New Beginnings: The NO_x Budget Trading Program and Infants' Health[*]

Nahid Tavassoli[†]

Abstract

This paper examines the impacts of the Nitrogen Oxide Budget Program (NBP), a program that created a cap-and-trade market to regulate ozone pollution, on infants' health outcomes. I employ the universe of birth records in the US from 1995 to 2008 and implement event studies under a triple-difference identification strategy. I show that while the NBP resulted in reductions in earnings and employment, the health benefits driven by pollution reduction resulted in net improvements for infants' health. A full exposure to the NBP reduces the incidence of low birth weight and very preterm birth by about 5.1% and 14.1%. A heterogeneity analysis suggests substantially larger effects among Black mothers, low-educated mothers, and single mothers. I provide empirical evidence to rule out endogenous changes in the selective fertility behavior of parents and changes in healthcare that could confound the estimates. A series of event studies does not support the concern that the effects reflect pre-existing trends in birth outcomes. I discuss the economic significance of the results in light of other exposures and their later-life impacts.

Keywords: Pollution Abatement, Cap-and-Trade Programs, Ozone, Birth Outcomes, Prenatal Development, In-Utero Exposures

JEL Codes: H75, I18, J13, Q48, Q52, Q53

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[†] Department of Economics, University of Wisconsin Milwaukee, Milwaukee, WI 53211, USA
Email: nahidtav@uwm.edu

1. Introduction

The infants' health production function depends on several inputs, including cross-generational genetic endowment, consumption of materials that directly affect health such as nutrition, and external factors like residential location that can indirectly impact health (Corman et al., 2017, 2018; Grossman, 1972). Studies in various settings highlight the relevance of maternal inputs and external factors during the prenatal development period for birth outcomes (Almond & Currie, 2011).

Public policies can influence both of these channels and change the trajectory of infants' health outcomes. For instance, studies document the health benefits for infants of several social insurance programs, including the Food Stamp (Almond et al., 2011), the Nutrition Program for Women, Infants, and Children (WIC) (Bitler & Currie, 2005), Medicaid (Goodman-Bacon, 2018), Earned Income Tax Credit (Hoynes et al., 2015), and Unemployment Insurance (Noghanibehambari & Salari, 2020).

A smaller strand of this literature evaluates the infants' health effects of environmental policies, specifically those implemented by the Environmental Protection Agency (EPA). For instance, DeCicca & Malak (2020) examine the impact of the Clean Air Interstate Rule (CAIR), an EPA program to reduce power plant emissions in the Eastern US. They find sizeable impacts on air quality and benefits for infants' health outcomes. Chay & Greenstone (2003b) document the benefits of the Clean Air Act Amendment (CAAA) on infant mortality.[3]

One notable example of these policies is the Nitrogen Oxide (NO_x)[4] Budget Program (hereafter, NBP). The NBP was an EPA initiative that created a cap-and-trade pollution abatement market for large power plants in several Eastern and Midwestern United states. Although the program was designed to reduce ground-level ozone (O_3) pollution during summer months, it affected a wide array of outcomes in exposed localities. There is evidence that the NBP reduced employment and workers' wages (Mark Curtis, 2018), firms' profits (Linn, 2010), housing prices in manufacturing-intensive areas (Agarwal et al., 2019), crime (Chen & Li, 2020), and pollution (Deschênes et al.,

[3] I should note that this literature does not always provide a conclusive association between these policies and birth outcomes. For instance, Gehrsitz (2017) evaluates the impact of the staggered introduction of low emission zones on pollution and infants' health. He finds reductions in average city-level particulate matter by about 4-8 percent. However, the results do not reveal a discernible impact on infants' health.

[4] Nitrogen oxide is the sum of nitric oxide (NO), nitrogen dioxide (NO_2), and other oxides of nitrogen.

2017). On the other hand, several strands of research link infants' health outcomes to changes in local areas' crime, housing wealth, pollution, income, and job prospects (Currie et al., 2022; Leifheit et al., 2020; Lindo, 2011). However, the literature on the net effects of the NBP on infants' health is limited. The current study aims to fill this gap.

From an empirical standpoint, the direction of the effects of the NBP is a priori unknown. Potential negative effects on local economic markets, workers' wages, and housing prices might be offset by benefits arising from reductions in crime and pollution. In this paper, I empirically examine this link. Specifically, I evaluate the effects of the NBP on infants' health outcomes.

I build on the study of Deschênes et al. (2017) to exploit three sources of variation in a triple-difference econometric framework: the variations across counties/states and over time in the introduction of the program and the policy design that the NBP was only in effect during summer months. Therefore, I compare birth outcomes of those in NBP states versus those in non-NBP states (first difference), in years after the program implementation versus years before (second difference), and who were exposed at differential dosages during the in-utero period due to cross-month variation in the program's design (third difference). I find that a full exposure to the program during pregnancy is associated with roughly 17 grams higher birth weight and about a 5.1 percent reduction in low birth weight. I also observe reductions in the incidence of very preterm birth by about 14.1 percent.

Several heterogeneity analyses reveal substantially larger effects among Blacks, low-educated mothers, and unmarried mothers. I find larger effects for exposure during late pregnancies and specifically in the third trimester. I also implement two-stage least-square estimations and show that a one-unit rise in ozone (from an average of 29 ppb[5]) is associated with 2.7 and 6.7 percent rise in low birth weight and very preterm birth, respectively, with respect to the mean of the outcomes. I implement a series of event studies and rule out the concerns that these effects are based on preexisting trends in outcomes. Moreover, I show that the effects are not driven by selective fertility and changes in the sociodemographic composition of births as a result of the program implementation.

The contribution of this paper to the literature is twofold. First, to my knowledge, this is the first study to examine the health effects of the NBP on infants. The NBP was part of the Clean Air Act

[5] Ppb stands for parts per billion.

Amendments (CAAA), a law passed by Congress and implemented by the Environmental Protection Agency (EPA). Public policies and government programs are justified partly by the calculations of their net benefits. Therefore, in the presence of overlooked externalities, policy measures are usually suboptimal. This paper provides empirical evidence regarding the overlooked benefits of a recent and successful environmental policy by revealing its effects on infants' health outcomes. Moreover, infants' health is an important outcome to study due to the later-life and life-cycle influences of health endowment at birth. Several studies link infants' birth outcomes directly to later-life cognition, education, earnings, and even old-age mortality outcomes (Behrman & Rosenzweig, 2004; Black et al., 2007; Cook & Fletcher, 2015; Fletcher, 2011; Joyce, 1999; Maruyama & Heinesen, 2020; Royer, 2009; Samaras et al., 2003; Shenkin et al., 2009). Studies that examine the Fetal Development Hypothesis and Fetal Origins of Adult Disease argue that changes in fetal programming and health endowment at birth change the life-course trajectory of individuals. Hence, negative shocks and adversities during the in-utero period could be detected in a wide range of later-life outcomes (Almond et al., 2012; Almond & Mazumder, 2005; Barker, 1994, 1995, 1997; Barker et al., 2002; Cunha & Heckman, 2007; Currie et al., 2014; Fletcher, 2018; Godfrey & Barker, 2000; Heckman, 2007; Noghanibehambari, 2022a, 2022b; Rosales-Rueda, 2018; Sotomayor, 2013).

Second, as I discuss in section 6.2, the main benefits of the program operate through reductions in pollution. The NBP specifically targeted ozone pollution by affecting the intermediary NO_x gases that are inputs for producing ozone. Examining health effects of ozone, by studying the NBP, is important for several reasons. Ozone has a ubiquitous presence and is a major component of smog, making it relevant to a large population. Az ozone also contributes to global warming both directly through its greenhouse gas properties[6] and indirectly through its interactions with atmospheric chemistry and dynamics, studying its effect is important in the context of climate change. Further, ozone has important impacts on respiratory health. Based on annual reports of the American Lung Association's 2023, about 120 million Americans, about 36 percent of the nation's population, live in cities with ozone levels below the set standards. These standards are usually controversial[7] due

[6] Ozone is a potent greenhouse gas. Like other greenhouse gases such as carbon dioxide (CO2) and methane (CH4), ozone absorbs and re-emits infrared radiation in the Earth's atmosphere. This process traps heat and contributes to the warming of the Earth's surface, leading to global warming.

[7] In the United States, the National Ambient Air Quality Standards (NAAQS) set limits for ozone levels at 70 ppb for an 8-hour daily maximum. However, the World Health Organization (WHO) Air Quality Guidelines' 2021

to my limited knowledge of a causal relationship between ozone and health.[8] Therefore, quantifying the effectiveness of the NBP on infants' health is important not only for informing policy designs but also for re-evaluating the thresholds for healthy ozone levels.

The rest of the paper is organized as follows. Section 2 provides a background of the program and related studies. Section 3 introduces data sources. Section 4 discusses the econometric method. Section 5 goes over the results. Section 6 discusses the mechanisms. The paper is concluded in section 7.

2. Background

2.1. Nitrogen Oxide Budget Program

The NO_x Budget Program (NBP) was initiated in 2003 as the third market-based cap-and-trade program implemented by the Environmental Protection Agency (EPA). Its primary objective was to limit ground-level ozone pollution during the ozone season (May to September) by reducing the emissions of nitrogen oxide (NO_x) from electric generating units and large industrial combustion sources, particularly boilers and turbines in the Eastern and Midwestern United States.[9]

Under the NBP, a cap was established for total NO_x emissions, and allowances were distributed to each affected source. Each allowance granted permission for a specific quantity of emissions, typically one ton. This approach provided individual sources with flexibility in how they complied with emission limits. They had the option to sell or bank any excess allowances if they managed to reduce emissions and had more than they required, or to purchase allowances if they were unable to keep their emissions within the allocated limit. As a result of this program, NOx emissions dropped by 43 percent between 2003 and 2008 (EPA, 2009).

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recommend that ambient ozone levels should not exceed 50 ppb as an 8-hour maximum moving average within a day.

[8] The studies that examine the ozone-health relationship almost exclusively rely on observational data with the potential for confounding factors. For instance, see the following studies that examine the effect of ozone on birth outcomes: (Salam et al., 2005), (Rappazzo et al., 2021), (Lin et al., 2015), (Bai et al., 2022), (Ekland et al., 2021), (Rammah et al., 2019), (Nuvolone et al., 2018), (Currie & Neidell, 2005), (Zhang et al., 2016), and (Currie et al., 2009). There is also a strand of literature that examines the impact of ozone on other health outcomes and health-related expenditure, e.g., (Bell et al., 2004), (Jerrett et al., 2009), (Neidell, 2009), (Lleras-Muney, 2010), (Moretti & Neidell, 2011), and (Dominici et al., 2014).

[9] The Ozone Transport Commission NO_x Budget Trading Program (OTC NBP) was the first cap-and-trade program that targeted ozone reduction in the US. It began in 1999 and ended in 2002. The OTC states include Connecticut, Delaware, Maryland, Massachusetts, New Hampshire, Maine, Vermont, New Jersey, New York, Pennsylvania, Rhode Island, and the District of Columbia.

Figure 1 depicts NBP states and non-NBP states in the final sample in green and red, respectively. I should note that although the NBP was implemented in 2003, several states adopted the policy in 2004. States that adopted the policy in 2003 include Connecticut, Pennsylvania, New York, New Jersey, Rhode Island, and Massachusetts. States that adopted the NBP in 2004 include West Virginia, Michigan, North Carolina, Indiana, Virginia, Tennessee, Illinois, Alabama, Missouri, Ohio, Kentucky, South Carolina, Maryland, Delaware, and District of Columbia.

I follow Deschênes et al. (2017) and remove the following states from my sample: Wisconsin, Iowa, Missouri, Georgia, Mississippi, Maine, New Hampshire, and Vermont. This exclusion is either due to the state geographic location being in downwind of the NBP states (e.g., Maine) or having a significant section of their border adjacent to the NBP areas. Therefore, the true treatment value is ambiguous for these states. However, in Appendix D, I show the robustness of the results to alternative state selections.

2.2. Literature Review

A narrow and recently growing literature evaluates the impacts of the NBP on various local measures, including crime, pollution, mortality, labor market, and housing prices. For instance, Deschênes et al. (2017) use a triple-difference method to examine the effect of the NBP on mortality, medication expenditure, and hospitalizations. Their results show that the NBP decreases the ground-level ozone pollution by reducing various NO_x components. They also find that the NBP leads to a significant reduction in mortality and medication purchases. However, they did not find a discernible effect on hospitalization. Chen & Li (2020) explore the effects of pollution on crime rates. They use a triple-difference strategy and exploit the location and timing of the NBP as a plausibly exogenous shock on pollution to estimate the causal effect of air pollution on criminal activities. They find a significant effects of the NBP on reducing violent and property crime rates. Moreover, the NBP could affect labor market outcomes by imposing significant costs on nonattainment industries. Curtis (2018) shows reductions in earnings for newly hired workers and specifically young workers in these industries. They show that the largest employment reduction occurred among workers in the manufacturing sectors. Agarwal et al. (2019) estimate the housing markets impact of the NBP among participating states. Their findings suggest that in regions with high manufacturing intensity, the NBP has a negative impact on housing prices. On the other end, regions with a low manufacturing intensity, they find an increase in housing prices.

These outcomes may operate as pathways between the NBP and birth outcomes as I discuss below. The effect of the NBP on labor market outcomes may be transferred into parental job loss and wage reductions. A small strand of literature evaluates the link between employment-income and infants' health outcomes (Adhvaryu et al., 2019; Mocan et al., 2015). For instance, Lindo (2011) employs the Panel Study of Income Dynamics data over several decades and examines the effect of parental job loss on birth outcomes. He finds that parental job loss during pregnancy is associated with about 5% reduction in birth weight. Moreover, the small literature evaluates the role of housing security for birth outcomes. For instance, Leifheit et al. (2020) employs data from the Fragile Families and Child Wellbeing Study and show that housing insecurity is associated with adverse birth outcomes. Combining their results with those reported by Agarwal et al. (2019), there may be benefits (costs) for birth outcomes associated with the NBP for regions with low (high) manufacturing intensity.

In relation to the influence of the NBP on crime (Chen & Li, 2020), several studies document the association between local area prevalence of crime and infants' health outcomes (Brown, 2018; Masi et al., 2007). For instance, Currie et al. (2022) link New York City crime records to mothers' exact addresses in birth records data to evaluate the effect of violent crime on infants' health outcomes. They find that in-utero exposure to violent assault significantly increases the incidence of low birth weight and preterm birth.

Another plausible channel of impact relates to the pollution reduction mechanism of the NBP. While the medical literature offers a testable explanation for the adverse effects of pollution[10], a relatively large literature documents the negative impacts of pollution on birth outcomes (Apergis et al., 2019; Currie & Neidell, 2005; Dave & Yang, 2022; Gehrsitz, 2017; Ha et al., 2014; Hill &

[10] The medical literature suggests several pathways through which toxic pollutants may affect infants' birth outcomes, including oxidative stress, pulmonary and placental inflammation, coagulation, changes in oxygen–hemoglobin dissociation curve, generating DNA adducts, and changes in epigenetic programming (Kannan et al., 2006; Pilsner et al., 2009; Sabarwal et al., 2018; Shah & Balkhair, 2011). For instance, nitrogen oxideoxide may lower the defense system, limit cell growth, release cytokines in maternal blood, and pass through the placenta and directly toxify the fetus (Maroziene & Grazuleviciene, 2002; Tabacova et al., 2010). Hence, it affects the probability of the survival of the fetus. As a response, the reproductive system of the mother responds by automatic changes in the the so-called fetal programming or epigenetic programming to limit the growth of infants or to accelerate delivery with the sole purpose of ensuring the fetus's survival. For instance, in-utero exposure to pollution lowers the survival of the fetus by decreasing the amount of oxygen. The reproductive system of the mother responds to this adverse condition by changes in epigenetic programming, i.e., turning off some genes associated with physical growth, in order to raise the likelihood of infants' survival. Conditional on survival, the infant has developed smaller physical elements and has generally a lower birth weight (Almond & Currie, 2011).

Ma, 2022; Jones, 2020). For instance, Currie et al. (2015) explore the impact of toxic plants' operation on air quality and infants' health. They employ a difference-in-difference approach to compare outcomes within 1 mile of a toxic plant to those farther away after plant opening versus before. They find significant increases in air pollution in the vicinity of a plant following the plant's operation. Likewise, operating of a toxic plant within one mile of mothers' residential location could increase the incidence of low birth weight by about 3 percent. Currie et al. (2009) examine the impact of in-utero exposure to air pollution on infants' birth outcomes. They employ data from New Jersey and implement mother fixed effects to compare birth outcomes of siblings who were exposed to differential levels of pollution during in-utero. They find negative effects of ozone, carbon monoxide, and PM_{10}[11], specifically for exposures during second and third trimesters. Currie & Neidell (2005) use a comprehensive data from California and employ highly parametrized models that isolate the effect of pollution on infant mortality. They find significant increases in infant mortality rates due to exposure to Carbon Monoxide (CO).

A growing strand of research investigates the effects of government-mandated environmental programs on infants' health outcomes. For instance, Gehrsitz (2017) evaluates the impact of the staggered introduction of low emission zones on pollution and infants' health. He finds reductions in average city-level particulate matter by about 4-8 percent. However, the results do not reveal a discernible impact on infants' health. Currie & Walker (2011) examine the impact of the introduction of electronic toll collection on air quality and infants' health. They exploit changes in birth outcomes among mothers residing in the vicinity of a toll compared to those living farther away after the introduction of tolls versus before. They find reductions in the incidence of low birth weight and preterm birth by about 10.8 and 11.8 percent, respectively.

These findings are also observed across many studies that employ data from other countries. For instance, Luechinger (2014) examines the impact of mandated desulfurization of power plants in Germany on air quality and infant mortality. He anticipates that, between the years 1985-2003, about 826-1460 infant lives were saved in Germany as a direct impact of this program on air quality.

[11] PM_{10} refers to particulate matter with a diameter of 10 micrometers or smaller. These particles can be composed of various substances, including dust, dirt, soot, and liquid droplets.

3. Data Sources and Sample Selection

The primary source of data for this study comes from county-identified restricted-use Natality birth records of Vital Statistics. This data are extracted from NCHS (2020). Natality data cover the universe of birth records in the US. It provides information on infants' birth weight and clinical estimation of gestational length, two important measures of physical health at birth. The data also provide information on maternal education, age, race, marital status, and the usage of prenatal care during pregnancy. In addition, it contains limited information on fathers' race and age, when such information is available and recorded. The advantage of the restricted-use version of Natality data is that it reports county and state of residence of mother, important variables in my identification strategy. Given the NBP spanned from 2003 to 2008, I restrict my sample to birth years 1995-2008, covering several years before the NBP period.[12] Hence, I limit the sample to birth years 1995-2008. I focus on singleton births as the outcomes of multiple births may be confounded by non-intrauterine-growth factors (Thekkeveedu et al., 2021; Qin et al., 2015). Furthermore, I exclude counties in Western states as the inclusion of these counties might confound the estimate, as several states of this region had implemented pollution abatement strategies during the same period (Collantes & Sperling, 2008; Cushing et al., 2018).[13] These selections leave us with a sample size of 23,780,301 births.

I use previous studies to define various derivative outcomes from information available in Natality files. My primary outcomes are birth weight (measured in grams) and gestational age (measured in weeks). I define low birth weight, very low birth weight, and extremely low birth weight as dummy variables, each equaling one if birth weight is less than 2,500, 1,500, and 1,000 grams, respectively, and zero otherwise. Similarly, I define preterm birth (very preterm birth) as binary variables indicating a gestational age of less than 37 (28) weeks. Finally, fetal growth is a continuous variable that captures average weekly weight gain of infants and is defined as birth weight divided by gestational age.

[12] The NBP was replaced by another EPA program in 2009: Clean Air Interstate Rule (CAIR). There is evidence of the benefits of CAIR for infants' health that might confound my estimates (DeCicca & Malak, 2020). Even though the Clean Air Interstate Rule (CAIR) replaced the NBP in 2009 (DeCicca & Malak, 2020). CAIR was initially put into effect in 2005, despite encountering legal challenges. To show that the results are not influenced by the impact of CAIR, as a robustness check, I restrict the sample to the years 2000-2006 and reproduce the main results. I report these results in Appendix G and find quite comparable point estimates.

[13] In Appendix D, I show that the effects of the NBP on birth outcomes are very similar to those found in my main analysis.

I extract state-year NBP status from Deschênes et al. (2017). Since the NBP was active during the summer months (May-September), I construct a state-year-month panel of the NBP status that equals one if the state is among the NBP states, the year is post-NBP, and the month falls within the May-September range. Otherwise, the NBP status dummy is zero. My purpose in merging this data with Natality data is to calculate the average prenatal exposure. In doing so, I utilize gestational age information to estimate the month and year of conception. I then calculate the average NBP status for the period between conception and birth and use it as the primary in-utero exposure measure. For instance, assume that an infant born in March 2005 in Illinois, with a gestational age of 37 weeks. This infant was likely conceived in June 2004. The NBP status is one for Illinois between May-September 2004. Therefore, about 33 percent of the infant's in-utero period was exposed to the NBP. The calculated average exposure measure for this infant is 0.33.

I also use pollution data extracted from the Daily Summary dataset of the Environmental Protection Agency (EPA). This dataset reports pollution measures at the monitor-pollutant-daily level. My focus is on nitrogen oxide and ozone. I aggregate the data to county-year-month level and assign it to each birth record based on gestation-specific average exposure, as explained above.

To control for other local confounders, I include a series of county-year controls. Population composition data are extracted from SEER (2019). Income and employment data are extracted from the Bureau of Economic Analysis. Finally, government expenditure data are extracted from Pierson et al. (2015).

Summary statistics of the final sample is reported in Table 1 for NBP states and non-NBP states in the left and right panels, respectively. The average birth weight is 3,309 (3,298) grams for NBP (non-NBP) states. The average gestational age is 38.6 and 38.5 weeks for NBP and non-NBP states, respectively. On average, 8 percent of infants are categorized as low birth weight in both NBP and non-NBP states. There are more White mothers in non-NBP states and more Black mothers in NBP states. About 19.72 percent of mothers are Black in NBP states, and 10.26 percent in non-NBP states. Moreover, approximately 51 and 44 percent of mothers in NBP and non-NBP states have some college education (i.e., years of schooling > 12). Among mothers in NBP states, the average pregnancy NBP exposure is 0.16.

4. Econometric Method

The identification strategy exploits three sources of variation. First, there are mothers residing in counties/states that implemented the NBP and those in counties/states that did not. Second, I observe individuals over several years, both pre-NBP and post-NBP. Third, within-year variation arises because the NBP is implemented from May to September, in accordance with the program's design.[14] This empirical strategy rests on the assumption that the birth outcomes of mothers with a higher pregnancy overlap with summer (in NBP states) follow the same path and are influenced by the same factors as those mothers with a lower overlap, except for the fact that they are exposed to the potential influence of the NBP-induced reductions in ozone. Specifically, I operationalize this triple-difference strategy using the following regressions:

$$y_{icrym} = \alpha_0 + \alpha_1 exposure_{cym} + \alpha_2 X_i + \alpha_3 Z_{ct} + \xi_{ry} + \zeta_{ym} + \gamma_{cm} + \varepsilon_{icrym} \qquad (1)$$

Where y represents birth outcome of infant i in county c, in census-region r, who was born in year y and month[15] m. The variable $exposure$ measures average pregnancy exposure to the NBP, as explained in section 3. Matrix X includes dummies for maternal race, age, and education. The variable Z contains county-level covariates that vary by year. The parameter ξ represents census-region-by-year fixed effects to account for all economic shocks that are common within a region in a given year. The parameter ζ represents year-by-month fixed effects to absorb all temporal and seasonal determinants of birth outcomes. Since my strategy exploits differences in pregnancy exposure dosage to the NBP, including county-month fixed effects (γ) allows for the absorption of the confounding effects from all other programs. Hence, I compare the outcomes of infants in two dimensions: one, exposure to the NBP and many other programs potentially affecting birth outcomes, and two, exposure to all other programs except the NBP. During the same period, many other programs were in place, specifically in the Eastern US. The idea is that these programs do not co-move with the NBP in both location and timing, i.e., they are not implemented during the same months of the year in the same states. To the extent that this assumption is true, county-

[14] Ozone is strongly associated with temperature (Chattopadhyay & Chattopadhyay, 2020; Ninneman & Jaffe, 2021). Indeed, ozone pollution is more of a problem during summer than in winter monthsozone. Since the main target of the NBP was to reduce ozone, the EPA enforced a summer-based program.

[15] In this setting, 'month' refers to one of the 12 calendar months, not a combination of year and month.

month fixed effects isolate the NBP-induced variations. Finally, ε is a disturbance term. I cluster standard errors at the county level to account for intra-county correlation in the error terms.[16]

I also employ event studies to flexibly observe the evolution of effects for pre-NBP and post-NBP birth cohorts. Specifically, I replace the exposure dummy in equation 1 with a series of event dummies around birth year, as follows:

$$y_{icrym} = \alpha_0 + \sum_{k=-\underline{T}}^{k=-1} \phi_k I(y - y_c^* = k) + \sum_{j=0}^{j=\overline{T}} \varphi_j I(y - y_c^* = j) + \alpha_2 X_i + \alpha_3 Z_{ct} + \xi_{ry} + \zeta_{ym} + \gamma_{cm} + \varepsilon_{icrym} \quad (2)$$

Where y^* represents the state-specific year of the NBP implementation. The parameter $I(.)$ is an indicator function that equals one if the inside argument is true and zero if false. Therefore, ϕ represents negative event times and pre-NBP coefficients. Similarly, φ represents the set of post-trend coefficients. All other parameters are defined as in equation 1.

5. Results

5.1. Event Studies

I start by estimating Equation 2 and observe the trend in the effects across different birth years. The results are reported in Figure 2 and Figure 3 for different outcomes in different panels. I observe quite small and statistically insignificant coefficients for birth weight (top-left panel of Figure 2) and gestational age (top-left panel of Figure 3) for the years prior to the NBP.[17] This pattern rules out the concerns regarding pre-existing trends in outcomes. Post-NBP, I observe coefficients that rise in magnitude and become statistically significant for 2 or more years after the state-specific program implementation year. I observe a similar pattern of effects for other derivative outcomes across other panels: very small and mostly insignificant pre-trend coefficients and a set of post-trend effects that rise in magnitude.

[16] In Appendix C, I show the robustness of my findings by using various alternative clustering levels.

[17] In Appendix A, I replicate the results using Sun & Abraham (2021)'s estimates to account for potential heterogeneity in the treatment effects. I also show the robustness of my results by employing the difference-in-difference method developed by De Chaisemartin & D'haultfoeuille (2022).

5.2. Main Results

The main results of the paper are reported in Table 2. A full exposure is associated with an approximately 17 grams higher birth weight (column 1) which is quite a small change relative to the mean of the outcome (0.5%). However, when I look at the effects for adverse outcomes related to birth weight and those at the lower tail of birth weight distribution, I find substantially larger impacts. Compared to the mean of the outcomes, the effects suggest a reduction of 5.1%, 11.6%, and 12.4% in low birth weight, very low birth weight, and extremely low birth weight, respectively (columns 2-4). To complete this picture, I move beyond conventional definitions of adverse birth weight dummies and evaluate the effects across a wide range of thresholds. Specifically, I define a series of dummy variables that equal one if birth weight is less than z grams where z varies between 1,000 to 3,000 in 100-gram increments. I then use each dummy as the outcome and replicate the regressions. The results are reported in Figure 4. In top panel, I report the marginal effects, and in the bottom panel, I depict the implied percentage change with respect to each outcome. Although not strictly monotone, an increasing percentage change is observed as I move toward lower birth weight thresholds, suggesting that the effects are more pronounced among women and infants at higher risks and at the bottom of birth weight distribution.

In column 5 of Table 2, I report the results for gestational age. I find a significant but small increase of about 0.8 days. I also observe relatively larger effects for adverse outcomes related to gestational age. I find a reduction of 2.7% for preterm birth. I also find a reduction in very preterm birth of about 11 basis points, off a mean of 0.008. This fact also suggests that the impacts are almost exclusively concentrated among infants at the bottom of the gestational age distribution.

Birth weight and gestational growth are very strongly correlated, and they are even partly mechanically intertwined. Therefore, one may argue the effects on one are driven mechanically by the effects on the other. To search for this, I examine the impacts on fetal growth. I observe a 0.24 grams per week of gestation rise in intrauterine weekly growth. The implied percentage change (0.29%) in comparison with the percentage change of birth weight (0.54%) and gestational age (0.32%) suggests that the effects operate through both outcomes: increases in weight and rises in gestational length.

5.3. Heterogeneity Analyses

A strand of literature suggests racial and sociodemographic inequality in air quality and provides evidence that the impacts of pollution are higher for the disadvantaged population (Morello-Frosch et al., 2017; Pearce et al., 2011; Su et al., 2011; Tessum et al., 2019). In Table 3, I explore the heterogeneity of the results across subsamples. In panel A, I replicate the results among Black mothers. I observe considerably larger coefficients across all outcomes. For instance, the effect on birth weight is more than twice the magnitude of the full sample (roughly 40 grams). The effect on fetal growth is about 2.8 times larger than that of the pooled sample.

In panel B, I examine the effects among low-educated mothers, i.e., those with less than high school education. I observe effect sizes that are several times those of the pooled sample. For instance, I observe reductions in low birth weight and very preterm birth of about 19 and 45 percent, respectively, with respect to the mean of the outcomes. The only exception is the small and insignificant impact on preterm birth.

Finally, I look at the subsample of single mothers as single mothers usually have lower access to material resources. Moreover, there is evidence that single parenthood is correlated with adverse socioeconomic conditions and adverse birth outcomes (Koops et al., 2021; Mikkonen et al., 2016; Shah et al., 2011). In panel C, I report the results among single mothers. Compared with the main results, I find considerably larger effects across all outcomes. Interestingly, I observe significant and relatively large reductions in preterm birth. The effects suggest about a 7.1 percent reduction in preterm birth and a 22.2 percent reduction in very preterm birth.

Studies also document heterogeneity in the effects across different trimesters. However, depending on the criteria pollutant, outcome, and study method, the evidence is mixed. For instance, some studies find larger effects of exposure during the third trimester (Bell et al., 2008; Stieb et al., 2012), second-trimester (Ha et al., 2014), or first-trimester (Chang et al., 2012). For instance, Currie et al. (2009) find larger effects on birth weight due to the second trimester of exposure to ozone, on low birth weight due to the first trimester exposure to ozone, and on gestational age for the third trimester of exposure to CO. I explore the heterogeneity of the NBP exposure across different trimesters in Table 4. I observe larger effects on birth weight during the third trimester. The effects on low birth weight are concentrated during the first and third trimesters. The effects of the first and third trimester are slightly larger than the second trimester for gestational age. The effects on preterm birth seem to be confined in the second trimester, in line with findings of Ha et al. (2014), who observe higher risks of preterm birth due to ozone exposure during the second trimester. The effects on fetal growth are also exclusively concentrated in late pregnancy. The relevance of third-trimester exposure for birth weight and fetal growth is not unexpected, as weight gain and physical growth of infants are slightly accelerated during the third trimester (Gaillard et al., 2014; Salam et al., 2005).

I further explore heterogeneity by maternal age and child gender in Appendix B. I find larger impacts on younger mothers (aged < 25). Besides, the effects appear to be very similar in magnitude among male and female infants.

5.4. Selective Fertility

One concern in interpreting the main results of the paper is selective selection of mothers into parenthood. Fertility choices are endogenous and could be a response to changes in local conditions. It could be the case that parents are aware of the program, improvements in air quality, and its benefits for infants. Also, they may choose to plan pregnancy in a way that benefit most from the program's implementation. If parents plan accordingly and this selective fertility behavior is also correlated with other determinants of birth outcomes, such as parental sociodemographic characteristics, then the results may reflect the compositional change in birth outcomes rather than the effects of the NBP. I empirically test this concern by regressing a series of observable and available parental characteristics on exposure measure, conditional on fixed effects of equation 1. The results are reported in Table 5. I do not observe a significant change in child's gender, maternal race, maternal marital status, and paternal age. The effects on several important observables are quite small in magnitude as the percentage changes from the outcomes' mean imply (row 6 of each panel). For instance, a full exposure ($exposure$ of 1 versus 0) is associated with 0.95%, 0.58%, 0.48%, 0.26%, and 2.4% change from child being female, mother being White, mother age, mother being married, and father being less than 30 years of age, respectively. Moreover, I do not detect a statistically significant association with "mother education $\leq$ 12" dummy (column 6). However, I observe small reductions in the share of mothers with some college (column 7). This is not very concerning for two reasons. First, it is not consistent across measures of education. Second, the impact is still economically small to explain the results. In my sample, conditional on all fixed effects and covariates, the marginal effect of "mother education>12" on birth weight is about 1.2

grams (se=0.01). This is less than 10 percent of the effects of exposure as reported in the main results. The big picture of Table 5 does not point to a robust and consistent pattern of endogenous fertility and selective parenthood based on observables. Hence, I can also rule out the endogenous compositional changes in the sample based on unobservables (Altonji et al., 2005; Fletcher et al., 2021).

A final concern is that states that implemented the NBP also implemented other social programs such as expansions in Medicaid which improves healthcare access, prenatal care, and hence birth outcomes. In Appendix E, I examine the effects of the NBP on prenatal care and prenatal visits and fail to find discernible and significant associations.

6. Mechanisms

6.1. Socioeconomic Characteristics

In this section, I explore potential mechanisms between the NBP and infants' health outcomes. Specifically, I employ alternative county-level data sources to examine the effects of the NBP on different measures of sociodemographic and socioeconomic characteristics of counties. These results are reported in Table 6. I do not see a significant change based on racial composition of counties (columns 1-2). However, I observe a reduction in the number of children less than five years old (column 3), suggesting a 2.3% drop from the mean. The most interesting results appear for income and employment measures. I observe a 2.4% reduction in per capita income (column 5), an 8.5% reduction in per capita proprietary income (column 6), a 2.3% drop in per capita employment (column 11), and a 2.5% reduction in per capita wage employment (column 12). Besides, changes in other socioeconomic aspects including supplemental income, government expenditure on health, government expenditure on police, and per capita government tax revenue are statistically insignificant. Although I observe reductions in farm employment (columns 13-14), their respective coefficients are relatively small in magnitude and statistically insignificant.

The observed reductions in employment and income are consistent with the previous literature and provide a pathway through which the NBP could negatively affect infants' health outcomes (Mark Curtis, 2018). To understand the magnitude of these negative effects, I use the estimates of previous literature. Lindo (2011) shows that parental job loss (equivalent to almost a 100%

reduction in income) results in about 150 grams lower birth weight. Using the percentage change reported in column 5 of Table 6, I can estimate a reduction of about 3.6 grams in birth weight.

6.2. Pollution

Given the policy target of reducing pollution and the literature that links pollution exposure during pregnancy and birth outcomes, pollution is an important mechanism. In this section, I examine the effects of the NBP on several criteria air pollutants. While the NBP was intended to decrease ground-level ozone pollution by reducing nitrogen oxide, a major contributor to ozone formation, it is probable that the program also impacted other pollutants. This could be due to power plants reacting by cutting their production or implementing more efficient combustion technologies and pollution control measures that simultaneously reduce multiple pollutants to reduce nitrogen oxide.

In doing so, I use my aggregated county-year-month pollution data and merge it with the NBP status database. I estimate the impact of NBP status on several pollutants, conditional on fixed effects of Equation 1, for summer months (May-September) and non-summer months. The results are reported in panels A and B of Table 7. In column 1 of panel A, I observe a negative coefficient on NO_2. The effect is equivalent to a 4.6 percent reduction from the mean, although it is statistically insignificant. On the other end, I observe a positive, insignificant, and smaller effect for non-summer months (panel B). In column 2, I observe a statistically significant reduction of 0.8 ppb ozone for summer months, off a mean of 34.4. In non-summer months, the effect becomes positive but smaller in size, and statistically insignificant. I also look at other pollutants that were not targeted by the NBP as a placebo test. I observe quite small effects on PM_{10} and $PM_{2.5}$[18] that are statistically insignificant. The only anomaly is regarding the effects on $PM_{2.5}$ during non-summer months, which is positive and significant.

In panel C, I show the results of a triple-difference specification, where I interact NBP status with the summer months. This represents the difference of the difference-in-difference results of panel A and panel B. I observe considerable reductions in NO_2, ozone, and $PM_{2.5}$. For instance, the triple interaction term suggests a 1.2 units reduction in ozone, off a mean of 28.9, equivalent to a reduction of about 4.2% from the mean. I also observe a statistically significant reduction of 0.83 ppb in ambient PM2.5, equivalent to a 6.5 percent decrease relative to the baseline mean.

[18] $PM_{2.5}$ refers to fine particulate matter with a diameter of 2.5 micrometers or smaller. Like PM_{10}, $PM_{2.5}$ consists of tiny particles suspended in the air that can easily be inhaled into the respiratory system.

Although PM2.5 is emitted directly as a primary pollutant from various natural and human-made emissions sources, it is also formed as a secondary pollutant through complex chemical reactions involving NO_x, sulfur oxide (SO_x), and certain organic compounds.[19]

6.3. Two-Stage Least-Square Analyses

In section 6.2, I showed that the NBP reduced several pollutants including $NO_2$and ozone. However, my interest lies in understanding how much of the effects operate through reductions in pollution. To achieve this, I use the NBP exposure as an instrument in a two-stage least-square estimation strategy to examine the effects of average ozone pollution exposure during pregnancy on birth outcomes. In this setting, I acknowledge that my instruments changes ozone in addition to other pollutants, all of which may be detrimental for birth outcomes. However, I are not interested in estimating the effect of ozone on birth outcomes per se, but rather on the effect of changing the entire mixture of pollutants which is more informative from a policy perspective and also helps us to disentangle the NBP-pollution pathway from other alternative pathways.

The results of two-stage least square estimations are reported in Table 8.[20] To interpret these results, I should note that the average ozone during pregnancy is 29.8, with a standard deviation of 6.2 (see Table 1). The bottom panel of Table 8 shows the first-stage results, where a full exposure to the NBP is associated with a 3.1 ppb decrease in ozone, representing a reduction of about 11 percent. The top panel presents the second-stage results. A 1 ppb rise in ozone during pregnancy is associated with a 7.9 grams lower birth weight, a 22 basis points increase in the probability of low birth weight (off a mean of 0.08), and a 5.4 basis points rise in the likelihood of very preterm birth (off a mean of 0.008). I fail to find significant effects for preterm birth.[21]

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[19] The mechanism behind $PM_{2.5}$ formation from NO_X and SO_X involves the following steps: Sulfur dioxide (SO_2) transforms into sulfuric acid droplets through interaction with ammonia, giving rise to various sulfate particles such as ammonium sulfate, ammonium bisulfate, and letovicite. Likewise, nitrogen dioxide (NO_2) undergoes conversion into nitric acid, which, upon reacting with ammonia, produces ammonium nitrate particles. These reactions contribute significantly to the formation of secondary $PM_{2.5}$.

[20] In Appendix F, I replicate the main results for the subsample of available ozone data used in Table 8.

[21] I can compare the IV estimations of Table 8 with other papers exploring the effects of ozone on birth outcomes. For instance, Salam et al. (2005) use data from California and implement highly parametrized ordinary least square models and find that each additional ppb of ozone is associated with about 3.9 grams lower birth weight and 2 percent increase in the risk of low birth weight. This is slightly lower than the estimates of column 1 (7.4 grams) and the implied change of column 2 (2.9%) in Table 8. Currie et al. (2009) use data from New Jersey and employ mother fixed effects to compare outcomes across siblings who were exposed to differential levels of pollution during pregnancy. They find that a rise of 1 ppb during the second trimester reduces birth weight by about 0.7 grams. They

One interesting avenue to explore is how much of the observed reduced-form results of Table 2 can be explained by the pollution (ozone) reduction of the program, by integrating those estimates with the NBP-pollution results of Table 7 and instrumental variable results of Table 8. The main goal is to measure how much of the NBP-induced effects on pollution could affect birth outcomes and whether this effect is comparable with that of the reduced-form results. For instance, implementation of the NBP reduces ozone by 1.2 ppb. Based on Table 8, a reduction of 1.2 units in ozone is associated with 9.4 grams higher birth weight, 26 basis points decrease in low birth weight, and a 6.5 basis points reduction in very preterm birth. These effects are comparable but slightly smaller than my findings in Table 2. The reduced form effects of the NBP on birth outcomes are higher by the following percentages as compared to the case where the effects operate solely through ozone: 89% for birth weight, 60% for low birth weight, and 76% for very preterm birth. One possible reason is that the NBP was not only successful in reducing ozone but also various other pollutants that are potentially detrimental to infants' health outcomes. Another reason is that the NBP affected many other outcomes, including housing prices and crime, which could positively affect birth outcomes (Agarwal et al., 2019; Chen & Li, 2020). This line of evidence is informative for environmental policy makers as it suggests that not all health benefits operate through reductions in pollution.

7. Conclusion

Pollution abatement policies are costly to implement and add a burden to society. Therefore, it is important to understand the full benefits of programs aim at reducing pollution. This paper reveals the benefits of one such program for infants' health outcomes: the introduction of the Nitrogen Oxide Budget Program (NBP). I used the universe of birth records in the US over the years 1995-2008 to examine the effects of the NBP on birth outcomes. My identification strategy exploited state-year changes in the introduction of the NBP combined with within-year cross-month differences in exposure dosage, resulting from the fact that the program was in place during summer months only. I found significant improvements in birth outcomes. A full exposure to the program throughout pregnancy is associated with 17 grams higher birth weight, a 5.1% decrease in the incidence of low birth weight, and a 14.1% reduction in very preterm birth. I documented

also find that the same increase during the first trimester is associated with a 4.3 basis points rise in the probability of low birth weight. My estimates suggest substantially larger effects.

that the effects are larger for infants at the lower tail of the birth weight and gestational age distribution. A series of heterogeneity analyses suggested that the effects are substantially larger among Black mothers, low-educated mothers, and single mothers.

It is important to note that the concentration of toxic pollutants is highly localized (e.g., those living in the vicinity of power plants) and could have no effect on a large portion of the population. The results of Table 3 point to effects that are several times larger for the disadvantaged population, subpopulations that are more likely to live in more polluted neighborhoods (Braubach & Fairburn, 2010; Christensen et al., 2022). Therefore, my results suggest that environmental policies might be effective in reducing the observed racial and sociodemographic gap in birth outcomes.

One way to understand the magnitudes of my findings is to compare them with other policies and other determinants of birth outcomes. For instance, Chung et al. (2016) examine the infants' health benefits of Alaska Permanent Fund payments, a cash transfer program to all Alaskan residents for the sale of natural resources. They document that an increase of $2,700[22] is associated with about 18 grams higher birth weight. Therefore, a full exposure to the NBP during pregnancy is equivalent to the benefit of about a $2,685 income transfer. Hoynes et al. (2015) investigate the effects of Earned Income Tax Credit (EITC) on birth outcomes. They find that an increase of $2,500 results in 13 grams higher birth weight. Using their estimation, a full NBP exposure is equivalent to about $3,442.

Another way to understand the magnitude of effects is to focus on the consequences of adverse health at birth. For instance, Fletcher (2011) estimates the schooling outcomes of people with low birth weight. He finds an increase of 3.9 percentage-points in the probability of having learning difficulty, about a 33% change from the mean. I estimate that the NBP-induced reduction in low birth weight reduces the probability of having a learning disability by about 1.2%. Currie & Moretti (2007) estimate the intergenerational correlation of health at birth. Their estimations suggest that low birth weight mothers are 2.9 percentage points more likely to have low birth weight children, an increase of 46%with respect to the baseline mean. Hence, a full exposure to the NBP reduces future generations' probability of low birth weight by about 1.7%.

[22] All dollar values are converted into 2020 dollars.

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Tables

Table 1 - Summary Statistics

	NBP States		*Non-NBP States*	
	Mean	SD	Mean	SD
Infants' Characteristics:				
Birth Weight	3308.62	624.8	3298.27	600.4
Low Birth Weight	.08	.28	.08	.27
Very Low Birth Weight	.02	.13	.01	.12
Ext. Low Birth Weight	.01	.09	.01	.08
Gestational Age	38.61	2.64	38.54	2.59
Preterm Birth	.13	.33	.13	.34
Very Preterm Birth	.01	.09	.01	.08
Fetal Growth	85.38	14.65	85.33	14.25
Child Female	.49	.5	.49	.5
Parental Characteristics:				
Mother White	.76	.43	.8	.4
Mother Black	.2	.4	.16	.37
Mother Hispanic	.11	.31	.28	.45
Mother Age	28.85	5.82	27.93	5.77
Mother Education $\leq$ 12	.49	.5	.56	.5
Mother Education $>$ 12	.51	.39	.44	.43
Mother Education Missing	.01	.1	.01	.09
Mother Married	.69	.46	.69	.46
Father Age $\leq$ 30	.12	.32	.15	.36
Average Exposure to NBP during Gestational Period	.16	.2	0	0
Average Exposure to Ozone during Gestational Period	29.75	6.15	28.72	3.87
Average Exposure to NO2 during Gestational Period	18.88	6.59	12.04	4.75
Average Exposure to NoNoxNoy during Gestational Period	26.37	141.89	13.56	8.36
County Covariates:				
Share of Whites	.8	.16	.83	.12
Share of Blacks	.17	.15	.13	.11
Share of Children $<$ 5 Years Old	.08	.01	.09	.01
Per Capita Supplemental Income	5921.07	2846.17	4919.58	1964.73
Per Capita Total Income	45012.35	13495.74	40342.89	10379.27
Per Capita Proprietary Income	3633.17	2613.94	4220.9	2901.09
Per Capita Government Health Expenditure	48.77	50	19.03	18.9
Per Capita Expenditure on Correctional Institutions	27.78	17.84	31.85	27.37
Per Capita Expenditure on Police	27.28	33.76	34.22	29.77
Per Capita Tax	244.2	244.03	169.24	100.83
Per Capita Employment	57.69	15.92	58.4	14.47
Per Capita Wage Employment	48.29	15.38	47.37	14.29
Per Capita Farm Proprietary Employment	.54	.96	1.02	1.95
Per Capita Non-Farm Proprietary Employment	8.86	2.4	10.01	2.44
Observations	16,137,412		8,016,878	

Notes. All dollar figures are converted into 2020 dollars. The data spans the years 1995-2008. ***Birth weight*** is the infant's weight at birth and is measured in grams. ***Gestational age*** is the clinical estimation of the period between conception and birth, and is measured in weeks. ***Fetal growth*** is the weekly intrauterine weight gain of infant and is calculated as birth weight divided by gestational age. Gestational age-adjusted birth weight is computed from the predicted value of a regression of birth weight on gestational age. ***Low birth weight*** is a dummy that equals one if birth weight is less than 2,500 grams. ***Very low birth weight*** is a dummy that equals one if birth weight is less than 1,500 grams. ***Extremely low birth weight*** is a dummy that equals one if birth weight is less than 1,000 grams. ***Preterm birth*** (premature birth) is a dummy that equals one if gestational age is less than 37 weeks. ***Very preterm birth*** is a dummy that equals one if gestational age is less than 27 weeks.

Table 2 - Reduced Form Effects of Prenatal Exposure to NBP on Birth Outcomes

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average In-Utero Exposure to NBP	17.8774***	-.00417**	-.00174***	-.00099***	.12381***	-.00351	-.00113***	.24434**
	(6.40252)	(.0017)	(.00044)	(.00033)	(.03089)	(.00274)	(.00033)	(.11927)
R-squared	.0543	.015	.00639	.00494	.019	.01343	.00542	.05444
Mean DV	3304.569	0.081	0.015	0.008	38.583	0.128	0.008	85.351
%Change	0.541	-5.143	-11.587	-12.378	0.321	-2.745	-14.100	0.286

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.

Observations: 23,780,301

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

Table 3 - Heterogeneity across Subsamples

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Subsamples of Blacks:								
Average In-Utero Exposure to NBP	39.27647***	-.01434***	-.00474***	-.00285**	.2149***	-.01447***	-.00368***	.68051**
	(14.34978)	(.00494)	(.00131)	(.0011)	(.0525)	(.00545)	(.00104)	(.29704)
R-squared	.02519	.01178	.00606	.00561	.01173	.01098	.00576	.02622
Mean DV	3098.805	0.136	0.032	0.018	38.110	0.187	0.018	80.794
%Change	1.267	-10.542	-14.802	-15.811	0.564	-7.735	-20.457	0.842
Panel B. Subsamples of Mothers with Schooling < High School:								
Average In-Utero Exposure to NBP	53.76873***	-.0137***	-.00305**	-.00215**	.06468	.00627	-.0027**	1.3374***
	(12.62095)	(.00428)	(.0015)	(.00099)	(.05172)	(.00663)	(.00112)	(.28267)
R-squared	.05736	.03075	.02361	.02186	.03222	.02685	.02187	.05642
Mean DV	3314.103	0.070	0.012	0.006	38.761	0.127	0.006	85.342
%Change	1.622	-19.577	-25.433	-35.844	0.167	4.934	-45.081	1.567
Panel C. Subsamples of Unmarried Mothers:								
Average In-Utero Exposure to NBP	33.86595***	-.01086***	-.00389***	-.00197***	.20339***	-.01148***	-.00289***	.51642***
	(9.82605)	(.00305)	(.0008)	(.00056)	(.04456)	(.00412)	(.00059)	(.1874)
R-squared	.03999	.0161	.00814	.00668	.01988	.01368	.00691	.03767
Mean DV	3175.373	0.111	0.023	0.012	38.404	0.162	0.013	82.319
%Change	1.067	-9.788	-16.927	-16.398	0.530	-7.088	-22.222	0.627

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.
Observations of Panel A: 4398095
Observations of Panel B: 1250264
Observations of Panel C: 7452615
*** p<0.01, ** p<0.05, * p<0.1

Table 4 - Heterogeneity in the Effects across Trimesters

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average Exposure to NBP during Trimesters:								
First Trimester	6.40567**	-.00166*	-.00108***	-.00051**	.04924***	-.0004	-.00082***	.08587
	(2.94152)	(.00091)	(.00035)	(.00025)	(.01445)	(.00161)	(.00026)	(.06234)
Second Trimester	4.30844**	-.00093	-.00011	-.00006	.0208**	-.00163*	.00028	.06494*
	(1.68683)	(.00073)	(.00031)	(.00019)	(.0092)	(.00096)	(.00021)	(.0393)
Third Trimester	6.50218**	-.00165*	-.00071**	-.00051**	.04547***	-.00087	-.00078***	.09604*
	(2.7808)	(.00088)	(.00033)	(.00021)	(.01533)	(.00139)	(.00022)	(.05316)
R-squared	.05312	.01475	.0065	.00508	.01803	.01304	.00549	.05381
Mean DV	3298.831	0.082	0.016	0.008	38.546	0.130	0.008	85.284

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.
Observations: 20,607,496
*** p<0.01, ** p<0.05, * p<0.1

Table 5 - Exploring Selective Fertility

	Outcomes:				
	Child Female	Mother White	Mother Black	Mother Hispanic	Mother Age
	(1)	(2)	(3)	(4)	(5)
Average In-Utero Exposure to NBP	.0047	.0045	-.0048	.0028	-.1387***
	(.0038)	(.0034)	(.0032)	(.0038)	(.0499)
R-squared	.0022	.1662	.178	.2631	.0692
Mean DV	0.489	0.774	0.185	0.166	28.546
%Change	0.959	0.585	-2.610	1.713	-0.486
	Mother Education ≤ 12	Mother Education > 12	Mother Education Missing	Mother Married	Father Age ≤ 30
	(6)	(7)	(8)	(9)	(10)
Average In-Utero Exposure to NBP	.0068	-.007*	.0017	-.0018	-.0031
	(.0042)	(.0038)	(.0019)	(.0057)	(.0028)
R-squared	.0664	.0561	.021	.0614	.0242
Mean DV	0.511	0.487	0.010	0.688	0.130
%Change	1.325	-1.432	17.249	-0.260	-2.402

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects.
Observations: 24,154,279
*** p<0.01, ** p<0.05, * p<0.1

Table 6 - Changes in County Characteristics Following Adoption of NBP

	Outcomes:						
	Share of Whites	Share of Blacks	Share of Children less than 5 Years Old	Per Capita Supplemental Income	Per Capita Total Income	Per Capita Proprietary Income	Per Capita Government Expenditure on Health
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NBP Status	.002	-.002	-.002***	82.078	-1055.739**	-349.282	-.156
	(.002)	(.002)	(0)	(70.093)	(416.455)	(239.805)	(1.943)
Observations	32970	32970	32970	32307	32307	32307	20680
R-squared	.996	.997	.958	.984	.97	.839	.843
Mean DV	0.815	0.129	0.082	5638.834	4.4e+04	4100.441	46.909
%Change	0.287	-1.313	-2.264	1.456	-2.399	-8.518	-0.332
	Per Capita Government Expenditure on Correctional Institutions	Per Capita Government Expenditure on Police	Per Capita Government Tax Revenue	Per Capita Employment	Per Capita Wage Employment	Per Capita Farm Proprietary Employment	Per Capita Non-Farm Proprietary Employment
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
NBP Status	1.4	-.637	2.66	-1.339***	-1.164***	-.002	-.173
	(1.95)	(1.397)	(6.538)	(.295)	(.272)	(.02)	(.133)
Observations	20680	20680	20680	32307	32307	32307	32307
R-squared	.8	.93	.973	.988	.989	.986	.923
Mean DV	34.021	34.850	203.084	57.623	47.212	0.658	9.754
%Change	4.114	-1.827	1.310	-2.324	-2.465	-0.364	-1.776

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county and region-year fixed effects.
*** p<0.01, ** p<0.05, * p<0.1

Table 7 – Exploring the Effects of NBP on Pollution

	NO_2	Ozone	PM_{10}	$PM_{2.5}$
	(1)	(2)	(3)	(4)
Panel A.Summer Months:				
NBP Status	-.7	-.84**	-.36	-.11
	(.69)	(.37)	(.65)	(.33)
Observations	13505	38724	26191	25810
R-squared	.9	.81	.68	.74
Mean DV	15.160	34.447	28.143	13.046
%Change	-4.621	-2.449	-1.283	-0.821
Panel B.Non-Summer Months:				
NBP Status	.41	.35	-.41	.72*
	(.69)	(.24)	(.67)	(.4)
Observations	18267	37955	36840	35978
R-squared	.89	.85	.66	.73
Mean DV	19.628	24.070	24.464	12.548
%Change	2.064	1.462	-1.681	5.778
Panel C.Triple-Difference:				
NBP Status × Summer	-1.11***	-1.2***	.05	-.83***
	(.26)	(.26)	(.34)	(.16)
NBP Status	.41	.35	-.41	.72*
	(.69)	(.24)	(.67)	(.4)
Observations	31772	76679	63031	61788
R-squared	.9	.88	.68	.74
Mean DV	17.744	28.989	25.994	12.756

Table 8 - The Results of Two-Stage Least-Square Estimations to Explore the Effects of NBP-Induced Changes in Ozone on Birth Outcomes

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Second Stage: Outcomes in Columns								
Average In-Utero Exposure to Ozone	-7.87799** (4.00366)	.00218** (.00104)	.00074** (.00029)	.00051** (.00021)	-.04509** (.02127)	.00155 (.0014)	.00054*** (.0002)	-.12656* (.06905)
R-squared	.04143	.01108	.00426	.00321	.00804	.00866	.00354	.04483
Mean DV	3300.414	0.083	0.016	0.008	38.562	0.130	0.008	85.274
First Stage (Outcome: Average In-Utero Exposure to Ozone)								
Average In-Utero Exposure to NBP	-3.13*** (0.93)							
Mean DV (Ozone)	29.44							
F-Statistics	11.23							

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.

Observations: 23,917,944

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

Figures

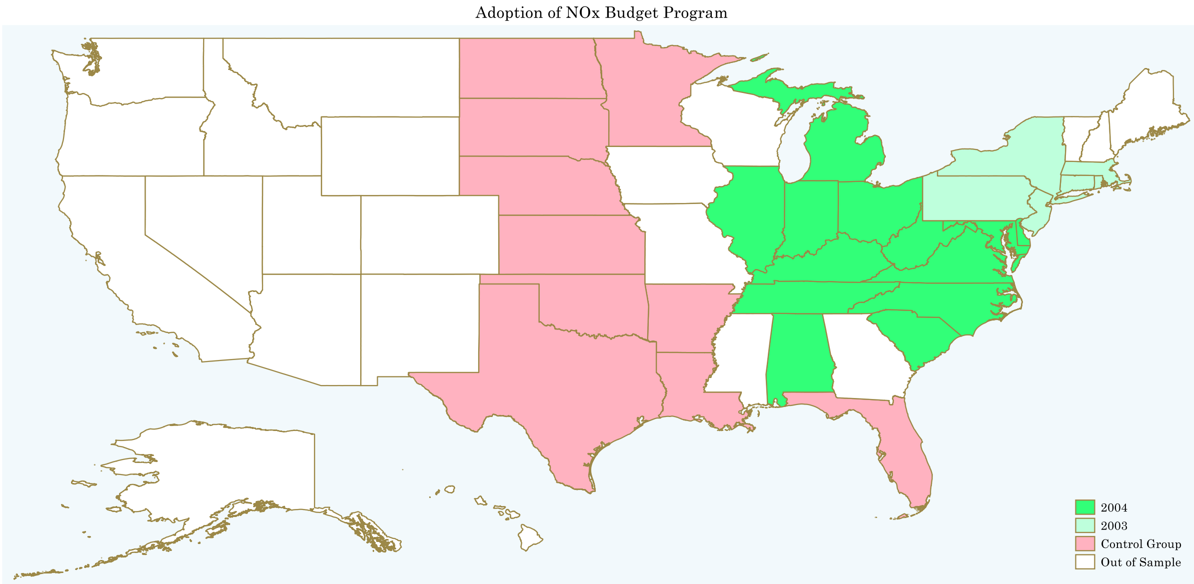


Figure 1 - Geographic Distribution of NBP Adoption across States

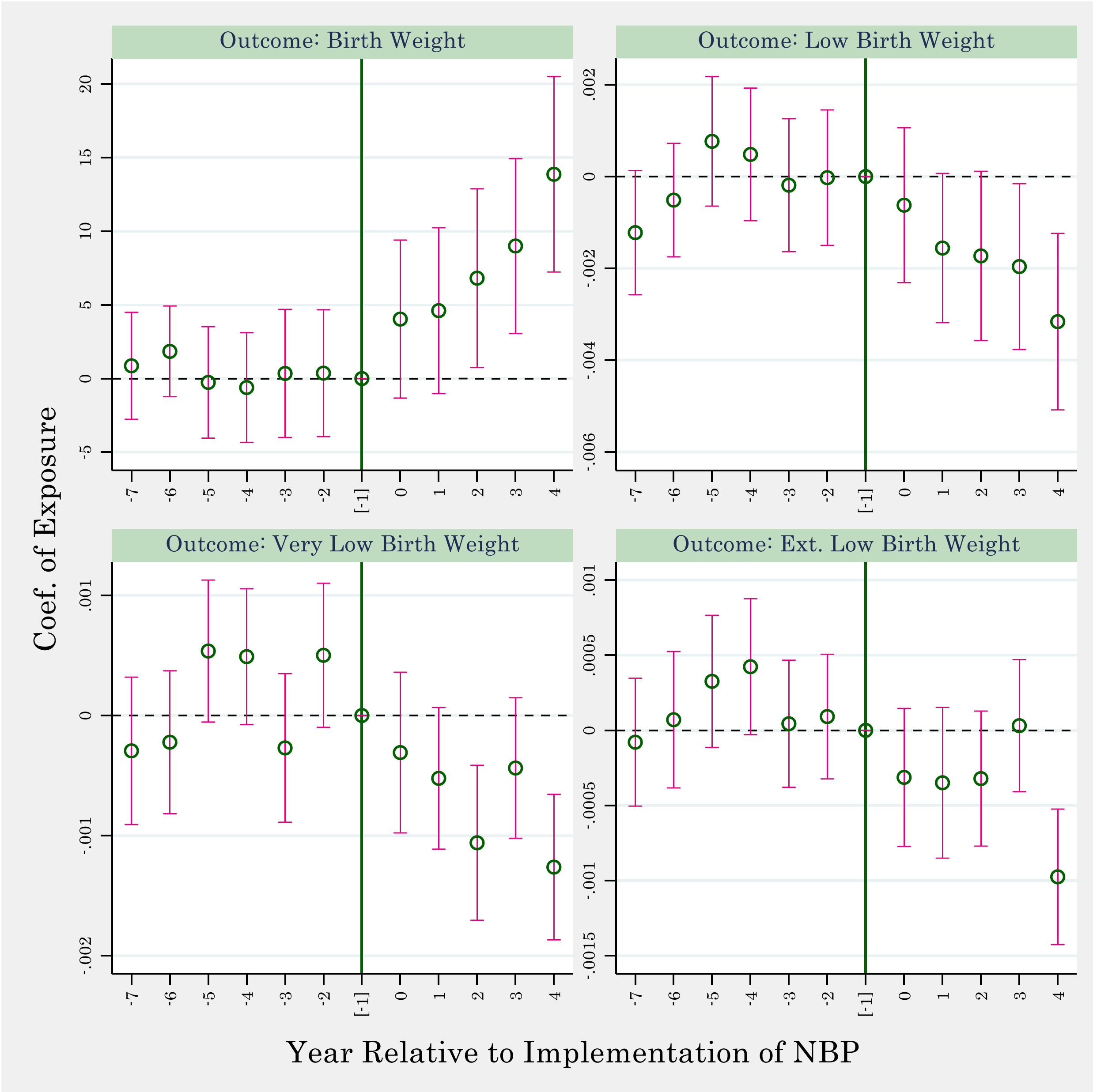


Figure 2 – Event Study Analysis of the Effects of NBP Adoption on Birth Outcomes

Notes. Point estimates and 95 percent confidence intervals are depicted. Standard errors are clustered on county. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.

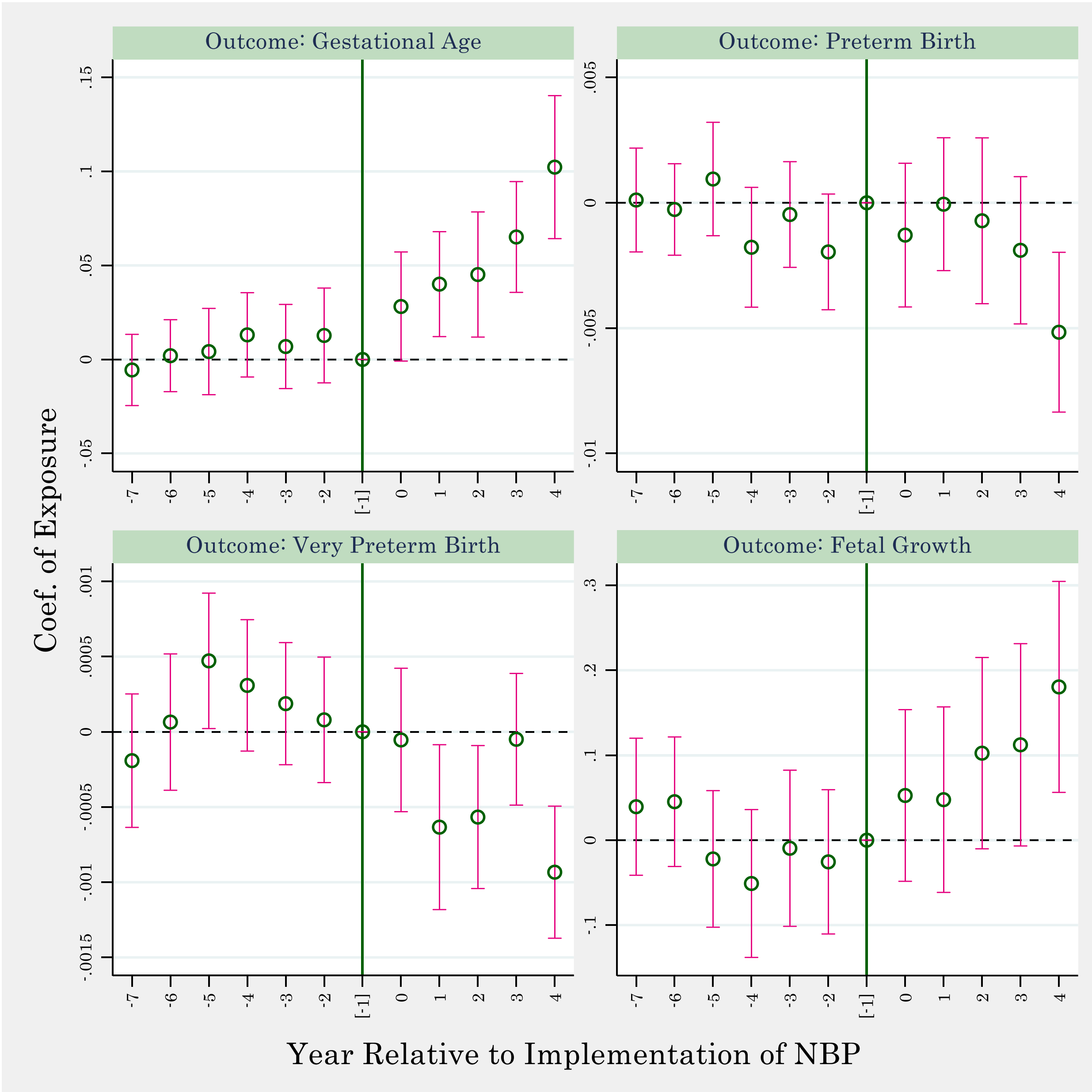


Figure 3 – Event Study Analysis of the Effects of NBP Adoption on Birth Outcomes

Notes. Point estimates and 95 percent confidence intervals are depicted. Standard errors are clustered on county. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.

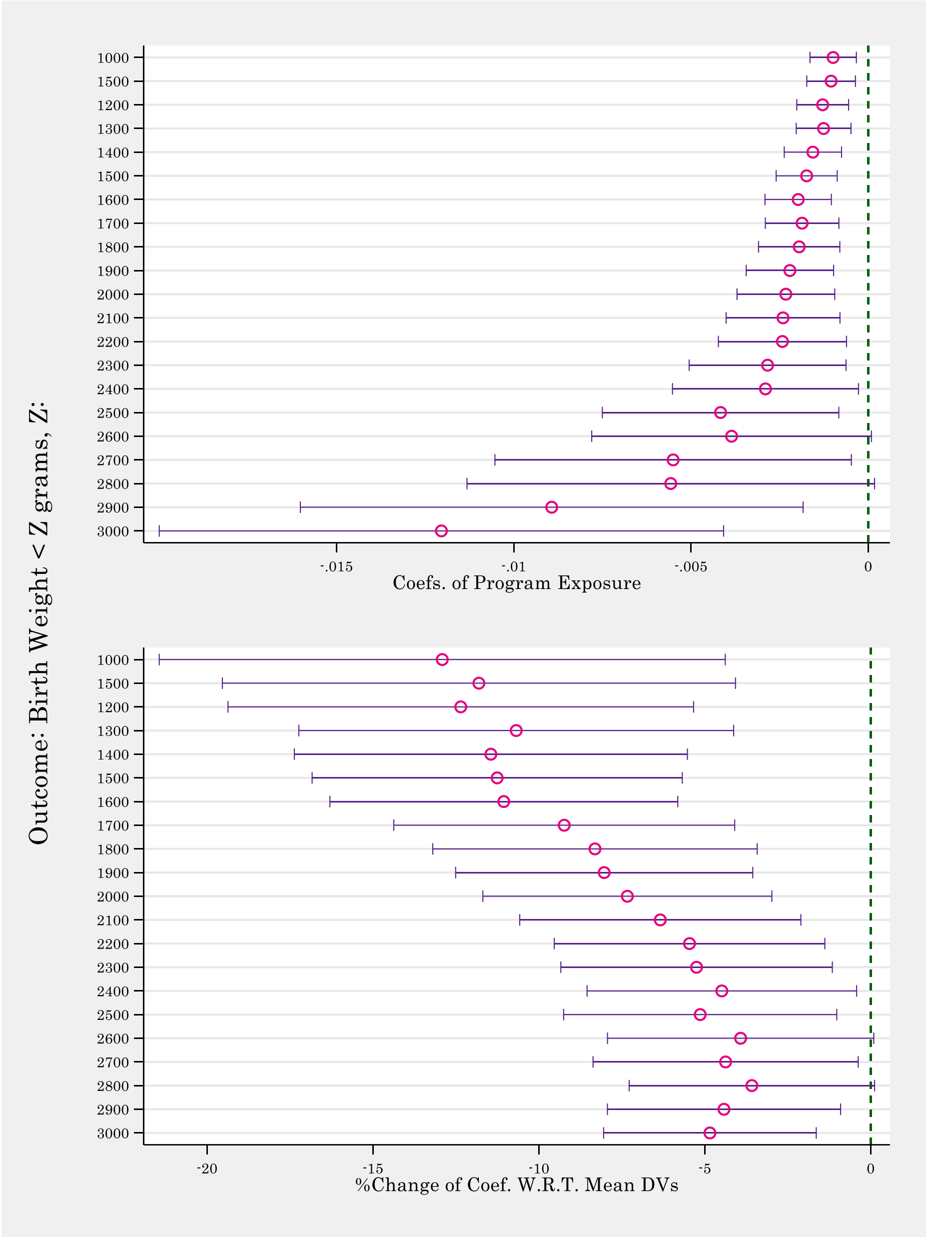


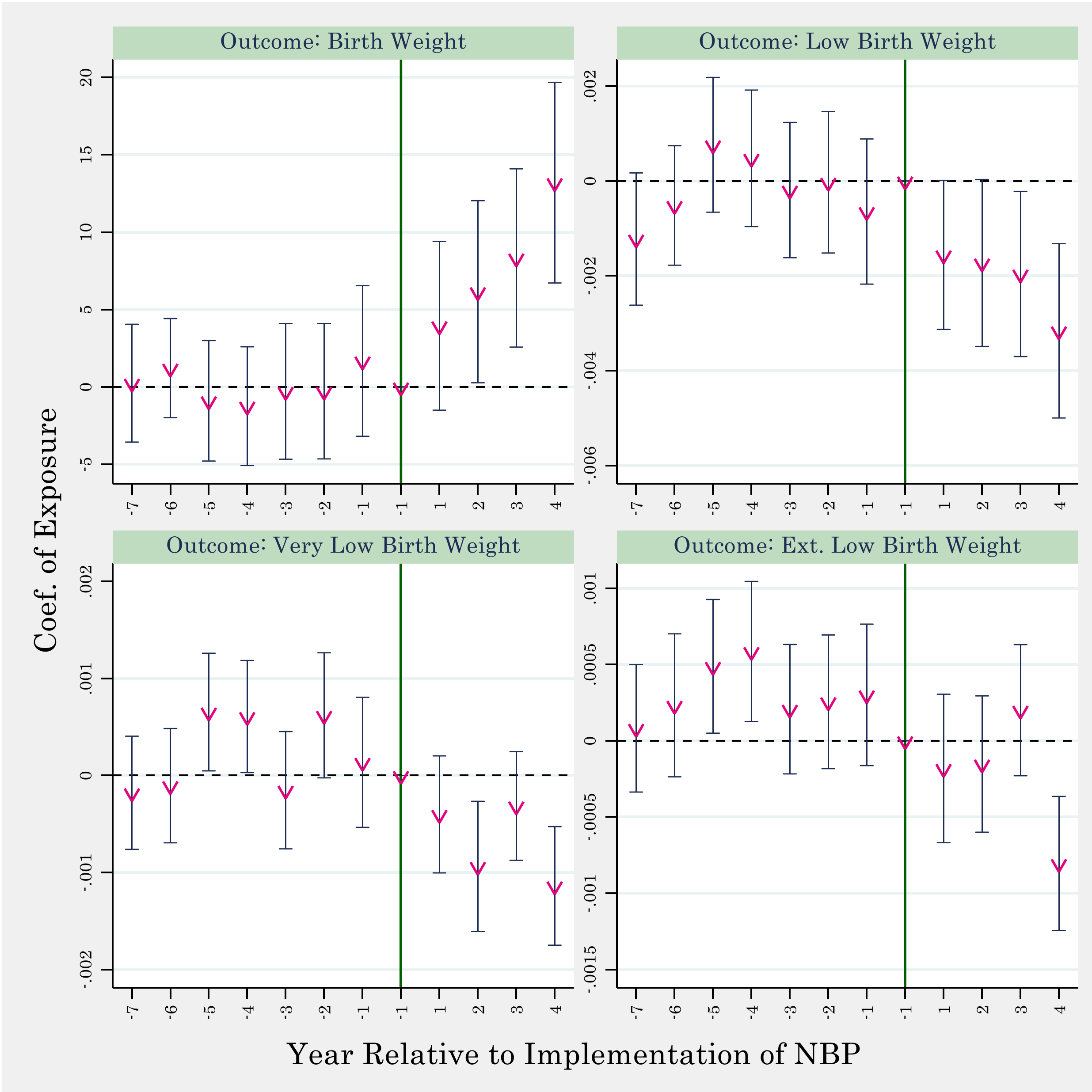
Figure 4 – Exploring the Heterogeneity in the Effects across Low Birth Weight Thresholds

Notes. Point estimates and 95 percent confidence intervals are depicted. Standard errors are clustered on county. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.

Appendix A

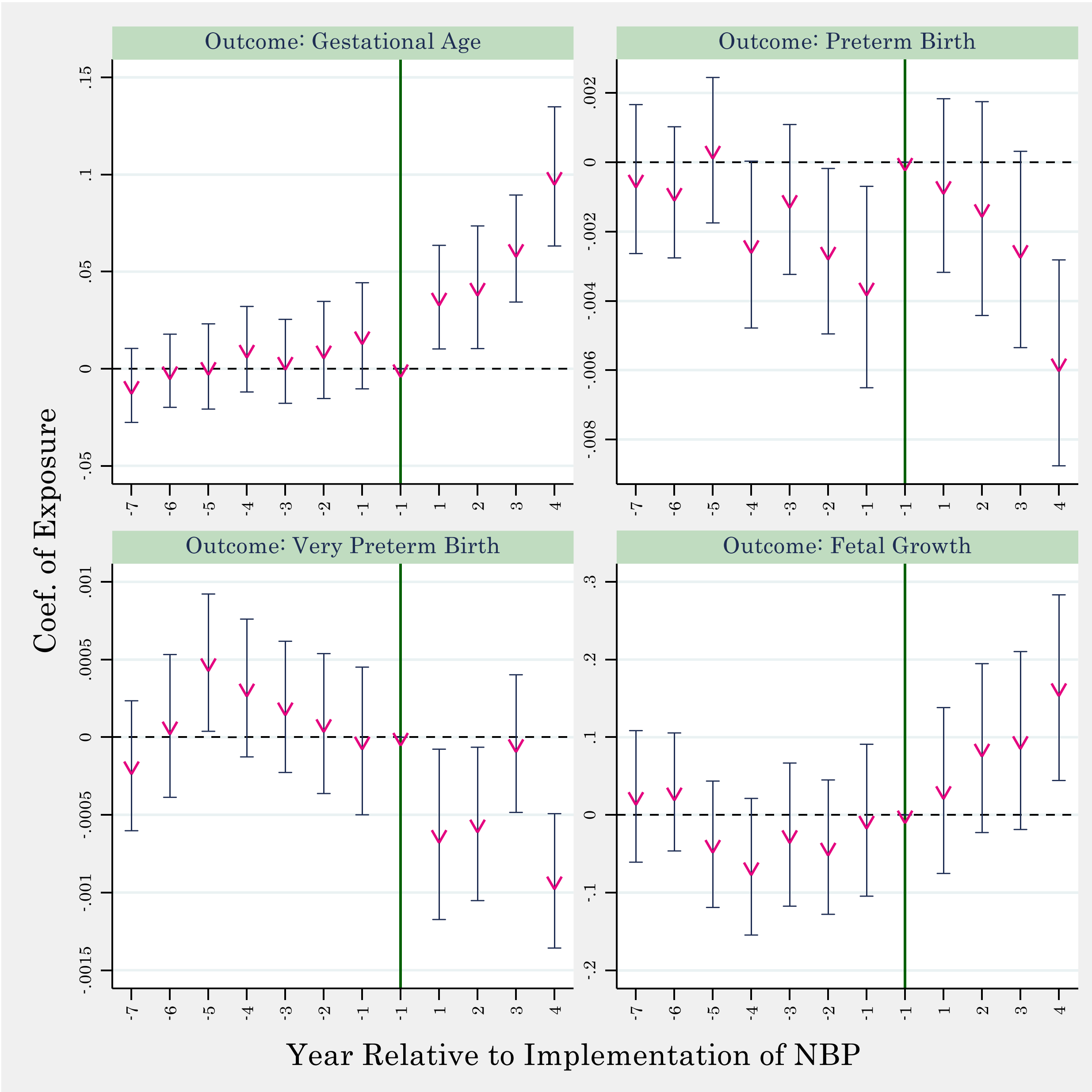
Recent developments in difference-in-difference method suggest that the estimates produced by ordinary least squares (OLS) could be biased when units are treated at different points in time (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sun & Abraham, 2021). The NBP was adopted partially in 2003 and fully in 2004. Although the time difference between adaptations is short, one might argue that the two-by-two comparisons of late-adopters and early-adopters could still confound the results. I use the strategy developed by Sun & Abraham (2021) to replicate the event studies. The results are illustrated in Appendix Figure A-1 and Appendix Figure A-2. I observe a very similar pattern as the event study analyses in Figure 2 and Figure 3, thereby limiting concerns regarding bias in OLS-produced estimates.

Further, I show the robustness of the event study results using the difference-in-difference method developed by De Chaisemartin & D'haultfoeuille (2022). The results are reported in Appendix Figure A-3 and Appendix Figure A-4. Although confidence intervals are larger and estimates are noisier, I observe a very similar pattern to the event studies reported in the paper.



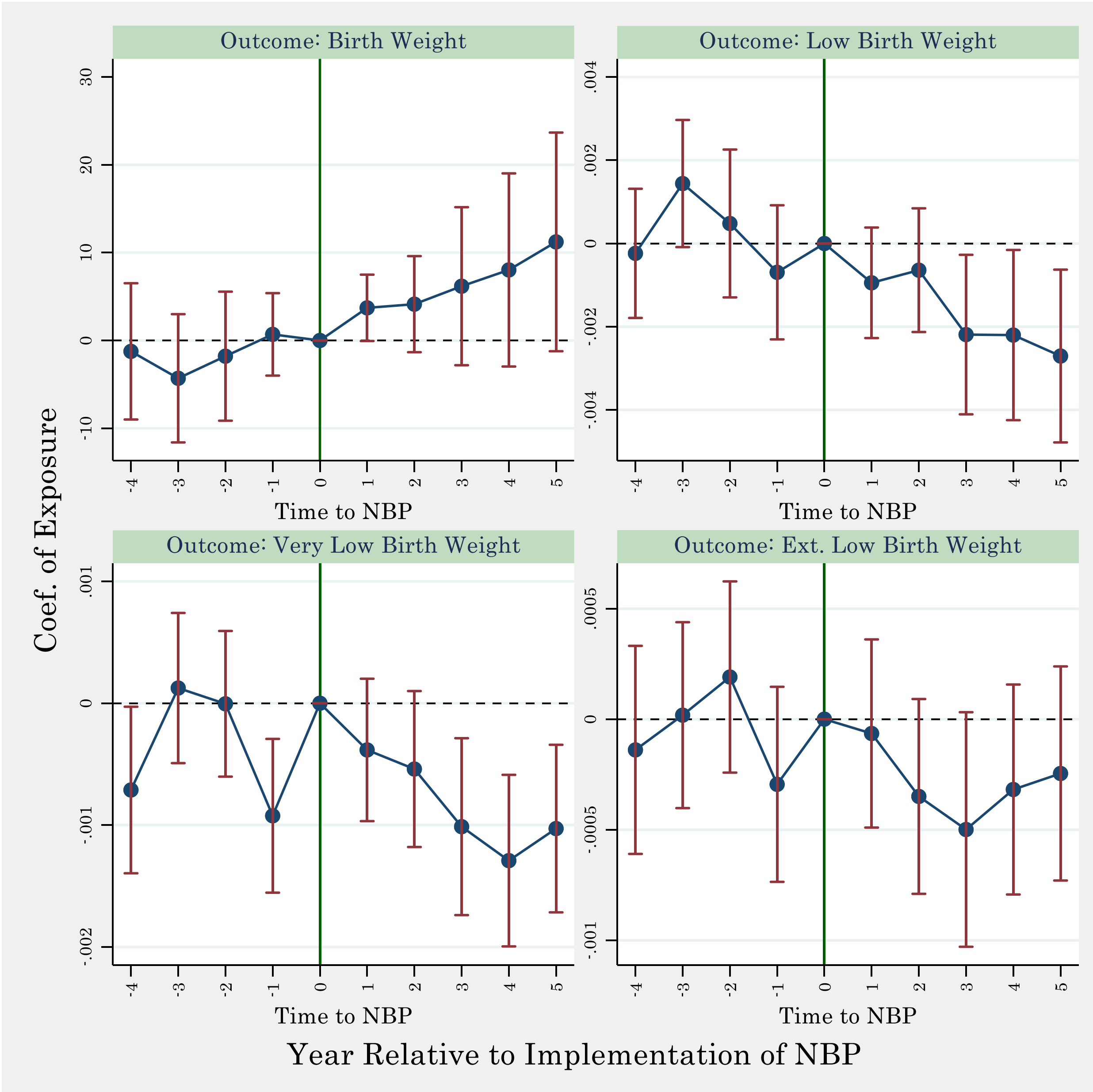
Appendix Figure A-1 - Sun & Abraham (2021) Event Study Analysis of the Effects of NBP Adoption on Birth Outcomes

Notes. Point estimates and 95 percent confidence intervals are depicted. Standard errors are clustered on county. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.



Appendix Figure A-2 - Sun & Abraham (2021) Event Study Analysis of the Effects of NBP Adoption on Birth Outcomes

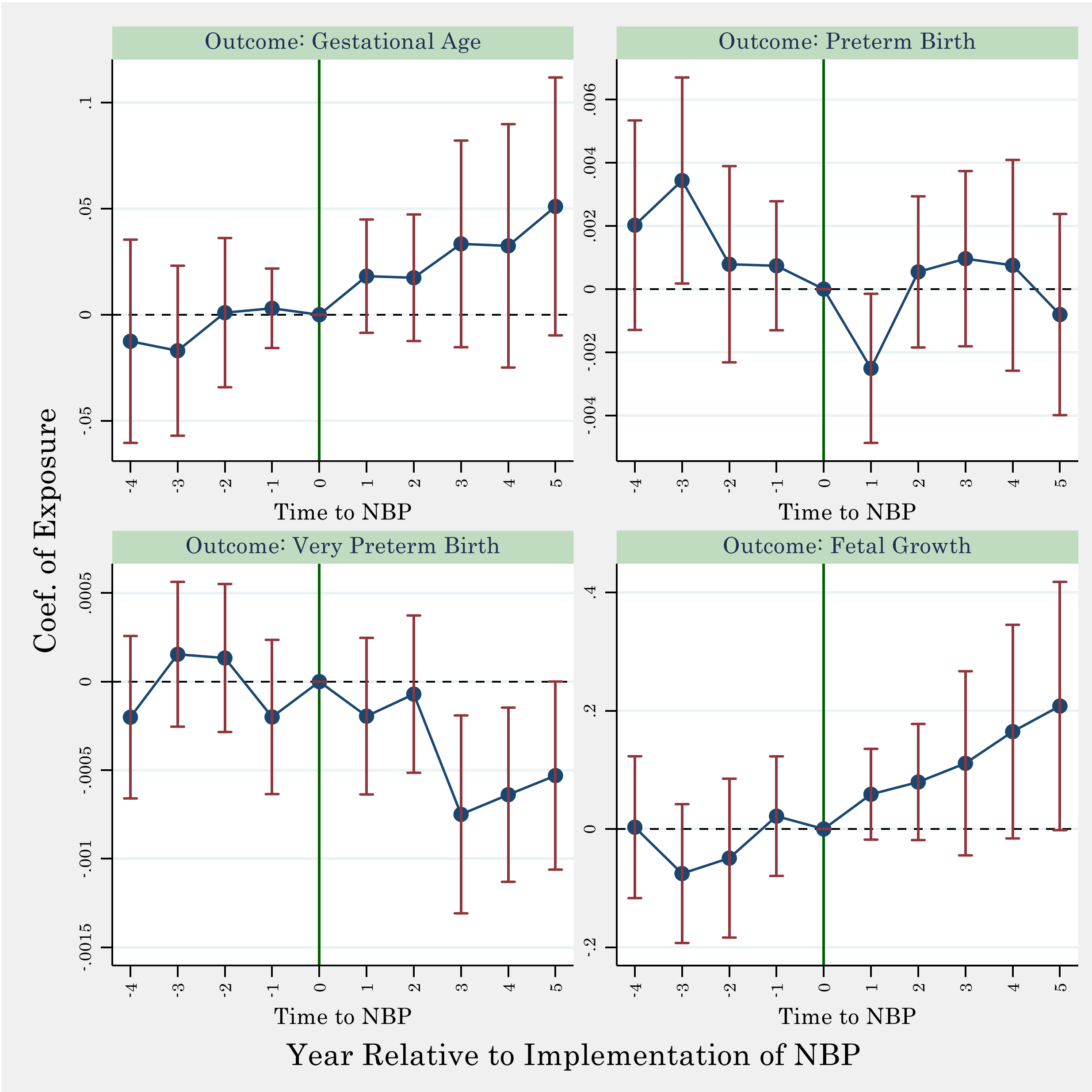
Notes. Point estimates and 95 percent confidence intervals are depicted. Standard errors are clustered on county. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.



Appendix Figure A-3 - de Chaisemartin & D'Haultfoeuille (2022) Event Study Analysis of the Effects of NBP Adoption on Birth Outcomes

Notes. Point estimates and 95 percent confidence intervals are depicted. Standard errors are clustered on county. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.

Observations: 23,780,301



Appendix Figure A-4 - de Chaisemartin & D'Haultfoeuille (2022) Event Study Analysis of the Effects of NBP Adoption on Birth Outcomes

Notes. Point estimates and 95 percent confidence intervals are depicted. Standard errors are clustered on county. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.

Observations: 23,780,301

Appendix B

In Appendix Table B-1, I explore the effects across subsamples of young mothers (age< 25) and subsamples of older mothers (age>35). I find slightly smaller effects for older mothers and slightly larger effects among younger mothers. Appendix Table B-2 reports the results across male and female infants. I do not find considerable heterogeneity by the child’s gender.

Appendix Table B-1 - Heterogeneity across Subsamples based on Mother Age

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Subsamples of Mothers Aged>35								
Average In-Utero Exposure to NBP	21.56821**	-.00334	-.00048	-.00051	.08937**	-.00111	0	.38512**
	(8.6507)	(.00287)	(.00109)	(.00077)	(.03581)	(.00374)	(.00077)	(.18832)
R-squared	.05497	.02675	.0157	.01301	.02794	.02483	.01422	.05278
Mean DV	3323.806	0.091	0.018	0.008	38.355	0.142	0.008	86.276
%Change	0.649	-3.665	-2.658	-6.384	0.233	-0.784	-0.039	0.446
Panel B. Subsamples of Mothers Aged<25								
Average In-Utero Exposure to NBP	24.26285***	-.00552**	-.00183**	-.00095*	.14445***	-.00351	-.00106**	.36833***
	(7.29261)	(.00224)	(.00073)	(.00054)	(.03514)	(.00328)	(.00051)	(.14205)
R-squared	.05346	.01705	.00868	.00719	.02195	.01554	.00759	.04926
Mean DV	3219.880	0.092	0.017	0.009	38.607	0.141	0.009	83.127
%Change	0.754	-6.001	-10.737	-10.586	0.374	-2.486	-11.817	0.443

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.
Observations of Panel A: 3030181
Observations of Panel B: 6640794
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix Table B-2 - Heterogeneity across Subsamples based on Child Gender

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Subsamples of Male Infants:								
Average In-Utero Exposure to NBP	17.11795**	-.00428**	-.0015***	-.00114***	.12455***	-.00374	-.00147***	.22501*
	(7.00987)	(.00172)	(.0005)	(.00038)	(.03115)	(.00298)	(.00041)	(.13568)
R-squared	.04623	.01388	.00709	.00576	.01853	.01354	.0063	.04337
Mean DV	3363.638	0.074	0.015	0.008	38.530	0.133	0.008	86.982
%Change	0.509	-5.781	-9.993	-14.267	0.323	-2.811	-18.337	0.259
Panel B. Subsamples of Female Infants:								
Average In-Utero Exposure to NBP	18.73871***	-.00413**	-.00199***	-.00083*	.12365***	-.00333	-.00077*	.26507**
	(6.20892)	(.00196)	(.00057)	(.00042)	(.03206)	(.00276)	(.0004)	(.11547)
R-squared	.04617	.01692	.00759	.00596	.02084	.01515	.00639	.04229
Mean DV	3242.819	0.088	0.016	0.008	38.638	0.123	0.008	83.646
%Change	0.578	-4.688	-12.453	-10.330	0.320	-2.708	-9.580	0.317

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.

Observations of Panel A: 12153834

Observations of Panel B: 11626389

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

Appendix C

In this appendix, I show the robustness to alternative standard error clustering. I show the robustness to using Huber-White robust standard errors (Appendix Table C-1), clustering by state (Appendix Table C-2), and two-way clustering by county and state-year (Appendix Table C-3).

Appendix Table C-1 - Exploring the Sensitivity of Standard Errors: Using Heteroscedastic Robust Standard Errors

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average In-Utero Exposure to NBP	17.8774***	-.00417***	-.00174***	-.00099***	.12381***	-.00351***	-.00113***	.24434***
	(1.54957)	(.00071)	(.00032)	(.00023)	(.00674)	(.00088)	(.00023)	(.0366)
R-squared	.0543	.015	.00639	.00494	.019	.01343	.00542	.05444

Notes. Standard errors are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.
Observations: 23780301
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table C-2 - Exploring the Sensitivity of Standard Errors: Clustering at State-Level

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average In-Utero	17.8774***	-.00417***	-.00174***	-.00099***	.12381***	-.00351*	-.00113***	.24434***
Exposure to NBP	(4.55069)	(.0014)	(.00047)	(.00034)	(.02123)	(.00194)	(.00035)	(.09337)
R-squared	.0543	.015	.00639	.00494	.019	.01343	.00542	.05444

Notes. Standard errors are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.
Observations: 23780301
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table C-3 - Exploring the Sensitivity of Standard Errors: Clustering at the County and State-Year-Level

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average In-Utero Exposure to NBP	17.8774***	-.00417***	-.00174***	-.00099***	.12381***	-.00351***	-.00113***	.24434***
	(1.74149)	(.00077)	(.00034)	(.00024)	(.00835)	(.00103)	(.00024)	(.03979)
R-squared	.0543	.015	.00639	.00494	.019	.01343	.00542	.05444

Notes. Standard errors are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.
Observations: 23780301
*** p<0.01, ** p<0.05, * p<0.1

Appendix D

The NBP was implemented in several Eastern, Midwestern, and Northeastern US states. In the main results of the paper, I excluded counties in Western states and argued that the inclusion of these counties might confound the estimate, as several states of this region had implemented pollution abatement strategies during the same period (Collantes & Sperling, 2008; Cushing et al., 2018). In Appendix Table D-1, I replicate the main results, including states in the Western region. I find effects that are very similar to those of the main results.

Appendix Table D-1 - Replicating the Main Results Including States in West Region

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average In-Utero Exposure to NBP	17.56891***	-.00388**	-.00158***	-.00087***	.1192***	-.00304	-.00089***	.24088**
	(6.07662)	(.0016)	(.00043)	(.00032)	(.0305)	(.00265)	(.00032)	(.11098)
R-squared	.05042	.01389	.00581	.00442	.01829	.01243	.00482	.05038
Mean DV	3316.176	0.077	0.014	0.007	38.624	0.124	0.007	85.585
%Change	0.530	-5.033	-11.272	-12.455	0.309	-2.452	-12.723	0.281

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.
Observations: 32480773
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix E

In Appendix Table E-1, I examine the effects on measures of prenatal care. Although the effects are statistically significant in columns 1-2 for number of prenatal visits and whether prenatal care began before the third trimester, the implied percentage changes are very small in magnitude. This suggests that the NBP policy changes are not associated with changes in healthcare access or contemporaneous improvements in prenatal care.

Appendix Table E-1 - Exposure to NBP and Prenatal Health Care Utilization

	Outcomes:		
	No of Prenatal Visits	Prenatal Care Began before Third Trimester	Month Prenatal Care Began
	(1)	(2)	(3)
Average In-Utero Exposure to NBP	.16958** (.08429)	-.00816** (.00356)	.03319 (.03694)
R-squared	.11776	.05525	.15366
Mean DV	11.309	0.941	2.614
%Change	1.500	-0.868	1.270

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.
Observations: 22,995,535
*** p<0.01, ** p<0.05, * p<0.1

Appendix F

In Table 8, I show the 2SLS results using ozone as the main endogenous pollutant. However, not all counties report ozone pollution. In Appendix Table F-1, I restrict the sample to those county-years with available data on ozone and replicate the reduced-form results of Table 2. I observe significant and slightly larger coefficients across most outcomes.

Appendix Table F-1 - Replicating the Main Results Excluding Observations with Missing Values on Ozone

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average In-Utero	24.66242**	-.00682***	-.00233***	-.00159***	.14115***	-.00486	-.00169***	.3962**
Exposure to NBP	(9.82186)	(.00263)	(.00065)	(.0005)	(.04577)	(.00412)	(.00047)	(.18252)
		16473385	16473385	16473385	16473385	16473385	16473385	16473385
R-squared	.05379	.01496	.00613	.00467	.01782	.01315	.00514	.05413
Mean DV	3299.672	0.083	0.016	0.008	38.560	0.130	0.008	85.257
%Change	0.747	-8.212	-14.556	-19.838	0.366	-3.740	-21.100	0.465

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment.
Observations: 16473385
*** p<0.01, ** p<0.05, * p<0.1

Appendix G

Although the NBP was replaced in 2009 by the Clean Air Interstate Rule (CAIR) (DeCicca & Malak, 2020), CAIR was initially implemented in 2005 but faced several court proceeding issues. To show that the results documented in this paper are not driven by the effects of CAIR, I limit the sample to the years 2000-2006 and replicate the main results. These estimates are reported in Appendix Table G-1. I observe comparable coefficients that are significant in most cases.

Appendix Table G-1 - Restricting the Sample to Years 2000-2006

	Outcomes:							
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Gestational Age	Preterm Birth	Very Preterm Birth	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average In-Utero Exposure to NBP	13.17437***	-.00315*	-.00176***	-.0009**	.06513***	.00191	-.00108**	.23807**
	(5.06578)	(.00166)	(.00055)	(.00044)	(.02406)	(.00266)	(.00043)	(.10578)
R-squared	.05245	.01521	.00731	.00587	.01735	.01337	.00626	.0541
Mean DV	3296.377	0.082	0.016	0.008	38.513	0.132	0.008	85.292
%Change	0.400	-3.842	-10.981	-11.270	0.169	1.445	-13.521	0.279

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county-by-month, region-by-year, and year-by-month fixed effects. Regressions include dummies for mother education, mother marital status, mother race, mother age, father age, and missing indicators for missing values of these covariates. Regressions also include county-level share of Whites, Blacks, children aged 0-5, real per capita income, and per capita employment. Observations: 12123034

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