

The 1973 Oil Embargo and Infant Health Outcomes: Evidence from a Macroeconomic Shock

Vikrant V Kamble*

October 29, 2025

[Latest Version]

Abstract

An extensive literature documents that economic shocks affect health outcomes, but mechanisms and distributional effects remain unclear. I study how energy shocks affect infant health using 1968-1979 Vital Statistics data. With an event-study framework that accounts for state-specific pre-trends, I find that states with higher oil dependence experienced increases in birthweight following the 1973 oil embargo. Gains are larger for less-educated, relatively younger, and Black mothers at 2.3-3.5 grams. These improvements result from increased prenatal care use, reduced air pollution, and shifts in maternal composition.

*University of Delaware, Department of Economics. Email: vvkamble@udel.edu

1 Introduction

On October 17, 1973, members of the Organization of Petroleum Exporting Countries (OPEC) announced an embargo on oil exports to countries supporting Israel during the Yom Kippur War, reducing production by 4.4 million barrels per day—approximately 7.5% of global output (Hamilton, 2013). This supply shock led oil prices to rise from about 3 to over 12 per barrel (Office of the Historian, Bureau of Administration, U.S. Department of State, 2025), and was followed by the most severe recession since World War II (Zarnowitz and Moore, 1977). While economists have extensively documented the macroeconomic consequences of this episode (e.g., Zulkifli and Haqem 2022; Barsky and Kilian 2002; Goel and Morey 1993; Bopp 1984), less is known about its effects on population health. In particular, little evidence exists on whether large economic disruptions alter outcomes among society’s most vulnerable, such as newborn children. Understanding these dynamics is important not only for interpreting the historical experience, but also for assessing how future economic shocks may propagate through populations with long-term consequences (e.g., Currie and Almond 2011; Almond 2006; Ruhm 2000). This paper examines the effects of the 1973–74 oil embargo on infant health outcomes across the United States.

Understanding these relationships has important implications for long-term population outcomes. Research indicates that economic conditions during pregnancy can influence cognitive development, educational attainment, and lifetime earnings (Dehejia and Lleras-Muney, 2004). The developmental origins hypothesis posits that early-life conditions establish biological and social pathways that persist throughout life, making the prenatal and infant periods particularly sensitive to external shocks (Almond and Currie, 2011). If macroeconomic disruptions systematically affect infant health, they may contribute to persistent inequalities in human capital and economic opportunity (Currie and Rossin-Slater, 2015). The 1973 oil embargo provides a well-defined, exogenous shock to study these effects. Its impact varied across U.S. states according to pre-existing characteristics, enabling a credible assessment of how large economic shocks influence population health (Kilian, 2008).

The analysis exploits cross-state variation in oil dependence—measured by motor fuel consumption per capita in 1972 collected by the U.S. Department of Transportation—to identify differences in birth outcomes across states following the embargo. Using a difference-in-differences with state-specific pre-trend controls framework following a similar method as Willage (2020) that accounts for pre-existing trends, the study finds that a 10% increase in pre-embargo motor fuel consumption per capita is associated with an increase in birthweight of approximately 2.2–3.5 grams, accompanied by reductions in adverse birth outcomes. The effects are largest among historically disadvantaged populations, including mothers with less than a high school education and Black mothers. These

results highlight the importance of early-life exposure to macroeconomic shocks and the role of socioeconomic and demographic factors in shaping population health outcomes.

The analysis uses three complementary datasets for examining the relationship between macroeconomic shocks and population health. The outcome data comes from Vital Statistics Natality Birth Data ([National Center for Health Statistics, 1979](#)), covering over 30 million individual births across all U.S. states from 1968 to 1979. This individual-level dataset includes information on birth outcomes and maternal characteristics at the county level. To address computational limits in analyzing large-scale data, observations are grouped to county-year-demographic cells following ([Kose et al., 2024](#)). For mechanism analysis, the study uses Environmental Protection Agency air pollution data from 1968 to 1979, focusing on carbon monoxide and nitrogen dioxide levels measured at daily pollution monitor levels. Motor vehicles are the main source of these pollutants, making up over 50% of carbon monoxide and 34% of nitrogen dioxide emissions ([Currie and Walker, 2011](#)). These measures are useful for studying environmental links between economic shocks and health outcomes.

The findings suggest that the embargo had unanticipated effects on infant health in more oil-dependent states. The results show statistically significant improvements across most outcomes. A 10% increase in pre-embargo per capita motor fuel consumption is associated with a 2.3 gram increase in birthweight. The largest effects are observed among historically disadvantaged populations. Mothers with less than a high school education experienced birthweight gains of approximately 3.4 grams, accompanied by reductions in the probability of low birthweight. Black mothers in oil-dependent states saw average gains of around 2.8 grams by the late 1970s. These estimates are both statistically and economically meaningful, underscoring that large macroeconomic shocks can generate heterogeneous health effects, with disadvantaged populations in some contexts experiencing measurable improvements.

The heterogeneity analysis shows that the gains from the embargo were not evenly shared. Relatively younger mothers, less-educated women, and Black mothers saw the largest improvements in birth outcomes, while more advantaged groups showed little change. These patterns suggest that disadvantaged populations had more to gain from improvements in healthcare access, cleaner air, or shifts in economic conditions. The results emphasize that economic shocks do not affect all groups in the same way and that, in some cases, vulnerable populations can experience relative benefits. This has direct policy relevance for current energy and climate transitions, where understanding who gains and who loses is essential for designing equitable policies.

To understand how oil dependence led to these health improvements, the study examines three possible mechanisms linking economic shocks to birth outcomes. First, the analysis looks at healthcare access through prenatal care use, both overall and in the first trimester. Mothers in oil-dependent states showed increases in prenatal care after

the embargo, with use rising by 0.002% of the mean. This pattern suggests that mothers may attend prenatal appointments more often during economic downturns. Second, the study looks at changes in maternal characteristics that could explain health improvements through selection effects. The data show an increase in maternal age and parity, along with reductions in teenage pregnancy rates in oil-dependent states after the embargo. These changes offer another explanation for health improvements, as maternal age, experience, and non-teenage status predict better birth outcomes (e.g., [Weng et al. 2014](#); [Chevalier and O’Sullivan 2007](#)). Third, the analysis looks at environmental pathways by testing whether reduced economic activity affects air pollution levels. Oil-dependent states had reductions in CO and NO_2 pollution after 1973, with levels declining through the rest of the decade. This pattern matches evidence linking economic downturns and pollution decreases ([Finkelstein et al., 2025](#)). These mechanisms likely worked together to produce the observed health benefits, showing that economic disruptions can affect population health through multiple pathways.

To test the robustness of these findings, the analysis conducts several checks. First, the study uses the method described in Appendix C.2 to confirm the main results. Second, the analysis tests alternative measures of oil dependence, including vehicle registration per capita in 1972 and per capita total petroleum products consumption in 1972. Results remain stable across these measures, indicating that findings reflect variation in fuel use rather than features specific to one measure. Third, the study tests binary treatment definitions (1 if above median) instead of continuous measures. Results hold when comparing high versus low oil-dependent states, showing that the continuous treatment approach captures variation in treatment intensity. Finally, the analysis confirms that results are robust when using individual-level data instead of aggregated cells.

This research builds upon existing literature examining how economic shocks affect infant health outcomes. Prior studies document effects of economic disruptions ([Charris et al., 2024](#)), immigration policy changes ([Timilsina, 2023](#)), and financial crises ([Finch et al., 2019](#)). However, these studies have not examined energy policy shocks as macroeconomic events that can affect population health outcomes. Moreover, existing research focuses on direct effects of economic shocks, leaving unexplored how these shocks affect infant health through multiple pathways. This research contributes by providing the first systematic evidence of how the 1973 oil embargo affected infant health outcomes, identifying environmental and compositional mechanisms that generated improvements for vulnerable populations. This aligns with recent research on the 2008 recession, which finds that the downturn reduced annual age-adjusted mortality, with larger effects among the elderly ([Finkelstein et al., 2025](#)).

A second strand of literature examines relationships between pollution from vehicular sources and infant health outcomes, with contributions from [Currie and Walker \(2011\)](#); [Alexander and Schwandt \(2022\)](#); [Knittel et al. \(2011\)](#). However, these studies focus on

direct interventions targeting pollution or traffic congestion. This research contributes by examining pathways through which economic shocks affect health outcomes through environmental changes. The findings reveal that environmental mechanisms explain part of observed health effects, suggesting that mechanisms linking economic shocks to infant health may involve behavioral, social, and institutional changes beyond the health-care, environmental, and compositional changes examined. These insights are relevant for discussions about energy transitions and climate policy, as societies transition away from fossil fuel dependence (Carleton and Hsiang, 2016). The findings suggest that with attention to healthcare access, environmental quality, and economic opportunities for disadvantaged communities, economic transitions can generate health benefits.

The remainder of the paper is structured as follows. Section 2 provides additional background on the 1973–74 oil embargo. Section 3 describes the data and sample construction. Section 4 outlines the empirical strategy, leveraging cross-state variation in oil dependence. Section 5 presents the main results, including analyses of heterogeneity and underlying mechanisms. Section 6 conducts robustness checks, and Section 7 discusses the implications of the findings and concludes.

2 The 1973-74 Oil Embargo: An Economic Shock

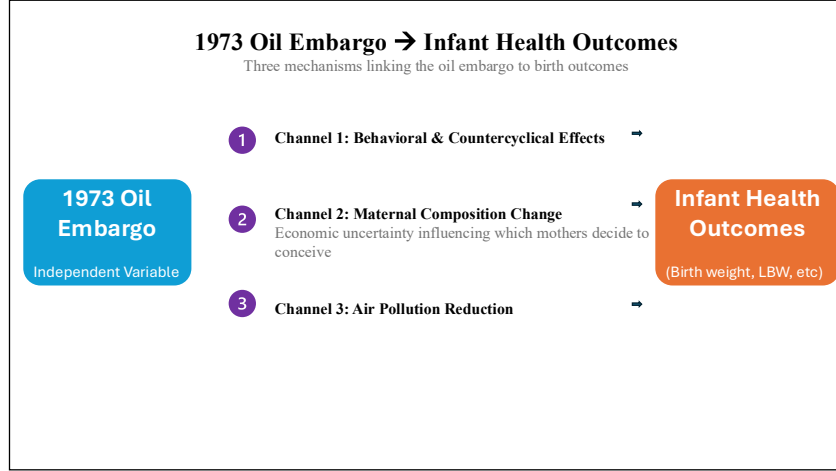
On October 6, 1973, Syria and Egypt launched a coordinated military offensive against Israel. Days later, Arab nations within the Organization of Petroleum Exporting Countries declared an embargo on oil exports to countries perceived as Israeli allies and reduced OPEC’s overall oil production. By November, production from Arab OPEC members had declined by 4.4 million barrels per day compared to September levels, representing a 7.5% reduction in global oil output (Hamilton, 2013).

The United States experienced this crisis during a period when the nation lacked energy independence. Americans faced petroleum product shortages, with long gasoline lines becoming a visible sign of the crisis. The economic costs went beyond fuel prices. Frech III and Lee (1987) documents that urban consumers incurred hidden costs through time spent waiting in gasoline lines—increasing the real cost of gasoline by 12% for city dwellers in December 1973, rising to 50% by March 1974.

The embargo lasted from October 1973 to March 1974, but its economic impact extended beyond this period. Oil prices increased from approximately \$3 per barrel to over \$12 per barrel, as shown in Figure A.1a (Acemoglu et al., 2013; Office of the Historian, Bureau of Administration, U.S. Department of State, 2025)—a fourfold increase. These elevated prices continued after the embargo ended, maintaining economic pressure throughout the period examined in this study.

2.1 Connecting Economic Disruption to Infant Health

Figure 1: Mechanism Testing Framework



Notes: This figure provides the mechanism framework under which oil embargo could affect the infant health outcomes.

This study examines the oil embargo’s impact on infant health outcomes through microeconomic behavioral responses.

The embargo created an exogenous supply shock that shifted petroleum supply curves leftward, raising prices and reducing quantities. This imposed binding budget constraints on households, requiring utility-maximizing agents to reconsider marginal costs and benefits across consumption categories. Economic shocks may lead households to make trade-offs with constrained budgets (Gertler and Gruber, 2002).

Under these constraints, the negative income shock may have increased relative demand for health-related investments. The opportunity cost of forgoing prenatal visits likely exceeded the opportunity cost of reducing discretionary consumption. Households facing liquidity constraints would reallocate expenditure toward essential healthcare services while reducing other consumption, following the equimarginal principle. This reallocation could generate positive health outcomes if families view infant health as a priority investment (Monheit et al., 2020).

This mechanism aligns with the countercyclical mortality hypothesis, which suggests health improvements during economic downturns. Evidence shows mortality rates often decline during recessions (Finkelstein et al., 2024), as individuals and healthcare systems reallocate resources toward prevention and essential care (Ruhm, 2000; Miller et al., 2009; Menclova, 2013). Prenatal care improves maternal health and pregnancy behaviors (Yan, 2017), supporting this channel.

Beyond reallocation effects, fertility timing decisions may have produced compositional effects. The oil embargo recession reduced women’s labor market opportunities,

lowering the opportunity cost of pregnancy and potentially shifting the timing and composition of conceptions toward periods of economic contraction. This study documents corresponding changes in maternal characteristics—increases in maternal age and parity, and reductions in teenage pregnancy rates in oil-dependent states. These shifts represent equilibrium responses to changing economic incentives and provide a selection-based explanation for observed health improvements, as maternal age, birth experience, and non-teenage status predict positive birth outcomes (Weng et al., 2014; Chevalier and O’Sullivan, 2007).

A third channel operates through environmental externalities. Market interdependence between petroleum and transportation means rising gasoline prices reduced consumer surplus in transportation markets, creating incentives to reduce vehicular traffic and associated negative externalities—determinants of infant health (Chay and Greenstone, 2003). The magnitude of these benefits depends on price elasticity of transportation demand (Hughes et al., 2008; Small and Dender, 2007). If consumers exhibited elastic responses, the reduction in quantity demanded would decrease pollution, benefiting pregnant women and developing fetuses who show higher sensitivity to environmental toxins (Currie et al., 2015). If demand remained inelastic, households maintained essential travel while eliminating discretionary trips.

These mechanisms demonstrate how supply shocks in one market propagate through economic systems to affect outcomes in other markets. Operating within the macroeconomic adjustment process, these channels created net positive welfare effects for infant health despite the negative economic shock.

3 Data

This study leverages multiple complementary data sources to construct a comprehensive analytical framework for examining how the 1973 oil embargo affected infant health outcomes. The integration of these datasets enables both rigorous causal identification and detailed investigation of underlying mechanisms, providing insight into how macroeconomic shocks affect population health.

3.1 Geographic Variation

To capture cross-state variation in oil dependence, I utilize the 1972 per capita quantity of total motor fuel consumed, as reported by the U.S. Department of Transportation.¹ This continuous measure captures cross-state variation in oil dependence prior to the 1973 embargo and serves as the primary treatment variable for identifying causal effects.

¹The term “motor fuel” refers to gasoline as well as all other fuels subject to state motor-fuel tax laws. “Special fuels” include diesel fuel, liquefied petroleum gases, and other fuels commonly referred to as “tractor fuel” or “power fuel” when used for operating vehicles on public highways.

The timing and comprehensiveness of this measure is particularly advantageous for causal identification. By measuring fuel consumption in 1972—the year immediately preceding the embargo—the variable captures pre-existing variation in oil reliance that would determine differential exposure to the subsequent price shock.

3.2 Birth Outcomes and Maternal Characteristics

The core analysis draws on the Vital Statistics Natality Birth Data from [National Center for Health Statistics \(1979\)](#), spanning 1968 to 1979.² This dataset provides individual-level birth records in each state by year, with five years of pre-embargo baseline data (1968-1972), the crisis period (1973-1974), and five years of post-embargo follow-up (1975-1979). This timeframe allows for testing parallel trends assumptions, estimating dynamic treatment effects, and examining both immediate and persistent impacts on infant health.

The dataset supports heterogeneity analysis. Each record contains demographic information including maternal age, educational attainment, and race, along with infant health metrics such as birthweight, gestational age, prenatal care indicators, and markers for low birthweight and preterm birth. The geographic precision extends to the county level for both maternal residence and birth location, allowing analysis of local variation in outcomes while maintaining statistical power for reliable analysis.

I conduct the main analysis at the county-year-cell level rather than the individual level, following established practices in the health economics literature that have been employed in studies examining health outcomes (e.g., [Kose et al. 2024](#); [East et al. 2023](#)). This analytical approach addresses computational constraints in analyzing datasets with over 30 million individual birth records while providing an established methodological foundation for the current analysis.

I implement cell-level aggregation following the methodology in [Kose et al. \(2024\)](#), creating units defined by county of birth, year, maternal race, age category, parity, education level, and marital status. For each cell, I calculate mean values for all health outcomes including birthweight, low birthweight indicators, gestational age, and prenatal care measures, while summing the total number of births and appropriate weights within each demographic-geographic-temporal grouping. This aggregation strategy preserves variation across demographic, geographic, and temporal dimensions while offering several analytical benefits. First, it maintains granularity to capture demographic heterogeneity across the dimensions relevant for birth outcomes—maternal age, education, and race—while enabling geographic and temporal exposure measurement. Second, when combined with analytical weights in the regression analysis, it weights observations by the number of births within each cell, giving greater influence to more populous geographic

²I started the data in 1968 because that is the first year the data is available and ended the data at 1979 because in 1980 there was another recession from 1980–1982 ([Stuart, 2022](#)).

areas and improving the precision of treatment effect estimates.

3.3 Environmental Mechanism Data

To investigate environmental pathways linking oil dependence to health outcomes, I analyze Air Pollution Concentration Data from the Environmental Protection Agency spanning 1968 to 1979.³ Motor vehicles represent a dominant contributor to ambient air pollution across the United States, accounting for more than half of all carbon monoxide (CO) emissions and approximately one-third of nitrogen dioxide (NO_2) concentrations in the atmosphere (Currie and Walker, 2011). In particular, why NO_2 ? According to Tiotiu et al. (2020), NO_2 primarily released from motor vehicle exhaust, is a well-established marker of traffic pollution, with transportation sources contributing up to 80% of ambient NO_2 concentrations. As a respiratory irritant, NO_2 can reach the lower lungs and is associated with coughing, wheezing, shortness of breath, bronchospasm, and, at high exposures, pulmonary edema. Given this substantial vehicular contribution to air pollution, the analysis focuses specifically on carbon monoxide and nitrogen dioxide measurements collected at the daily pollution monitor level. Following the methodology established in Isen et al. (2017), I aggregate these daily monitor observations to construct the average pollution concentration in birth county c and year t , weighted by the number of monitor observations in that county-year, yielding county-level annual mean concentrations. These measures are particularly well-suited for mechanism testing due to their direct connection to vehicular emissions and established associations with adverse health outcomes documented in prior research (Currie and Walker, 2011; Baran et al., 2025a).

4 Empirical Strategy

The empirical strategy relies on a difference-in-differences (DiD) and event study approach that exploits continuous 1972 geographic variation in fuel dependence across U.S. states. This approach combines cross-sectional variation in 1972 total motor fuel consumption per capita as a proxy for pre-embargo fuel dependence with temporal variation around the 1973 embargo. State with higher 1972 motor fuel consumption levels are interpreted as being more oil dependent. The identifying assumption is that, in the absence of oil embargo, there would be no differential changes in outcomes across states based on their 1972 motor fuel consumption levels.

Analysis is conducted at the county-year-demographic cell level. The regression specification is

³I am grateful to the author of the paper Deryugina and Reif (2023) for generously sharing the dataset with me.

$$Y_{cst} = \gamma_s + \gamma_t + \sum_{\tau=1968, \tau \neq 1972}^{1979} \beta_\tau [Treat_s \times \mathbb{1}\{t = \tau\}] + \epsilon_{cst} \quad (1)$$

In this specification, Y_{cst} denotes birth outcomes (e.g., birthweight, gestational age, low birthweight, preterm, etc.) for infants born in county c , state s , and calendar year t . The parameters γ_s and γ_t represent state and birth year fixed effects, respectively. $\mathbb{1}\{t = \tau\}$ is an indicator for year τ . The treatment variable, $Treat_s$, is a continuous measure of 1972 motor fuel consumption per capita in state s , expressed in logarithmic form.⁴ This specification traces the evolution of treatment effects over time relative to baseline year 1972. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year cell. All event studies rely on the continuous geographic variation in oil dependence. Under the level-log model specification, β_τ captures the effect whereby a 10% increase in fuel consumption per capita in pre-embargo year yields a $\frac{\beta_\tau}{10}$ gram change in birth outcomes for year t relative to 1972 (for example, $\beta_\tau = \mathbb{E}[\text{Birthweight}|\text{year } \tau] - \mathbb{E}[\text{Birthweight}|\text{year } 1972]$).

The key identification assumption is the *parallel trends* assumption: conditional on state and year fixed effects, changes in infant health outcomes after the embargo are systematically related to the intensity of state-level oil reliance and would have followed similar trends in the absence of the oil embargo. I assess this assumption by plotting the event-study coefficients. However, the event studies using Equation 1 in Figure B.4 reveal substantial deviations from parallel trends. To address this, I follow a similar approach as Wolfers (2006) and Willage (2020). I adjust for state-specific pre-trends to account for these violations. This approach relies on the relative trends assumption.⁵

Specifically, I detrend all health outcomes manually using state-specific pre-1973 linear trends. This procedure mentioned in Appendix C.1⁶ removes each state’s historical trajectory prior to the embargo, isolating deviations attributable to the embargo and improving the causal interpretation of the estimated effects. The identifying assumption

⁴I use a level-log specification where birthweight remains in levels while fuel consumption per capita is logged. This addresses right-skewness in fuel consumption and reduces the influence of outlier states. The specification also allows interpretation as semi-elasticities—the change in birthweight for a percentage change in fuel consumption.

⁵The relative trends assumption allows treatment and control groups to have different pre-policy growth rates—for example, treatment states growing at 3% annually while control states grow at 1%—but assumes this 2% differential would have persisted absent the intervention ($\beta_T - \beta_C = \text{constant}$). Unlike the stricter parallel trends assumption that requires identical growth rates ($\beta_T = \beta_C$), this approach accommodates heterogeneous trajectories while maintaining identification. Parallel trends is thus a special case where the baseline differential equals zero. Post-policy changes in the 2% baseline differential can then be attributed to the treatment effect.

⁶The detrending procedure follows four steps. First, I estimate the event study regression from Equation 1 to obtain year-specific treatment effect coefficients. Second, I fit a linear trend to the pre-embargo coefficients from 1968 to 1971. Third, I project this estimated trend across all sample years to construct a counterfactual trajectory. Fourth, I subtract the projected trend from the original coefficients to obtain detrended estimates, with confidence intervals adjusted accordingly.

is the *relative trends* between oil-dependent and non-oil-dependent states would have remained constant in the absence of the treatment. Note that the parallel trends assumption is a special case of the relative trends assumption where outcomes in treatment and control groups follow similar trends before the policy or shock. Then I implemented a similar specification equation as [Wolfers \(2006\)](#) and [Willage \(2020\)](#), fully accounting for pre-trends by normalizing the pre-period estimates to zero and estimating the post-period deviations relative to this normalized pre-trend. The specification is [Equation 2](#) and results are depicted in [Figure 4](#). The event studies using [Appendix C.1](#) and [Equation 2](#) yield similar results.

$$Y_{cst} = \gamma_s + \gamma_t + \sum_{\tau=1973}^{1979} \beta_{\tau} [Treat_s \times \mathbb{1}\{t = \tau\}] + [\Phi_s \times State_s \times Year_t] + \eta_{cst} \quad (2)$$

In [Equation 2](#), the terms $\Phi_s \times State_s \times Year_t$ capture state-specific linear trends over time, identified exclusively from the pre-embargo period.⁷ The post-embargo interactions, $Treat_s \times \mathbb{1}\{t = \tau\}$, measure the dynamic effects of the 1973 oil embargo across the post-treatment years through the coefficients β_{τ} relative to the pre-period linear trend.

To obtain the adjusted difference-in-differences static estimates that account for pre-period trends, I employ the following regression specification:

$$Y_{cst} = \gamma_s + \gamma_t + \beta^{DD} (Treat_s \times \mathbb{1}(t \geq 1973)) + [\Psi_s \times State_s \times (t - 1974) \times \mathbb{1}(t < 1973)] + \zeta_{cst} \quad (3)$$

In this [Equation 3](#), γ_s and γ_t represent state and year fixed effects, respectively. The indicator $\mathbb{1}(t \geq 1973)$ constitutes a binary variable equal to one for post-embargo periods. Given that the $Treat_s$ variable enters in logarithmic form, the β^{DD} coefficient represents the effect relative to the pre-treatment trends whereby a 10% increase in pre-embargo per capita motor fuel consumption generates a $\frac{\beta^{DD}}{10}$ unit change in the dependent variable.

State-specific pre-1973 linear trends are captured through the interaction of three distinct terms: $State_s$, continuous time centered at 1974 ($t - 1974$), and a pre-period indicator $\mathbb{1}(t < 1973)$. Notably, these state-specific trends are identified exclusively using pre-treatment periods. The inclusion of state pre-trends eliminates differential secular trends across states. The estimates using [Equation 3](#) are smaller in magnitude than the event studies in [Equation 2](#) because of the covariance matrix.

The identification strategy requires the assumptions that adjusted pre-trends exhibit approximate linearity and that no contemporaneous shocks correlate with both per capita

⁷By doing this, the model accounts for the possibility that states were already on different trajectories prior to the embargo, ensuring that the estimated post-embargo effects (β_{τ}) are not confounded by pre-existing differences in trends.

motor fuel consumption and infant health outcomes.⁸ Standard errors are clustered at the state level to account for within-state serial correlation, and all regressions are weighted by birth counts.

5 Results

5.1 Main Results

Figure B.4 presents raw event study estimates using Equation 1 examining the evolution of treatment effects over time, using 1972 as the reference year. However, these raw estimates reveal substantial violations of the parallel trends assumption, with oil-dependent states exhibiting systematically different pre-embargo trajectories in birth outcomes. Figure B.4a shows that states with higher oil dependence were already experiencing declining birthweight trends prior to 1973, making it difficult to isolate embargo-specific effects. Similar pre-existing differential trends are evident across other outcomes as well, undermining the causal interpretation of the raw estimates.

The detrending procedure outlined in Appendix C.1 removes each state’s historical trajectory prior to the embargo, isolating deviations specifically attributable to the oil shock. Figure 4 addresses these parallel trends violations by presenting adjusted event study estimates that detrend all infant health outcomes using state-specific pre-1973 linear trends. Across all panels in Figure 4, the pre-treatment trend is eliminated, revealing clear deviations in the post-treatment period from the zero-slope line.

Birthweight exhibits an upward trajectory following the oil embargo in states that demonstrated greater pre-embargo oil dependence. Gestation age, an alternative measure of infant health, similarly demonstrates positive treatment effects. Correspondingly, both low birthweight incidence and preterm birth rates decline following the embargo. Each annual coefficient represents the treatment effect in year t relative to the pre-period treatment trend.

The corresponding pre-trend adjusted difference-in-differences estimates derived from Equation 3 are reported in Table 2. These point estimates exhibit modest attenuation relative to the dynamic treatment effects documented in Figure 4 because of the covariance matrix. The birthweight coefficient of approximately 23 grams implies that a 10% increase in pre-embargo per capita motor fuel consumption generates a 2.3 gram improvement in birth weight. I find that a 10% increase in pre-embargo motor fuel consumption generates a 0.07% birth weight increase relative to sample birthweight mean of 3,302 grams. The low birthweight outcome represents the fraction of births falling below the

⁸To support this, I show in Figure D.2 and Figure D.3 that other time-varying, state-level characteristics do not exhibit systematic changes in response to the oil embargo. This evidence helps alleviate concerns about the likelihood that the estimated treatment effects (β_τ) are driven by confounding unobserved shocks.

threshold of $< 2500g$. A 10% increase in per capita motor fuel consumption corresponds to approximately a 0.0004 reduction in the low birthweight probability—equivalent to a 0.04 percentage point decline, or approximately a 0.57% ($\frac{-0.0004}{0.07}$) proportional reduction from the baseline incidence of 7%. In addition, there is a statistically significant increase in gestation age in weeks and a reduction in preterm births, but this preterm coefficient is not statistically significant.

I find countercyclical health effects following the oil embargo, which differ from procyclical patterns where economic downturns harm infant health. [Bozzoli and Quintana-Domeque \(2014\)](#) show this procyclical pattern in Argentina, where economic crisis reduced birthweight through credit constraints and maternal stress. My countercyclical findings are consistent with [Dehejia and Lleras-Muney \(2004\)](#), who show that U.S. recessions improve infant outcomes through maternal compositional changes—shifts in which mothers conceive during downturns. They find a 0.26% to 0.5% reduction in low birthweight for each percentage point increase in unemployment. I observe similar selection effects through reduced teenage pregnancy and higher maternal age in oil-dependent states. My results indicate that a 10% increase in per capita motor fuel consumption yields approximately a 0.54% relative reduction from the baseline low birthweight incidence of 7%, a magnitude comparable to the countercyclical effects documented by [Dehejia and Lleras-Muney \(2004\)](#).

This effect size is also consistent with safety net policy studies (e.g., WIC, food stamps, community health centers), where most interventions show birthweight effects ranging from 2 to 6 grams (Food Stamps: [Almond et al. \(2011\)](#); WIC: [Hoynes et al. \(2011\)](#); community health centers: [Kose et al. \(2024\)](#)). My estimated 0.07% increase in birthweight relative to the sample mean of 3,302 grams is similar to [Kose et al. \(2024\)](#), who report a 0.09% increase in birthweight following the introduction of community health centers at the county level. This indicates that the oil embargo’s health effects through environmental and compositional mechanisms are comparable in magnitude to direct healthcare interventions.

5.2 Heterogeneity Analysis

5.2.1 Heterogeneous Effects by Maternal Education: Less than HS

It is possible that low socioeconomic status families, and in particular mothers with less than a high school education, experienced stronger improvements in birthweight because they were more vulnerable to these shocks. [Figure 5](#) examines whether the oil embargo’s effects on birth outcomes exhibited systematic variation across maternal education levels. The results reveal pronounced heterogeneity, with the most substantial treatment effects concentrated among mothers with less than high school education. [Figure 5a](#) demonstrates that for this demographic, birthweight effects exhibit increasingly positive

trajectories following 1973, achieving improvements of 3.5-5 grams by the late 1970s for a 10% increase in pre-embargo per capita motor fuel consumption. Correspondingly, [Figure 5b](#) demonstrates significant improvements in gestational age for this subgroup. Reductions in low birthweight incidence for the same demographic are demonstrated in [Figure 5c](#), with preterm birth exhibiting similar declines among mothers with less than high school education.

The adjusted difference-in-differences estimates are reported in [Table 3](#). The birthweight coefficient of approximately 34 grams implies that a 10% increase in pre-embargo per capita motor fuel consumption generates a 3.4 gram improvement in birth outcomes. Additionally, a 10% increase in motor fuel consumption per capita is associated with an increase in gestational age of approximately 0.021 weeks (0.147 days), representing a small effect size. Furthermore, low birthweight incidence exhibits a statistically significant decline. Preterm birth also decline, though this effect fails to achieve statistical significance.

These patterns suggest that infants born to less educated mothers in oil-dependent states experienced health improvements following the embargo. In contrast, mothers with high school or greater than high school education show minimal and statistically insignificant effects across most outcomes and time periods. The differential impacts by education level may reflect varying exposure to economic channels through which oil dependence affected household resources.

5.2.2 Heterogeneous Effects by Other Maternal Characteristics

When I examine treatment effects by race in [Figure 6](#), pronounced disparities emerge in how the oil embargo affected birth outcomes across different communities. According to [Aizer and Currie \(2014\)](#), Black mothers are more likely than White mothers to have low-birth-weight or preterm infants. These early-life disparities can persist into adulthood, influencing long-term health and the accumulation of human capital ([Currie, 2011](#)). Black mothers in oil-dependent states experienced remarkably positive birth outcomes following 1973, with their infants gaining substantial weight—ultimately achieving improvements of 2.8-3 grams by the decade’s end relative to the preperiod treatment trend. This represents a clinically meaningful difference that likely translated into better health trajectories for these children. These mothers additionally demonstrate increased gestation age. The benefits extended beyond birthweight and gestation age; Black mothers furthermore experienced reductions in low birthweight deliveries and preterm births. White mothers, by comparison, exhibited considerably more modest responses throughout the period, with birthweight effects that remained relatively small. [Table 4](#) presents adjusted difference-in-differences estimates that corroborate this narrative regarding infant health outcomes. The racial disparities in treatment response underscore how macroeconomic shocks can

generate vastly different consequences across demographic subgroups.

Figure 7 shows that relatively younger mothers emerge as the primary demographic experiencing beneficial effects from oil dependence following the 1973 embargo. Relatively younger maternal age group (20-24 to 30-34) demonstrates an upward trajectory in birthweight outcomes, with effects building progressively from near-zero in 1973 to substantial gains of 3-3.5 grams by 1978-1979 relative to the pre-period treatment trend. This improvement translates into meaningful reductions in adverse birth outcomes, particularly evident in the declining low birthweight births for this age group (Panel c). The pattern stands in contrast to older maternal cohorts, where effects remain consistently clustered around zero across the post-embargo period. Gestational age responses show modest across all groups, while preterm birth measure displays minimal systematic pattern. The age-specific concentration of benefits suggests that relatively younger mothers in oil-dependent states may have experienced unique advantages during the embargo period, potentially through self care, or reduced exposure to stressors during pregnancy. My results are consistent with the past literature (e.g., Noelke et al. 2019; Bertoli et al. 2023) on differential impacts across education, age, and race provide insights into the pathways through which macroeconomic shocks affect population health.

5.3 Other Outcomes and Potential Mechanisms

Analysis of additional infant health outcomes (Figure A.3 and Table A.1) reveals consistent patterns across very low birth weight, high birth weight, and small for gestational age classifications. States with greater pre-embargo oil reliance exhibit significant reductions in very low birth weight, increases in high birth weight, and decreases in small for gestational age births, suggesting broad-based improvements in infant health outcomes.

The mechanism testing framework is outlined in Figure 1. I examine three primary mechanisms that could potentially explain the observed improvements in infant health outcomes. Prior research suggests that recessions may generate positive external health effects, such as improvements in healthcare quality and reductions in pollution (Finkelstein et al., 2025; Stevens et al., 2015; Chay and Greenstone, 2003), with additional evidence linking declines in pollution to better infant health outcomes (Currie and Walker, 2011; Alexander and Schwandt, 2022; Knittel et al., 2011; Baran et al., 2025b). First, I investigate the prenatal care utilization during periods of economic contraction. Second, I analyze compositional changes in the maternal population—specifically examining which mothers are deciding to conceive during such macroeconomic events. Finally, I evaluate the pollution channel, whereby economic downturns may reduce vehicular traffic and industrial activity, thereby diminishing ambient pollution exposure, particularly vehicular emissions.

5.3.1 Access to Health Care

To understand the pathways through which oil dependence generated the observed birth outcome improvements, I investigate changes in healthcare access patterns. First, to demonstrate evidence of the economic downturn, I present two figures. [Figure A.2](#) shows the seasonally adjusted unemployment rate, which demonstrates that nationwide, the unemployment rate rose sharply following the oil embargo. Second, [Figure 8b](#) shows the adjusted event study estimates for each year relative to the pre-period treatment trend, revealing a consistent decline in employment following the embargo.

Individuals tend to increase self-care behaviors during periods of economic downturn, which is precisely what I find in [Figure 8c](#) and [Figure 8d](#). These figures examine prenatal care utilization and first-trimester prenatal care initiation, revealing that mothers in oil-dependent states experienced substantial increases in healthcare access following the embargo. The event study estimates in [Figure 8c](#) demonstrate a clear upward trajectory beginning in 1973, with prenatal care utilization rising systematically. Given that the baseline prenatal care rate was 99%, a 10% increase in motor fuel consumption per capita shows an increase of approximately 0.002 additional prenatal care visits, as shown in [Table 5](#), representing approximately 0.002% of the mean. This underscores the importance of preventive healthcare behaviors in response to macroeconomic shocks. My findings are consistent with prior research showing that prenatal care improves maternal health and pregnancy behaviors ([Yan, 2017](#)), contributing to better infant outcomes.

5.3.2 Maternal Compositional Change

Second potential mechanism underlying the observed birth outcome improvements involves changes in the demographic composition of mothers giving birth in oil-dependent states. When I examine how the embargo affected maternal characteristics shown in [Figure 9](#), clear compositional shifts emerge that could partially explain the health improvements observed among newborns. [Figure 9a](#) demonstrates that oil-dependent states experienced systematic increases in maternal age following 1973. This aging pattern suggests that economic conditions in these states may have influenced fertility timing decisions, leading women to delay childbearing. [Figure 9b](#) reveals a parallel increase in parity, with mothers in oil-dependent regions having approximately additional children on average by the decade's end, indicating shifts toward higher-order births. Perhaps most notably, [Figure 9c](#) shows that the proportion of teenage mothers declined substantially in oil-dependent states after the embargo.

[Table 6](#) show the static estimates for the adjusted DiD. The results reveal that areas with higher baseline motor fuel consumption per capita (measured in 1972) experienced differential changes after the embargo shock. A 10% higher 1972 per capita fuel consumption is associated with an increase in maternal age of about $0.306 \times \ln(1.1) = 0.029$ years

(roughly 0.12% of the mean of 24.47 years). Similarly, parity rises by $0.229 \times \ln(1.1) = 0.022$ births (about 1.0% of the mean of 2.09). At the same time, the share of teen mothers declines by $-0.045 \times \ln(1.1) = -0.0043$, corresponding to a 0.43 percentage point decrease (2.4% of the mean teen motherhood rate of 18%). Finally, the coefficient for the share of Black mothers, -0.024 , translates into $-0.024 \times \ln(1.1) = -0.0023$, a reduction of about 0.23 percentage points (1.5% of the mean of 15%), though this effect is not statistically significant. Since maternal age, parity, and teenage pregnancy status are all strong predictors of birth outcomes—with relatively older, more experienced, and non-teenage mothers typically having healthier babies—these compositional changes provide an important alternative explanation for the observed health improvements. These changes provide an important alternative explanation for observed health improvements since maternal age, experience, and non-teenage status are strong predictors of positive birth outcomes (e.g., [Weng et al. 2014](#); [Chevalier and O’Sullivan 2007](#)).

5.3.3 Reduction in Pollution

The mechanism linking fuel consumption to health outcomes also operates through air quality changes. As [Hecq et al. \(1993\)](#) document, increased fuel consumption generates higher atmospheric pollution levels from combustion byproducts including nitrogen oxides (NO_2) and volatile organic compounds. The substantial decrease in motor fuel consumption as shown in [Figure 3a](#) following the oil embargo therefore likely reduced ambient pollution exposure, providing a plausible pathway for the observed birth weight improvements.

[Figure 10](#) explores environmental channels by examining carbon monoxide pollution levels, which serve as a proxy for broader air quality conditions. The adjusted estimates demonstrate in [Figure 10a](#) that oil-dependent states experienced notable reductions in CO pollution after 1973, declining consistently throughout the remainder of the decade but are somewhat noisy. In [Figure 10b](#), the decline in NO_2 concentration reveals that areas with higher baseline motor fuel consumption per capita experienced larger reductions in NO_2 concentrations following the 1973 oil crisis. Following [Table 7](#), a 10% higher baseline per capita motor fuel consumption is associated with about a 4.7% reduction in CO concentration after the embargo. Given the mean CO concentration of 7.47, this translates into a reduction of approximately 0.35 units from the mean. This demonstrates a positive environmental externality where energy price shocks generated substantial air quality improvements in the most fuel-dependent regions. This pattern suggests that embargo-induced changes in economic activity, transportation patterns, or industrial operations may have inadvertently improved local environmental conditions in these regions. This finding is also consistent with reduced pollution exposure particularly benefiting pregnancies that are most sensitive to environmental stressors.

To contextualize the magnitude of the environmental mechanism, I compare my pollution-health effects to [Currie and Walker \(2011\)](#), who exploit the introduction of electronic toll collection (E-ZPass) in New Jersey and Pennsylvania, which reduced traffic congestion and vehicle emissions near highway toll plazas. Their study documents a 10.8% NO₂ reduction post E-ZPass and an 11.8% decrease in low birthweight among mothers near toll plazas. While their study does not directly measure CO, existing traffic studies (example [Venigalla and Krimmer \(2006\)](#)) suggest a 10% NO₂ reduction would correspond to at least a 40% CO reduction. My estimates indicate that a 10% increase in fuel consumption generates a 4.7% CO reduction and a 0.57% relative reduction in low birthweight probability (0.04 percentage point decline from 7% baseline). Though direct comparison is complicated by differences in pollutants measured and exposure intensity, my results suggest the environmental pathway operates at economically meaningful magnitudes. The concentrated exposure near toll plazas likely generates stronger pollution-health effects than the state-level variation in oil dependence I exploit, explaining the differences in effect sizes.

6 Robustness Checks

To ensure the reliability of the main findings, I conduct several robustness checks that test the sensitivity of my baseline estimates to alternative specifications and measurement approaches. First, I utilize the alternative way of detrending showed in [Figure 11](#). The maroon squares represent point estimates and confidence intervals obtained following the procedure outlined in [Appendix C.2](#). This bootstrap-based approach appropriately accounts for estimation uncertainty in the confidence intervals that conventional methods may inadequately capture, thereby providing more reliable statistical inference in finite samples with clustered standard errors. As a complementary robustness exercise, the orange triangles present estimates derived from the similar methodology as [Goodman-Bacon \(2021\)](#), which directly detrends the dependent variable—e.g., birthweight itself—prior to estimation rather than incorporating detrending through regression controls. This alternative specification mitigates potential concerns regarding functional form assumptions and ensures that the estimated treatment effects are not driven by particular modeling choices in the detrending process. The convergence of results across these methodologically distinct approaches provides substantial evidence for the robustness of the documented treatment effects, demonstrating that the observed improvements in birth outcomes are invariant to reasonable modifications in alternative specification and inferential procedures.

Second, I examine whether the results depend on the specific measure of oil dependence by replacing 1972 motor fuel consumption with log vehicle registration per capita in 1972 as an alternative proxy for state-level oil reliance. [Figure 12](#) and [Figure 13](#) demon-

strates that the adjusted event study estimates using this alternative measure produce virtually identical patterns to the main results, with comparable effect sizes and temporal dynamics across all birth outcomes. This consistency suggests that the findings are not driven by idiosyncrasies in the motor fuel consumption measure but reflect genuine variation in oil dependence.

Third, I explore whether the continuous treatment approach masks important nonlinearities by implementing a binary treatment specification that compares high versus low oil-dependent states (used median as a cutoff). [Figure 14](#) shows that this discrete approach yields results that are qualitatively consistent with the continuous specification, though with somewhat different effect magnitudes as expected when collapsing continuous variation into binary categories. Finally, results using the individual-level birth data in [Figure 15](#) show robust findings consistent with the main results. Across all robustness checks, the core findings remain stable, providing confidence that the main results reflect genuine treatment effects rather than specification-dependent artifacts.

7 Discussion and Conclusion

In this study, I examine the causal effects of the 1973 oil embargo on birth outcomes by exploiting cross-state variation in oil dependence measured by 1972 motor fuel consumption per capita. My analysis yields several important findings that contribute to our understanding of how macroeconomic shocks affect population health, particularly among vulnerable demographic groups.

My central finding is that while raw event study estimates suggest systematic deterioration in birth outcomes among oil-dependent states, these apparent effects largely reflect pre-existing differential trends. After I adjust for state-specific pre-trends, the overall treatment effects become substantially improved and statistically significant across most outcomes.

Additionally, when I examine heterogeneity across different demographic groups, a more nuanced story emerges. I find substantial benefits for specific vulnerable populations. Mothers with less than high school education, relatively younger mothers, and Black mothers all experienced meaningful improvements in birth outcomes following the embargo. These differential effects suggest that oil-dependent states may have provided unique advantages for disadvantaged groups during this period, potentially through improved self-care, or reduced exposure to environmental stressors that disproportionately affected these populations.

Through my mechanism analysis, I provide plausible explanations for these improvements consistent with multiple theoretical frameworks. First, I find evidence that oil-dependent states experienced increased prenatal care utilization following the embargo, supporting the countercyclical mortality hypothesis whereby economic downturns prompt

resource reallocation toward prevention and essential care (Ruhm, 2000; Miller et al., 2009; Menclova, 2013; Finkelstein et al., 2024). Prenatal care improves maternal health and pregnancy behaviors (Yan, 2017), supporting this channel. Second, I document compositional changes in maternal characteristics—including increases in maternal age and parity alongside reductions in teenage pregnancy rates—that partly explain the observed health improvements. These shifts reflect equilibrium responses to altered opportunity costs of conception timing during the recession, as women facing reduced market work opportunities adjusted their fertility decisions accordingly. Maternal age, birth experience, and non-teenage status predict positive birth outcomes (Weng et al., 2014; Chevalier and O’Sullivan, 2007), providing a selection-based explanation for the observed improvements. Third, I find evidence of reduced air pollution levels in oil-dependent states, consistent with declining transportation demand as rising gasoline prices incentivized consumers to reduce vehicular travel (Hughes et al., 2008; Small and Dender, 2007). These reductions benefited pregnant women and developing fetuses who show heightened sensitivity to environmental toxins (Currie et al., 2015). These findings suggest multiple pathways through which macroeconomic shocks can influence population health: through healthcare system responses, behavioral adaptations to changing economic incentives, and environmental externalities from altered consumption patterns.

My findings reveal countercyclical health effects following the oil embargo, contrasting with procyclical patterns documented in other contexts. While Bozzoli and Quintana-Domeque (2014) show that Argentina’s economic crisis reduced birthweight among children of low-educated mothers through credit constraints and nutritional deprivation, I find that the oil embargo improved birth outcomes for disadvantaged populations. However, these countercyclical effects operate through mechanisms distinct from those in the general recession literature. The oil embargo generated region-specific changes in air pollution that particularly benefited vulnerable populations. The compositional changes I document, including reductions in teenage pregnancy and increases in maternal age, further reflect selection mechanisms unique to energy price shocks rather than consumption constraints emphasized in traditional recession studies. These findings demonstrate that countercyclical health improvements can arise through shock-specific pathways, with energy price disruptions creating distinct mechanisms that differentially affect oil-dependent regions and their most vulnerable populations.

These findings have broader implications for understanding distributional effects of economic shocks. The concentration of benefits among disadvantaged groups that I document challenges simple narratives about how oil price volatility affects population welfare. Rather than uniform negative impacts, my results suggest that vulnerable populations may sometimes benefit from the economic reorganization that follows major shocks (Finkelstein et al., 2024), particularly when these changes improve access to healthcare, reduce environmental hazards, or alter social and economic opportunities in ways that

favor disadvantaged communities.

These insights are particularly relevant for contemporary policy discussions about energy transitions and climate change adaptation. As policymakers consider interventions that may similarly disrupt oil-dependent economic structures, my research demonstrates how such changes differentially affect population subgroups, which becomes crucial for designing equitable transition policies. The evidence I present that some of society’s most vulnerable members—relatively younger mothers, less-educated women, and racial minorities—can experience health improvements during periods of oil market disruption suggests that well-designed policies could harness similar mechanisms to improve population health outcomes.

7.1 Welfare Implications of Early-Life Health Improvements During Economic Shocks

The birth outcome improvements documented after the oil embargo have potentially significant welfare implications that merit discussion within the broader literature on recession impacts. While conventional analyses of macroeconomic shocks such as recessions focus primarily on consumption volatility (Blanchard, 1993; Flatters and Willmott, 2009; Meyer and Sullivan, 2013) and employment disruptions (Kalleberg and Von Wachter, 2017), the health capital gains identified in this study represent an additional welfare channel that could operate through improved lifetime trajectories for affected cohorts.

These findings connect to recent work (Dehejia and Lleras-Muney, 2004; Ruhm, 2000; Miller et al., 2009) examining how health effects during economic downturns alter traditional welfare calculations of recession costs. The documented improvements in birth-weight and gestational age could generate substantial lifetime returns through established pathways linking early-life health endowments to subsequent labor market outcomes and lifetime earnings. Such effects suggest that comprehensive welfare assessments of macroeconomic disruptions may need to account for health capital adjustments that partially offset standard consumption-based welfare losses.

The magnitude of these potential welfare effects remains an important question for future research. The cross-sectional variation in fuel dependence that identifies the health effects provides a natural framework for future welfare analyses, offering the possibility of quantifying how economic shocks generate heterogeneous welfare consequences. These considerations highlight how macroeconomic events may have complex welfare implications that extend beyond immediately observable consumption and employment effects, suggesting fertile ground for future research integrating health capital into standard welfare frameworks.

My future research aims to explore the longer-term consequences of the birth outcome improvements I document, investigate whether it has any longer term consequences on

education or the income (Stuart, 2022; East et al., 2023).

In conclusion, my study demonstrates that the health effects of macroeconomic shocks are highly heterogeneous. My finding that oil embargo impacts were concentrated among society's most vulnerable populations, producing health improvements rather than deterioration, offers important lessons for both historical understanding and contemporary policy design in an era of ongoing energy and climate transitions.

References

- Acemoglu, D., Finkelstein, A., and Notowidigdo, M. J. (2013). Income and health spending: Evidence from oil price shocks. *Review of Economics and Statistics*, 95(4):1079–1095. [5](#), [47](#)
- Aizer, A. and Currie, J. (2014). The intergenerational transmission of inequality: maternal disadvantage and health at birth. *science*, 344(6186):856–861. [14](#)
- Alexander, D. and Schwandt, H. (2022). The impact of car pollution on infant and child health: Evidence from emissions cheating. *The Review of Economic Studies*, 89(6):2872–2910. [4](#), [15](#)
- Almond, D. (2006). Is the 1918 influenza pandemic over? long-term effects of in utero influenza exposure in the post-1940 us population. *Journal of political Economy*, 114(4):672–712. [2](#)
- Almond, D. and Currie, J. (2011). Killing me softly: The fetal origins hypothesis. *Journal of economic perspectives*, 25(3):153–172. [2](#)
- Almond, D., Hoynes, H. W., and Schanzenbach, D. W. (2011). Inside the war on poverty: The impact of food stamps on birth outcomes. *The review of economics and statistics*, 93(2):387–403. [13](#), [58](#), [59](#), [60](#), [61](#)
- Bailey, M. J. (2012). Reexamining the impact of family planning programs on us fertility: evidence from the war on poverty and the early years of title x. *American Economic Journal: Applied Economics*, 4(2):62–97. [58](#), [59](#), [60](#), [61](#)
- Baran, C., Currie, J., Dursun, B., and Tekin, E. (2025a). Clean rides, healthy lives: The impact of electric vehicle adoption on air quality and infant health. Working Paper 34278, National Bureau of Economic Research. [9](#)
- Baran, C., Currie, J., Dursun, B., and Tekin, E. (2025b). Clean rides, healthy lives: The impact of electric vehicle adoption on air quality and infant health. Technical report, National Bureau of Economic Research. [15](#)
- Barsky, R. B. and Kilian, L. (2002). Oil and the macroeconomy since the 1970s. *Journal of Economic Perspectives*, 18(4):115–134. [2](#)
- Bertoli, P., Grembi, V., and Nguyen, T. L. B. (2023). Birth outcomes in hard times among minority ethnic groups. *Journal of Population Economics*, 36(1):263–294. [15](#)
- Blanchard, O. (1993). Consumption and the recession of 1990-1991. *The American Economic Review*, 83(2):270–274. [21](#)

- Bopp, A. E. (1984). Tests for structural change in us oil consumption, 1967–1982. *Energy economics*, 6(4):223–230. [2](#)
- Bozzoli, C. and Quintana-Domeque, C. (2014). The weight of the crisis: Evidence from newborns in argentina. *Review of Economics and Statistics*, 96(3):550–562. [13](#), [20](#)
- Carleton, T. A. and Hsiang, S. M. (2016). Social and economic impacts of climate. *Science*, 353(6304):aad9837. [5](#)
- Charris, C., Branco, D., and Carrillo, B. (2024). Economic shocks and infant health: Evidence from a trade reform in brazil. *Journal of Development Economics*, 166:103193. [4](#)
- Chay, K. Y. and Greenstone, M. (2003). The impact of air pollution on infant mortality: evidence from geographic variation in pollution shocks induced by a recession. *The quarterly journal of economics*, 118(3):1121–1167. [7](#), [15](#)
- Chevalier, A. and O’Sullivan, V. (2007). Mother’s education and birth weight. Technical report, IZA Discussion Papers. [4](#), [7](#), [17](#), [20](#)
- Currie, J. (2011). Inequality at birth: Some causes and consequences. *American economic review*, 101(3):1–22. [14](#)
- Currie, J. and Almond, D. (2011). Human capital development before age five. In *Handbook of labor economics*, volume 4, pages 1315–1486. Elsevier. [2](#)
- Currie, J., Davis, L., Greenstone, M., and Walker, R. (2015). Environmental health risks and housing values: evidence from 1,600 toxic plant openings and closings. *American Economic Review*, 105(2):678–709. [7](#), [20](#)
- Currie, J. and Rossin-Slater, M. (2015). Early-life origins of life-cycle well-being: Research and policy implications. *Journal of policy Analysis and management*, 34(1):208–242. [2](#)
- Currie, J. and Walker, R. (2011). Traffic congestion and infant health: Evidence from e-zpass. *American Economic Journal: Applied Economics*, 3(1):65–90. [3](#), [4](#), [9](#), [15](#), [18](#)
- Dehejia, R. and Lleras-Muney, A. (2004). Booms, busts, and babies’ health. *The Quarterly journal of economics*, 119(3):1091–1130. [2](#), [13](#), [21](#)
- Deryugina, T. and Reif, J. (2023). The long-run effect of air pollution on survival. Technical report, National Bureau of Economic Research. [9](#)
- East, C. N., Miller, S., Page, M., and Wherry, L. R. (2023). Multigenerational impacts of childhood access to the safety net: Early life exposure to medicaid and the next generation’s health. *American Economic Review*, 113(1):98–135. [8](#), [22](#)

- Finch, B. K., Thomas, K., and Beck, A. N. (2019). The great recession and adverse birth outcomes: evidence from california, usa. *SSM-population health*, 9:100470. [4](#)
- Finkelstein, A., Notowidigdo, M. J., Schilbach, F., and Zhang, J. (2024). Lives vs. livelihoods: The impact of the great recession on mortality and welfare. Technical report, National Bureau of Economic Research. [6](#), [20](#)
- Finkelstein, A., Notowidigdo, M. J., Schilbach, F., and Zhang, J. (2025). Lives versus livelihoods: The impact of the great recession on mortality and welfare. *The Quarterly Journal of Economics*, page qjaf023. [4](#), [15](#)
- Flatters, P. and Willmott, M. (2009). Understanding the post-recession consumer. *Harvard Business Review*, 87(7/8):106–112. [21](#)
- Frech III, H. and Lee, W. C. (1987). The welfare cost of rationing-by-queuing across markets: Theory and estimates from the us gasoline crises. *The Quarterly Journal of Economics*, 102(1):97–108. [5](#)
- Gertler, P. and Gruber, J. (2002). Insuring consumption against illness. *American economic review*, 92(1):51–70. [6](#)
- Goel, R. K. and Morey, M. J. (1993). Effect of the 1973 oil price embargo: A non-parametric analysis. *Energy economics*, 15(1):39–48. [2](#)
- Goldenberg, R. L. and Cliver, S. P. (1997). Small for gestational age and intrauterine growth restriction: definitions and standards. *Clinical obstetrics and gynecology*, 40(4):704–714. [49](#)
- Goodman-Bacon, A. (2018). Public insurance and mortality: evidence from medicaid implementation. *Journal of Political Economy*, 126(1):216–262. [58](#), [59](#), [60](#), [61](#)
- Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of econometrics*, 225(2):254–277. [18](#), [38](#), [56](#)
- Hamilton, J. D. (2013). Historical oil shocks. In *Routledge handbook of major events in economic history*, pages 239–265. Routledge. [2](#), [5](#)
- Hecq, W., Borisov, Y., and Totte, M. (1993). Daylight saving time effect on fuel consumption and atmospheric pollution. *Science of the total environment*, 133(3):249–274. [17](#)
- Hoynes, H., Page, M., and Stevens, A. H. (2011). Can targeted transfers improve birth outcomes?: Evidence from the introduction of the wic program. *Journal of Public Economics*, 95(7-8):813–827. [13](#)

- Hughes, J. E., Knittel, C. R., and Sperling, D. (2008). Evidence of a shift in the short-run price elasticity of gasoline demand. *The Energy Journal*, 29(1):113–134. [7](#), [20](#)
- Isen, A., Rossin-Slater, M., and Walker, W. R. (2017). Every breath you take—every dollar you’ll make: The long-term consequences of the clean air act of 1970. *Journal of Political Economy*, 125(3):848–902. [9](#)
- Kalleberg, A. L. and Von Wachter, T. M. (2017). The us labor market during and after the great recession: Continuities and transformations. *RSF: The Russell Sage Foundation Journal of the Social Sciences*, 3(3):1–19. [21](#)
- Karadavut, A. B., Smits, I., van Dillen, J., and Hogeveen, M. (2025). The criteria to classify body-proportionality of the small for gestational age newborn: a scoping review. *BMC Pregnancy and Childbirth*, 25(1):1–16. [49](#)
- Kilian, L. (2008). Exogenous oil supply shocks: how big are they and how much do they matter for the us economy? *The review of economics and statistics*, 90(2):216–240. [2](#)
- Knittel, C. R. (2014). The political economy of gasoline taxes: lessons from the oil embargo. *Tax Policy and the Economy*, 28(1):97–131. [30](#), [47](#)
- Knittel, C. R., Miller, D. L., and Sanders, N. J. (2011). Caution, drivers! children present: Traffic, pollution, and infant health. Technical report, National Bureau of Economic Research. [4](#), [15](#)
- Kose, E., O’Keefe, S., and Rosales-Rueda, M. (2024). Does the delivery of primary health care improve birth outcomes?: Evidence from the rollout of community health centers. *Journal of Human Resources*. [3](#), [8](#), [13](#), [49](#), [58](#), [59](#), [60](#), [61](#)
- Menclova, A. K. (2013). The effects of unemployment on prenatal care use and infant health. *Journal of Family and Economic Issues*, 34(4):400–420. [6](#), [20](#)
- Meyer, B. D. and Sullivan, J. X. (2013). Consumption and income inequality and the great recession. *American Economic Review*, 103(3):178–183. [21](#)
- Miller, D. L., Page, M. E., Stevens, A. H., and Filipski, M. (2009). Why are recessions good for your health? *American Economic Review*, 99(2):122–127. [6](#), [20](#), [21](#)
- Monheit, A. C., Grafova, I. B., and Kumar, R. (2020). How does family health care use respond to economic shocks? realized and anticipated effects. *Review of Economics of the Household*, 18(2):307–334. [6](#)
- National Center for Health Statistics (1968–1979). Vital statistics natality birth data. National Bureau of Economic Research. [3](#), [8](#)

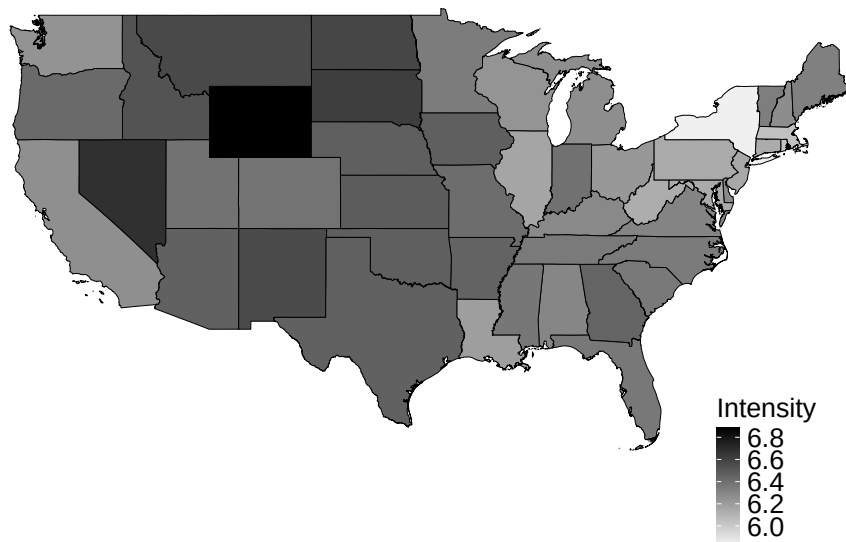
- Noelke, C., Chen, Y.-H., Osypuk, T. L., and Acevedo-Garcia, D. (2019). Economic downturns and inequities in birth outcomes: evidence from 149 million us births. *American journal of epidemiology*, 188(6):1092–1100. 15
- Office of the Historian, Bureau of Administration, U.S. Department of State (2025). Oil embargo, 1973–1974. <https://history.state.gov/milestones/1969-1976/oil-embargo>. Accessed: August 16, 2025. 2, 5
- Ruhm, C. J. (2000). Are recessions good for your health? *The Quarterly journal of economics*, 115(2):617–650. 2, 6, 20, 21
- Small, K. A. and Dender, K. V. (2007). Fuel efficiency and motor vehicle travel: the declining rebound effect. *The energy journal*, 28(1):25–52. 7, 20
- Stevens, A. H., Miller, D. L., Page, M. E., and Filipski, M. (2015). The best of times, the worst of times: understanding pro-cyclical mortality. *American Economic Journal: Economic Policy*, 7(4):279–311. 15
- Stuart, B. A. (2022). The long-run effects of recessions on education and income. *American Economic Journal: Applied Economics*, 14(1):42–74. 8, 22
- Timilsina, L. (2023). Immigration policy shocks and infant health. *Economics & Human Biology*, 51:101309. 4
- Tiotiu, A. I., Novakova, P., Nedeva, D., Chong-Neto, H. J., Novakova, S., Steiropoulos, P., and Kowal, K. (2020). Impact of air pollution on asthma outcomes. *International journal of environmental research and public health*, 17(17):6212. 9, 37
- Venigalla, M. and Krimmer, M. (2006). Impact of electronic toll collection and electronic screening on heavy-duty vehicle emissions. *Transportation research record*, 1987(1):11–20. 18
- Weng, Y.-H., Yang, C.-Y., and Chiu, Y.-W. (2014). Risk assessment of adverse birth outcomes in relation to maternal age. *PloS one*, 9(12):e114843. 4, 7, 17, 20
- Wilcox, A. J., Cortese, M., McConnaughey, D. R., Moster, D., and Basso, O. (2021). The limits of small-for-gestational-age as a high-risk category. *European Journal of Epidemiology*, 36(10):985–991. 49
- Willage, B. (2020). Unintended consequences of health insurance: Affordable care act’s free contraception mandate and risky sex. *Health economics*, 29(1):30–45. 2, 10, 11
- Wolfers, J. (2006). Did unilateral divorce laws raise divorce rates? a reconciliation and new results. *American economic review*, 96(5):1802–1820. 10, 11

- Yan, J. (2017). The effects of prenatal care utilization on maternal health and health behaviors. *Health economics*, 26(8):1001–1018. [6](#), [16](#), [20](#)
- Zarnowitz, V. and Moore, G. H. (1977). The recession and recovery of 1973-1976. In *Explorations in Economic Research, Volume 4, number 4*, pages 1–87. NBER. [2](#)
- Zulkifli, N. and Haqem, D. (2022). The opec oil shock crisis (1973): An analysis. *Asian Journal of Research in Business and Management*, 4(1):136–148. [2](#)

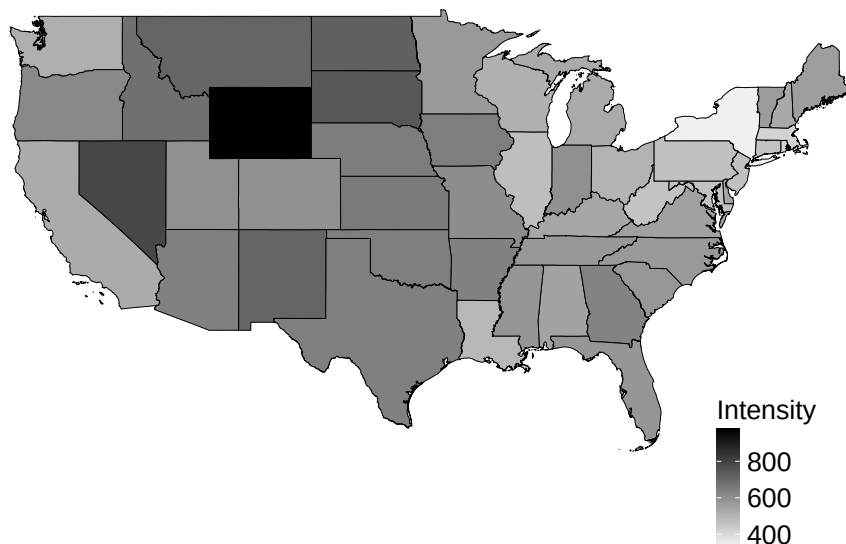
FIGURES

Figure 2: Pre-embargo geographic variation: Motor fuel consumption per capita (gallons per person)

Pre-embargo state level fuel consumption per capita (in logs)



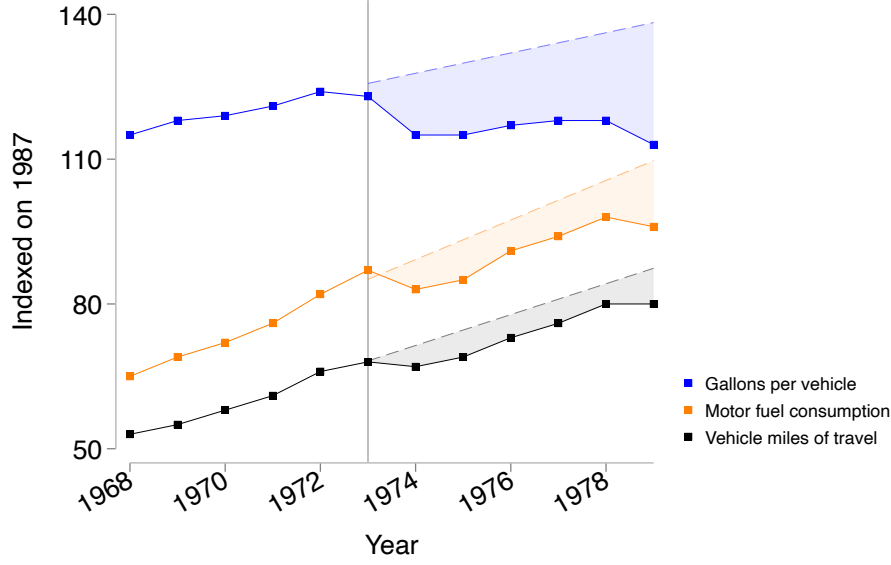
Pre-embargo state level fuel consumption per capita (in levels)



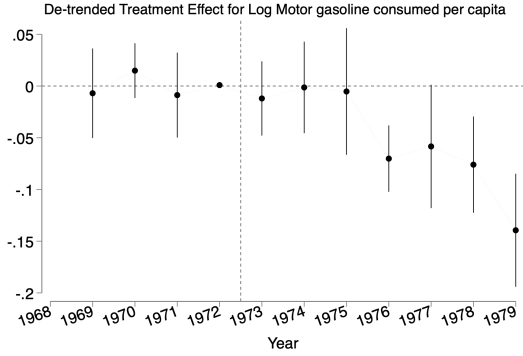
Notes: The top panel shows the natural logarithm of 1972 motor fuel consumption per capita, while the bottom panel shows levels. Both maps exploit continuous geographic variation in oil dependence by state.

Source: U.S. Department of Transportation

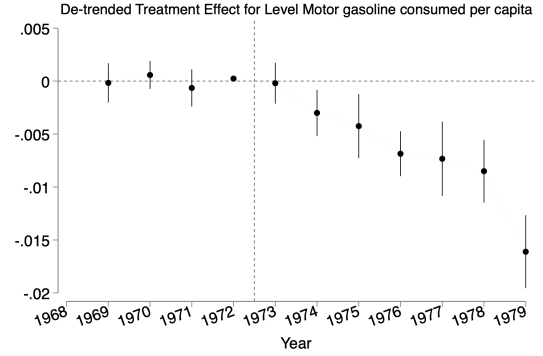
Figure 3: Motor fuel consumption, gallons per vehicle, vehicle miles of travel, and motor gasoline consumed per capita (1968–1979)



(a) Motor fuel consumption, gallons per vehicle, and vehicle miles of travel (1968–1979)



(b) Motor gasoline consumed per capita (in logs)



(c) Motor gasoline consumed per capita (in levels)

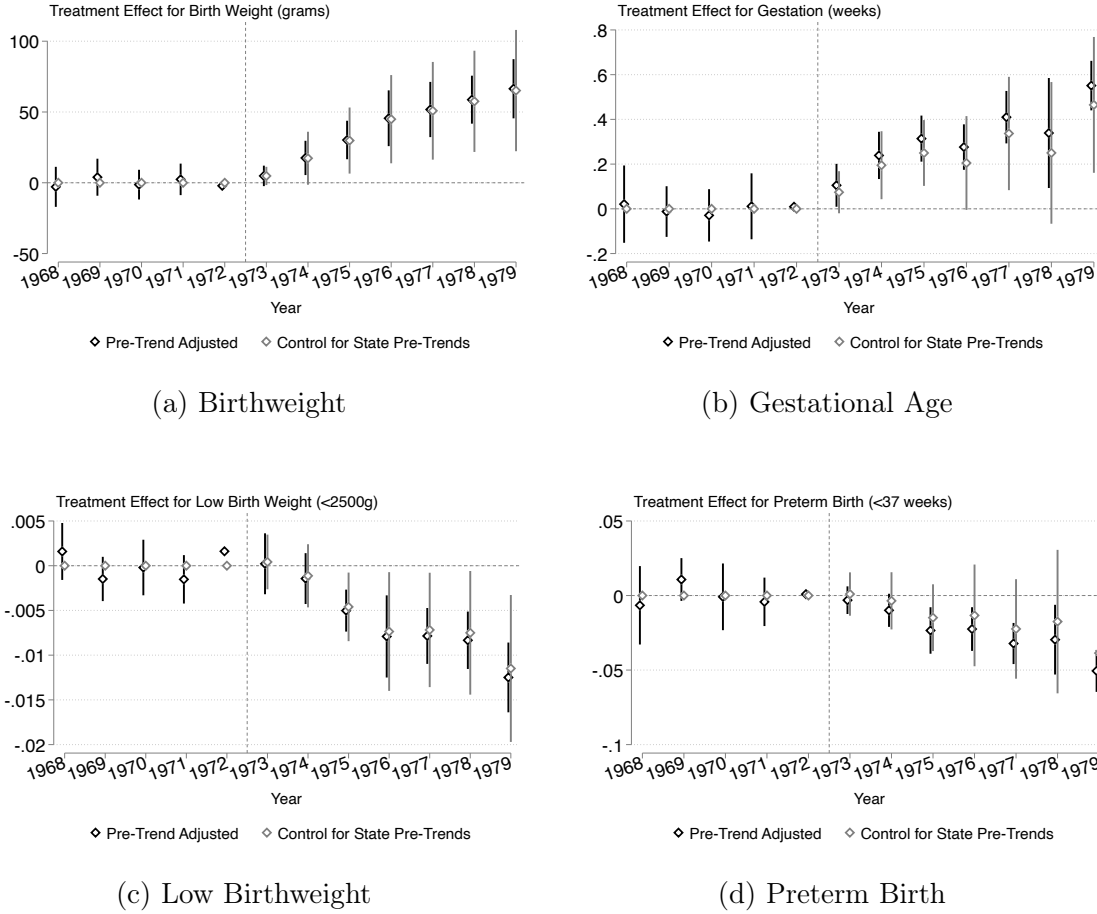
Notes: The relationship shown in Figure 3a is consistent with a observation of an inverse relationship between real oil prices and US consumption oil levels (Knittel, 2014). Figure 3b and Figure 3c are produced using detrended method mentioned in Appendix C.1 and traces the evolution of treatment effects over time relative to pre-period treatment trend. The dependent variable is log and level of motor gasoline consumed per capita, respectively. The treatment variable measures 1972 motor fuel consumption per capita by state in log form, exploiting continuous geographic variation in oil dependence. The regression includes state and year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level. Regressions are weighted by state population.

Source: Bureau of Transportation Statistics, U.S. Department of Transportation

MAIN RESULTS

Adjusted Event Study

Figure 4: Adjusted Event Study Estimates



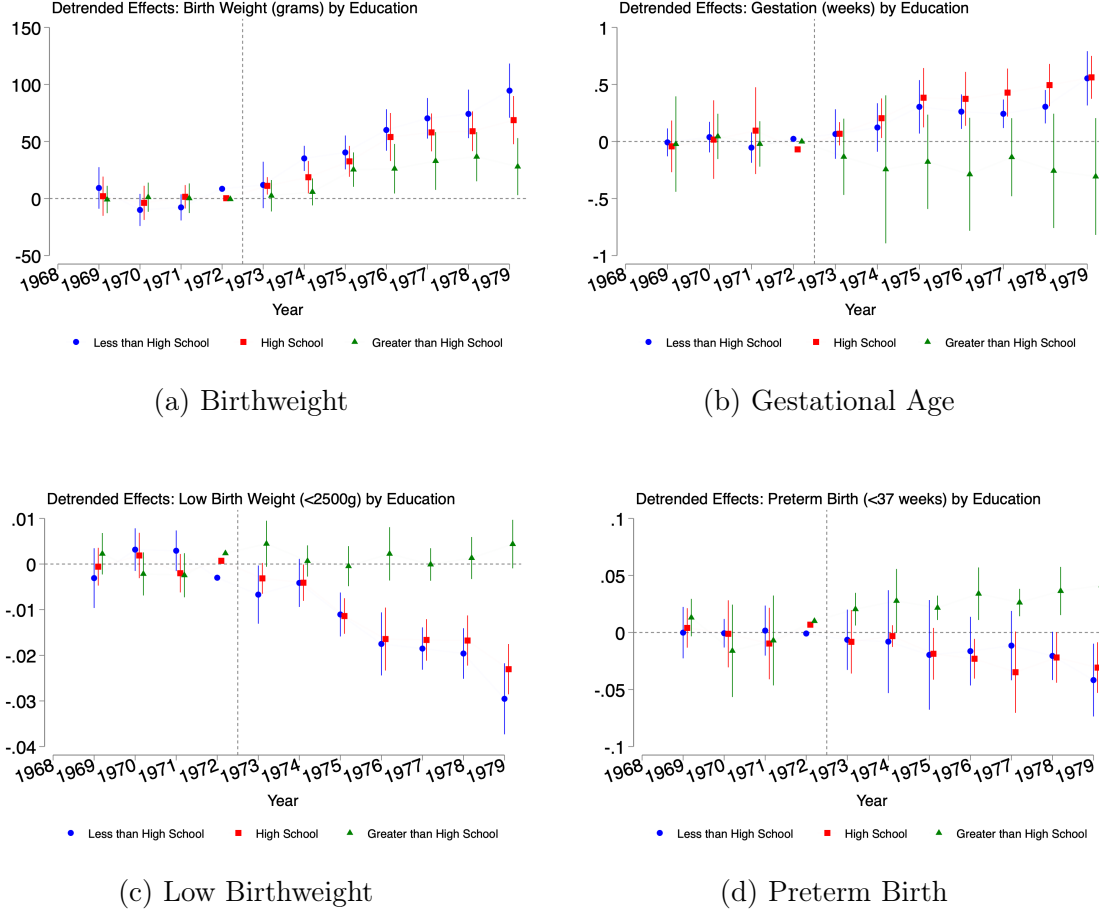
Notes: Black points are produced using detrended Equation 1 version mentioned in Appendix C.1 and trace the evolution of treatment effects over time relative to pre-period treatment trend. Grey points are generated using Equation 2. The treatment variable measures 1972 motor fuel consumption per capita by state in log form, exploiting continuous geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year-cell.

Source: National Vital Statistics System (NVSS) (1968-1979)

Heterogeneity

By Education

Figure 5: Heterogeneous Effects of Treatment by Maternal Education

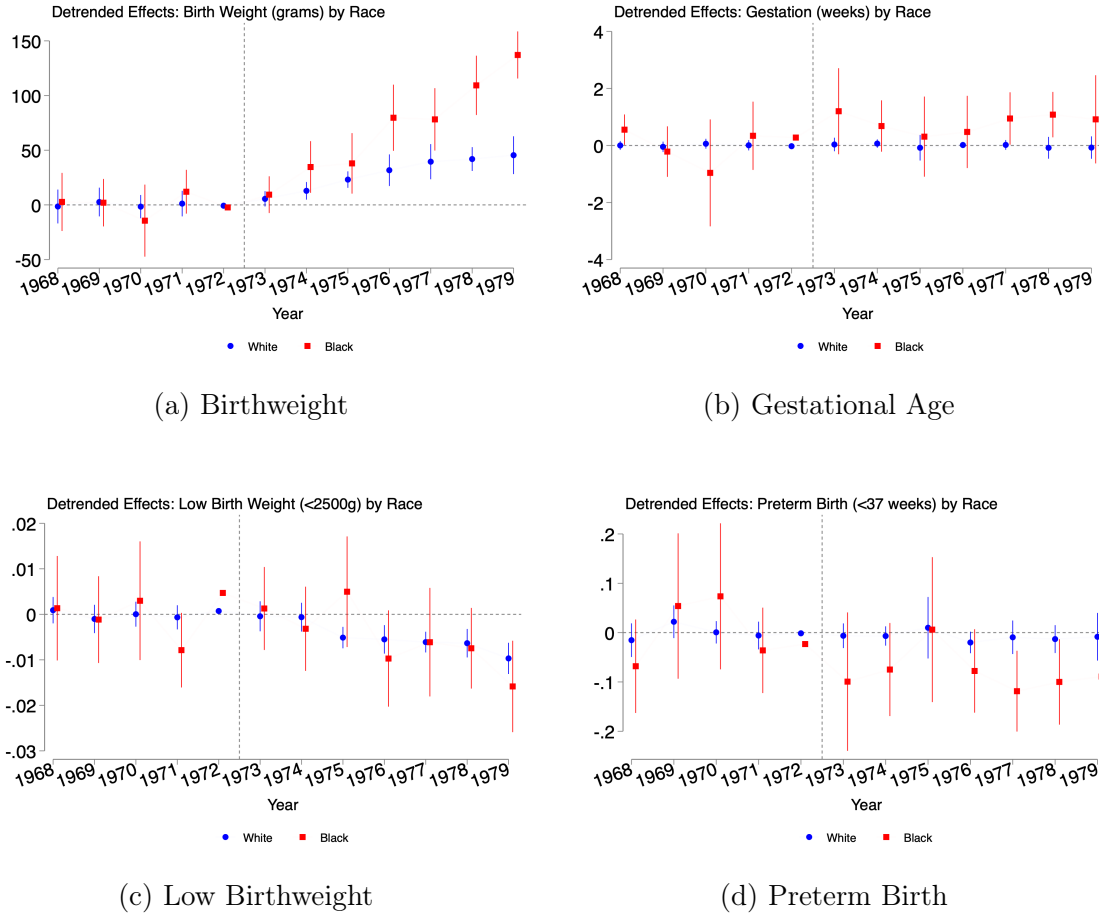


Notes: These figures are produced using detrended [Equation 1](#) version mentioned in [Appendix C.1](#) and trace the evolution of treatment effects over time relative to pre-period treatment trend. The treatment variable measures 1972 motor fuel consumption per capita by state in log form, exploiting continuous geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year-education.

Source: National Vital Statistics System (NVSS) (1968-1979)

By Race

Figure 6: Heterogeneous Effects of Treatment by Race/Ethnicity

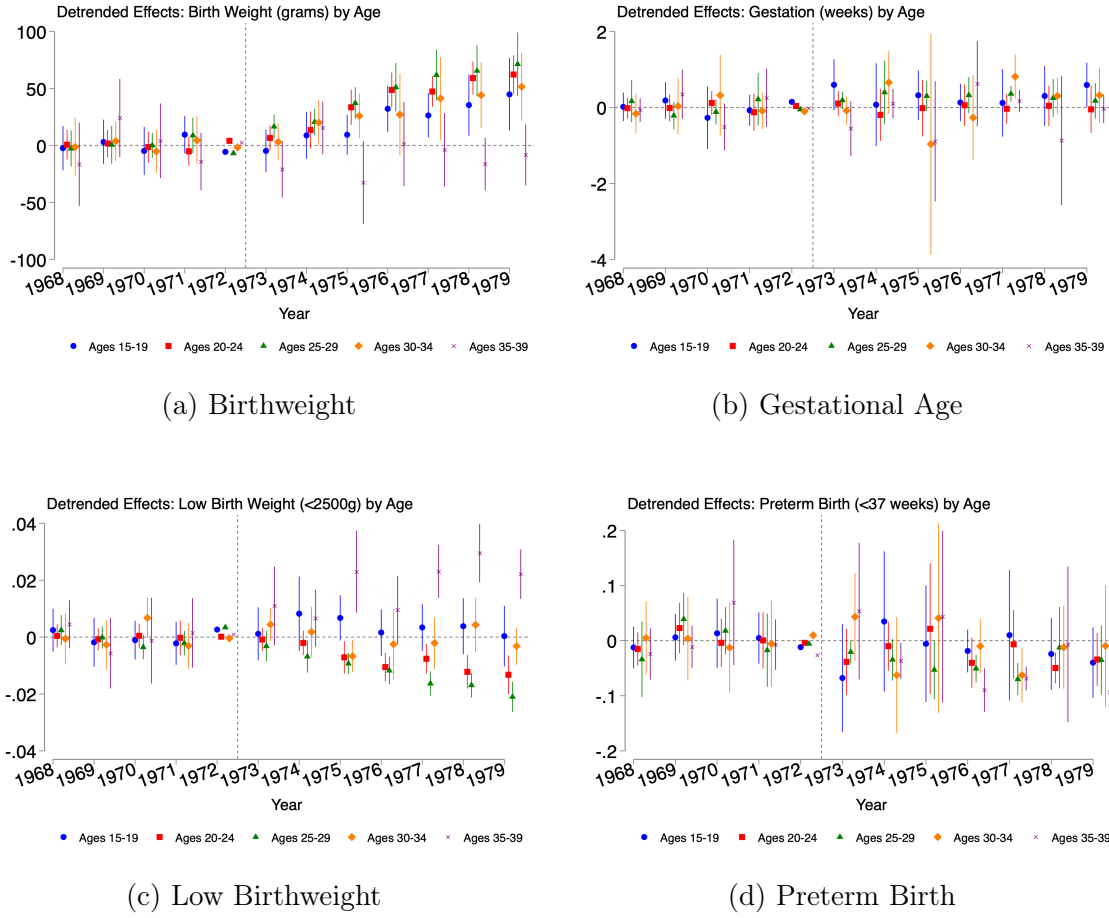


Notes: These figures are produced using detrended [Equation 1](#) version mentioned in [Appendix C.1](#) and trace the evolution of treatment effects over time relative to pre-period treatment trend. The treatment variable measures 1972 motor fuel consumption per capita by state in log form, exploiting continuous geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year-race.

Source: National Vital Statistics System (NVSS) (1968-1979)

By Age

Figure 7: Heterogeneous Effects of Treatment by Maternal Age



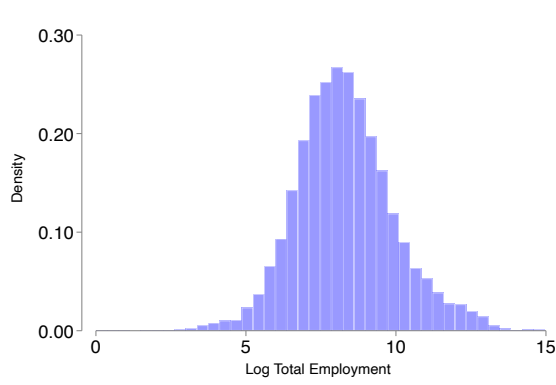
Notes: These figures are produced using detrended [Equation 1](#) version mentioned in [Appendix C.1](#) and trace the evolution of treatment effects over time relative to pre-period treatment trend. The treatment variable measures 1972 motor fuel consumption per capita by state in log form, exploiting continuous geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year-age.

Source: National Vital Statistics System (NVSS) (1968-1979)

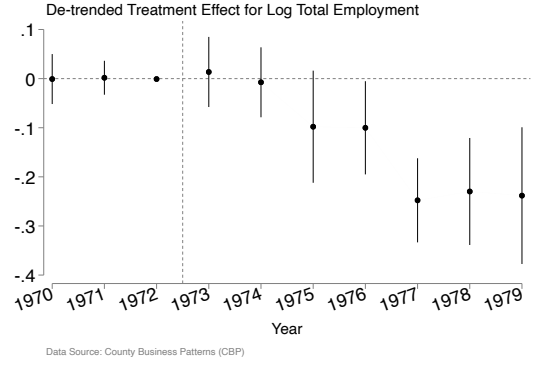
Potential Mechanisms

Economic Conditions and Prenatal Care

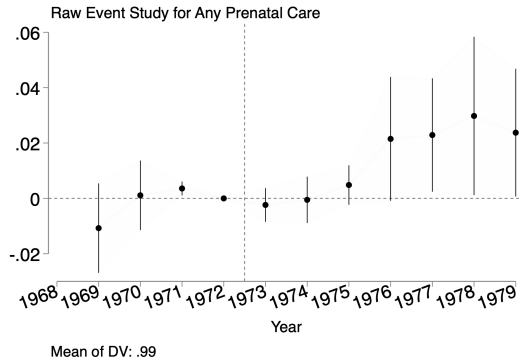
Figure 8: Economic Conditions and Prenatal Care



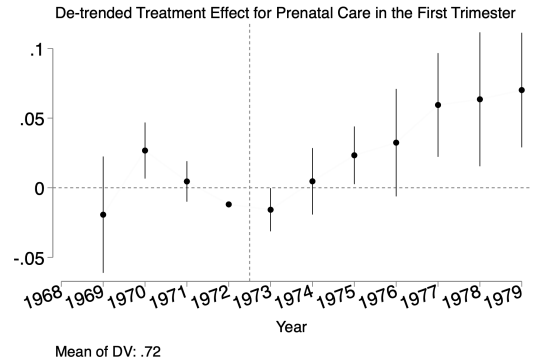
(a) Log Total Employment



(b) Adjusted Event Study Estimates for Log Total Employment



(c) Any Prenatal Care



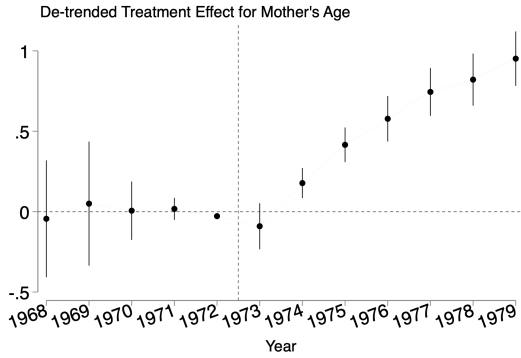
(d) Prenatal Care in the First Trimester

Notes: These figures are produced using detrended Equation 1 version mentioned in Appendix C.1 (except Figure 8a and Figure 8c) and trace the evolution of treatment effects over time relative to pre-period treatment trend. The treatment variable measures 1972 motor fuel consumption per capita by state in log form, exploiting continuous geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level. Regressions are weighted by the number of births in each county-year cell in Figure 8c and Figure 8d. In Figure 8b, unit of analysis is county-year and regression is weighted by the 1972 population.

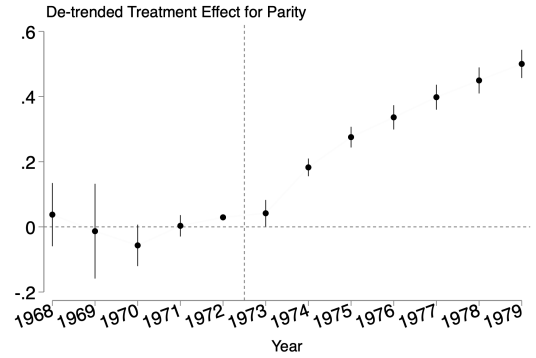
Source: National Vital Statistics System (NVSS) for Figure 8c and Figure 8d; County Business Patterns (CBP) for Figure 8a and Figure 8b.

Maternal Compositional Change

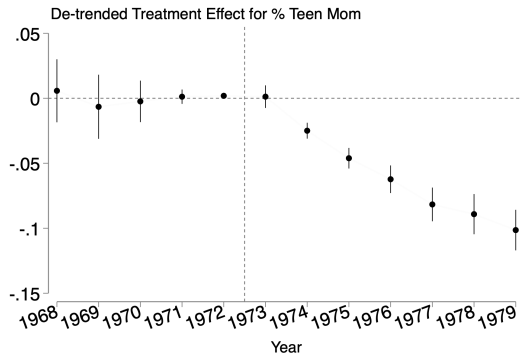
Figure 9: Adjusted Event Study Estimates by Maternal Composition



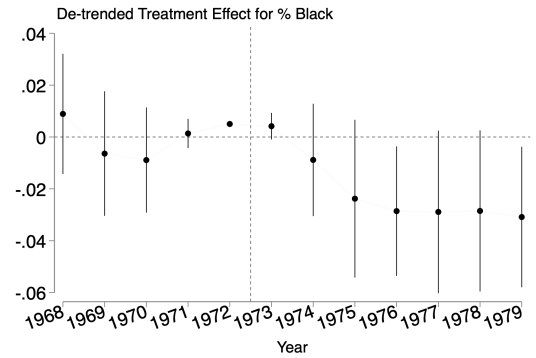
(a) Maternal Age



(b) Parity



(c) % Teen Moms



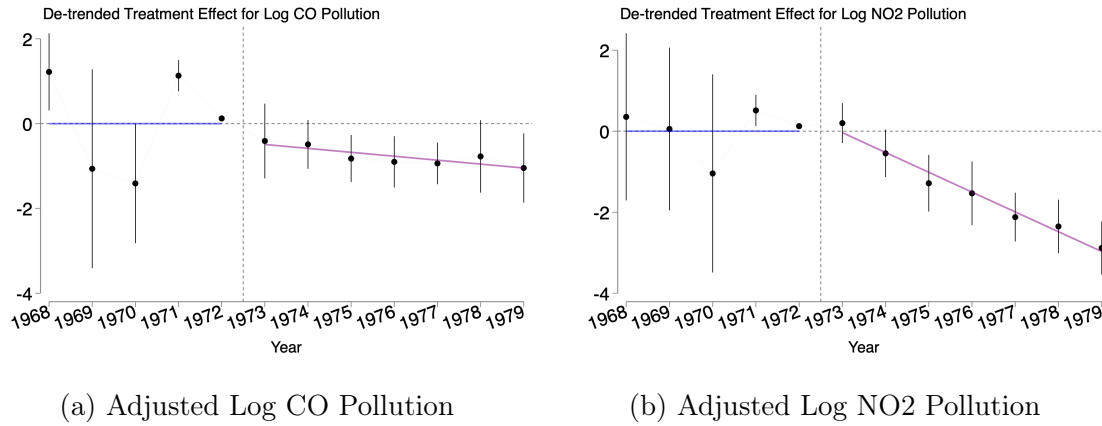
(d) % Black Mothers

Notes: These figures are produced using detrended [Equation 1](#) version mentioned in [Appendix C.1](#) and trace the evolution of treatment effects over time relative to pre-period treatment trend. The treatment variable measures 1972 motor fuel consumption per capita by state in log form, exploiting continuous geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year.

Source: National Vital Statistics System (NVSS) (1968-1979)

Reduction in Pollution

Figure 10: Mechanism Estimates: Pollution



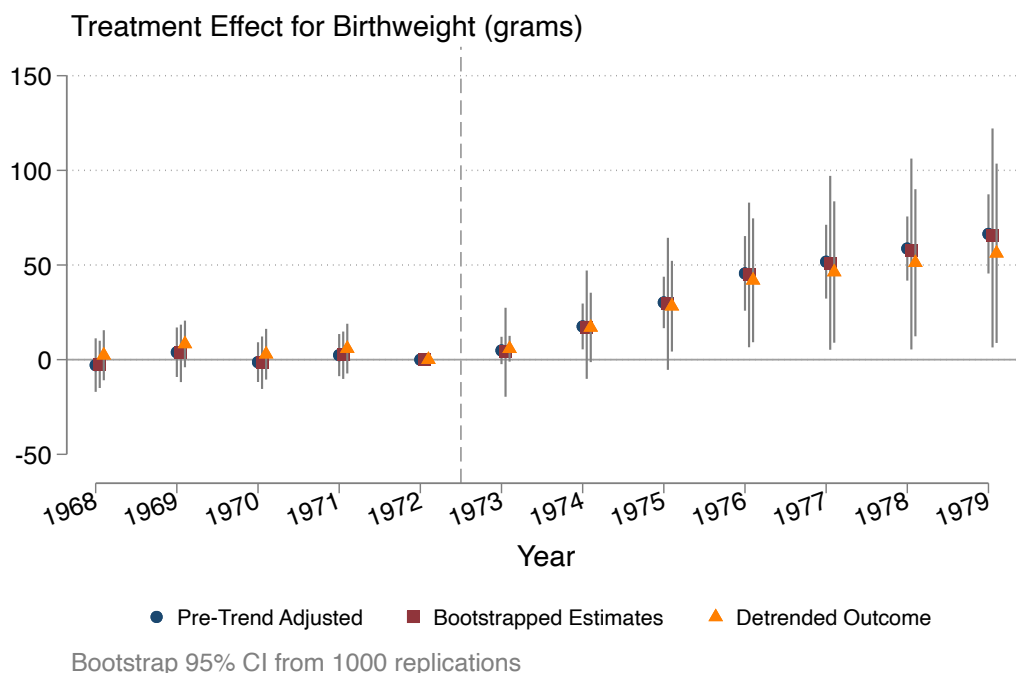
Notes: These figures are produced using detrended [Equation 1](#) version mentioned in [Appendix C.1](#) and trace the evolution of treatment effects over time relative to pre-period treatment trend. [Figure 10a](#) shows the effect on CO pollution. [Figure 10b](#) shows the effect on NO_2 pollution. These figures present results from an event study examining how baseline motor fuel consumption patterns mediate the effects of the 1973 oil crisis on Carbon Monoxide (CO) and nitrogen dioxide (NO_2) concentrations. The key explanatory variable is the interaction between birth year indicators and the logarithm of per capita motor fuel consumption in 1972 relative to pre-period treatment trend. Regressions include state and birth year fixed effects and are weighted by 1972 state population. Standard errors are clustered at the state level. The specification identifies how areas with different baseline fuel consumption intensities experienced differential changes in air pollution following the 1973 oil shock. The negative elasticities reveal that areas with higher baseline motor fuel consumption per capita experienced larger reductions in CO and NO_2 concentrations, respectively, following the 1973 oil crisis. Specifically, higher fuel consumption intensity in 1972 is associated with a greater decrease in pollution levels in subsequent years. This demonstrates a positive environmental externality where energy price shocks generated substantial air quality improvements in the most fuel-dependent regions.

Subnote: Why NO_2 ? According to [Tiotiu et al. \(2020\)](#), NO_2 primarily released from motor vehicle exhaust, is a well-established marker of traffic pollution, with transportation sources contributing up to 80% of ambient NO_2 concentrations. As a respiratory irritant, NO_2 can reach the lower lungs and is associated with coughing, wheezing, shortness of breath, bronchospasm, and, at high exposures, pulmonary edema.

Robustness Checks

Adjusting Standard Errors

Figure 11: Adjusting Standard Errors for the Birthweight Treatment Effect



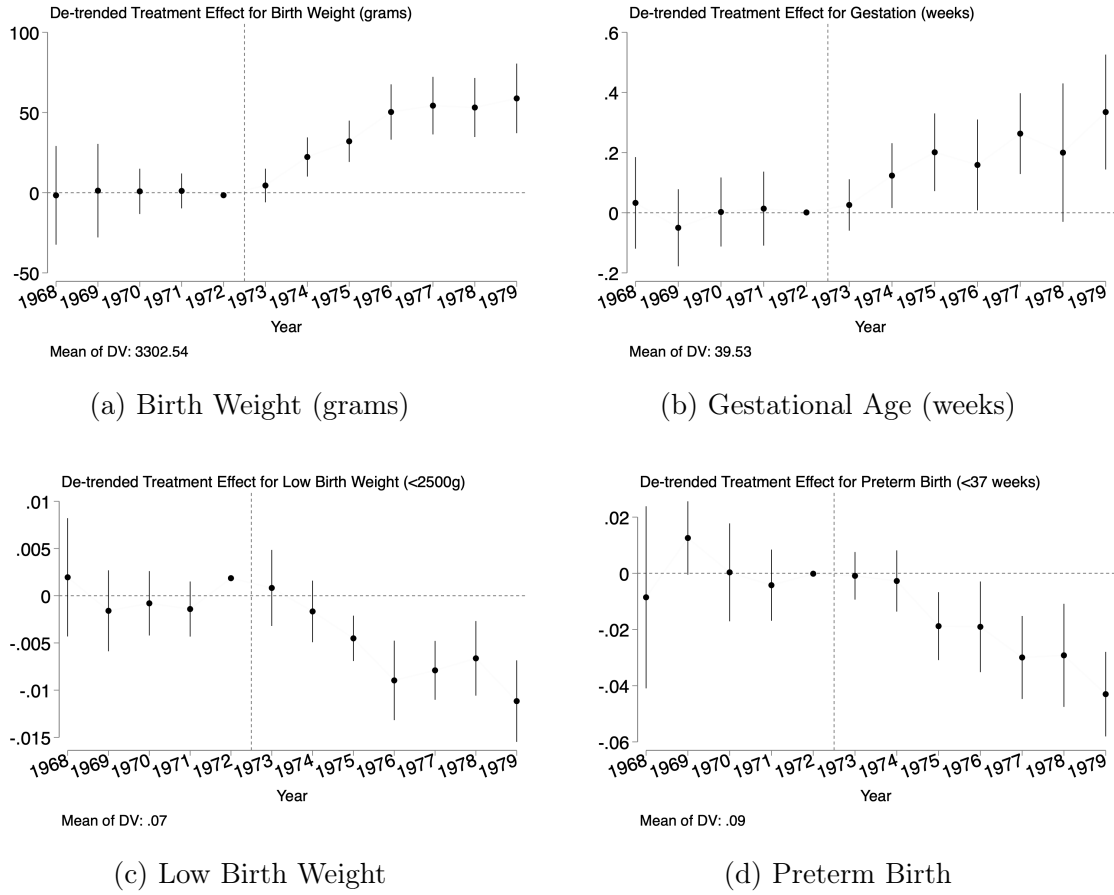
Notes: This figure is produced using detrended [Equation 1](#) version mentioned in [Appendix C.1](#) and trace the evolution of treatment effects over time relative to pre-period treatment trend. The treatment variable measures 1972 motor fuel consumption per capita by state in log form, exploiting continuous geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year-cell.

Blue dots are detrended baseline estimates. The maroon squares represent the estimates and confidence intervals obtained by following the process as described in [Appendix C.2](#). This method adequately accounts for the uncertainty in confidence intervals that the original approach might have missed. An additional robustness check is shown by the orange triangles, where I followed the [Goodman-Bacon \(2021\)](#) method that detrends the outcome variable—here, birthweight itself. All methods support the evidence that the estimates are robust.

Source: National Vital Statistics System (NVSS) (1968-1979)

Alternative Measure of Oil Dependence: Log of Per Capita Vehicle Registrations, 1972

Figure 12: Adjusted Event Study Estimates Using Alternative Oil Dependence Measure

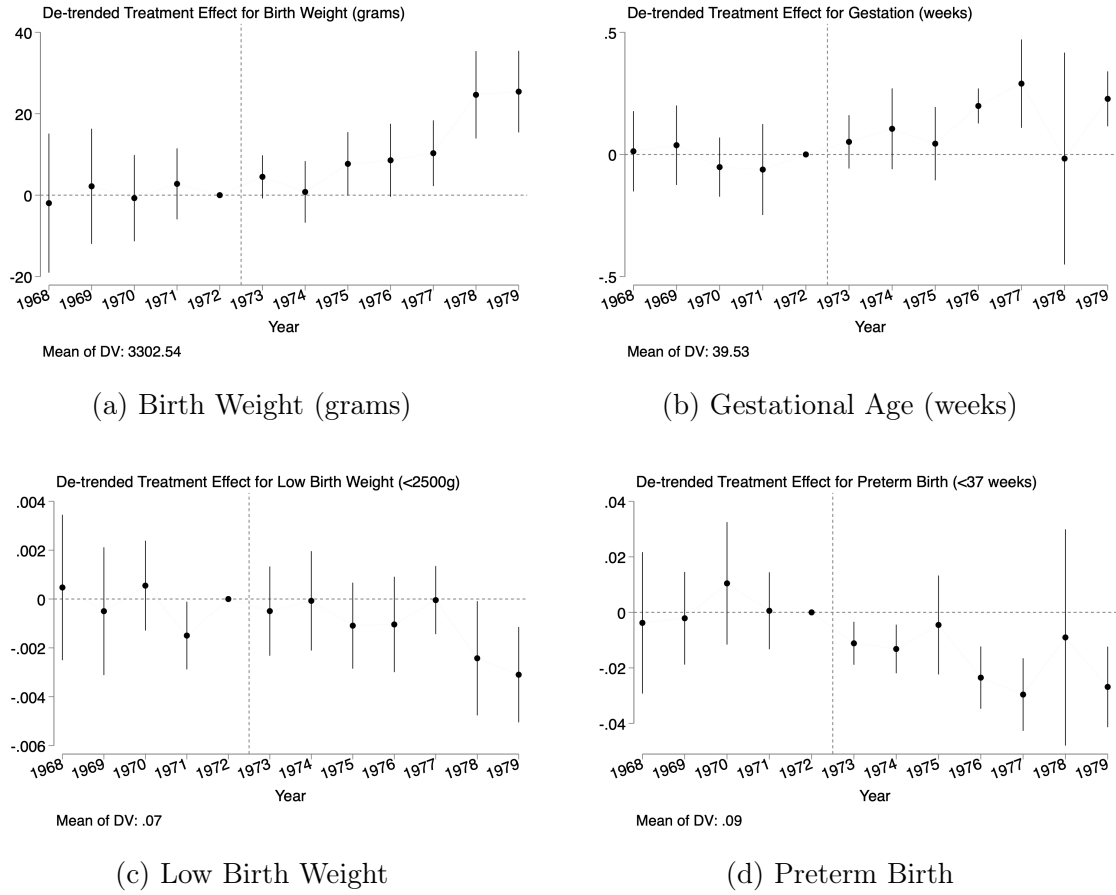


Notes: These figures are produced using detrended [Equation 1](#) version mentioned in Appendix C.1 and trace the evolution of treatment effects over time relative to pre-period treatment trend. The treatment variable measures 1972 vehicle registration per capita by state in log form, exploiting continuous geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year-cell.

Source: National Vital Statistics System (NVSS) (1968-1979); U.S. Department of Transportation

Alternative Measure of Oil Dependence: Log of Per Capita Total Petroleum Products Consumption, 1972

Figure 13: Adjusted Event Study Estimates Using Alternative Oil Dependence Measure

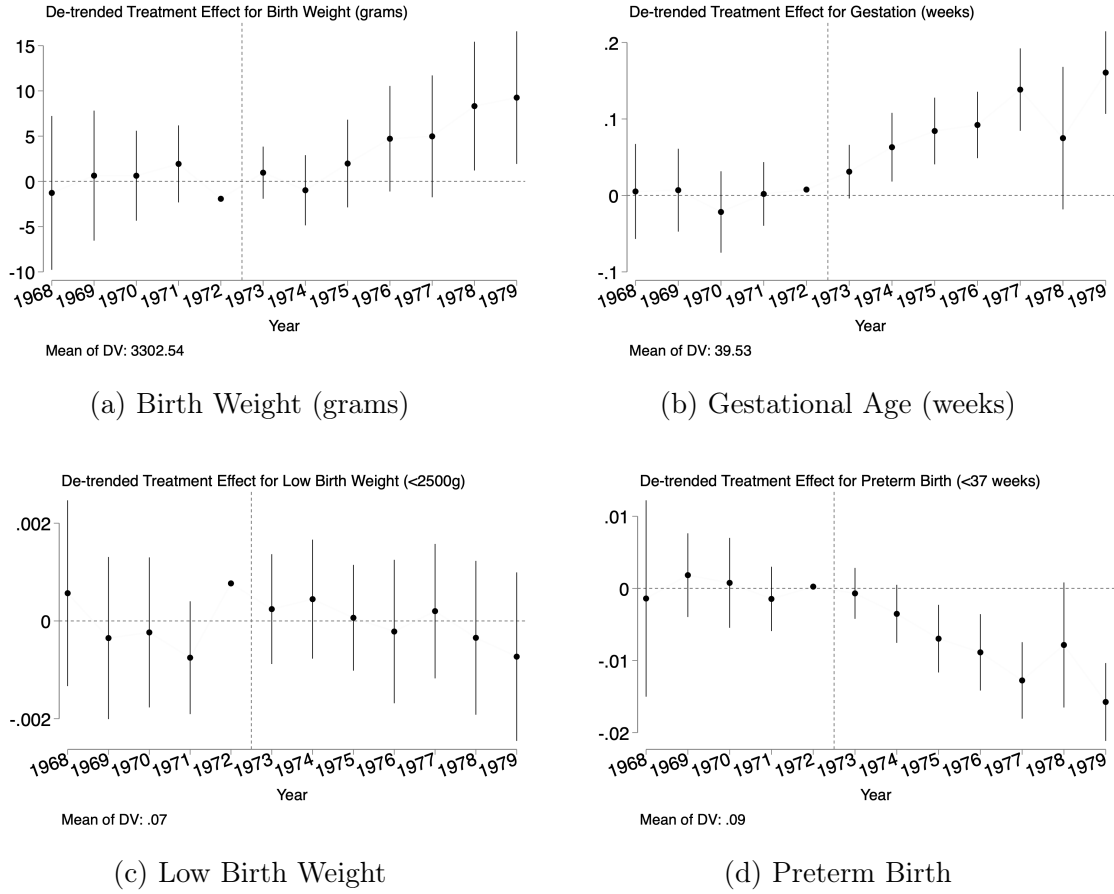


Notes: These figures are produced using detrended [Equation 1](#) version mentioned in Appendix C.1 and trace the evolution of treatment effects over time relative to pre-period treatment trend. The treatment variable measures 1972 total petroleum products consumption per capita by state in log form, exploiting continuous geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year-cell.

Source: National Vital Statistics System (NVSS) (1968-1979); State Energy Data System (SEDS)

Using Binary Treatment instead of Continuous

Figure 14: Event Study Estimates Using Binary Treatment Instead of Continuous Measure

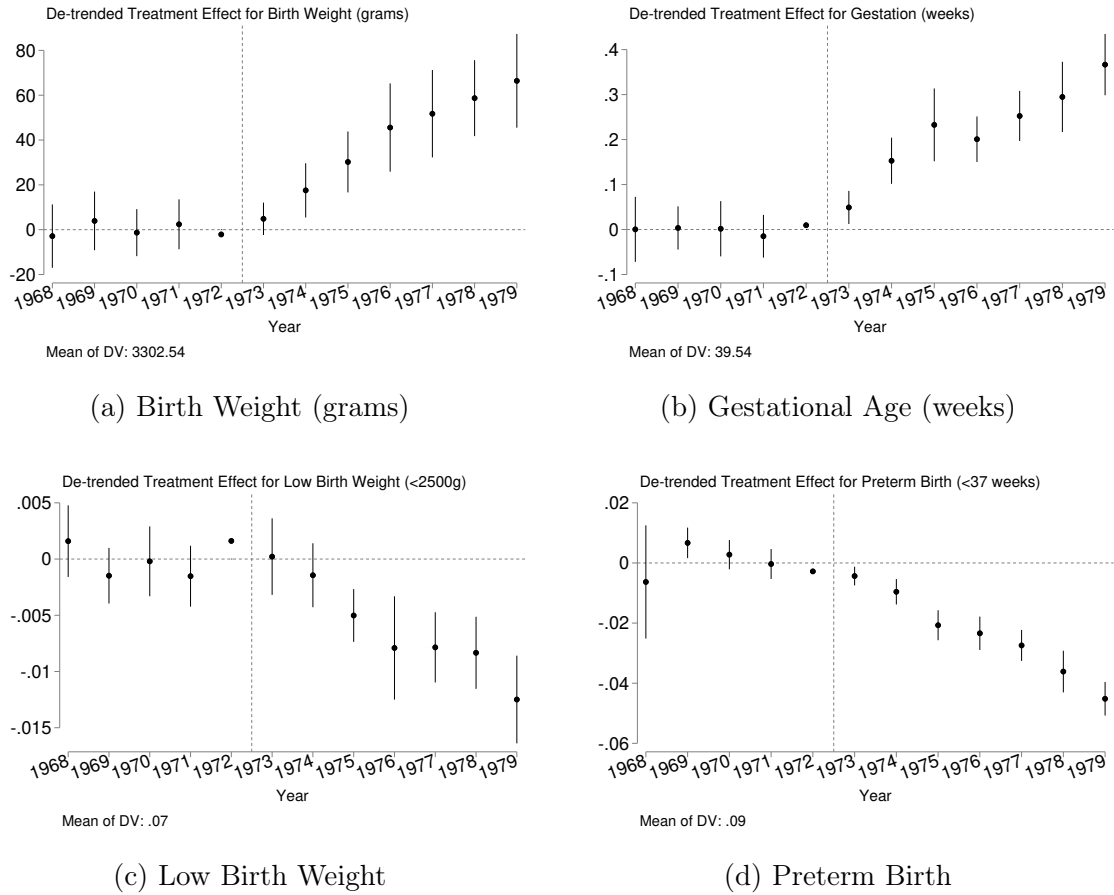


Notes: These figures are produced using detrended [Equation 1](#) version mentioned in [Appendix C.1](#) and trace the evolution of treatment effects over time relative to pre-period treatment trend. The treatment variable measures 1972 motor fuel consumption per capita by state in log form (converted to binary using median threshold), exploiting geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year-cell.

Source: National Vital Statistics System (NVSS) (1968-1979)

Using Individual Level Data

Figure 15: Event Study Estimates Using Individual Level Data



Notes: These figures are produced using detrended [Equation 1](#) version mentioned in Appendix [C.1](#) and trace the evolution of treatment effects over time relative to pre-period treatment trend. The treatment variable measures 1972 motor fuel consumption per capita by state in log form, exploiting geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level. These figures use the Individual-level data.

Source: National Vital Statistics System (NVSS) (1968-1979)

TABLES

Table 1: Summary Statistics

	Mean	SD
Birthweight(grams)	3302.544	223.187
Gestational Age	39.526	1.135
LBW	.074	.091
VLBW	.011	.037
SGA	.096	.108
PNC (any prenatal)	.985	.052
PNC (first trimester)	.722	.231
HBW	.015	.041
Mother's Age	24.467	.86
Parity	2.086	.179
% Teen Mom	.177	.054
% Black	.153	.158
% Married	.867	.109
% High School+	.728	.143

Notes: This table presents descriptive statistics for birth outcomes, with means and standard deviations reported in columns 1 and 2, respectively. I classify birth outcomes using standard clinical thresholds: low birth weight (<2,500g), very low birth weight (<1,500g), preterm birth (<37 weeks gestation), small for gestational age (<10th percentile for gestation age), and high birth weight (>4,500g). *Source:* National Vital Statistics System (NVSS) (1968-1979)

Table 2: Effect of Oil Embargo on Infant Health Outcome (Primary Outcomes)

	(1) Birthweight	(2) Gestation Age	(3) LBW	(4) Preterm
Post $\times$ Treat	22.590** (10.351)	0.183*** (0.061)	-0.004** (0.002)	-0.008 (0.010)
N	2,121,406	1,647,988	2,121,406	1,647,988
Mean of DV	3302.54	39.53	.07	.09

Standard errors in parentheses

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

Notes: Estimates obtained from [Equation 3](#) with state and year-of-birth fixed effects. Regressions include state-specific linear pre-trends estimated using pre-1973 data to control for differential state trends prior to embargo. All specifications are weighted by birth counts with standard errors clustered at the state level.

Source: National Vital Statistics System (NVSS), 1968-1979.

Table 3: Effect of Oil Embargo on Infant Health Outcome (Subsample LHS)

	(1) Birthweight	(2) Gestation Age	(3) LBW	(4) Preterm
Post $\times$ Treat	33.738*** (10.373)	0.219*** (0.068)	-0.010*** (0.003)	-0.012 (0.017)
N	28,483	27,336	28,483	27,336
Mean of DV	3211.07	39.3	.1	.13
Standard errors in parentheses				
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$				

Notes: Estimates obtained from Equation 3 with state and year-of-birth fixed effects. Regressions include state-specific linear pre-trends estimated using pre-1973 data to control for differential state trends prior to embargo. All specifications are weighted by birth counts with standard errors clustered at the state level.

Source: National Vital Statistics System (NVSS), 1968-1979.

Table 4: Effect of Oil Embargo on Infant Health Outcome (Subsample Black Mothers)

	(1) Birthweight	(2) Gestation Age	(3) LBW	(4) Preterm
Post $\times$ Treat	28.184*** (10.151)	0.554* (0.308)	0.001 (0.004)	-0.050 (0.040)
N	21,927	17,113	21,927	17,113
Mean of DV	3083.34	38.63	.13	.17
Standard errors in parentheses				
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$				

Notes: Estimates obtained from Equation 3 with state and year-of-birth fixed effects. Regressions include state-specific linear pre-trends estimated using pre-1973 data to control for differential state trends prior to embargo. All specifications are weighted by birth counts with standard errors clustered at the state level.

Source: National Vital Statistics System (NVSS), 1968-1979.

Table 5: Effect of Oil Embargo on Prenatal Care

	(1) PNC	(2) First Trimester
Post $\times$ Treat	0.017*** (0.006)	0.004 (0.035)
N	1,819,184	1,819,184
Mean of PNC	.99	
Mean of First Trim		.72
Standard errors in parentheses		
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$		

Notes: Estimates in column (1) are obtained from [Equation 1](#) and in column (2) are obtained from [Equation 3](#) with state and year-of-birth fixed effects. Column (2) regression includes state-specific linear pre-trends estimated using pre-1973 data to control for differential state trends prior to embargo. All specifications are weighted by birth counts with standard errors clustered at the state level.

Source: National Vital Statistics System (NVSS) (1968-1979)

Table 6: Effect of Oil Embargo on Maternal Composition

	(1) Mother's Age	(2) Parity	(3) % Teen Mom	(4) % Black
Post $\times$ Treat	0.306** (0.133)	0.229*** (0.032)	-0.045*** (0.009)	-0.024 (0.018)
N	35,965	35,965	35,965	35,965
Mean of DV	24.47	2.09	.18	.15
Standard errors in parentheses				
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$				

Notes: Estimates obtained from [Equation 3](#) with state and year-of-birth fixed effects. Regressions include state-specific linear pre-trends estimated using pre-1973 data to control for differential state trends prior to embargo. All specifications are weighted by birth counts with standard errors clustered at the state level.

Source: National Vital Statistics System (NVSS), 1968-1979.

Table 7: Effect of Oil Embargo on Log CO Concentration

	(1) Log CO Concentration
Post $\times$ Treat	-0.474* (0.243)
N	1,952
Mean of DV	7.47
Standard errors in parentheses	
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$	

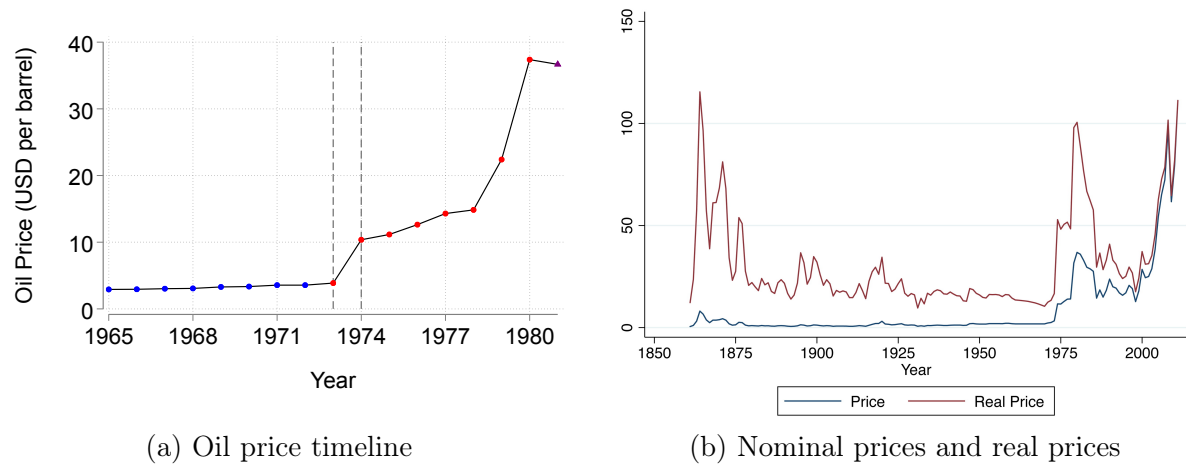
Notes: Estimates obtained from [Equation 3](#) with state and year fixed effects. Regressions include state-specific linear pre-trends estimated using pre-1973 data to control for differential state trends prior to embargo. All specifications are weighted by 1972 state population with standard errors clustered at the state level.

Source: EPA (1968-1979)

APPENDICES

A Figures

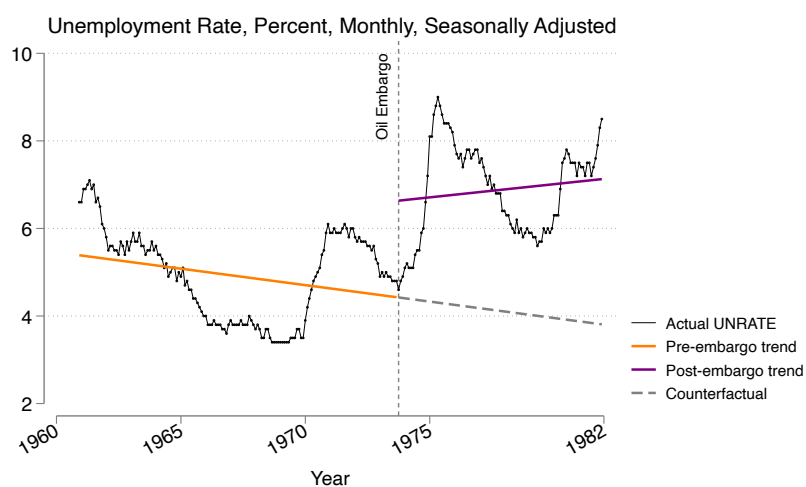
Figure A.1: Annual Oil Prices, 1965-1981



Notes: Figure A.1a shows average annual oil prices calculated from the monthly spot prices in the West Texas Intermediate series. This graph uses the data from the (Acemoglu et al., 2013). The graph shows that oil prices remained stable until the 1973 oil embargo, after which they began to rise sharply.

This exact Figure A.1b is taken from (Knittel, 2014). Both nominal prices (actual dollar amounts) and real prices (inflation-adjusted) experienced dramatic spikes during the 1973 oil crisis.

Figure A.2: Unemployment Rate, Percent, Seasonally Adjusted

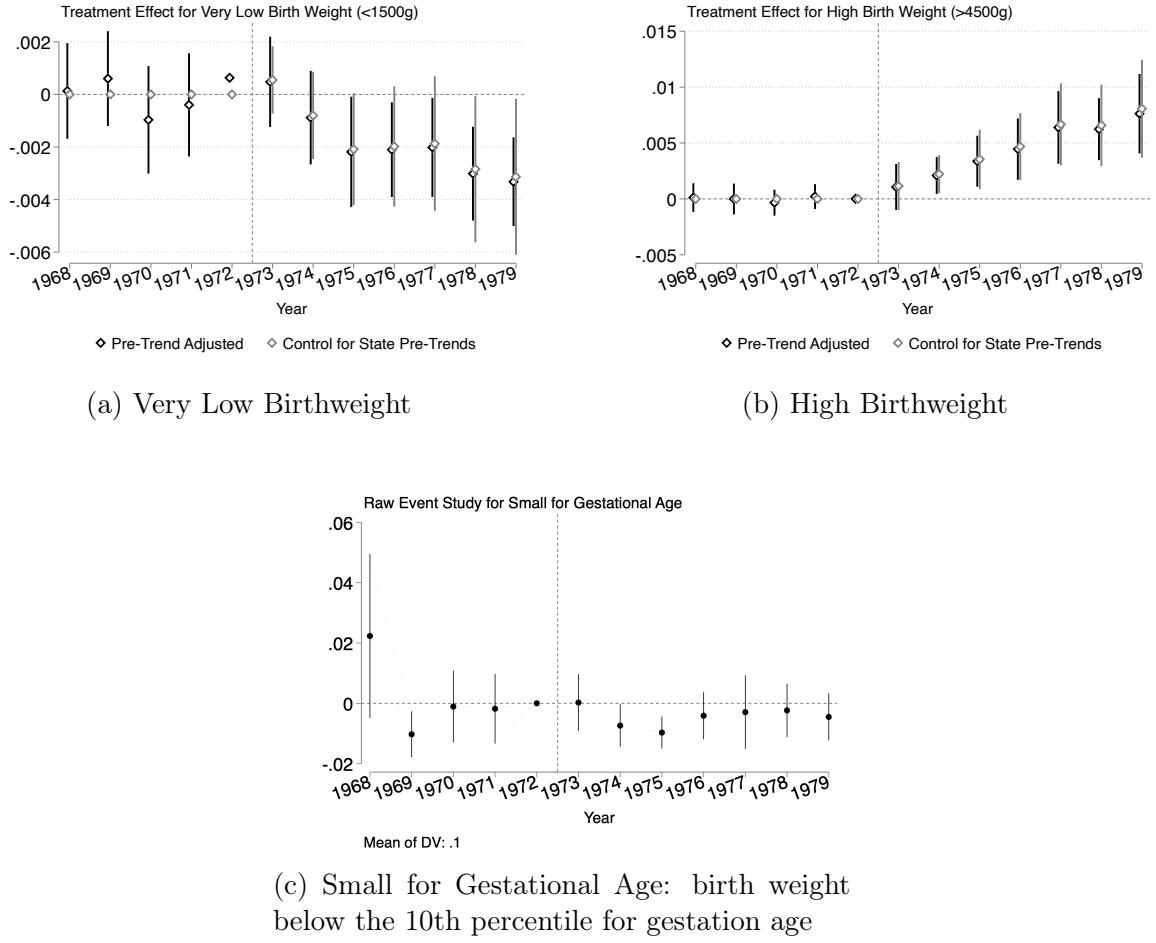


Source: U.S. Bureau of Labor Statistics, Unemployment Rate [UNRATE], retrieved from FRED, Federal Reserve Bank of St. Louis; <https://fred.stlouisfed.org/series/UNRATE>, September 6, 2025.

Notes: The unemployment rate is calculated as the share of individuals without a job compared to the total labor force. The labor force includes only people aged 16 and above who live in one of the 50 U.S. states or the District of Columbia. It excludes those living in institutions such as prisons, mental health facilities, or nursing homes, as well as individuals serving on active duty in the military.

Source: FRED

Figure A.3: Secondary Birth outcomes



Notes: The black points in [Figure A.3a](#) and [Figure A.3b](#) are produced using detrended [Equation 1](#) version mentioned in [Appendix C.1](#) and trace the evolution of treatment effects over time, relative to pre-period treatment trend. Grey points are generated using [Equation 2](#). [Figure A.3c](#) is produced using [Equation 1](#) and trace the evolution of treatment effects over time relative to baseline reference year 1972. The treatment variable measures 1972 motor fuel consumption per capita by state in log form, exploiting continuous geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year-cell.

Source: National Vital Statistics System (NVSS) (1968-1979)

Analysis of small for gestational age (SGA) outcomes offers additional validation of the positive birthweight effects documented in my primary findings.⁹ SGA defined as birth weight below the 10th percentile for gestational age (e.g., ([Kose et al., 2024](#)); ([Karadavut et al., 2025](#))). My raw event study estimates in [Figure A.3c](#) demonstrate consistent negative coefficients during the post-embargo years, indicating meaningful decreases in SGA incidence within oil-dependent states after 1973. With approximately 10% baseline SGA

⁹This outcome was not part of the main analysis because, although it is an important outcome to consider, gestational age shows strong performance as a risk assessment tool while SGA classification reveals surprisingly weak capabilities for detecting high-risk babies ([Wilcox et al., 2021](#)). Additionally, the standard for defining SGA at birth and how the threshold is decided remains under discussion ([Goldenberg and Cliver, 1997](#)).

prevalence, these coefficient magnitudes represent substantively important improvements in fetal development outcomes that reinforce my birthweight findings. The observed SGA reductions confirm that embargo-related changes produced genuine improvements in fetal development.

Table A.1: Effect of Oil Embargo on Infant Health Outcome (Secondary Outcomes)

	(1) VLBW	(2) HBW	(3) SGA
Post $\times$ Treat	-0.001 (0.001)	0.003** (0.001)	0.004 (0.007)
N	2,121,406	2,121,406	1,647,988
Mean of DV	.01	.02	.1

Standard errors in parentheses

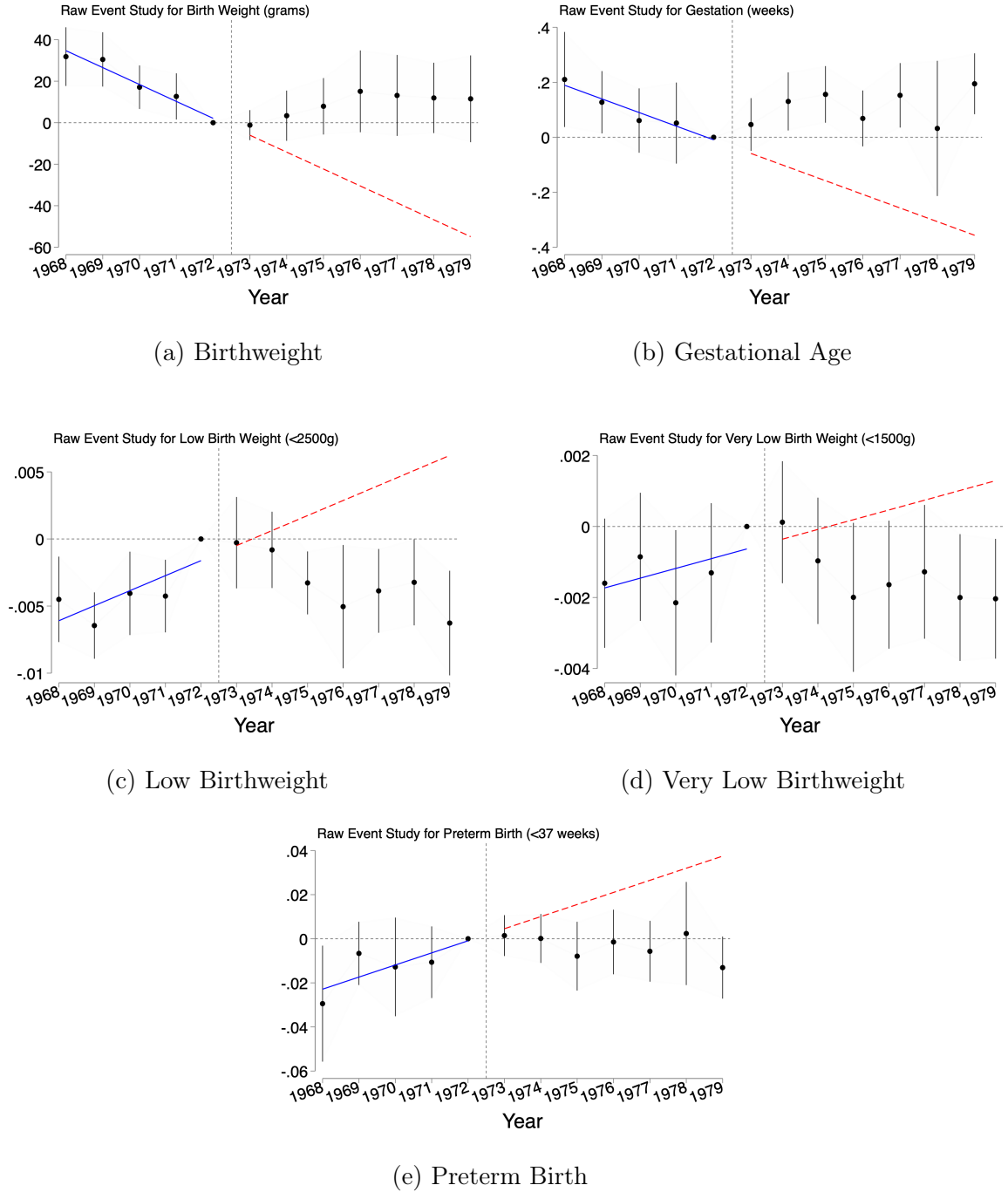
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates obtained from [Equation 3](#) with state and year-of-birth fixed effects. Regressions include state-specific linear pre-trends estimated using pre-1973 data to control for differential state trends prior to embargo. All specifications are weighted by birth counts with standard errors clustered at the state level.

Source: National Vital Statistics System (NVSS), 1968-1979.

B Raw Event Studies

Figure B.4: Raw Event Study Estimates



Notes: These figures are produced using [Equation 1](#) and trace the evolution of treatment effects over time relative to baseline reference year 1972. The treatment variable measures 1972 motor fuel consumption per capita by state in log form, exploiting continuous geographic variation in oil dependence. The specification includes state and birth year fixed effects. Uncapped vertical bars show 95% confidence intervals. Standard errors are clustered at the state level, and all regressions are weighted by the number of births in each county-year-cell. The dashed line after 1973 represents the counterfactual.

Source: National Vital Statistics System (NVSS) (1968-1979)

C Adjusted Event Studies Process

The event study estimates derived from [Equation 1](#) reveal substantial violations of the parallel trends assumption fundamental to causal identification in difference-in-differences designs. Specifically, oil-dependent states exhibit systematically different pre-embargo trajectories in birth outcomes relative to less oil-dependent states, undermining the credibility of standard event study inference. To address these violations while preserving causal interpretation, I implement an adjustment procedure that directly removes pre-existing linear trends from the outcome variables.

This detrending approach relaxes the stringent parallel trends requirement and substitutes a more plausible identifying assumption of maintaining relative trends. The standard approach assumes that all states would have experienced identical changes in birth outcomes if the oil embargo had never happened. This is often unrealistic because different states naturally have different health trends for many reasons unrelated to oil dependence.

My alternative approach is more flexible and realistic. It allows oil-dependent and non-oil-dependent states to have completely different baseline trends in birth outcomes. For instance, some oil-dependent states might have been experiencing declining birth outcomes even before the embargo, while non-oil-dependent states might have been seeing improvements. This is perfectly acceptable under my approach.

The key requirement is that the gap between these different state trends should remain stable over time if the oil embargo had not occurred. To illustrate: imagine that before the embargo, oil-dependent states were losing 2 grams of average birthweight each year, while non-oil-dependent states were gaining 1 gram each year. This creates a 3-gram annual gap between the two groups. My approach assumes this 3-gram gap would have continued unchanged if the embargo had never happened. Any change in this gap after the embargo can then be attributed to the oil shock's actual impact.

This method substantially improves our ability to identify the true causal effects of the oil embargo by removing the influence of pre-existing differences in state health trends that could otherwise make it appear as though the embargo had effects when it actually did not, or vice versa.

C.1 Detrending Methodology

The detrending process follows a systematic four-step approach to remove pre-existing linear trends from the event study estimates:

1. **Main Event Study Regression:** I first estimate the event study specification

from Equation 1:

$$Y_{cst} = \gamma_s + \gamma_t + \sum_{\tau=1968, \tau \neq 1972}^{1980} \beta_\tau [Treat_s \times \mathbb{1}\{t = \tau\}] + \epsilon_{cst}$$

where Y_{cst} represents birth outcomes in county c , state s , year t , $Treat_s$ measures state-level oil dependence (log motor fuel consumption per capita in 1972), $\mathbb{1}\{t = \tau\}$ is an indicator for year τ , γ_s and γ_t are state and year fixed effects, and β_τ captures the treatment effect in year τ relative to the reference year 1972.

2. **Pre-trend Estimation:** I extract the pre-embargo coefficients $\{\beta_\tau\}_{\tau=1968}^{1971}$ and estimate the linear pre-trend relationship:

$$\beta_\tau = \alpha + \delta \times \tau + u_\tau \quad \text{for } \tau \in \{1968, 1969, 1970, 1971\} \quad (1)$$

where α represents the intercept, δ captures the linear trend slope, and u_τ is the error term.

3. **Trend Projection:** Using the estimated parameters $\hat{\alpha}$ and $\hat{\delta}$ from the pre-period regression, I project the counterfactual trend across all sample years:

$$\hat{\beta}_\tau^{\text{trend}} = \hat{\alpha} + \hat{\delta} \times \tau \quad \text{for } \tau \in \{1968, 1969, \dots, 1980\} \quad (2)$$

This projected trend represents what the event study coefficients would have been if the pre-embargo linear relationship had continued unchanged throughout the sample period.

4. **Detrending Adjustment:** The final detrended coefficients are obtained by subtracting the projected trend from the original estimates:

$$\beta_\tau^{\text{detrended}} = \beta_\tau - \hat{\beta}_\tau^{\text{trend}} \quad (3)$$

The confidence intervals are similarly adjusted to maintain the appropriate statistical inference:

$$[\text{CI}_\tau^{\text{lower, detrended}}, \text{CI}_\tau^{\text{upper, detrended}}] = [\text{CI}_\tau^{\text{lower}} - \hat{\beta}_\tau^{\text{trend}}, \text{CI}_\tau^{\text{upper}} - \hat{\beta}_\tau^{\text{trend}}] \quad (4)$$

This procedure effectively removes the state-specific pre-embargo linear trend from each event study coefficient, isolating deviations from the historical trajectory that can be more credibly attributed to the oil embargo. The resulting detrended estimates provide valid causal inference under the more realistic assumption that relative trends between oil-dependent and non-oil-dependent states would have remained constant in the absence

of the treatment.

C.2 Bootstrap Procedure for Correct Confidence Intervals

Potential problem: The confidence interval adjustment in Equation 4 treats the detrending correction $\hat{\beta}_\tau^{\text{trend}}$ as deterministic, ignoring uncertainty in predicting the counterfactual. Specifically, the projection $\hat{\beta}_\tau^{\text{trend}} = \hat{\alpha} + \hat{\delta} \times \tau$ extrapolates from only four pre-embargo years (1968-1971) to predict what would have happened in later periods (1973-1980) absent the oil shock. This counterfactual prediction is inherently uncertain because: (1) the pre-trend parameters $\hat{\alpha}$ and $\hat{\delta}$ are estimates rather than known values, and (2) extrapolating any trend involves increasing uncertainty the further one projects from the estimation period. The original approach ignores this projection uncertainty entirely, leading to confidence intervals that may be substantially too narrow.

Potential solution: I implement a stratified random sampling bootstrap procedure that repeats the entire 4-step detrending process mentioned in Appendix C.1 1000 times with different subsamples to obtain empirical confidence intervals that properly account for uncertainty.

C.2.1 Bootstrap Sampling and Regression

For each bootstrap replication $b = 1, 2, \dots, B$ where $B = 1000$:

1. **Stratified Subsampling:** Within each state s , I randomly sample 50% of observations without replacement:

$$\mathcal{S}_s^{(b)} \sim \text{Random sample of 50\% from state } s \quad (5)$$

$$\mathcal{D}^{(b)} = \bigcup_{s=1}^{48} \mathcal{S}_s^{(b)} \quad (6)$$

where $\mathcal{S}_s^{(b)}$ is the 50% subsample from state s in bootstrap replication b , and $\mathcal{D}^{(b)}$ is the complete bootstrap dataset for replication b formed by combining all 48 state subsamples.

2. **Main Event Study Regression:** Using bootstrap sample $\mathcal{D}^{(b)}$, I estimate:

$$Y_{cst}^{(b)} = \gamma_s^{(b)} + \gamma_t^{(b)} + \sum_{\tau=1968, \tau \neq 1972}^{1980} \beta_\tau^{(b)} [Treat_s \times \mathbb{1}\{t = \tau\}] + \epsilon_{cst}^{(b)} \quad (7)$$

where $Y_{cst}^{(b)}$ represents birth outcomes in county c , state s , year t for bootstrap sample b , $Treat_s$ is state-level oil dependence (log motor fuel consumption per capita in 1972), $\mathbb{1}\{t = \tau\}$ is an indicator for year τ , $\gamma_s^{(b)}$ and $\gamma_t^{(b)}$ are state and year fixed

effects for bootstrap sample b , $\beta_\tau^{(b)}$ captures the treatment effect coefficients for bootstrap sample b , and $\beta_{1972}^{(b)} = 0$ (reference year).

3. **Pre-trend Estimation:** I extract pre-embargo coefficients $\{\beta_\tau^{(b)}\}_{\tau=1968}^{1971}$ and estimate:

$$\beta_\tau^{(b)} = \alpha^{(b)} + \delta^{(b)} \times \tau + u_\tau^{(b)} \quad \text{for } \tau \in \{1968, 1969, 1970, 1971\} \quad (8)$$

where $\alpha^{(b)}$ is the intercept of the pre-trend for bootstrap sample b , $\delta^{(b)}$ is the slope of the pre-trend for bootstrap sample b , and $u_\tau^{(b)}$ is the error term for bootstrap sample b .

4. **Trend Projection:** Using the bootstrap-specific estimates $\hat{\alpha}^{(b)}$ and $\hat{\delta}^{(b)}$:

$$\hat{\beta}_\tau^{\text{trend},(b)} = \hat{\alpha}^{(b)} + \hat{\delta}^{(b)} \times \tau \quad \text{for } \tau \in \{1968, 1969, \dots, 1980\} \quad (9)$$

where $\hat{\beta}_\tau^{\text{trend},(b)}$ represents the projected counterfactual trend for year τ in bootstrap sample b .

5. **Detrending Adjustment:** The bootstrap-specific detrended coefficients are:

$$\beta_\tau^{\text{detrended},(b)} = \beta_\tau^{(b)} - \hat{\beta}_\tau^{\text{trend},(b)} \quad (10)$$

where $\beta_\tau^{\text{detrended},(b)}$ represents the detrended treatment effect for year τ in bootstrap sample b , relative to the pre-treatment trend.

C.2.2 Empirical Confidence Intervals

After completing all $B = 1000$ bootstrap replications, for each year τ , I obtain the bootstrap distribution:

$$\Theta_\tau = \{\beta_\tau^{\text{detrended},(1)}, \beta_\tau^{\text{detrended},(2)}, \dots, \beta_\tau^{\text{detrended},(B)}\} \quad (11)$$

where Θ_τ is the set of all detrended treatment effects for year τ across all bootstrap replications.

The bootstrap mean estimate is:

$$\bar{\beta}_\tau^{\text{detrended}} = \frac{1}{B} \sum_{b=1}^B \beta_\tau^{\text{detrended},(b)} \quad (12)$$

where $\bar{\beta}_\tau^{\text{detrended}}$ is the average detrended treatment effect across all bootstrap samples.

The empirical 95% confidence interval is constructed using percentiles:

$$\text{CI}_\tau^{0.95} = [Q_\tau^{0.025}, Q_\tau^{0.975}] \quad (13)$$

where Q_τ^p is the p -th percentile of the bootstrap distribution for year τ .

C.2.3 Final Result

The bootstrap procedure yields, for each year $\tau \in \{1968, 1969, \dots, 1980\}$:

$$(\bar{\beta}_\tau^{\text{detrended}}, Q_\tau^{0.025}, Q_\tau^{0.975}) \quad (14)$$

These provide bootstrap-corrected point estimates and confidence intervals that properly propagate uncertainty through the entire 4-step detrending procedure, addressing the uncertainty.

C.3 Detrending the Outcome Variable

C.3.1 Baseline Specification

My baseline empirical specification is:

$$Y_{cst} = \gamma_s + \gamma_t + \sum_{\tau=1968, \tau \neq 1972}^{1980} \beta_\tau [\text{Treat}_s \times \mathbb{1}\{t = \tau\}] + \epsilon_{cst} \quad (15)$$

where Y_{cst} represents birth outcomes in county c , state s , year t , Treat_s measures state-level oil dependence (log motor fuel consumption per capita in 1972), $\mathbb{1}\{t = \tau\}$ is an indicator for year τ , and γ_s and γ_t are state and year fixed effects, respectively. The coefficients β_τ capture the differential effect of oil embargo on birth outcomes in year τ relative to the reference year 1972.

C.3.2 Addressing Pre-Treatment Trends

A key identifying assumption in difference-in-differences estimation is that treatment and control units would have followed parallel trends in the absence of treatment. To address potential violations of this assumption stemming from state-specific pre-trends, I implement a similar detrending procedure suggested by (Goodman-Bacon, 2021).

1. Pre-Trend Estimation

I estimate state-specific pre-trends using only pre-treatment observations ($t \leq 1971$):

$$\bar{Y}_{st} = \alpha_s + \delta_s \cdot t + \varepsilon_{st} \quad \text{for } t \leq 1971 \quad (16)$$

where $\bar{Y}_{st}$ represents an average outcome for state s in year t , α_s is a state-specific intercept, δ_s captures the state-specific pre-trend, and ε_{st} is an error term.

2. Pre-Trend Prediction and Extrapolation

Using the estimated coefficients, I predict the pre-trend across ALL years (extrapolating beyond the pre-treatment period):

$$\widehat{\text{pretrend}}_{st} = \hat{\alpha}_s + \hat{\delta}_s \cdot t \quad \text{for all } t \in [1968, 1980] \quad (17)$$

where $\hat{\alpha}_s$ and $\hat{\delta}_s$ are the estimated coefficients from the pre-treatment regression.

3. Outcome Detrending

I construct the detrended outcome variable by subtracting the predicted pre-trend from the original outcome:

$$\tilde{Y}_{st} = \bar{Y}_{st} - \widehat{\text{pretrend}}_{st} \quad (18)$$

where $\tilde{Y}_{st}$ represents the detrended outcome.

4. Main Analysis

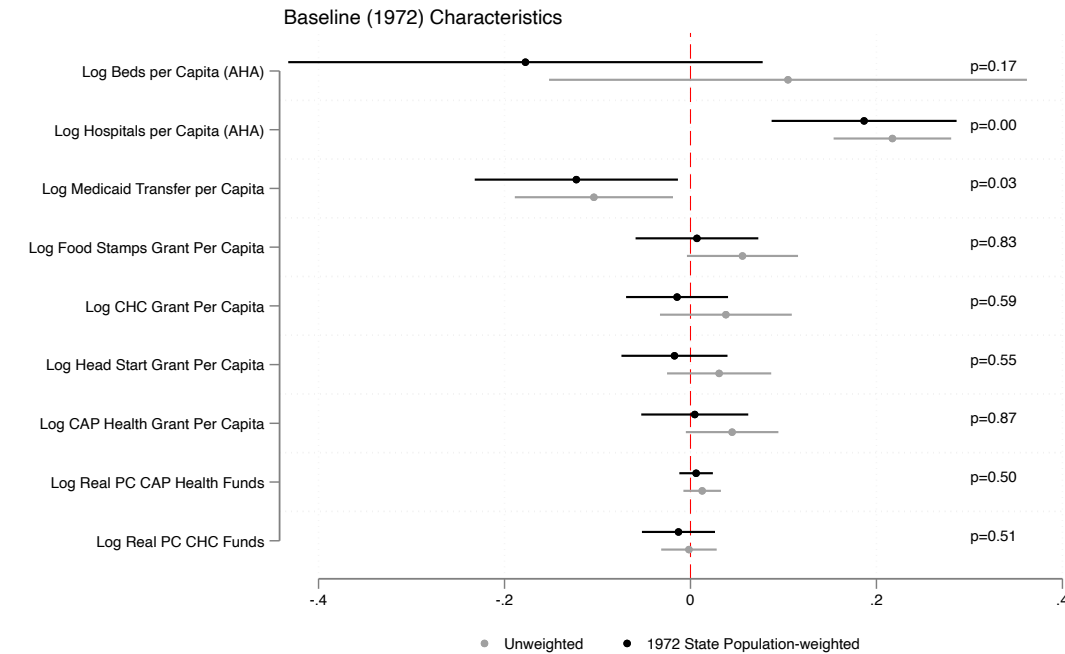
I then estimate my main specification using the detrended outcome:

$$\tilde{Y}_{st} = \gamma_s + \gamma_t + \sum_{\tau=1968, \tau \neq 1972}^{1980} \beta_{\tau} [\text{Treat}_s \times \mathbb{1}\{t = \tau\}] + u_{st} \quad (19)$$

This approach is particularly valuable when treatment and control units exhibit different pre-treatment trajectories. By removing these systematic differences, the detrended specification provides more credible identification of the causal effects of oil shocks on birth outcomes.

D State level Characteristics

Figure D.1: Baseline Cross Sectional State Characteristics Balance Checks

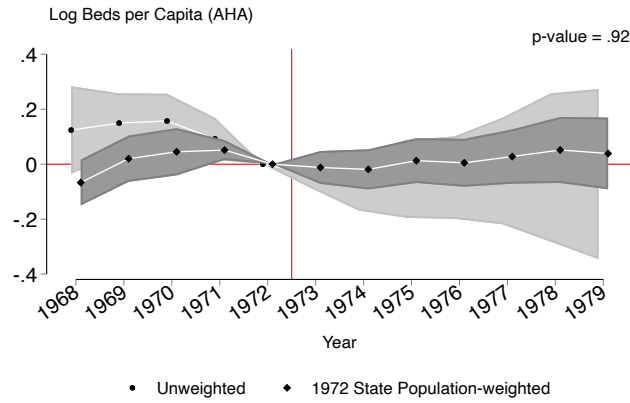


Standard errors clustered at state. Dependent variable is log of motor fuel consumption per capita. Unit of analysis is state. Baseline variables come from Goodman-Bacon (2018); Almond et al., (2011); Bailey (2012); Kose et al., (2024)

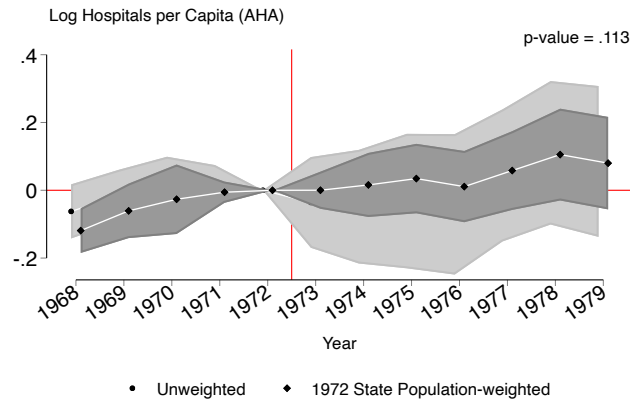
Notes: This figure reports estimates from an OLS regression of log per capita motor fuel consumption in 1972 at state level on baseline 1972 state level characteristics. Regressions are weighted by the 1972 population. Standard errors are clustered at the state level.

Source: Baseline variables come from (Goodman-Bacon, 2018; Almond et al., 2011; Bailey, 2012; Kose et al., 2024)

Figure D.2: Time varying state level characteristics–Health System Capacity



(a) Log Bed Per Capita

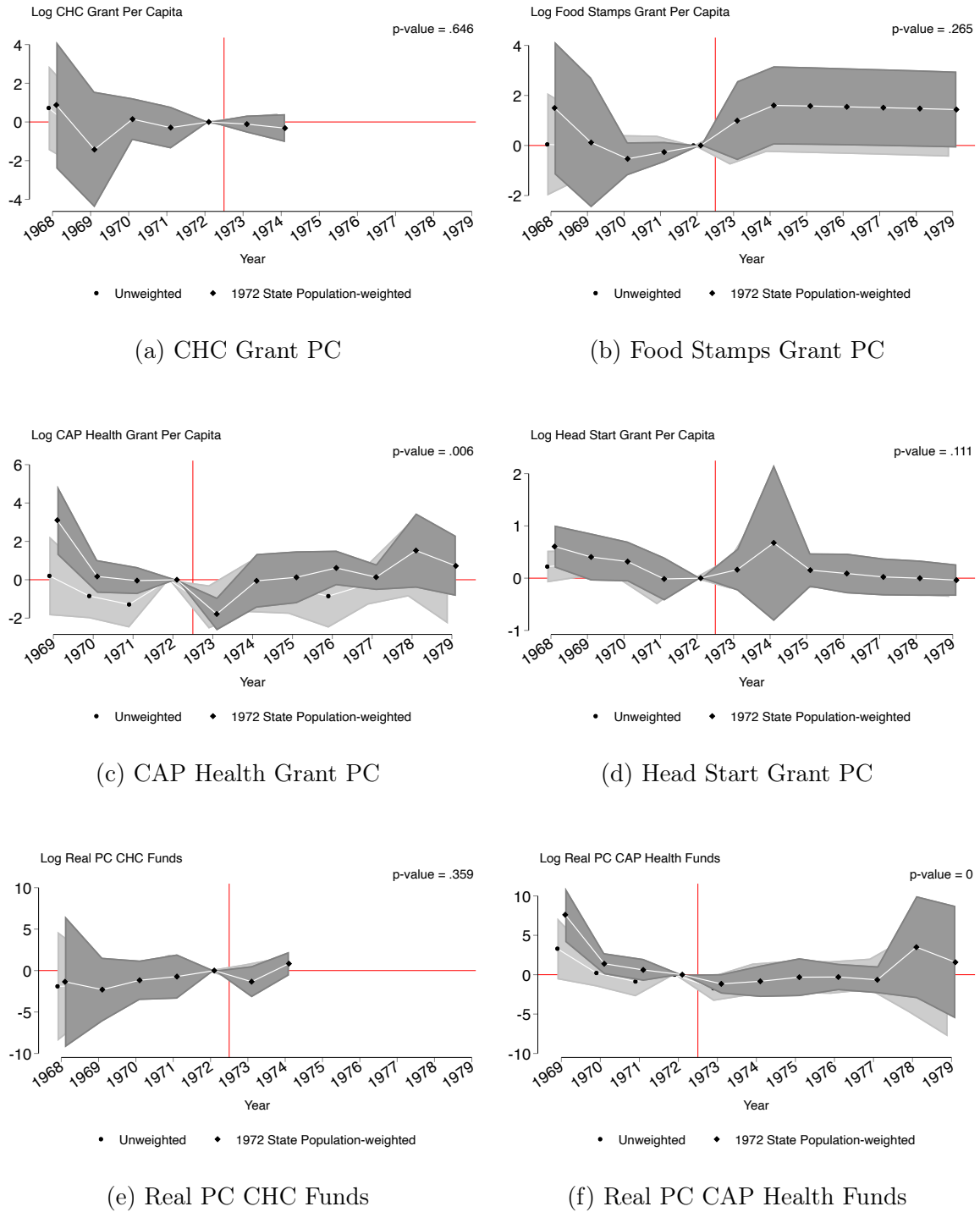


(b) Log Hospital Per Capita

Notes: These figures present results from a regression of time varying state level characteristics on the interaction between log per capita motor fuel consumption in 1972 and year dummies (with 1972 as the omitted category). Unit of analysis is state-year. Regression Specification includes state FE and year FE. Regressions are weighted by the 1972 population. Standard errors are clustered at the state level. Each outcome is in log form. “PC” meaning per capita.

Source: Outcome variables come from (Goodman-Bacon, 2018; Almond et al., 2011; Bailey, 2012; Kose et al., 2024)

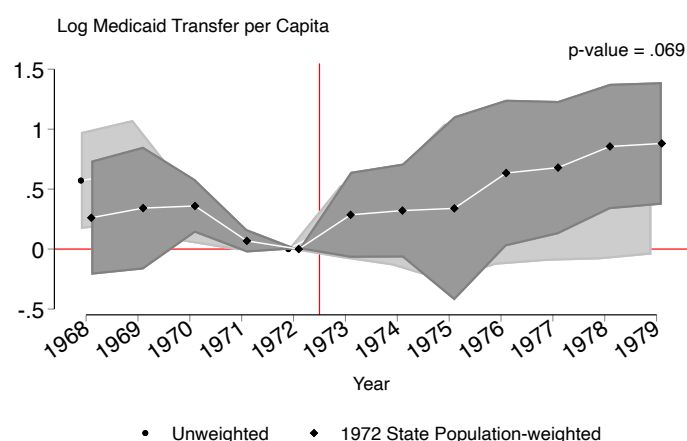
Figure D.3: Time varying state level characteristics – Grants and Funds



Notes: These figures present results from a regression of time varying state level characteristics on the interaction between log per capita motor fuel consumption in 1972 and year dummies (with 1972 as the omitted category). Unit of analysis is state-year. Regression Specification includes state FE and year FE. Regressions are weighted by the 1972 population. Standard errors are clustered at the state level. Each outcome is in log form. “PC” meaning per capita.

Source: Outcome variables come from (Goodman-Bacon, 2018; Almond et al., 2011; Bailey, 2012; Kose et al., 2024)

Figure D.4: Log Medicaid Transfer Per Capita



Notes: This figure presents result from a regression of the Log Medicaid Transfer Per Capita on the interaction between log per capita motor fuel consumption in 1972 and year dummies (with 1972 as the omitted category). Unit of analysis is state-year. Regression Specification includes state FE and year FE. Regressions are weighted by the 1972 population. Standard errors are clustered at the state level. “PC” meaning per capita.

Source: Outcome variables come from (Goodman-Bacon, 2018; Almond et al., 2011; Bailey, 2012; Kose et al., 2024)