire per week reduces birth weight by a statistically significant 23 grams. This effect represents 0.14 standard deviations as reported at the bottom of the table, reflecting variation across municipality-birthdate cells. Measured across infants, the standard deviation of birth weight is 525, implying an effect of 0.04 standard deviations. The other two gestational periods exhibit no significant effects, although they are more prone to concerns about selection from prematurity. Consistent with these concerns, the differential effect for weeks 13-25 is positive though insignificant.

The remainder of Table 4 reports differential effects of upwind fires on the other outcomes. Columns (6) and (7) show that the birth weight effects are felt at the lower tail of the birth weight distribution. An additional fire per week in the last gestational period leads to a 10% increase in the incidence of low birth weight (< 2500 grams)—an effect of 8 per 1000 on a base risk of 81 per 1000—and a 41% increase in the incidence of very low birth weight (< 1500 grams)—an effect of 5 per 1000 on a base risk of 12 per 1000. Some part of these reductions in birth weight may be attributable to earlier birth. In column (8), we find that the average length of gestation—as measured by our imputed gestational age variable—falls by a statistically significant 0.6 days for an additional upwind fire per week in the final period of gestation. Rates of preterm birth (<37 weeks) rise insignificantly, while rates of very preterm births (<32 weeks) rise significantly, by 5 per 1000 on a base rate of 12 per 1000. We again find no evidence of detrimental effects of upwind fires in the earlier gestational periods.

To unpack these differentials into their upwind and non-upwind components, Figure 3 plots the underlying 13-week sums of coefficients in a series of bar graphs. Consistent with detrimental pollution effects and beneficial economic effects, upwind fires in the final gestational period are associated with deterioration in all six outcomes, while other fires in the same period are associated with improvements, significantly so in most cases. For earlier gestational periods, the upwind associations show no clear pattern, but non-upwind fires generally continue to be marginally significantly associated with better health at birth. These results suggest that the increased economic activity has benefits for much gestation, while pollution from field fires harms fetuses primarily toward the end of gestation.

5.2.2 Perinatal Morbidity and Mortality

Besides birth weight and gestational age at birth, vital statistics and hospitalization data provide other useful measures of health in the period immediately surrounding birth, the perinatal period. Using the same approach as Table 4, Tables 5 and 6 analyze these data, focusing on fetal mortality first, early neonatal mortality (first week of life) next, and morbidity among the living last.

Table 5 presents estimates of the differential effects of upwind fires on the reported stillbirth rate and the number of births. Because stillbirths are underreported, we use the number of births as a broader proxy for *in utero* survival, a strategy similar to that employed by Jayachandran (2009). The number of births may increase due to prematurity, so a negative effect would amount to a lower bound on the number of fetal deaths. This approach assumes that the number of conceptions is unrelated to the relative incidence of upwind fires, which is again more reasonable for fire exposure during the last period of gestation, 0-12 weeks before the (potential) birthdate. In column (1), we consider reported stillbirths, finding that one additional upwind fire per week in the final gestational period differentially increases the stillbirth rate by 4 per 1000 births, more than half of the reported stillbirth rate. Column (2) looks at the logarithm of the daily number of *live* births, a proxy of *in utero* survival.[19] Here, an additional upwind fire per week in the final gestational period differentially decreases the number of live births by 2.8%, providing further evidence of fetal death. If we bundle stillbirths with live births to count the *overall* number of births, we find an upwind-fire-related decline of 2.3%, as reported in column (3), further suggesting *in utero* mortality. However, this result serves only as a robustness check for the lower bound, as stillborns should be excluded from the measure of *in utero* survival.

If stillborn babies are drawn from the lower tail of the underlying health distribution, then by focusing exclusively on live births, Table 4 may underestimate the negative effects of upwind fires. The remainder of Table 5 addresses this concern by adding stillborns to the sample and repeating the analyses in Table 4. For birth weight, the differential effects of upwind fire exposure in the final gestational period strengthen to 27 grams, with a 10-point increase in the incidence of low birth weight and a 6.5-point increase in the incidence of very low birth weight. Upwind fire exposure appears to increase death risk in smaller fetuses, biasing downward the estimates in Table 4. Effects on gestational age are similar; effects on preterm birth rates strengthen to 6.7 points, though still insignificant; while effects on very preterm birth rates increase to 5.7 points.

To conclude our main estimates, Table 6 presents results for early-life morbidity and mortality. For mortality, we use the share of live infants in a municipality-birthdate cell that died within a day or a week of birth. As displayed in columns (1)-(2), neither measure of newborn death shows a significant effect from fire exposure late in gestation, although the point estimates are positive.[20] However, it is worth noting that given the rarity of both outcomes, we lack the power to detect meaningful effects; their means of 3 to 6 (per 1000 live births) are only 2-3 times the standard errors of the effect estimates. For morbidity, we consider rates of hospital admissions (for all causes and for causes related to fetal growth faltering and prematurity) charged to the public health care

[19] Less than 1% of cells have zero births, so using logarithms does not meaningfully alter the sample.

[20] 1-week mortality significantly increases from upwind fire exposure in the earliest gestational period, but we do not emphasize this result because of concerns about selection and endogenous fertility.

system by the end of the first full day of life, as well as average 1- and 5-minute APGAR scores, which are meant to reflect how well the newborn tolerates the delivery process and conditions outside the womb, respectively. Results presented in columns (3)-(6) point to no significant effects on these outcomes.

5.2.3 Robustness and Heterogeneity

The broad takeaways from Tables 4-6 are that pollution from sugarcane harvest fires causes smaller babies, shorter pregnancies, and more *in utero* mortality, without statistically significant effects on morbidity and mortality soon after birth. However, while equation (2) is an exacting regression specification, it may still be subject to concerns about potential confounders like maternal characteristics, location-specific time trends, and location-specific seasonality. In principle, these factors should not be differentially associated with fires by wind direction, so they should not bias our estimates, but we can confirm this claim empirically. For our primary birth outcomes, Table 7 takes two approaches to addressing these concerns. First, it investigates robustness to alternative regression specifications that include additional covariates or interactions between location and time fixed effects. Second, it carries out a series of falsification exercises designed to assess the likelihood that our results would arise spuriously. To contain the size of the table, we focus on the estimand we have emphasized throughout the discussion of the results, the differential effect of upwind fires in the final gestational period. For the same purpose, we also report only coefficients and standard errors (not p-values) for the robustness tests.

After column (1) reprints the original estimate from Table 4, the next five columns of Table 7 focus on the alternative regression specifications. First, in column (2), we add municipality-birthdate averages of maternal and infant characteristics as covariates in equation (2), finding no substantive change in our main parameter estimates. We include averages of maternal age, education level, marital status, parity, and number of past miscarriages, as well as infant race and sex, all known determinants of infant health. The robustness of our results to these additional covariates suggests that they may not be differentially associated with upwind fires, a finding that would support our treatment of the interaction between wind and fires as exogenous. In Appendix Table A6, we directly test our identifying assumption by using these variables as outcomes in equation

(2). We find no evidence that they are differentially associated with fires by wind direction.

In our second set of robustness checks, which appear in columns (3)-(6), we experiment with alternative specifications for annual and seasonal confounders. Column (3) replaces the date fixed effects in equation (2) with year and day-of-year fixed effects; column (4) then replaces the year fixed effects with station-by-year fixed effects, keeping the day-of-year fixed effects; column (5) then reintroduces date fixed effects, keeping the station-by-year fixed effects; column (6) introduces station-by-week-of-year fixed effects. Except for preterm birth (for which Table 4 did not show a statistically significant effect), our results are broadly robust across these specifications.

The remaining columns of Table 7 present three falsification exercises for our primary birth outcomes. In column (7), we estimate the original model except with dependent variables (average birth outcomes in a municipality-birthdate cell) measured at a five-year lag. Implicitly, this exercise checks whether season-location specific effects not captured by our model could explain the results. They do not; all but one of the 5-year lagged coefficients are of the wrong sign, and all t-statistics are less than 1.6. In columns (8) and (9), we carry out an in-sample falsification exercise by reassigning fire counts from either a random year (for the same municipality and day of the year) or a random location (for the same birthdate). We perform each randomization 100 times and report the proportion placebo samples in which the point estimate was less extreme (i.e., closer to zero) than our main estimates in Table 4. In all cases except preterm birth (for which Table 4 did not show a statistically significant effect), our main estimate is more extreme than 95% of the placebo estimates.[21]

To shed light on heterogeneity, Appendix Table A7 estimates differential effects of upwind fires on our primary birth outcomes in subsamples defined by child sex, maternal age, and maternal education. For birth weight outcomes, these estimations suggest stronger effects among more vulnerable groups, such as less-educated mothers and those in the early or late years of the reproductive periods (younger than 25 or older than 35), although differences in effects across subsamples are not statistically significant. No clear patterns emerge across subsamples for outcomes related to gestational age at birth, although measurement error may be more of a concern for more

[21]In unreported results, we also estimated equation (2) in enlarged samples that include municipalities within 10km, 15km, 17.5km, or 20km of the air monitoring stations. Consistent with increased measurement error in wind, the estimated effect on birth weight shrinks as we include municipalities further from the stations. For these respective samples, our point estimates (standard errors) for the final gestational period are -20.3 (6.2), -14.6 (7.3), -11.0 (5.6), and -9.0 (6.0).

vulnerable groups (e.g., the less-educated) if they are more prone to misreporting the date of the last menstrual period.

5.3 Mechanism

The effects estimated in Section 5.2 are reduced-form, so they are limited in their ability to speak to mechanisms. Nonetheless, the negative effect of pollution exposure in the final gestational period on both birth weight and gestational age at birth is consistent with inflammation in the mother inducing preterm labor. This section explores further evidence on this mechanism.

5.3.1 Proximate Determinants of Low Birth Weight

One important question relating to this mechanism is the extent to which the shortening of gestation can explain the reduction in birth weight. Low birth weight has two proximate determinants, intrauterine growth restriction—which leads to babies that are small for gestational age—and preterm delivery—in which babies who may have been developing healthily in the womb are small because they are born early. A theory involving late-pregnancy inflammation would predict an important role for the latter determinant.

Practically, distinguishing intrauterine growth restriction from preterm delivery as the reason for smallness at birth is difficult because we only have a noisy measure of gestational age at birth. In our data, the most straightforward approach to studying this issue is to exclusively analyze infants born at term (37-41) weeks. Fetal growth is slower during this period than at any point over the previous 10 weeks (Olsen et al. 2010), when the vast majority of preterm infants are born, so this approach minimizes (but does not eliminate) concerns about variations in the timing of birth within the category. When we focus on this subsample, we estimate that an additional upwind fire per week during the last three months of gestation differentially decreases birth weight by 4.5 grams, with a standard error of 6.5. The corresponding estimates for low and very low birth weight are -2.3 (std. err. = 3.4) and 0.4 (std. err. = 1.2), respectively. Because these estimates are much smaller than the effects we find in the full sample, they suggest that shorter gestation can account for much of the full sample effect.

At the same time, we cannot entirely rule out fetal growth restriction in the womb. Notably, maternal inflammation could restrict fetal growth in addition to inducing preterm labor, so this alternative mechanism is not inconsistent with the proposed theory. As mentioned above, growth at the start of the final gestational period is rapid, leaving much scope for growth restriction from placental inflammation before the fetus reaches term. Unfortunately, the course coding of gestational age limits us from investigating these issues more definitively.

5.3.2 Maternal Health

An effect of environmental conditions during gestation presumes a mechanism involving the mother's body, which may be verifiable in the data. A likely cause of pollution-related inflammation in the mother is respiratory infection, which in extremely severe cases leads to hospital admissions. As such, we draw on DATASUS data on hospital admissions charged to the public health care system, estimating the differential effects of upwind fires on hospitalizations among prenatal care patients in the public system and among women of childbearing age (15-45). Unfortunately, we lack the statistical power to detect effects by specific causes of hospitalization like respiratory infection, so we analyze counts of all hospitalizations.[22] To avoid introducing new methods, we use equation (1), our pollution specification, for estimation. That is to say, we count hospitalizations in the week leading up to date t and then regress the count on upwind fires, non-upwind fires, and covariates, all measured during the same rolling week.

We leave these data as counts because up-to-date estimates of the population at risk are not available, especially for prenatal care patients in the public system. Although a Poisson fixed effect model seems suitable for this application, the large number of date fixed effects introduces an incidental parameters problem, so we use a linear model with three alternative transformations of the dependent variable. First, we take the logarithm of the count, so that we measure proportional changes in hospitalizations within a municipality. This approach works well for all women 15-45, for whom 0 hospitalizations occur in fewer than 0.1% of rolling week cells, but less well for public prenatal care patients, who have 0 hospitalizations in nearly one quarter of cells. As such, in a second approach, we take the logarithm of the count plus one. This alternative avoids sample selectivity but also somewhat obscures the proportional interpretation of the logarithm. In the third approach, we move away from logarithms and standardize counts by their municipality-

[22]We have verified that overnight stays after the delivery of a child do not account for our results.

specific means and standard deviations.

Appendix Table A8 reports the results, revealing positive but only marginally significant differential effects of upwind fires on the extreme event of hospitalization. Unlike with the infant health outcomes, the analytic and bootstrap p-values differ substantially here. In the case of prenatal patients, each additional upwind fire is associated with 3-4% more hospitalizations, or one-twentieth of a standard deviation, with analytic p-values ranging from 0.046 to 0.13 and bootstrap p-values ranging from 0.13 to 0.24. In the case of prenatal patients, each additional upwind fire is associated with 1.5% more hospitalizations, or one-tenth of a standard deviation, with analytic p-values ranging from 0.04 to 0.09 and bootstrap p-values ranging from 0.15 to 0.25. Overall, these estimates provide suggestive evidence that maternal infection may explain the negative effects of pollution on birth outcomes.

6 Conclusions

Farmers have used fire as a tool in agriculture for thousands of years, but the health effects of this source of moderate but repeated pollution are not fully understood. We use data from a major global sugar-producing area in Brazil to study the health effects of pollution from fires used during harvest season. Using wind direction to untangle these health effects from confounding local business cycle variation, we find a causal pathway running from smoke exposure in the last three months of gestation to reduced birth weight, shorter gestation, and increased risk of fetal death. These effects are strongly significant and robust to a range of alternative model specifications. Furthermore, our reliance on wind direction is key for causal identification; by itself, panel variation in fires leads to results of the wrong sign and magnitude, likely because harvest activities are so deeply intertwined with the local economy.

The fact that field fires in our setting increase pollution only moderately, starting from a moderate base, distinguishes our study from much other research on air pollution and infant health, which tends to focus either on more extreme pollution shocks or on settings with graver baseline levels. Unfortunately, it is difficult to benchmark the magnitudes of our estimates against this existing literature because of its extremely varied findings. Stieb et al. (2012) review the epidemiological literature on *in utero* pollution exposure and birth outcomes, finding a wide range of positive to negative associations between PM_{10} exposure and birth weight, both over the entire pregnancy and in specific trimesters. In their meta-analysis, the pooled estimate for PM_{10} exposure over the entire pregnancy is significantly negative but moderate, about one-tenth the magnitude that would be implied by our final trimester fire effects if they were entirely driven by PM_{10}. But in light of the wild variation in effect sizes across studies, this pooled estimate does not provide a useful basis for comparison. In fact, this variation reinforces the motivation for our research design: that cross-section, time-series, and panel variation in air pollution is highly correlated with other economic, ecological, and climatic determinants of health, confounding typical estimation strategies.

Our findings suggest that small increases in air pollution can damage early-life health even in relatively unpolluted areas. An additional fire per week in the last gestational period leads to a 10% increase in the incidence of low birth weight (< 2500 grams)—an effect of 8 per 1000 on a base risk of 81 per 1000—and a 41% increase in the incidence of very low birth weight (< 1500 grams)—an effect of 5 per 1000 on a base risk of 12 per 1000. One implication is that the relevant dose response relationship is steep even at low pollution levels, so concerns about the health effects of air pollution should not stop at the periphery of cities or industrial centers. The pollution levels in our study are virtually ignored by environmental agencies across the globe, yet they appear to be a significant health threat. Our estimates highlight the trade-off between economic activity and pollution externalities, which is central to the study of sustainability in economic development (Dasgupta 2007).

Another implication is that the health burden of anthropogenic air pollution is not a phenomenon exclusive to the modern era. The use of fire to cook food and tend to fields long predates the industrial revolution (Pyne 1997; Scott et al. 2013), and so too does the contamination of the air by human activities. That said, the health burden of agricultural fires is likely becoming more severe in the modern era, as increases in population density and market integration intensify use of fire in rural areas, with spillovers to urban areas. Examples can by found in India, where field fires in rural Punjab send a cloud of particulate matter to nearby Delhi every November (Pande and Sugathan 2015), and in Indonesia, where land-clearing fires in the country's peatlands export haze to several Southeast Asian countries (Tacconi 2016).

However, the modern era also brings a way to mitigate the intensification of fire activity. For many (but not all) crops, combines, mechanical harvesters, and ploughs eliminate the need for fire, implying potentially large health gains from mechanization, *ceteris paribus*. In the specific case of sugarcane, its potential as a path to energy sustainability makes a broad assessment of its costs, with and without mechanization, imperative. Cost-benefit analyses that ignore either the health impacts of traditional harvesting methods or the costs of mechanization overstate the benefits of sugar-based ethanol. While adoption of these pollution-reducing technologies is proceeding rapidly in some settings, such as sugar plantations in Brazil, other settings are mechanizing more slowly. Smallholder sugar farms in Brazil are one example, as are smallholder farms across sub-Saharan Africa, where slash-and-burn agriculture remains prevalent (Andreae 1991). Because the transportability of our estimates to these varied settings is unclear, policymakers would benefit from a geographically broader assessment of the health impacts of agricultural fires. How the health impacts vary and persist over the lifecycle is another fruitful area for future research. On both fronts, careful attention to both the observed and unobserved linkages between burning and other forms of economic activity will be key to producing useful knowledge.

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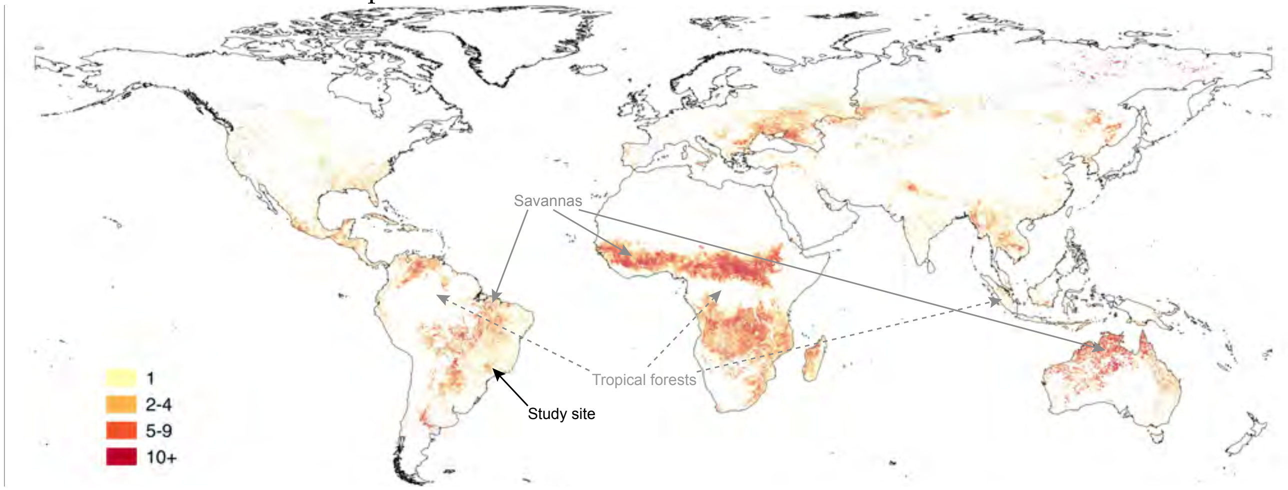
A. Fires per 5-arcminute cell, November 2000 – October 2001



B. Share of 5-arcminute cell under cropland

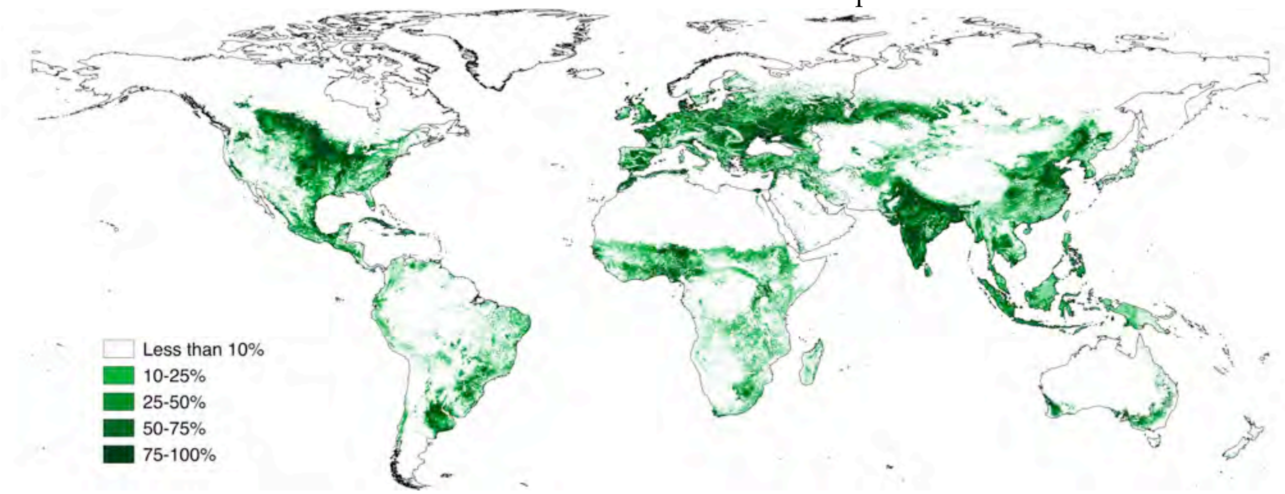


Figure 1: Global Distribution of Fires and Croplands

Note: 5-arcminute cells are approximately 100 km^2 at the Equator. Panel A is based the authors' calculations using remote sensing data from the TERRA satellite, described further in Section 3. Counts correspond to the number of 1km-by-1km pixel-days detected to contain fire; counts are not adjusted by detection confidence. Panel B is from Ramankutty et al. (2008), who estimated cropland cover by combining satellite remote sensing and agricultural inventory data.

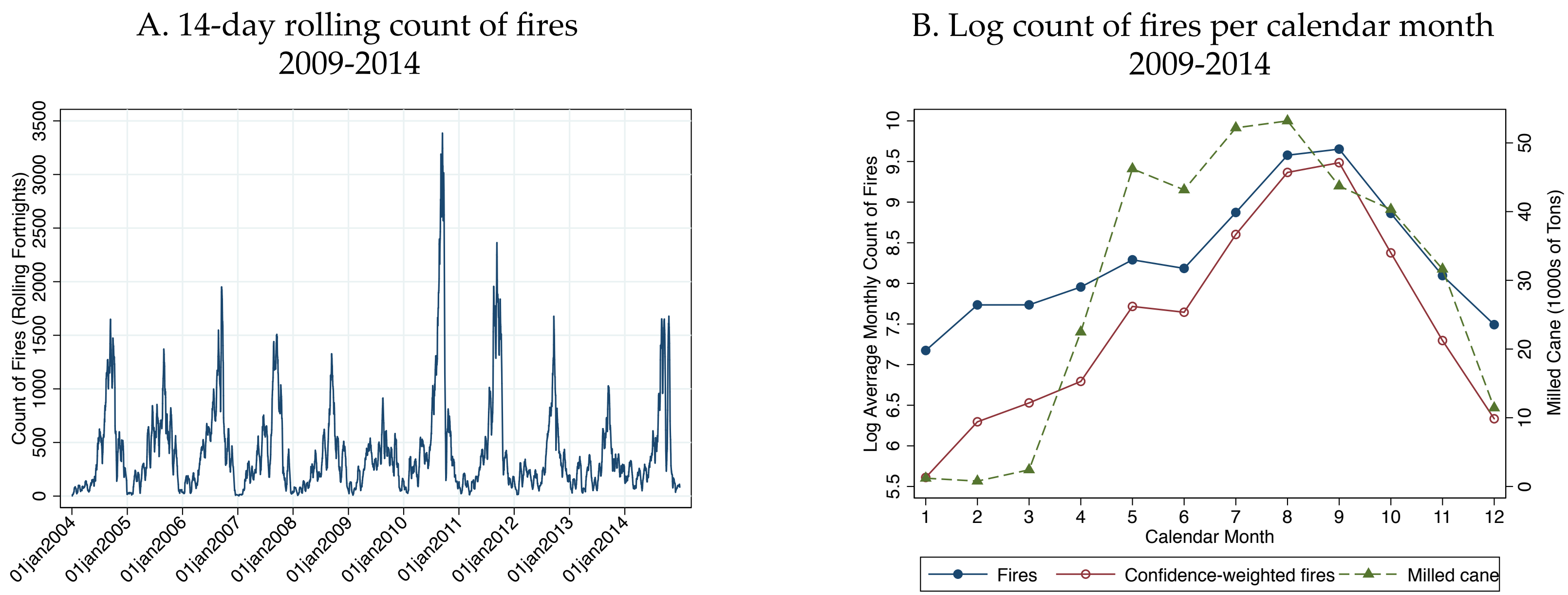


Figure 2: Trends and Seasonality in Satellites Based Fire Counts, State of São Paulo

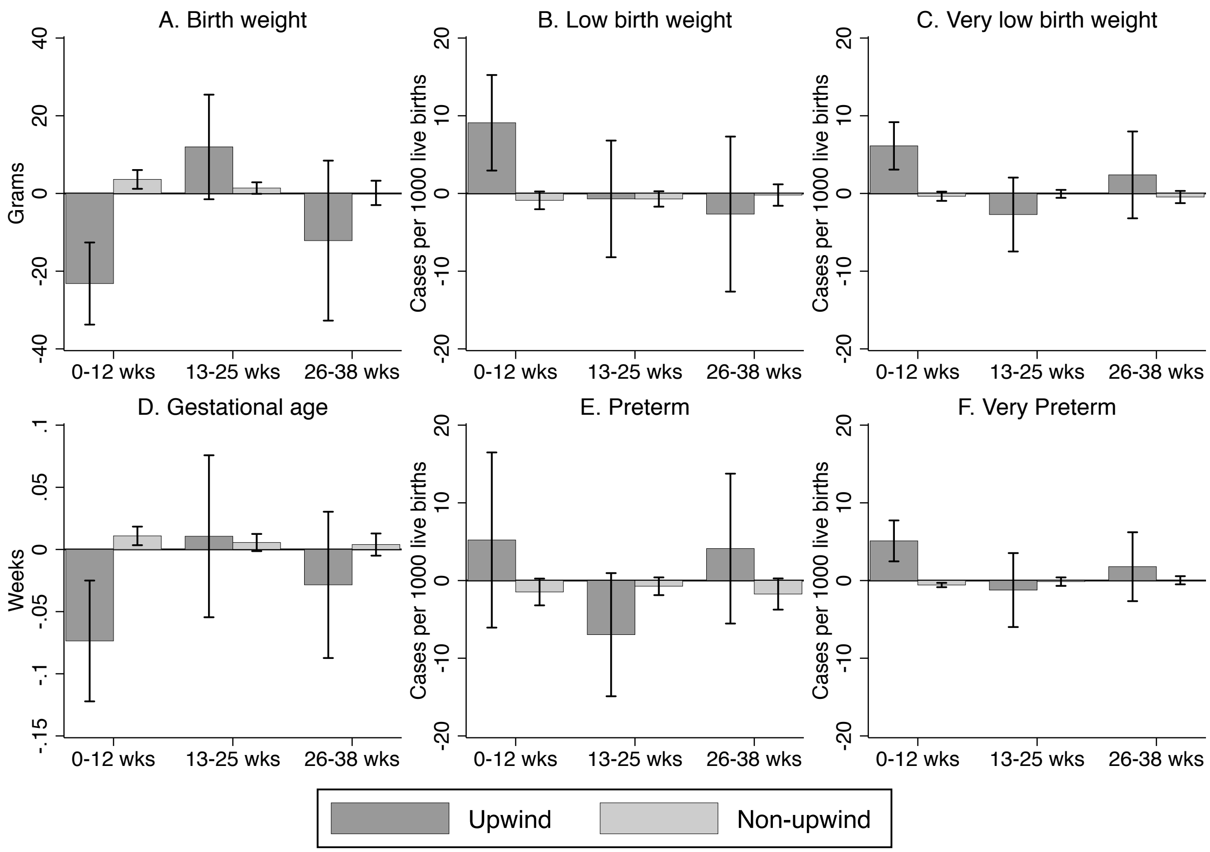


Figure 3: Fires and Birth Outcomes by Wind Direction

Note: Coefficient estimates from the models in Table 4. Capped spikes represent 95% confidence intervals based on critical values from the *t* distribution with degrees of freedom set to the number of clusters minus 1.

Table 1: Sugarcane Production, Economic Activity, and Fires

Dependent variable: Fires per 100km²

	Muni-year panel (Fixed effect)				Muni-month panel (First difference)
	[1]	[2]	[3]	[4]	[5]
1{Harvesting sugarcane in year}	0.37	0.13	0.18	0.16	
	(0.14)	(0.14)	(0.14)	(0.14)	
Share of area harvested (ref < 5%)					
5%-15%		0.57	0.58	0.53	
		(0.15)	(0.16)	(0.15)	
15%-25%		1.13	1.06	0.95	
		(0.19)	(0.20)	(0.20)	
25%-35%		1.78	1.95	1.82	
		(0.25)	(0.27)	(0.26)	
35%-50%		1.61	1.70	1.54	
		(0.32)	(0.35)	(0.35)	
50%-75%		1.53	1.73	1.54	
		(0.42)	(0.48)	(0.47)	
>75%		1.96	2.27	2.07	
		(0.75)	(1.06)	(1.06)	
Log municipal GDP per capita				0.98	
				(0.33)	
Jobs created (per 1000 residents in 2000)					0.91
					(0.19)
Jobs destroyed (per 1000 residents in 2000)					-1.49
					(0.36)
Mean of dependent variable	2.8	2.8	2.8	2.8	0.27
SD of dependent variable	3.8	3.8	3.8	3.8	0.88
Municipality FE	X	X	X	X	
Year FE	X	X	X	X	
Month FE					X
Observations	7,095	7,095	5,805	5,805	45,795
Municipalities	645	645	645	645	645
Years covered	2004-2014	2004-2014	2004-2012	2004-2012	2009-2014

Note: Parentheses contain standard errors clustered at the municipality level. All columns present means and standard deviations in levels rather than changes.

Table 2: Descriptive Statistics, Vital and Hospital Records

		Standard deviations		Sample sizes	
	Mean	Individual	Cell	Individual	Cell
	[1]	[2]	[3]	[4]	[5]
A. Birth ouctomes					
Birth weight (in grams)	3,159.3	525.7	161.8	287,506	26,190
Low birth weight per 1,000	80.8	272.4	83.7	287,506	26,190
Very low birth weight per 1,000	12.4	110.7	34.2	287,506	26,190
Imputed gestational age (in weeks)	38.6	1.7	0.5	287,506	26,190
Preterm per 1,000	97.1	296.1	93.6	287,506	26,190
Very preterm per 1,000	12.4	110.7	34.4	287,506	26,190
B. Perinatal Mortality and Morbidity					
Stillbirth per 1,000	7.7	87.6	26.9	289,748	26,198
Mortality witin 1 day of birth per 1,000	2.6	.	18.0	.	26,190
Mortality within 1 week of birth per 1,000	5.7	.	26.8	.	26,190
Hospital admissions in first full day of life per 1,000	36.7	.	63.2	.	26,190
Fetal-growth-related hospital admissions in first full day of life p	1.1	.	9.5	.	26,190
APGAR 1 minute	8.5	1.34	0.5	287,505	26,189
APGAR 5 minute	9.5	0.8	0.3	287,505	26,189
C. Infant and Maternal Demographics					
Male	0.51	0.50	0.15	287,506	26,190
White	0.76	0.43	0.17	287,506	26,190
Brown/Mullato	0.18	0.38	0.14	287,506	26,190
Mom younger than 25	0.35	0.48	0.15	287,506	26,190
Mom between 25 and 35	0.54	0.50	0.15	287,506	26,190
Mom had previous miscarriage	0.11	0.32	0.11	287,506	26,190
Mom had previous live birth	0.51	0.50	0.15	287,506	26,190
Mom formally married	0.48	0.50	0.16	287,506	26,190
Mom informally married/cohabiting	0.11	0.31	0.16	287,506	26,190
Mom college educated	0.25	0.43	0.14	287,506	26,190

Note: Cells include all births occuring in the same municipality on the same day. All cell-level statistics are weighted by the number of births. Stillbirths are per 1,000 births dead or alive; all other rates are per 1,000 live births. Mortality and hospialization data do not have individual-level values because we do not individually link them to births.

Table 3: Effects of Fires on Average Weekly Air Pollution

	PM_{10} [1]	PM_{10} [2]	PM_{10} [3]	PM_{10} [4]	PM_{10} [5]	NO_x [6]	O_3 [7]
Fires in week ending in t	0.471	0.423	0.599				
	(0.104)	(0.097)	(0.140)				
	$p^A<.01$, $p^B<.01$	$p^A<.01$, $p^B<.01$	$p^A<.01$, $p^B<.01$				
Upwind fires				2.490	2.513	-0.107	0.598
				(0.541)	(0.567)	(0.229)	(0.336)
				$p^A<.01$, $p^B<.01$	$p^A=.001$, $p^B<.001$	$p^A=.65$, $p^B=.66$	$p^A=.10$, $p^B=.11$
Downwind fires				0.631			
				(0.289)			
				$p^A=.018$, $p^B=.020$			
Other fires				0.468			
				(0.140)			
				$p^A<.01$, $p^B<.01$			
Non-upwind fires					0.489	0.088	-0.079
					(0.134)	(0.066)	(0.149)
					$p^A=.003$, $p^B=.012$	$p^A=.21$, $p^B=.20$	$p^A=.61$, $p^B=.68$
Diff. UPWIND - NON-UPWIND					2.024	-0.195	0.677
					(0.568)	(0.205)	(0.278)
					$p^A<.01$, $p^B<.01$	$p^A=.36$, $p^B=.35$	$p^A=.03$, $p^B=.02$
Date FE	X	X	X	X	X	X	X
Location FE	X	X	X	X	X	X	X
Weather covariates		X	X	X	X	X	X
Confidence-adjusted fire counts			X	X	X	X	X
Mean of dep. variable	31.4	31.4	31.4	31.4	31.4	15.3	39.5
SD of dep. variable	16.1	16.1	16.1	16.1	16.1	9.1	13.3
Observations	*26,342*	*26,342*	*26,342*	*26,342*	*26,342*	*23,448*	*27,159*

Note: Observations are at the station-date level, with fires and pollution measured over the preceding week. Robust standard errors in parentheses are clustered at the station level. p^A refers to the asymptotic p-value; p^B refers to the wild-cluster bootstrap p-value. 12 stations are observed between 2009 and 2014; one station is observed between 2009 and 2013. Fire counts include fires within 5 to 50km of the municipal population centroid; all specifications control for fires within 5km. Weather controls include average temperature, relative humidty and their interactions, direction of wind (fixed octants), periods of calm and non-measured winds.

Table 4: Effects of Fires on Birth Weight and Gestational Age at Birth

	Birth weight (in grams)					Low birth wt. per 1,000	V. low birth wt. per 1,000	Gestational age (weeks)	Preterm per 1,000	V. preterm per 1,000
			Main specification (fires by wind direction)							
	All fires	All fires	Upwind	Non-upwind	Difference	Difference	Difference	Difference	Difference	Difference
	[1]	[2]	[3]	[4]	[5] =[3]-[4]	[6]	[7]	[8]	[9]	[10]
Fires bet. weeks t and t-12	1.33	1.91	-19.66	3.35	-23.01	8.26	5.10	-0.083	4.32	5.43
	(0.80)	(1.02)	(5.50)	(0.92)	(5.54)	(2.97)	(1.74)	(0.022)	(5.54)	(1.11)
	$p^A=.12$, $p^B=.11$	$p^A=.08$, $p^B=.13$	$p^A<.01$, $p^B=.02$	$p^A<.01$, $p^B<.01$	$p^A<.01$, $p^B<.01$	$p^A=.02$, $p^B=.02$	$p^A=.01$, $p^B=.06$	$p^A<.01$, $p^B=.02$	$p^A=.45$, $p^B=.54$	$p^A<.01$, $p^B<.01$
Fires bet. weeks t-13 and t-25	1.13	2.18	10.56	2.12	8.44	0.55	-1.54	0.000	-5.55	-0.62
	(0.64)	(0.78)	(7.65)	(0.73)	(7.70)	(4.21)	(2.28)	(0.030)	(3.77)	(2.21)
	$p^A=.10$, $p^B=.12$	$p^A=.02$, $p^B=.04$	$p^A=.19$, $p^B=.20$	$p^A=.01$, $p^B=.03$	$p^A=.29$, $p^B=.32$	$p^A=.89$, $p^B=.83$	$p^A=.51$, $p^B=.55$	$p^A=.99$, $p^B=.96$	$p^A=.17$, $p^B=.14$	$p^A=.79$, $p^B=.80$
Fires bet. weeks t-26 and t-38	0.60	-0.95	-9.54	-0.26	-9.28	-3.54	1.55	-0.032	3.38	1.65
	(0.90)	(1.42)	(8.87)	(1.35)	(8.71)	(4.23)	(2.21)	(0.025)	(4.06)	(1.97)
	$p^A=.52$, $p^B=.50$	$p^A=.59$, $p^B=.94$	$p^A=.30$, $p^B=.32$	$p^A=.85$, $p^B=.89$	$p^A=.30$, $p^B=.27$	$p^A=.42$, $p^B=.64$	$p^A=.51$, $p^B=.55$	$p^A=.22$, $p^B=.23$	$p^A=.42$, $p^B=.39$	$p^A=.42$, $p^B=.47$
Date FE	X	X		X		X	X	X	X	X
Municipality FE	X	X		X		X	X	X	X	X
Weather covariates		X		X		X	X	X	X	X
Mean of dep. variable	3159.3	3159.3		3159.3		80.7	12.4	38.6	96.9	12.4
SD of dep. variable	161.8	161.8		161.8		83.7	34.2	0.7	93.5	34.4
Observations	*26,190*	*26,190*		*26,190*		*26,190*	*26,190*	*26,190*	*26,190*	*26,190*

Note: Estimates are 13-week sums of coefficients on weekly fire counts. Robust standard errors in parentheses are clustered at the municipality level. p^A refers to the asymptotic p-value; p^B refers to the wild-cluster bootstrap p-value. Observations are at the municipality-day level and are weighted by the size of local birth-cohort in that municipality-day. Fire counts are weighted by confidence and include fires within 5 to 50km of the municipal population centroid; all specifications control for fires within 5km. Weather controls include average temperature, relative humidty and their interactions, direction of wind (fixed octants), periods of calm and periods of non-measured winds.

Table 5: Differential Effects of Upwind Fires on Fetal Mortality

	Reported stillbirths per 1,000	Log live births x 100	Log births x 100	Including reported stillborns: Birth weight	Low birth wt. per 1,000	V. low birth wt. per 1,000	Gestational age (weeks)	Preterm per 1,000	V. premature per 1,000
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Fires bet. weeks t and t-12	3.91	-2.75	-2.34	-26.80	9.98	6.48	-0.084	6.69	5.68
	(1.00)	(0.95)	(1.01)	(4.88)	(3.00)	(1.51)	(0.023)	(5.62)	(1.17)
	$p^A<.01$, $p^B<.01$	$p^A<.01$, $p^B=.03$	$p^A=.04$, $p^B=.048$	$p^A<.01$, $p^B<.01$	$p^A<.01$, $p^B<.01$	$p^A<.01$, $p^B<.01$	$p^A<.01$, $p^B=.03$	$p^A=.25$, $p^B=.27$	$p^A<.01$, $p^B<.01$
Fires bet. weeks t-13 and t-2	-1.63	-0.11	-0.29	10.59	0.01	-2.66	0.005	-6.23	-1.08
	(1.51)	(1.07)	(1.15)	(6.19)	(3.49)	(2.20)	(0.030)	(3.78)	(2.17)
	$p^A=.30$, $p^B=.36$	$p^A=.92$, $p^B=.94$	$p^A=.81$, $p^B=.84$	$p^A=.11$, $p^B=.10$	$p^A>.99$, $p^B=.92$	$p^A=.25$, $p^B=.26$	$p^A=.87$, $p^B=.80$	$p^A=.13$, $p^B=.11$	$p^A=.63$, $p^B=.63$
Fires bet. weeks t-26 and t-3	2.08	0.42	0.63	-12.29	-2.45	2.85	-0.032	5.85	1.75
	(0.80)	(1.93)	(1.98)	(9.37)	(4.44)	(2.69)	(0.027)	(4.21)	(2.05)
	$p^A=.02$, $p^B=.048$	$p^A=.83$, $p^B=.84$	$p^A=.76$, $p^B=.76$	$p^A=.21$, $p^B=.18$	$p^A=.59$, $p^B=.64$	$p^A=.31$, $p^B=.28$	$p^A=.25$, $p^B=.28$	$p^A=.19$, $p^B=.13$	$p^A=.41$, $p^B=.45$
Mean of dep. variable	7.7	215.9	216.6	3147.6	85.9	16.4	38.6	101.7	12.5
SD of dep. variable	26.9	73.6	73.6	168.6	85.9	39.0	0.7	95.1	34.3
Observations	*26,198*	*26,190*	*26,198*	*26,197*	*26,197*	*26,197*	*26,197*	*26,197*	*26,197*

Note: Estimates are 13-week sums of upwind/non-upwind coefficient differences. Robust standard errors in parentheses are clustered at the municipality level. p^A refers to the asymptotic p-value; p^B refers to the wild-cluster bootstrap p-value. Observations are at the municipality-day level and are weighted by the size of local birth-cohort in that municipality-day. Fire counts are weighted by confidence and include fires within 5 to 50km of the municipal population centroid. All models include location and date fixed effects, as well as fires within 5km, average temperature, relative humidty and their interactions, direction of wind (fixed octants), periods of calm and periods of non-measured winds.

Table 6: Differential Effects of Upwind Fires on Neonatal Mortality and Morbidity

	Mortality (per 1,000)		Hospital admissions in first full day of life (per 1,000)		APGAR score (0-10)	
	1st-day	1st-week	All causes	Fetal growth causes	1-minute	5-minute
	[1]	[2]	[3]	[4]	[5]	[6]
Fires between week t and t-12	0.422	0.519	0.546	0.821	0.020	-0.013
	(1.330)	(1.798)	(4.028)	(0.586)	(0.031)	(0.035)
	p^A=.76, p^B=.73	p^A=.78, p^B=.82	p^A=.89, p^B=.92	p^A=.19, p^B=.22	p^A=.53, p^B=.57	p^A=.72, p^B=.89
Fires between week t-13 and t-	-0.202	1.760	2.431	0.458	0.052	0.014
	(0.749)	(1.621)	(4.508)	(0.400)	(0.034)	(0.023)
	p^A=.79, p^B=.72	p^A=.30, p^B=.36	p^A=.60, p^B=.59	p^A=.28, p^B=.31	p^A=.15, p^B=.22	p^A=.56, p^B=.51
Fires between week t-26 and t-	1.497	3.260	1.811	0.257	-0.042	-0.017
	(1.034)	(1.440)	(4.683)	(0.506)	(0.031)	(0.027)
	p^A=.17, p^B=.18	p^A=.04, p^B=.06	p^A=.71, p^B=.69	p^A=.62, p^B=.58	p^A=.21, p^B=.30	p^A=.54, p^B=.62
Mean of dep. variable	2.6	5.7	36.7	1.1	84.7	95.4
SD of dep. variable	18.0	26.8	63.2	9.5	4.6	3.2
Observations	*26,190*	*26,190*	*26,190*	*26,190*	*26,189*	*26,189*

Note: See Table 5.

Table 7: Robustness and Falsification Checks for Differential Effects of Upwind Fires in the Final Gestational Period on Birth Weight and Gestational

			Alternative fixed effect specifications				Falsification exercises		
	Base model	Base model + covariates					Out-of-sample births (5-year lag)	In-sample random year (Fraction less extreme)	In-sample random location (Fraction less extreme)
Outcome	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Birth weight	-23.01	-22.54	-20.64	-24.58	-29.98	-26.12	1.93	0.99	0.99
	(5.54)	(6.79)	(4.55)	(3.88)	(6.18)	(7.27)	(3.33)		
LBW	8.26	8.05	6.21	5.76	9.99	8.16	-4.26	0.96	0.94
	(2.97)	(3.26)	(2.57)	(2.80)	(3.33)	(3.45)	(3.05)		
VLBW	5.10	5.18	4.56	3.94	4.79	3.51	-0.20	0.99	0.99
	(1.74)	(1.77)	(1.37)	(1.20)	(1.25)	(1.49)	(1.37)		
Gestational age	-0.083	-0.082	-0.083	-0.054	-0.040	-0.064	-0.005	0.99	0.990
	(0.022)	(0.023)	(0.022)	(0.021)	(0.024)	(0.020)	(0.038)		
Preterm	4.32	3.92	4.32	-2.15	-1.16	4.17	-5.40	0.81	0.83
	(5.54)	(5.52)	(5.54)	(4.89)	(4.60)	(3.95)	(5.26)		
Very preterm	5.43	5.49	3.81	2.22	4.13	4.48	5.19	0.99	0.99
	(1.11)	(1.11)	(1.25)	(1.19)	(1.20)	(0.82)	(3.41)		
Date FE	X	X			X	X	X	X	X
Station FE	X	X	X				X	X	X
Weather covariates	X	X	X	X	X	X	X	X	X
Infant/mom covariates		X							
Year FE			X						
Day-of-year FE			X	X					
Station*Year FE				X	X				
Station*Week-of-year FE						X			
Observtions	*26,190*	*26,190*	*26,190*	*26,190*	*26,190*	*26,190*	*25,896*		
Replications								*100*	*100*

Note: Estimates are the sum of upwind/non-upwind coefficient differences over the final 13 weeks of the pregnancy. Robust standard errors in parentheses are clustered at the municipality level. Observations are at the municipality-day level and are weighted by the size of local birth-cohort in that municipality-day. Fire counts are weighted by confidence and include fires within 5 to 50km of the municipal population centroid; all specifications control for fires within 5km. Weather controls include average temperature, relative humidty and their interactions, direction of wind (fixed octants), periods of calm and periods of non-measured winds. Infant and maternal controls include the variables in Panel C of Table 2. Columns [8]-[9] perform 100 within-sample randomizations and report the fraction of placebo estimates that are less extreme than the effect estimated in Table 4.

ONLINE APPENDIX – NOT FOR PUBLICATION

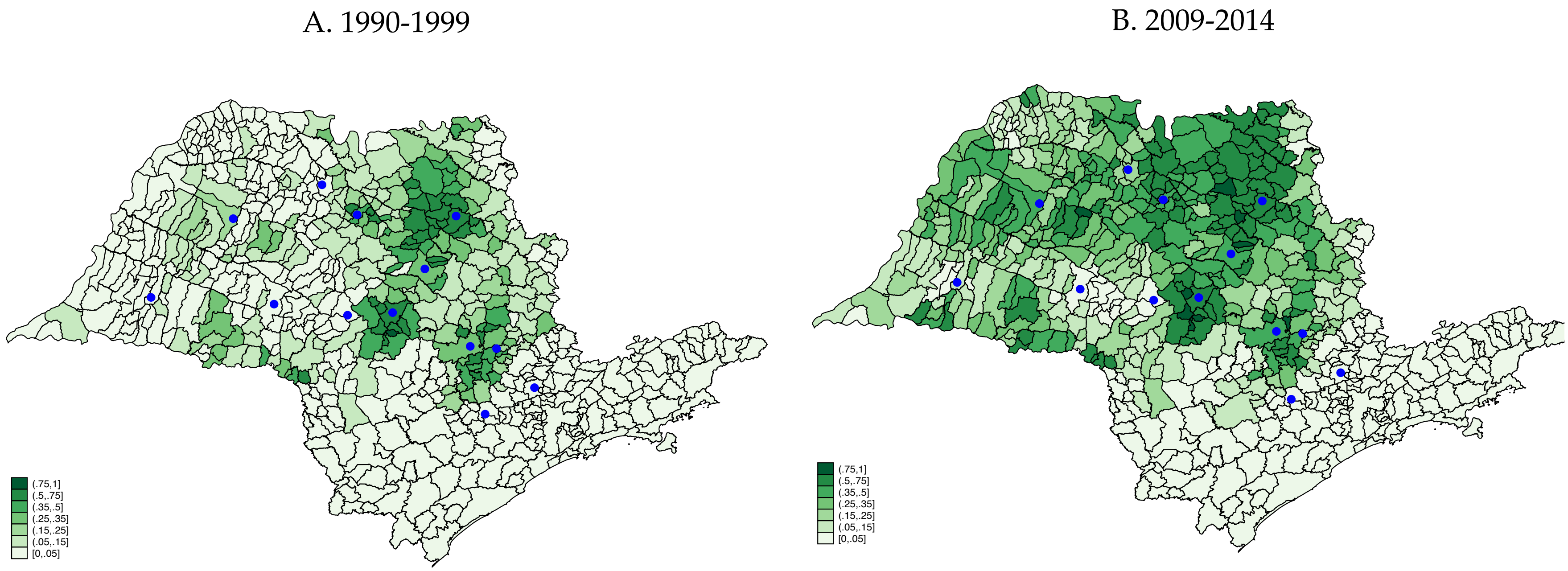


Figure A1: Sugarcane Planting as Share of Municipal Land Area
(Points indicate locations of air monitoring stations. State is approx. 900km wide.)

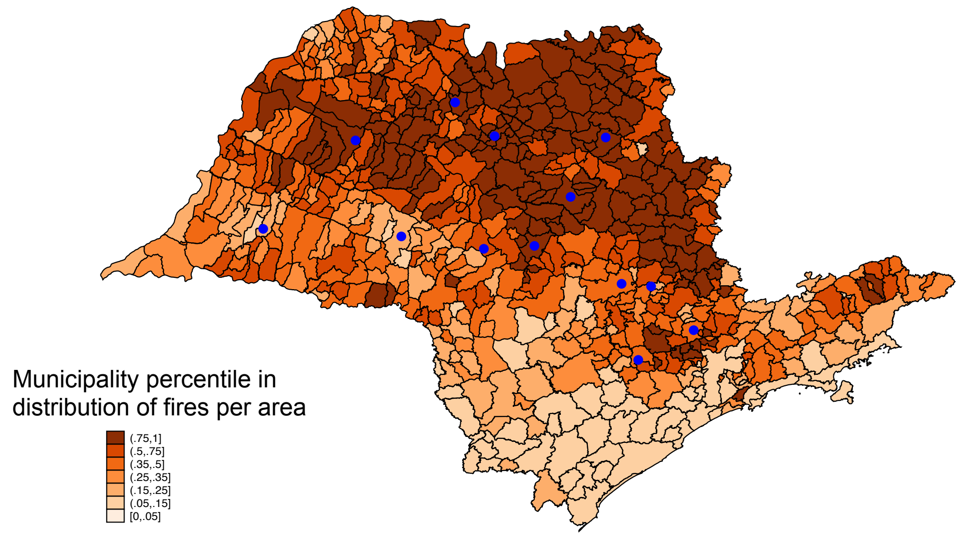


Figure A2: Spatial Distribution of Average Annual Fire Intensity, 2009-2014
(Points indicate locations of air monitoring stations)

Table A1: Share of Rolling Weeks with Missing Data, Air Monitoring Stations

	Pollutants			Weather	
	Particulate Matter	Ozone	Nitogen Oxides	Relative Humidity	Temperature
	[1]	[2]	[3]	[4]	[5]
Station 1 (Jan-1-2008 to Dec-31-2014)	0.037	0.037	0.054	0	0
Station 2 (May-19-2008 to Dec-31-2014)	0.264	0.028	1.000	0	0
Station 3 (Jul-11-2008 to Dec-31-2014)	0.026	0.036	0.055	0.017	0.006
Station 4 (Aug-20-2008 to Dec-31-2014)	0.189	0.021	0.317	0	0
Station 5 (May -9-2008 to Dec-31-2014)	0.017	0.004	0.027	0	0
Station 6 (Oct -15-2008 to Dec-31-2014)	0.014	0.002	0.013	0.192	0.009
Station 7 (Sep-25-2008 to Dec-31-2014)	0.028	0.051	0.161	0	0
Station 8 (Apr-30-2008 to Dec-31-2014)	0.042	0.057	0.076	0.152	0
Station 9 (Sep-2-2008 to Dec-31-2014)	0.017	0.032	0.196	0	0.102
Station 10 (May-15-2008 to Dec-31-2014)	0.064	0.053	0.091	0	0
Station 11 (Aug-20-2008 to Dec-4-2013)	0.045	0.031	0.084	0	0
Station 12 (Jun-26-2008 to Dec-31-2014)	0.034	0.024	0.049	0	0
Station 13 (Apr-15-2009 to Dec-31-2014)	0.036	0.034	0.072	0	0

Notes: For rolling week computations a variable was considered missing if more than 3 days had no readings within that week.

Table A2: Means/Standardard Deviations, Air Monitoring Stations

	Pollutants			Weather	
	Particulate Matter	Ozone	Nitogen Oxides	Relative Humidity	Temperature
	[1]	[2]	[3]	[4]	[5]
Station 1 (Jan-1-2008 to Dec-31-2014)	33.42/12.4	39.49/12.1	20.41/11.7	72.80/10.3	21.82/2.9
Station 2 (May-19-2008 to Dec-31-2014)	36.13/14.1	34.00/12.8	-	67.78/9.9	22.72/2.7
Station 3 (Jul-11-2008 to Dec-31-2014)	32.09/15.8	41.83/12.4	14.87/6.9	65.47/12.4	23.12/2.5
Station 4 (Aug-20-2008 to Dec-31-2014)	33.83/17.6	43.67/12.4	6.37/3.5	70.03/11.4	24.40/2.6
Station 5 (May -9-2008 to Dec-31-2014)	30.47/15.6	37.10/11.4	15.82/8.9	66.91/9.6	23.41/3.0
Station 6 (Oct -15-2008 to Dec-31-2014)	28.80/11.5	36.64/13.3	24.30/9.5	74.24/8.3	21.05/3.0
Station 7 (Sep-25-2008 to Dec-31-2014)	29.60/14.8	41.77/12.2	12.59/4.8	73.33/10.6	22.81/2.8
Station 8 (Apr-30-2008 to Dec-31-2014)	22.03/10.6	48.08/13.0	10.25/3.1	66.27/11.9	22.86/2.7
Station 9 (Sep-2-2008 to Dec-31-2014)	36.35/16.2	41.33/13.5	18.99/8.6	75.47/9.8	22.30/2.9
Station 10 (May-15-2008 to Dec-31-2014)	20.94/11.1	44.28/13.5	11.36/4.7	66.52/11.6	24.04/2.8
Station 11 (Aug-20-2008 to Dec-4-2013)	31.53/16.0	32.82/12.7	13.66/4.9	62.70/10.3	24.86/2.7
Station 12 (Jun-26-2008 to Dec-31-2014)	36.81/20.5	35.41/12.5	19.02/11.1	63.19/11.8	24.53/2.4
Station 13 (Apr-15-2009 to Dec-31-2014)	37.58/18.9	36.3/12.1	13.96/6.9	63.74/11.5	22.94/2.5
Units	$\mu g/m^3$	$\mu g/m^3$	*ppb (parts per billion)*	*Percentage*	*Celsius*

Notes: Weather condition values for missing weeks were imputed using station-specific week-of-year averages.

Table A3: Descriptive Statistics, Wind Direction

	Percent of winds originating from octant…									
	NNE [1]	ENE [2]	ESE [3]	SSE [4]	SSW [5]	WSW [6]	WNW [7]	NNW [8]	Missing [9]	% of days witout prevailing wind [10]
Station 1 (Jan-1-2008 to Dec-31-2014)	10.4	12.2	26.2	10.1	7.0	8.0	8.0	7.5	10.8	53.7
Station 2 (May-19-2008 to Dec-31-2014)	12.5	10.3	15.4	18.5	5.0	4.5	5.4	10.7	18.0	40.8
Station 3 (Jul-11-2008 to Dec-31-2014)	15.9	22.7	16.2	10.6	7.6	2.9	4.8	11.7	7.5	37.1
Station 4 (Aug-20-2008 to Dec-31-2014)	8.0	15.6	19.4	27.3	7.3	7.1	4.4	6.1	4.9	33.2
Station 5 (May -9-2008 to Dec-31-2014)	7.3	13.8	31.5	14.3	4.3	4.4	6.5	8.9	9.0	50.0
Station 6 (Oct -15-2008 to Dec-31-2014)	3.5	3.9	27.7	35.3	2.5	3.8	7.7	10.2	5.3	39.2
Station 7 (Sep-25-2008 to Dec-31-2014)	10.0	6.6	5.6	49.2	8.8	3.6	3.4	7.8	5.1	44.8
Station 8 (Apr-30-2008 to Dec-31-2014)	4.3	9.1	33.6	22.1	5.1	4.0	8.3	4.1	9.5	31.2
Station 9 (Sep-2-2008 to Dec-31-2014)	8.5	18.1	22.6	12.5	12.1	7.7	7.9	3.5	7.2	47.5
Station 10 (May-15-2008 to Dec-31-2014)	7.2	19.3	34.1	6.4	3.1	8.0	5.5	3.6	12.8	26.9
Station 11 (Aug-20-2008 to Dec-4-2013)	6.1	13.7	24.7	15.7	10.4	3.9	6.6	9.3	9.6	43.8
Station 12 (Jun-26-2008 to Dec-31-2014)	10.0	23.7	14.6	13.4	11.2	10.0	5.5	4.1	7.5	40.2
Station 13 (Apr-15-2009 to Dec-31-2014)	11.7	9.4	19.0	19.0	5.4	3.6	4.6	8.9	18.4	34.2

Notes: Days assigned prevailing wind only if the wind blows for 8 or more hours from the same octant. Prevailing wind octant is definied by a grid search moving around the circle in 10-degrees incremements. More details in the text.

Table A4: Means/Standard Deviations, Fires

	Fire counts 5-50km (mean/SD), per satellite per week		
	Fires	Confidence-adjusted fires	Confidence-adjusted fires in prevailing-wind octant
	[1]	[2]	[3]
Station 1 (Jan-1-2008 to Dec-31-2014)	1.62/3.51	1.14/3.04	0.03/0.23
Station 2 (May-19-2008 to Dec-31-2014)	3.20/4.44	1.64/3.12	0.11/0.35
Station 3 (Jul-11-2008 to Dec-31-2014)	3.96/5.07	2.53/4.86	0.20/0.70
Station 4 (Aug-20-2008 to Dec-31-2014)	2.83/4.80	2.05/4.61	0.21/0.78
Station 5 (May -9-2008 to Dec-31-2014)	1.93/2.66	1.10/2.30	0.09/0.33
Station 6 (Oct -15-2008 to Dec-31-2014)	2.51/5.83	1.56/4.79	0.04/0.19
Station 7 (Sep-25-2008 to Dec-31-2014)	3.23/4.14	1.98/3.64	0.13/0.41
Station 8 (Apr-30-2008 to Dec-31-2014)	0.96/1.45	0.63/1.28	0.05/0.22
Station 9 (Sep-2-2008 to Dec-31-2014)	2.43/3.42	1.24/2.38	0.08/0.27
Station 10 (May-15-2008 to Dec-31-2014)	1.15/1.76	0.74/1.64	0.09/0.32
Station 11 (Aug-20-2008 to Dec-4-2013)	4.67/6.85	3.12/6.30	0.09/0.42
Station 12 (Jun-26-2008 to Dec-31-2014)	3.40/4.15	2.36/4.10	0.21/0.61
Station 13 (Apr-15-2009 to Dec-31-2014)	4.15/4.70	2.76/4.59	0.22/0.56
All	*2.77/4.45*	*1.75/3.92*	*0.12/0.46*

Notes: Prevailing wind octant is definied by a grid search moving around the circle in 10-degrees incremements. More details in the text.

Table A5: Air Pollution Robustness Checks

	Base	Alternative angles		Alternative radii				Alternative fixed effects				Log transform x 100
		30°	90°	40km	30km	20km	10km					
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
A. PM10												
Diff. upwind-non-upwi	2.024	0.818	0.917	1.901	2.106	3.611	1.266	1.738	1.592	1.592	1.310	4.204
in week ending in t	(0.568)	(0.645)	(0.330)	(0.718)	(0.886)	(1.331)	(2.855)	(0.427)	(0.478)	(0.478)	(0.405)	(1.184)
Observations	*26,342*	*26,342*	*26,342*	*26,342*	*26,342*	*26,342*	*26,342*	*26,342*	*26,342*	*26,342*	*26,342*	*26,342*
B. O3												
Diff. upwind-non-upwi	0.677	0.571	0.103	0.720	0.775	0.411	1.723	0.745	0.430	0.430	0.522	1.014
in week ending in t	(0.278)	(0.287)	(0.201)	(0.248)	(0.307)	(0.459)	(1.500)	(0.196)	(0.208)	(0.208)	(0.228)	(0.486)
Observations	*27,159*	*27,159*	*27,159*	*27,159*	*27,159*	*27,159*	*27,159*	*27,159*	*27,159*	*27,159*	*27,159*	*27,159*
Date FE	X	X	X	X	X	X	X			X	X	X
Station FE	X	X	X	X	X	X	X	X				X
Weather	X	X	X	X	X	X	X	X	X	X	X	X
Year FE								X				
Day-of-year FE								X	X			
Station*Year FE									X	X		
Station*Week-of-year FE											X	

Note: Robust standard errors in parentheses are clustered at the station level. See additional notes in Table 3.

Table A6: (Placebo) Differential Effects of Upwind Fires in the Gestational Period on Infant/Maternal Demographics

	Estimate for weeks t to t-12	(se)
A. Infant Demographics		
Male	-0.580	(0.905)
White	-0.529	(1.820)
Brown/Mullato	1.610	(1.780)
B. Maternal Demographics		
Younger than 25	-0.793	(0.774)
Aged between 25 and 35	-0.009	(0.892)
Had previous miscarriage	-0.937	(1.490)
Had previous live birth	1.270	(1.830)
Formally married	-1.300	(0.773)
Informally married/Cohabiting	0.046	(3.740)
College eduation	0.031	(0.934)

Notes: Point estimates are based on linear probability models using the same specification of Table 4. Estimates are multiplied by 100 to facilitate interpretation as percentage points.

Table A7: Stratifications for Differential Effects of Upwind Fires in the Final Gestational Period on Birth Weight and Gestational Age

	Boys [1]	Girls [2]	Mom < 25 [3]	Mom 25 to 35 [4]	Mom > 35 [5]	HS or less [6]	College [7]
Birth weight	-25.02	-23.07	-32.28	-17.64	-27.10	-22.86	-18.19
	(8.69)	(12.52)	(13.67)	(7.54)	(13.20)	(10.97)	(12.50)
Low birth weight per 1,000	8.04	9.08	4.62	11.23	1.19	7.75	7.54
	(5.62)	(7.51)	(6.09)	(3.20)	(9.54)	(5.10)	(8.68)
Very low birth weight per 1,000	4.40	6.19	8.70	2.09	9.93	5.87	2.09
	(1.49)	(2.95)	(3.12)	(2.04)	(4.62)	(2.52)	(2.24)
Gestational age (in weeks)	-0.106	-0.066	-0.080	-0.088	-0.105	-0.081	-0.085
	(0.030)	(0.047)	(0.062)	(0.027)	(0.098)	(0.036)	(0.030)
Preterm per 1,000	4.93	4.89	-4.47	10.03	11.97	2.74	6.40
	(7.70)	(8.43)	(12.11)	(4.78)	(22.01)	(8.07)	(8.59)
Very Preterm per 1,000	6.70	3.90	8.33	3.33	7.02	5.98	4.10
	(1.45)	(2.60)	(3.22)	(2.15)	(4.64)	(1.72)	(3.46)
Date FE	X	X	X	X	X	X	X
Municipality FE	X	X	X	X	X	X	X
Weather covariates	X	X	X	X	X	X	X
Observations	*25,312*	*25,170*	*24,423*	*25,342*	*15,868*	*26,043*	*21,544*

Note: Robust standard errors in parentheses are clustered at the station level. See notes in Table 4.

Table A8: Differential Effects of Upwind Fires on Adult Female Hospital Admissions

	Log(count) excl. zeros x 100	Log(count + 1) x 100	Standardized count
	[1]	[2]	[3]
A. Prenatal Patients			
Diff. upwind - non-upwind	2.894	3.913	0.054
in week ending in t	(1.785)	(1.761)	(0.035)
	p^A=.131, p^B=.220	p^A=.046, p^B=.128	p^A=.152, p^B=.244
Observations	*21,093*	*27,608*	*27,608*
B. Women 15 to 45			
Diff. upwind - non-upwind	1.511	1.544	0.106
in week ending in t	(0.829)	(0.811)	(0.045)
	p^A=.094, p^B=.248	p^A=.081, p^B=.224	p^A=.036, p^B=.152
Observations	*27,591*	*27,608*	*27,608*

Note: Robust standard errors in parentheses are clustered at the municipality level. p^A refers to the asymptotic p-value; p^B refers to the wild-cluster bootstrap p-value. Dependent variables are rolling week counts. The standardized count subtracts the location specific mean and divides by the location-specific standard deviation. Fire counts are weighted by confidence and include fires within 5 to 50km of the municipal population centroid. All models include location and date fixed effects, as well as fires within 5km, average temperature, relative humidty and their interactions, direction of wind (fixed octants), periods of calm and periods of non-measured winds.