Opioid Supply, Trade Liberalization Shocks, and Infant Health Outcomes

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This paper examines the interaction of factors affecting opioid supply and opioid demand and their impact on neonatal health outcomes. I exploit variation in the triplicate status of states in 1996 and a county-level trade liberalization shock as natural variations for opioid supply and demand, respectively. The results indicate that the counties that experienced high opioid demand and supply had an increase in the incidence of preterm births and births with low birth weight. Specifically, relative to the baseline mean, the preterm birth rate increased by 3.8%, and the rate of births with low birth weight increased by 5% in those counties experiencing high opioid supply and increased economic distress. I also find that trade liberalization shocks independently led to a significant decrease in the incidence of preterm births and births with low birth weight suggesting that there might be positive maternal selection into fertility. In contrast, the triplicate status of states, by itself, does not significantly explain infant health outcomes. My findings indicate that the triplicate laws had a protective effect on infant health in counties experiencing higher economic distress and therefore, higher opioid demand.

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1 Introduction

The opioid epidemic has impacted all aspects of adult well-being. Since 1999, more than 500,000 individuals have died from opioid overdoses with recent data indicating that 44 people die every day from prescription opioid misuse (Hedegaard et al., 2021). Recent findings suggest that it imposes substantial burdens on children right from birth, primarily due to the rise in maternal opioid use disorders (OUDs). In 2020, almost six out of every 1,000 live births resulted in a diagnosis of neonatal opioid dependency (Hirai et al., 2021). Furthermore, maternal OUD has also been linked to several poor outcomes at birth, including being born preterm and having low birth weight (Pac et al., 2023; Brogly, 2019).

Broadly, the opioid crisis is driven by factors affecting its supply and demand. On the supply side, various factors, such as the prescribing practices of healthcare providers, the presence of "pill mills",[1] and the proliferation of synthetic opioids, have resulted in increased opioid-related mortality and morbidity (Chang et al., 2016; Schnell, 2017). Concurrently, on the demand side, individual circumstances or person-based factors, including unemployment, financial distress, and mental health challenges, have also been identified as drivers of risky drug use, in particular opioid use (Finkelstein et al., 2022; Charles et al., 2019; Dasgupta et al., 2018; Hollingsworth et al., 2017). Both demand and supply side factors that cause opioid consumption to increase have dire consequences for infant health outcomes.

It is interesting that over the same period of exploding opioid use, the country's employment-to-population ratio decreased from 64.3 percent to 60.4 percent with some of the largest declines in those areas hit hardest by the opioid crisis (Abraham and Kearney, 2020). Yet because of how endogenous opioid supply and individual economic and personal circumstances are, we know very little about how opioid demand and supply factors interact to affect infant health. Are the effects of supply and demand forces linear and additive, or does their interaction produce a more pronounced effect in comparison to their independent effects? By exploiting two natural variations in structural

[1] Pill mills are medical establishments, typically a doctor or pain management clinics or pharmacies, that engage in unscrupulous and inappropriate prescribing of controlled substances (Rigg et al., 2010).

unemployment originating from trade liberalization shocks and opioid supply, I investigate how changes in the demand and supply of opioids interact and affect infant health outcomes.

I use two sources of variation. Recent evidence from court cases against Purdue Pharma, the manufacturer of Oxycontin, has uncovered evidence to support that the manufacturer systematically marketed opioids in the initial years to states that did not have triplicate laws. Triplicate laws mandated the reporting of prescriptions for Schedule II drugs to the state drug agency, serving as a mechanism to control opioid over-prescribing in states where such laws were in place. As a consequence, triplicate states experienced a much lower level of opioid prescriptions and opioid-related mortality in the years following the introduction of Oxycontin compared to non-triplicate states (Alpert et al., 2022).

Around the same time, the US experienced a steady decline in employment, especially in the manufacturing sector largely attributable to increased import competition from China. A notable development was the Permanent Normal Trade Relations with China in 2000, which increased import competition differently across counties in the US. In turn, these counties experienced varying levels of manufacturing employment decline after 2000 according to the intensity of trade shock they experienced (Pierce and Schott, 2016). What these two events present is a scenario in which counties that experienced high trade shocks that raised the demand for opioids had different supply of opioids because of their triplicate law status in 1996. As long as these two events are unrelated and independent of each other, the interaction between the two shocks provides an estimate of the interaction effect between opioid demand and supply factors on infant health outcomes.

My main data source for the analysis is the restricted-use Natality files from 1996-2018. The data include information on the universe of all births in the US along with information about mothers, such as their county and state of residence as well as county and state of occurrence of birth, which helps me identify their exposure to trade shocks as well as opioid supply. I also obtain demographic information of mothers including their age at the time of childbirth and self-reported race and ethnicity. For my main outcome variables, I use information on the infant's gestation period (in weeks), the birth weight of the infant, and various details concerning the mother's pregnancy-related behaviors including whether the mother had at least 5 prenatal visits and whether they reported using tobacco during pregnancy. I construct four main outcome variables: the number of preterm births (<37 weeks) and extremely preterm births (<32 weeks), and the number of births with low birthweight (<2500 grams) and very low birthweight (<1500 grams) in each county and in each year per 100,000 newborns.

I find that counties with high opioid demand and supply experienced an increase of 442.5 preterm births and 391.4 births with low birth weight per 100,000 newborns. Relative to the baseline mean, these estimates translate into a 3.8 and 5 percent increase in the rate of preterm births and low birth weight births respectively. These estimates are statistically significant at the 1 percent level. Additionally, I find that these areas also experienced an increase in the number of extremely preterm births, births with very low births, maternal tobacco use incidence, and a decrease in the number of births who reported at least 5 prenatal visits. The increases in preterm births and births with low birth weight are exclusively driven by White children, with almost zero effect on Black or Hispanic children.

Interestingly, I find that trade shock independently leads to an improvement in infant health outcomes. Relative to the baseline mean, counties experiencing high trade shocks in 2000 saw a decrease in the number of children being born prematurely and with low birth weight by 3.39 percent and 2.65 percent respectively. While the evidence on trade shocks and infant health is scant in the United States, this finding strongly indicates a strong positive relationship between trade shocks and infant health, attributed to the choice of mothers to give birth during times of high unemployment and economic downturns. In addition, I also note that the triplicate status of the states, by itself, seems to have no significant impact on infant health outcomes. These findings indicate that the interaction between independent conditions that increase opioid supply and opioid demand is harmful to infant's health. Put another way, the results suggest that historical policies limiting the availability of opioids may have had a positive impact on infant health outcomes.

I contribute to the existing literature in several ways. First, I study how both the opioid supply and trade shocks affect infant health independently. Previous work has largely focused on the effect of opioid policies (e.g. Prescription Drug Monitoring Programs, Naloxone Access Laws, etc) on infant health and neonatal outcomes. For example, Gihleb et al. (2020) finds that the introduction of operational Prescription Drug Monitoring Programs reduced neonatal abstinence syndrome by 10 percent with no impact on overall infant mortality and infant health outcomes. Ziedan and Kaestner (2020) observes modest improvement in infant health outcomes because of state policies that control prescription opioid supply. Within this space, there is very little evidence on how the opioid crisis and independent factors affecting opioid supply have affected infant health outcomes. In this paper, I use a natural variation to specifically test if initial differences in opioid supply have a long-term impact on infant health outcomes. I find that initial supply differences in Oxycontin, by itself, have no impact on infant health outcomes. However, they mitigate poor infant health outcomes in counties with high economic distress. I also find that these counties also experienced a higher rate of mothers reporting tobacco use as well as a lower rate of prenatal visits, both of which are outcomes correlated with higher opioid use. These findings suggest that while the overall impact of opioid supply policies might be modest, there might be significant heterogeneity in their efficacy depending on external individual and economic circumstances.

Similarly, while previous work in economics has explored the role of trade shocks on adult mortality and morbidity (Pierce and Schott, 2020), there is very little work on the role of trade shocks on infant mortality or infant health outcomes. To the extent that trade shocks result in long-term adult job displacement, heightened opioid consumption, and diminished family resources, they could negatively impact the health of newborns. Concurrently, trade shocks result in periods of elevated unemployment, and such periods have been associated with adverse infant health outcomes due to the selective decision of mothers to conceive during these times. Therefore, it is uncertain how trade shocks affect infant health. I observe that trade shocks decrease the incidence of poor infant health suggesting that there might be selective fertility in those counties experiencing high trade shocks.

Second, apart from their independent effects, I offer a novel contribution by studying the interaction effect between factors affecting opioid demand and supply. While from the outset it is intuitive to assume that both trade shocks as well as the opioid crisis are detrimental to infant health, we know little about the extent to which these shocks might be interacting with each other. The thought experiment of two events having an interaction effect on intergenerational outcomes is related to the literature that examines the joint impact of more than one intervention or event on children's outcomes (Almond et al., 2018). For example, Aguilar and Vicarelli (2022) studies the joint effect of a positive and negative shock in Mexico (a conditional cash transfer program and rainfall shocks) on children's cognitive outcomes. Gunnsteinsson et al. (2022) studies the opposing effect of a vitamin A supplementation program and a tornado during pregnancy and infancy on infant birth weight and other child health outcomes. While these were examples of studies that interact a positive and a negative shock, Rossin-Slater and Wüst (2020) investigates how two coinciding positive events, including a nursing home visit program and a child care program, influence long-run educational outcomes.

In a similar vein, this paper provides insight into how circumstances that increase parental opioid demand and opioid supply interact with each other and affect infant health outcomes. This is also closely related to the recent attempt at separating ”place-specific factors” and ”person-specific factors” in exacerbating the opioid epidemic Finkelstein et al. (2022). Place-specific factors can be defined as those variables that contribute to an increase in opioid supply. For instance, these include physician propensity to over-prescribe, the presence of pill mills, and state-specific prescription opioid monitoring programs. By contrast, person-specific factors are individual circumstances that drive opioid demand and can include personal characteristics including mental health, financial distress, and prior substance use behaviors. I contribute to this literature by separately and jointly studying factors influencing ”place-specific” and ”person-specific” drivers of opioid use in affecting infant health outcomes which have been increasingly affected by the opioid crisis.

2 Background

2.1 The Opioid Crisis and Triplicate Laws

The opioid crisis is responsible for bringing about an era of increased adult mortality and morbidity, successively fueling other drug crises within the country. Previous literature across disciplines has found that the opioid epidemic significantly and adversely affects the development of human capital in both adults and children. Specifically, this crisis has led to a range of detrimental outcomes for adults, encompassing deteriorating physical and mental well-being, reduced employment opportunities and earnings, and a higher incidence of criminal activities. Concurrently, the crisis exerts an indirect yet substantial influence on children's outcomes, giving rise to negative consequences such as decreasing infant health outcomes, increasing occurrences of home displacements and placements into foster care, and an uptick in incidents of child maltreatment (See Maclean et al. (2020) for a review of the effect of the opioid crisis on adult as well as children's economic outcomes).

Recent evidence has uncovered the role of the triplicate prescription laws in mediating the intensity of the crisis since 1996. Multiple copy prescription programs (MCPPs) or more commonly called triplicate prescription regulations required physicians to send copies of prescriptions to law enforcement and government health agencies (Fishman et al., 2004). New York was the first state to introduce this regulation in 1918 followed soon by California and Hawaii as a means to control the ongoing opium and cocaine drug crises. Several states introduced similar triplicate regulations in the 1970s and 1980s with a number of them discontinuing their program in the 1990s. The primary goal of these programs was to monitor the prescribing of Schedule II medications with research showing that the states with these triplicate regulations saw, on average, a 50% decline in Schedule II prescribing after the initiation of the program (Fishman et al., 2004).

Alpert et al. (2022) finds that the presence of these regulations in 5 states namely New York, Texas, California, Idaho, and Illinois severely limited the marketing of Oxycontin in 1996 by Purdue Pharma. Purdue Pharma introduced their new "blockbuster" drug, Oxycontin, in January of

1996 as a replacement for their existing MS-Contin. The aggressive marketing and promotion of the new drug exponentially increased its sale from $48 million in 1996 to $1.1 billion in 2000 Van Zee (2009). Alpert et al. (2022) observes that Purude decided not to target states with triplicate regulations in their aggressive marketing campaigns because of the additional prescription supervision in these states. They find that this consequently had a long-run impact on opioid and opioid-related mortality within these states even 20 years after the introduction of Oxycontin.

Further research using this variation in opioid supply has also found significant impacts on living arrangements of children as well as labor market flows. In particular, Buckles et al. (2023) find that children born in non-triplicate states were more likely to be living away from their parents and in households headed by their grandparents. Mukherjee et al. (2023) finds that individuals were more likely and quicker to transition from unemployment to employment in triplicate states. To sum up, what this variation provides is an natural variation in the supply of prescription Oxycontin across states in the country that resulted in a significant difference in the intensity of the opioid crisis in these states.

2.2 Unemployment, Labor Market Shocks, and Drug Use

Since 1999, the country has been experiencing a structural change in its labor force participation rates fueled by a gradual decline of the manufacturing sector in the country. A number of studies have linked how unemployment spells might be correlated with an increase in drug use, especially opioid use. Mukherjee et al. (2023) finds a positive association between being unemployed and reported misuse of drugs. Hollingsworth et al. (2017) finds that increases in county unemployment rates increase opioid death rates as well as instances of opioid overdoses. Further, Charles et al. (2019) observes that local decline in manufacturing unemployment was associated with an increase in prescription opioid use and opioid overdoses. This is further substantiated by Dean and Kimmel (2019) who finds that trade-related jobs in a county were associated with a substantial increase in opioid-related deaths. In summary, unemployment shocks or trade shocks has the potential to increase opioid demand within the impacted locality.

A number of papers have also examined the impact of declining opioid supply on local labor market conditions. For instance, Beheshti (2022) studies the impact of hydrocodone rescheduling, which decreased the supply of the drug, on labor market outcomes. They find positive improvements in labor market conditions including unemployment rates as well as a decrease in social security participation. Similarly, Harris et al. (2020) finds that increases in instrumented opioid prescriptions decrease labor force participation.

The main instrument I use for opioid demand is the county-level trade shocks from the introduction of the Permanent Normal Trade Relations Agreement with China in 2000. The PNTR agreement substantially reduced import tariffs on Chinese imports after 2000. Pierce and Schott (2016) finds that the counties facing import competition also experienced a decline in its manufacturing employment after 2000 which may have increased demand for drugs. This is further substantiated by the fact that Pierce and Schott (2020) also finds that the counties most affected by this trade shock also saw a significant increase in adult mortality after 2000. Taken together, I have a variation at the state level that exogenously modifies opioid supply and another variation at the county level that affects opioid demand. This paper delves into the interaction between the demand and supply forces of opioids on infant health outcomes.

Concurrently, a set of literature also explores the independent impact of unemployment on infant health. The general consensus is that the relationship between unemployment and infant health is procyclical. In other words, gestational unemployment rates are negatively associated with poorer health outcomes while babies conceived during periods of high unemployment are healthier (Dettling and Kearney, 2023). This is due to the positive selection of healthier mothers who choose to conceive and give birth during periods of high unemployment. Similarly, others have also examined the impact of local labor demand shocks on child health to find that gender-specific labor demand shocks cause a negative impact on parent-reported child health outcomes (Page et al., 2019). It is interesting that the opioid crisis intersected with a period of lowering labor force participation in the country, both of which could have had an impact on infant health. Another contribution of my study is an attempt at separating the impact of trade shocks and the opioid crisis on infant health. Using the two variations I explore here, I am able to separately identify the estimate of trade shocks devoid of the opioid crisis on infant health outcomes.

2.3 Opioid Crisis and Infant Health Outcomes

Since 1999, the number of reported case of maternal opioid use disorders increased threefold (Haight et al., 2018). It is well established that maternal opioid use is associated with poor infant health outcomes. There is a strong relationship between maternal opioid use disorders (MOUD) and the incidence of neonatal opioid withdrawal syndrome (NOWS) in turn increasing the risk of infant mortality (Grossarth et al., 2023). Moreover, MOUDs are also associated with an increased probability of infants born with low birth weight as well as infants born prematurely (Krans et al., 2021; Pac et al., 2022). Within this background, there is relatively little work that examines the causal impact of the opioid crisis on infant outcomes.

A small set of papers has examined the impact of opioid policies on infant health. Ziedan and Kaestner (2020) and Gihleb et al. (2020) study the impact of opioid policies, in particular Prescription Drug Monitoring Programs, on infant health to find modest improvements in infant health with the introduction of these programs. Furthermore, a number of papers have also studied the impact of state-level prenatal substance use policies, including punitive policies that criminalize substance use during pregnancy, on infant health outcomes (Atkins and Durrance, 2020; Meinhofer et al., 2022). More recent work has looked into the impact of the opioid epidemic, by itself, on infant health outcomes. Arteaga and Barone (2022) studies the role of the opioid crisis in affecting infant health outcomes by proxying the intensity of the crisis to the proportion of cancer patients within a commuting zone in 1996. They find that the crisis fueled poorer infant health outcomes while observing an increase in the number of non-marital childbirths in commuting zones with higher opioid supply.

3 Data

The primary data source for this study comes from the restricted-use National Vital Statistics System (NVSS) Natality files provided by the National Center for Health Statistics. I combine data for the years 1991-2018 which includes the universe of all births in the United States for these years. For the main outcome variables in my analysis, I make use of information on the number of weeks of gestation as well as the birth weight (in grams) of each recorded birth. In my study, a birth is deemed premature if the number of gestational weeks is less than 37 weeks and extremely premature if the number of gestational weeks is less than 32 weeks (Quinn et al., 2016). Similarly, I define a birth being low birth weight if they weigh less than 2,500 grams and as very low birth weight if they weigh less than 1,500 grams (Cutland et al., 2017).

I also obtain a number of demographic information from the data including the mother's race and ethnicity, the mother's age at the time of the birth, state and county of occurrence of the birth, state and county of residence of the mother as well as birth year and month. Since my empirical variation assumes that the county and state of birth are the same as their residence, I restrict my sample of births to only those where the county (and state) of occurrence is the same as the county (and state) of residence. For other outcome variables, I also use information on the number of prenatal visits made by the mother as well as self-reported tobacco use by the mother during pregnancy.

From the universe of all births between 1991 and 2018, I calculate the number of total births, preterm births, extremely preterm births, low birth weight births, and very low birth weight births for each state, county, year, mother's age group (18-34 and 35-49), and mother's race/ethnicity groups (White non-Hispanic, Black non-Hispnanic, and Hispanic). For my main outcome variables, I divide the total number of preterm births, extremely preterm births, low birth weight births, and very low birth weight births by the total births for each group and multiply them by 100,000. For secondary outcome variables, I also calculate the number of births that were associated with at least 5 prenatal visits and the number of births where the mother reported using tobacco during pregnancy.

For the next part of my analysis, I calculate a measure of trade shock intensity used previously by Pierce and Schott (2020). With the passing of the Permanent Normal Trade Relations Agreement with China in 2000, import tariff rates for Chinese imports decreased significantly. Pierce and Schott (2020, 2016) defines $NTRGap_j$ as the difference between tariff rates for each 4-digit SIC industry with and without Normal Trade Relations:

$$NTRGap_j = NonNTRRate_j - NTRRate_j$$

I obtain industry-wise NTR and non-NTR Tariff data from Feenstra et al. (2002); Pierce and Schott (2016) to construct the $NTRGap_j$.[2] A larger trade gap estimate is associated with a higher exposure to trade liberalization for the industry. The industry-level NTR Gap estimate is further condensed to a county-level measure using the following definition:

$$NTRGap_c = \sum_j \frac{L_{jc,1990}}{L_{c,1990}} NTRGap_j$$

Here, $\frac{L_{jc,1990}}{L_{c,1990}}$ is the fraction of the population employed in industry 'j' over total employment in county 'c' in 1990. I obtain data for employment in each industry and in each county from the County Business Patterns Data 1990.[3] I link natality data to county-level NTR Gap measures using county identifiers. In Figure 1, I map the distribution of NTR Gap across counties in the US. The figure demonstrates that the trade shock was mostly present in the northern and eastern states with the intensity lower in the central and western states. Apart from this measure of trade shock, I also link the natality data with county-wise unemployment rates obtained from the Bureau of Labor Statistics Local Area Unemployment Statistics (BLS LAU).

I also include controls for other opioid and drug policies including indicators for mandatory Prescription Drug Monitoring Programs, Naloxone Access Laws, medical marijuana laws, and good samaritan laws. These data are obtained from the RAND OPTIC-Vetted Policy datasets.[4] I further control for county and year-level demographics including the percent of the population that is white non-Hispanic, black non-Hispanic, Hispanic, aged 18-34, aged 34-49, aged ¿ 50, male, and

[2]The data is accessible from https://faculty.som.yale.edu/peterschott/international-trade-data/. Detailed data notes are available here: https://www.nber.org/system/files/working˙papers/w9387.

[3]The CBP data can be downloaded from https://www.census.gov/data/datasets/1990/econ/cbp/1990-cpb.html.

[4]Data can be accessed from https://www.rand.org/health-care/centers/optic/resources/datasets.html

female. These data are obtained from the National Cancer Institute Surveillance, Epidemiology, and End Results Program County Population data files.

Table 1 presents the summary statistics for my sample of births. 10.74% and 11.7% of births were born prematurely in triplicate and non-triplicate states respectively. The national rate for preterm births in 2000 was noted to be around 11.6% (Moore, 2002). Similarly, 7.32% and 7.92% of births were born with low birth weight in triplicate and non-triplicate states respectively. These estimates are also consistent with the national averages for the country (Martin et al., 2002). Around 11.64% of the sample in non-triplicate states had mothers who reported using tobacco during pregnancy while only 4.2% of the sample in triplicate states reported using tobacco during pregnancy. Finally, 95% of the sample in both triplicate and non-triplicate states reported having at least 5 prenatal visits during pregnancy. Average unemployment rates were higher in triplicate states than in non-triplicate states. At the same time, triplicate counties experienced lower NTR gaps than non-triplicate counties.

4 Research Design

My analysis relies on the differential exposure of states to high opioid supply and of counties to high trade shocks causing them to experience varied levels of opioid demand. Five states experienced lower levels of opioid supply due to their triplicate status at the time of the introduction of Oxycontin in 1996. These are New York, Illinois, California, Texas, and Idaho. There is also within-state variation across counties in their intensity of exposure to the trade shock in 2000. Given these two levels of variation in opioid supply as well as opioid demand, my baseline regression estimation takes the form:

$$\begin{aligned} y_{csgt} = \beta_0 + \beta_1 Hightrade_c \cdot Non-Triplicate_s \cdot Post2000_t + \beta_2 Hightrade_c \\ \cdot Post2000_t + \beta_3 Non-Triplicate_s \cdot Post2000_t + \gamma_c + \delta_t + \eta_g + X_{st} + \epsilon_{csgt} \end{aligned} \tag{1}$$

Here, y_{cst} is the number of low preterm births, extremely preterm births, births with low birth weight, and births with very low birth weight per 100,000 births in a county c, state s, mother's race and age group g, and year t. $Hightrade_c$ is equal to 1 if the county had an above-median value of NTR Gap after 2000 and 0 otherwise. In an alternate estimation, I check the robustness of my estimates by using the continuous measure of NTR Gap. $Non-Triplicate_s$ is equal to 1 for all non-triplicate states and 0 otherwise. The main coefficient of interest is β_2 which measures the differential effect of being in a county experiencing high trade liberalization shock in a non-triplicate state after the PNTR trade shock in 2000. I account for all double-interaction terms including β_2 and β_3 which measure the impact of being in a non-triplicate state after 2000 as well as the effect of being in a county with high trade liberalization shock after 2000.

The equation also includes county fixed effects indicated by γ_c as well as year fixed effects denoted by δ_t. County fixed effects control for county-level time-invariant characteristics that might be correlated with infant health outcomes. Similarly time (or year) fixed effects account for year-wise changes in infant health outcomes that might be specific to each birth cohort. η_g controls for the mother's age and race group. All analyses are weighted using year-county-group level female population. Finally, ϵ_{cst} is the error term that is robust and is clustered at the state level for all analyses to account for state-level correlation of errors across counties. Note that the research design I employ here does resemble a triple differences-in-difference strategy. However, I do not include the interaction term $Triplicate_s \cdot Hightrade_c$ since it is subsumed under county fixed effects.

There are four assumptions that I rely on to interpret β_1 as the causal effect of trade liberalization shocks and triplicate laws on infant health outcomes. First, opioid supply was higher in triplicate states and there were no external unobservable changes to opioid supply in 2000 that coincided with the PNTR agreement in 2000 that might have caused opioid supply to be higher in triplicate states compared to non-triplicate states. To check this, I collect year-wise Automation of Reports and Consolidated Order Systems (ARCOS) data on Oxycodone distribution in each state per 100,000 population for the years 1997 to 2005. Oxycodone was the primary component of Oxycontin, the painkiller manufactured by Purdue Pharma. Figure 2 plots the average Oxycodone consumption per 100,000 population across triplicate and non-triplicate states between 1999 and 2008. I observe that opioid supply was consistently higher in non-triplicate states compared to triplicate states even after 10 years of the introduction of Oxycontin. I also do not observe any discernible changes in opioid supply across triplicate and non-triplicate states that coincided with the trade liberalization in 2000. This evidence is further backed by Alpert et al. (2022) who finds that Oxycontin supply was consistently higher in non-triplicate states compared to triplicate states using Medicaid and Medical Expenditure Panel Survey data.

Second, I assume that trade liberalization shocks of equal magnitude increased opioid demand equally across triplicate and non-triplicate states. However, they were met with different supply conditions in triplicate and non-triplicate states. Note that I am unable to observe individual opioid demand. However, using zip-code-level ACROS data on Oxycodone distribution from 2000-2005, I compare Oxycodone distribution across triplicate and non-triplicate states across the range of the trade shock measure. Figure 3 graphs the evolution of population-weighted county-level total oxycodone sales (in grams weight per 100,000 population) across high trade shock and low trade shock counties in triplicate and non-triplicate states from 1999 to 2008. I observe two things of interest here. First, as noted before, mean Oxycodone sales for triplicate states are lower than non-triplicate states across high trade shock and low trade shock counties. Second, high trade shock counties in non-triplicate states experienced higher sales of Oxycodone after 2000 compared to counties experiencing lower trade shock in non-triplicate states. This relationship is consistent across all years after 2000. .

Third, and more importantly, I require there to be sufficient variation in trade shock intensity across triplicate and non-triplicate states. As noted earlier, the mean NTR Gap was lower in triplicate states than in non-triplicate states (see Table 1). I further plot a kernel density plot of the NTR Gap measure for triplicate and non-triplicate states (see Figure 4). Non-triplicate states have a larger range than triplicate states, even though more than 95 percent of the distribution coincides with each other. To ensure that these outliers with high NTR gap values in non-triplicate states are not influencing my estimates I check the robustness of my main estimates by excluding counties that have an NTR Gap value outside the common support (>18).

Last, another threat to my identification is if high trade shock and low trade shock counties across triplicate and non-triplicate states were systematically different with respect to time trends in infant health outcomes. I test this using an event study estimation where I compare the infant health outcomes between high trade shock and low trade shock counties across triplicate and non-triplicate states. I find that infant health outcomes between high and low trade shock counties were trending similarly before 2000 across high trade shock and low trade shock counties. However, after 2000, there was a significant increase in the incidence of preterm births and births with low birth weight in non-triplicate states (See Figures 5 and 6). In contrast, there was a slightly positive change in infant health outcomes in high trade shock counties after 2000.

5 Results

5.1 Baseline Results

I first plot the relationship between trade liberalization shock and infant health outcomes for triplicate and non-triplicate states in Figures (7) and (8). The figures depict a binned scatter plot of county-level mean NTR Trade Gap in the x-axis against the mean number of preterm births (and extremely preterm births) and births with low birth weight (and very low birth weight) per 100,000 newborns after 2000. I observe a steep positive relationship between the intensity of trade liberalization shocks and poor infant health across the 4 outcomes in non-triplicate states. I interpret this as providing suggestive evidence that higher opioid demand is associated with poorer infant health outcomes in non-triplicate states, where opioid supply is high. In contrast, triplicate states seem to have a negative relationship between high trade shock intensity and poor infant health outcomes.

The diverging trend in infant health outcomes by triplicate and non-triplicate states is further supported by Table 2. Here, I discuss the estimates from estimating Equation (1) where I interact treatment indicators for high opioid supply and high opioid demand. I find that being born in an area with high opioid demand and high opioid supply is associated with poor infant health outcomes. In particular, I find that in these counties, the number of preterm births increased by 442.5 and the number of births of births with low birth weight increased by 391.4 for 100,000 newborns. The estimates are significant at the 1% level. Relative to the baseline mean, these estimates approximately translate to a 4% and 5% increase in preterm births and low birth weight births. I find similar effects for extreme infant health outcomes including being born very premature and being born with very low birth weight. High opioid supply and demand areas saw an increase of extremely premature births by 219.5 per 100,000 total births and an increase of 81.9 births with very low birth weights per 100,000 total births (statistically insignificant at the 10% level).

In comparison, I find that trade shocks, by itself, substantially improve infant health outcomes. In particular, relative to the baseline mean, high trade shock areas are associated with 3.39% and 2.65% decrease in the rate of preterm births and births with low birthweight after 2000. The evidence is scant on the impact of trade shocks and infant health in the United States. However, these results are in line with Dettling and Kearney (2023) and De Cao et al. (2022) who find that there is positive maternal selection in times of high unemployment and financial stress. Therefore, babies born during periods of high unemployment and economic distress have better infant health outcomes. I also find that triplicate laws independently have a low and insignificant effect on infant health outcomes. To summarise these results, triplicate laws were protective of infant health in areas of high opioid demand and trade shocks were detrimental to infant health only in areas with high opioid supply.

I next check the effect of this interaction effect by varying time periods. I divide the post-period 2001-2018 into two time periods 2001-2007 and 2008-2018. The rationale behind this division is that the country underwent a major financial crisis from 2008-2009 which might have essentially transformed the relationship between the trade shocks, opioid demand, and infant health within counties in the country. Table 3 presents estimates from dividing the post-period into two periods. While the estimates are slightly larger in the first period 2001-2007, I observe a consistent relationship between high opioid supply and demand and poor infant health outcomes across the two time periods. These estimates are consistent with Alpert et al. (2022) who find a significant impact of triplicate laws on opioid mortality even 20 years into the introduction of Oxycontin. The results suggest that the triplicate laws were protective of infant health even in the long run.

Next, I study the interaction effects by race of the child. Table 4 presents estimates from the estimation of Equation (1) by White, Black, and Hispanic children for the number of preterm births and low birth weight births per 100,000 newborns. Overall, the rate of black infants with poor infant health outcomes is much higher than White and Hispanic children. I find that the interaction between high opioid supply and high opioid demand counties significantly affects White children for both outcomes. In particular, I find that the rate of preterm births went up by 381 births and the rate of births with low birth weight by 296 births for White children. The results are significant at the 5 percent level.

6 Additional Outcomes

6.1 Effect on Prenatal Visits and Tobacco Use during Pregnancy

In the next part, I explore potential reasons for observing poor infant health outcomes in counties with high opioid demand and supply. An obvious cause is that these areas experienced higher maternal opioid abuse. While I cannot observe mothers opioid use directly, I use two indicators of prenatal risky health behaviors that are suggestive of higher opioid abuse by mothers. In particular, I study mother's prenatal care behavior and tobacco use during pregnancy. The Centers for Disease Control (CDC) recommends at least 7 prenatal visits to ensure a healthy pregnancy. Prenatal care involving less than 5 visits is considered extremely risky, potentially increasing the risks of having a preterm birth or a low birth weight birth. Prenatal care is also important for screening potential maternal opioid use and protecting infants against opioid dependence and other opioid-related adverse health outcomes (Brogly et al., 2018). Qualitative evidence suggests that substance use problems were a key reason behind women not seeking proper prenatal care (Friedman et al., 2009).

Similarly, I use another indicator of risky maternal behavior: whether mothers report using tobacco during pregnancy. Smoking during pregnancy is associated with poor infant health outcomes including premature births and low birth weight births (Stone et al., 2014; Ion and Bernal, 2015). Moreover, many studies have indicated the high correlation between tobacco use and opioid use (Rajabi et al., 2019) with more than 88 percent of pregnant women enrolled in medication-assisted treatment for opioids also reporting smoking cigarettes (Akerman et al., 2015). To study these behaviors, I use information from the Natality data that records whether a particular birth was associated with at least 5 prenatal visits by the mother and whether the mother reported tobacco use during pregnancy. While around 95 percent of the births in triplicate and non-triplicate states report having at least 5 prenatal visits, a stark difference exists between reported tobacco use in triplicate and non-triplicate states. In particular, 4.2% of mothers report using tobacco in triplicate states, and 11.64% of mothers report using tobacco in non-triplicate states. I use the same estimating equation as Equation (1) to study whether these behaviors are systematically different in areas with high opioid demand and supply.

Table 5 presents estimates for these outcomes. I find that there is a significant interaction effect in areas experiencing high opioid demand and supply. In particular, these areas saw an increase in the incidence of mothers reporting tobacco use during pregnancy and a decrease in the incidence of mothers who had at least 5 prenatal visits. Relative to the mean, the estimates translate into a 5% increase (statistically significant at the 1 percent level) and a 0.29% decrease (statistically significant at the 1 percent level) in these outcomes respectively. Insofar as these variables are indicators of risky opioid use during pregnancy, I find suggestive evidence that there was an increase in risky maternal opioid use in areas characterized by high opioid demand and supply.

7 Robustness Checks

I check the robustness of my estimates by using an alternate control group for triplicate states. As stated before, state-level triplicate drug prescribing laws were initiated much earlier than the opioid crisis, starting with New York in 1918 and California in 1939 to combat the then-ongoing opium and cocaine drug crisis. A number of other states followed suit including Hawaii, Idaho, Illinois, Rhode Island, Michigan, Texas, and Pennsylvania. However in 1996, at the time of the introduction of Oxycontin, there were only 5 with an active triplicate program with other states either discontinuing the program or beginning their transition to an electronic prescription monitoring program. Insofar as these early initiating states were similar in their policy background, they could serve as an optimal control group for triplicate states in my study.

Using Hawaii, Rhode Island, Michigan, and Pennsylvania as an alternate control group, I re-estimate equation (1). Results are presented in Table 6. I find that the interaction effect between high opioid supply and demand again leads to worse infant health outcomes. The effect sizes are stronger than my main estimates. In particular, non-triplicate counties with high trade shocks experience 621.5 more preterm births and 459.5 more births with low birth weight per 100,000 newborns. I also observe stronger results for extreme outcomes including being born less than 32 weeks and being born with very low birth weight. Trade shocks, by itself, again seem to have a positive and significant effect on infant health outcomes. The larger interaction effect might be because of a higher drug abuse potential in these states, which could have been exacerbated with the introduction of Oxycontin.

I next check if the main estimates are robust to using the continuous NTR Gap measure instead of a binary treatment variable. I present results in Table 7 where the main interaction is defined as $Post_{2000} * Non-Triplicatestate_s s * NTRGap_c$. The interaction term is again positive and significant indicating that high opioid demand and supply conditions lead to poorer infant health outcomes.

Last, I re-estimate equation (1) excluding counties in non-triplicate states that had NTR Gap values outside the common support. Table 8 presents the results from this estimation. I again find consistent estimates for the interaction effects indicating that counties with high NTR Gap values might not be driving my primary findings.

8 Conclusion

In this paper, I explore the interaction effect of factors affecting opioid supply and opioid demand on infant health outcomes including being born preterm and with low birth weight. I use natural variations arising from county-level variation in exposure to trade shocks and differences in opioid supply derived from the triplicate status of states in 1996. Using National Vital Statistics - Natality data, I find that the interaction between high opioid demand and supply leads to an increase in the number of preterm infants and the number of infants with low birthweight per 100,000 infants. I also observe that trade shock, by itself, leads to positive improvements in infant health.

The results indicate that the triplicate laws might be protective of infant health in areas that are hit by the trade shocks and therefore susceptible to increased opioid abuse. Further, I find that these areas also experienced lower maternal risky behaviors with much lower incidences of maternal tobacco use and higher rates of appropriate prenatal care. Finally, I also find that high unemployment rates are short-lived in high trade shock areas with lower opioid supply indicating the lower persistence of financial distress in these areas that might be contributing to lower opioid abuse.

There are several implications of this paper for policy. First, the results suggest that further examination of infant health may not be done in isolation. There might be various factors at play in an individual's, in this case, mother's, environment that might affect their infant's health outcomes. This is especially important as public policies to combat the ongoing opioid crisis are usually enacted at the state level. Additional local-level efforts might be necessary to provide services to improve health outcomes of mothers and infants including provision of rehabilitation services and substance abuse treatment centers.

My findings also suggest that supply-side policies play a huge role in negating factors that lead to an increase in demand for opioids. However, the recent decade has been characterised by the presence of illicit and synthetic opioids including fentanyl which is highly potent and has greater abuse potential than traditional opioids. Consequently, policies addressing prescription opioid misuse might not be effective in the presence of these synthetic drugs. Hence, there might be a need to re-evaluate supply-side policies that could effectively reduce the supply of synthetic opioids.

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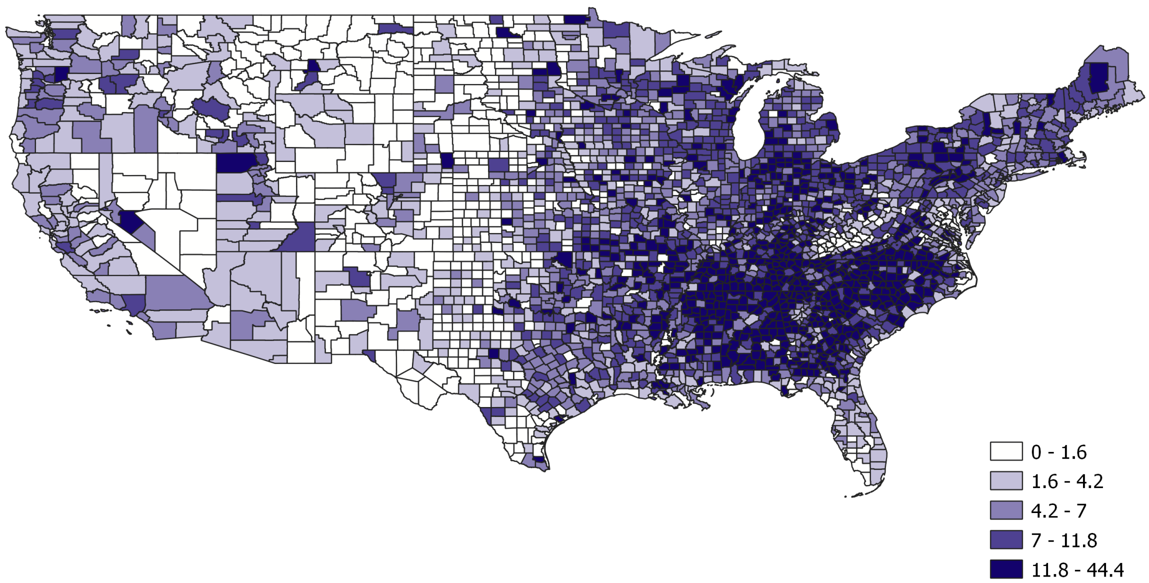
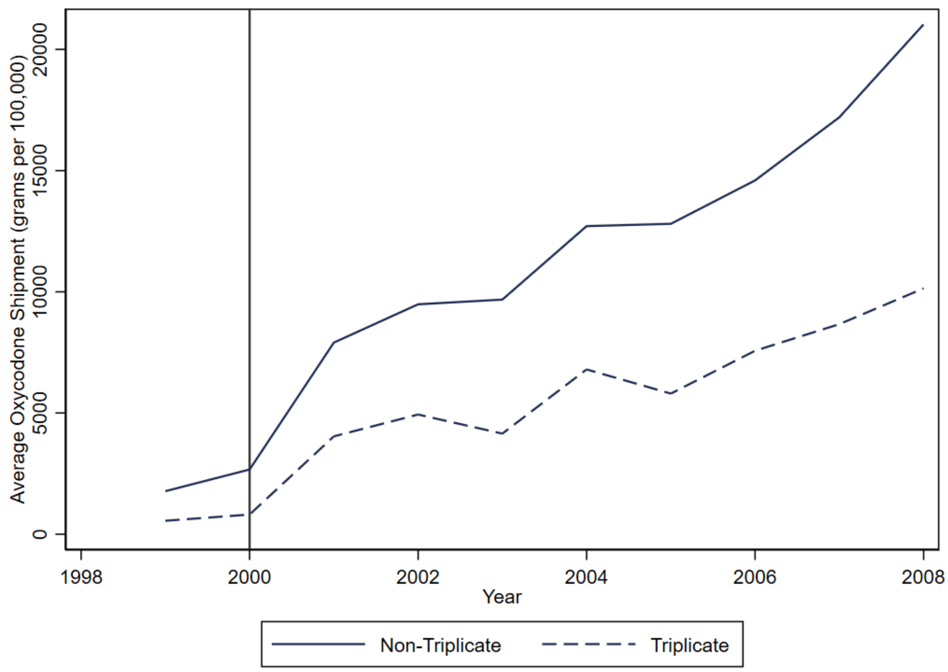
Figure 1: Distribution of NTR Gap across Counties in the US



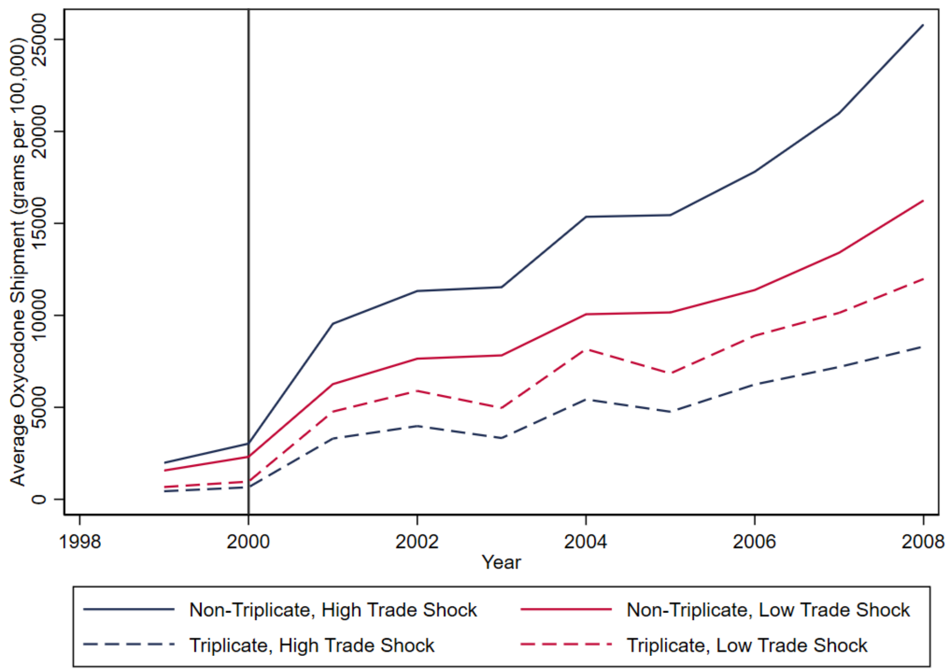
Notes: The figure maps the distribution of NTR Gap across counties in the US. Values presented in the legend indicate unweighted NTR Gaps at the county level with darker shades indicating higher trade liberalization in the county. Data for constructing the NTR Gap are obtained from Pierce and Schott (2016); Feenstra et al. (2002) and County Business Patterns, 2000.

Figure 2: Average Oxycodone distribution in grams per 100,000 across Triplicate and Non-Triplicate States



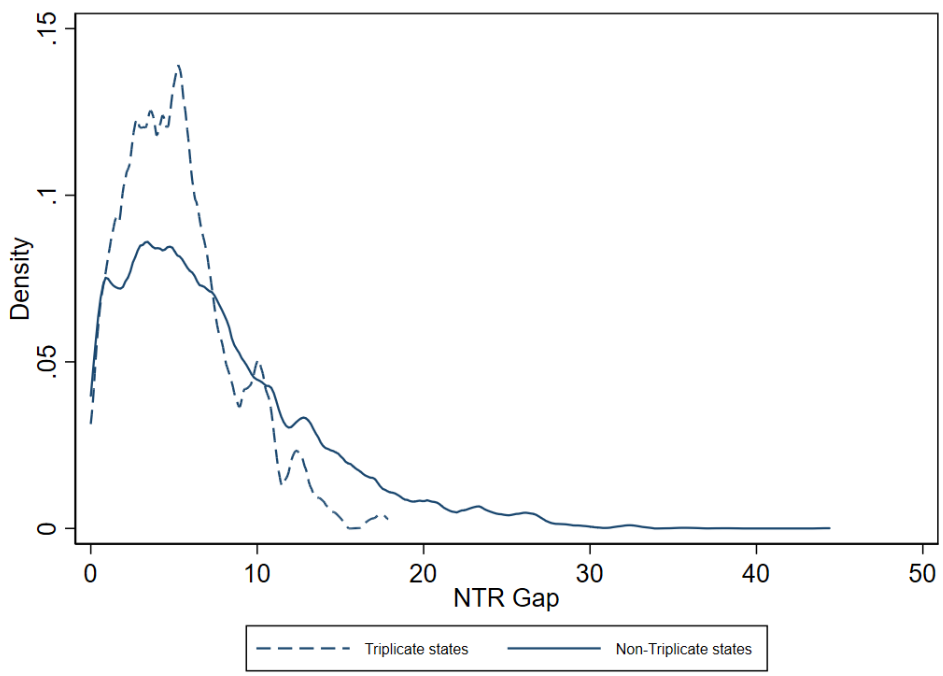
Notes: The figure plots the average Oxycodone distribution across triplicate and non-triplicate states from 1999 to 2008. State-wise Oxycodone data is obtained from https://www.deadiversion.usdoj.gov/arcos/retail˙drug˙summary/archive/index.html.

Figure 3: Average Oxycodone distribution in grams per 100,000 across Triplicate and Non-Triplicate States- - By intensity of Trade Shock



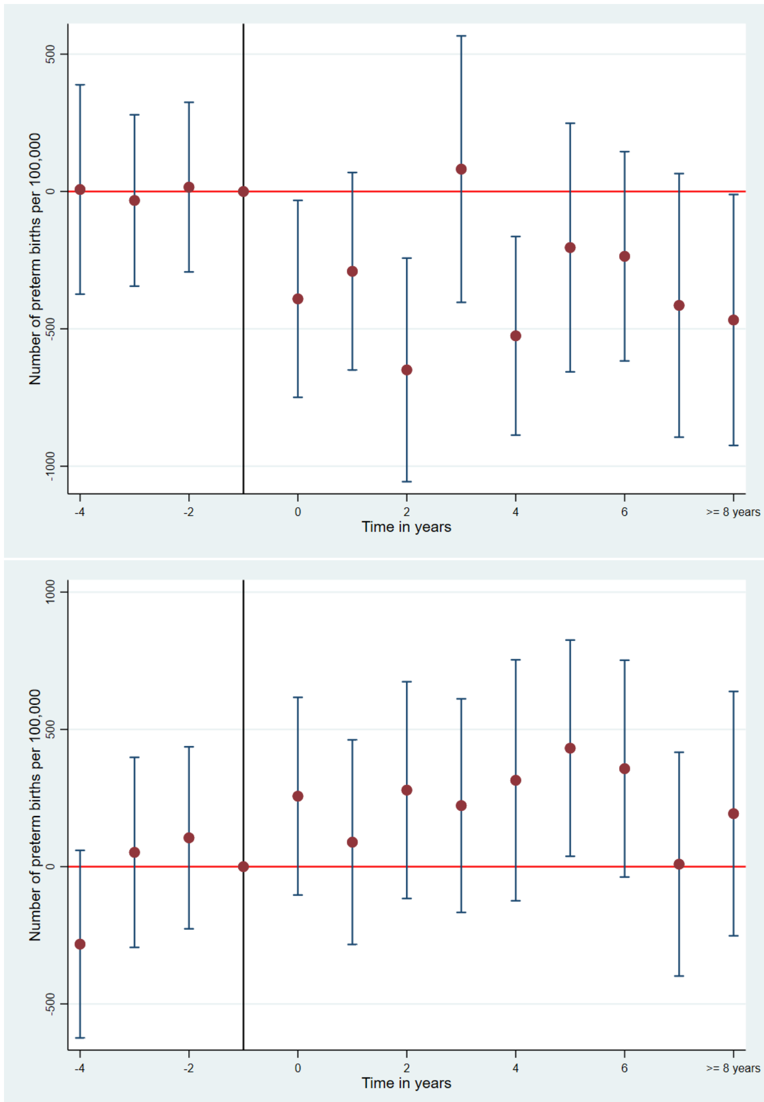
Notes: The figure plots the relationship between average Oxycodone sales (MMEquivalents) and NTR Gap across triplicate and non-triplicate states from 1999 to 2008 using a binned scatter plot. Zip-code level Oxycodone data is obtained from https://www.deadiversion.usdoj.gov/arcos/retail˙drug˙summary/archive/index.html.

Figure 4: Distribution of NTR Gap across Triplicate and Non-Triplicate states



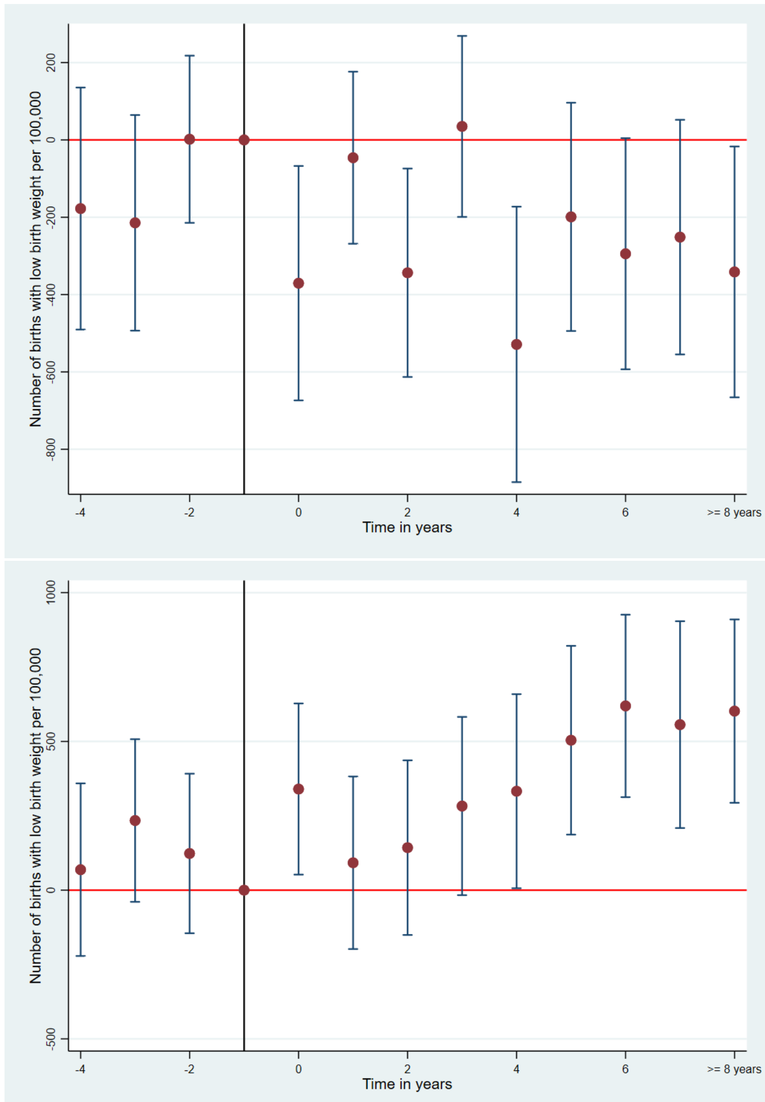
Notes: The figure shows the distribution of NTR Gap values across triplicate and non-triplicate states using a kernel density plot. Data for constructing the NTR Gap are obtained from Pierce and Schott (2016); Feenstra et al. (2002) and County Business Patterns, 2000.

Figure 5: Event study estimates - Rate of preterm births



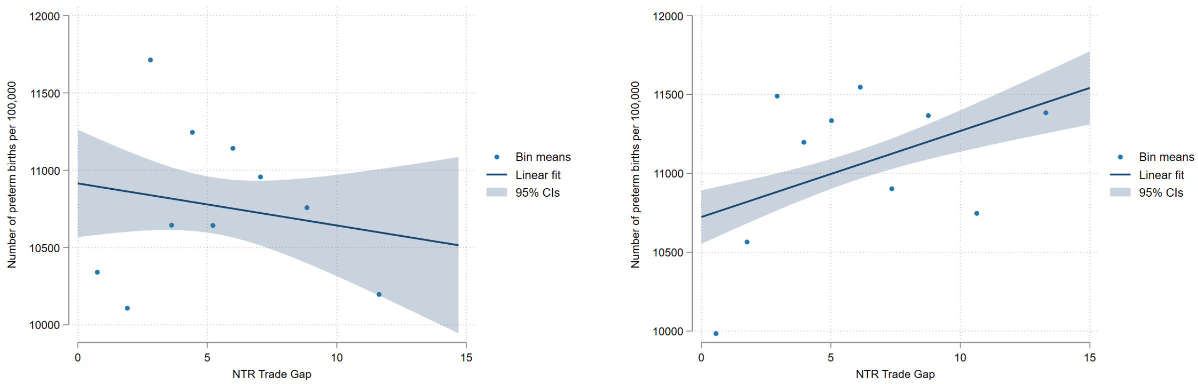
Notes: The figure plots the event study estimates comparing the rate of preterm births in high trade shock counties compared to low trade shock counties in triplicate (top) and non-triplicate states (bottom). The estimation uses county and year-fixed effects with state-year-level controls. Standard errors are clustered at the state level and a 95 percent confidence interval is estimated for each point estimate.

Figure 6: Event study estimates - Rate of births with low birth weight



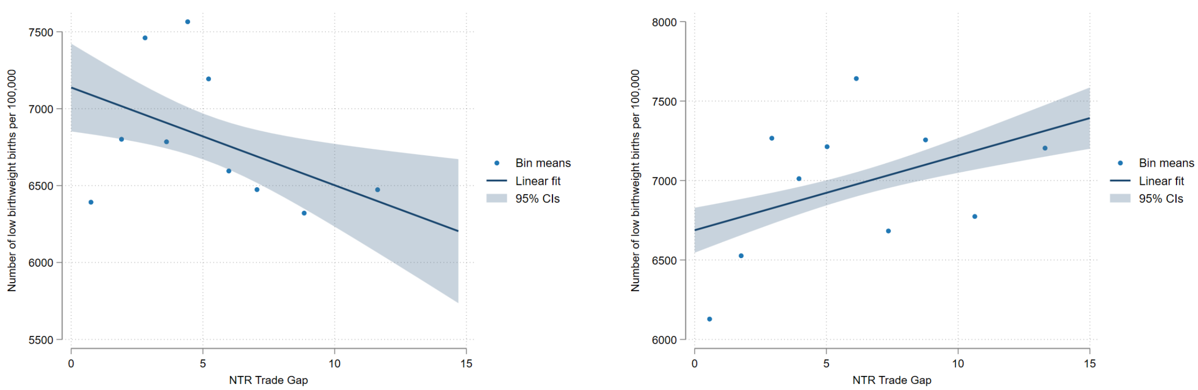
Notes: The figure plots the event study estimates comparing the rate of births with low birth weight in high trade shock counties compared to low trade shock counties in triplicate (top) and non-triplicate states (bottom). The estimation uses county and year-fixed effects with state-year-level controls. Standard errors are clustered at the state level and a 95 percent confidence interval is estimated for each point estimate.

Figure 7: Relationship between Trade Liberalization Shocks and Rate of Preterm Births



Notes: The figure shows binned scatter plots of the rate of preterm births against NTR Gap values for triplicate (left) and non-triplicate states (right). The estimation uses year-fixed effects and controls for the mother's race and age group.

Figure 8: Relationship between Trade Liberalization Shocks and Rate of Low Birthweight births



Notes: The figure shows binned scatter plots of the rate of low birthweight births against NTR Gap values for triplicate (left) and non-triplicate states (right). The estimation uses year-fixed effects and controls for the mother's race and age group.

Table 1: Summary Statistics

	Triplicate		Non-Triplicate	
	Mean	SD	Mean	SD
Percent of Preterm births (<37 weeks)	10.74	4.59	11.7	8.69
Percent of extremely Preterm births (<32 weeks)	1.69	2.06	2.1	5.5
Percent of births with Low birth weight (<2500 grams)	7.32	3.99	7.92	7.63
Percent of births with very low weight births (<1500 grams)	1.28	1.73	1.53	4.82
Tobacco use during pregnancy	4.2	7.67	11.64	10.92
At least 5 prenatal visits	95.28	5.97	94.67	9.91
County level NTR Trade Gap	5.272	3.303	7.573	5.83
Percent of High trade shock counties	49.8	5	61.7	48.5
Mean Unemployment rate	6.358	3.211	5.75	2.678
2000 census				
Percent with less than high school degree	20.47	3.65	18.34	5.42
Male labor force participation rate	70.81	2.16	71.04	4.75
Female labor force participation rate	56.99	1.93	58.55	5.39
Employed in agriculture, forestry, fishing and hunting, and mining	2.43	2.01	2.75	2.29
Employed in construction	6.64	1.34	7.009	1.07
Employed in manufacturing	12.81	2.17	13.25	5.39
Employed in wholesale trade	3.75	0.27	3.32	0.57
Employed in retail trade	11.44	0.84	11.9	1.08
Employed in transportation, warehousing, and utilities	5.33	0.63	5.15	0.91
Employed in information	3.26	0.73	2.76	0.87
Employed in Finance, Insurance, real estate, rental and leasing	7.11	1.38	6.46	1.38
Employed in Professional, scientific, management, administrative, and waste management services	9.87	1.32	6.46	2.42
Median income	37881.33	4269.73	36898.13	7518.3

Notes: The table presents mean and standard deviation for key variables across triplicate and non-triplicate states. Infant health variables are derived from the NVSS Natality Birth data. Unemployment rates are derived from BLS Local Area Unemployment Files. The bottom panel compares key variables from the 2000 census across triplicate and non-triplicate states.

Table 2: Interaction Effect Between Trade Liberalization Shocks and Non-triplicate Laws on Infant Health

	(1)	(2)	(3)	(4)
	Rate of preterm births (less than 37 weeks)	Rate of births with low BW (less than 2500g)	Rate of extreme preterm births (less than 32 weeks)	Rate of births with very low BW (less than 1500g)
Non-Triplicate state*High trade shock*Post 2000	442.5***	391.4***	219.5**	81.9
	(147.040)	(128.421)	(100.821)	(87.303)
High trade shock* Post 2000	-387.1***	-205.3*	-128.2	-69.6
	(121.196)	(105.850)	(83.101)	(71.958)
Non-Triplicate state*Post 2000	87.0	-94.0	79.7	31.4
	(109.085)	(95.273)	(74.797)	(64.768)
Mean y.	11402.32	7739	1460.55	1976.35
No. of Observations	187,639	187,639	187,639	187,639
R Squared	0.291	0.295	0.145	0.137

Notes: All specifications control for county and year-fixed effects with controls for state and year-wise demographic controls. Regressions are weighed using the female population count in each county, year, race, and age group. Errors are clustered at the state level. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels respectively.

Table 3: Interaction Effect Between Trade Liberalization Shocks and Non-triplicate Laws on Infant Health by Time Periods 2001-07 and 2008-18

	(1)	(2)	(3)	(4)
	Rate of preterm births (less than 37 weeks)	Rate of births with low BW (less than 2500g)	Rate of extreme preterm births (less than 32 weeks)	Rate of births with very low BW (less than 1500g)
Non-Triplicate state*High trade shock*2001-2007	485.3***	482.4***	314.2***	161.4*
	(153.144)	(133.751)	(105.004)	(90.925)
Non-Triplicate state*High trade shock*2008-2018	410.7***	323.8**	149.0	22.9
	(150.444)	(131.392)	(103.152)	(89.321)
High trade shock* Post 2000	-386.5***	-204.1*	-126.9	-68.6
	(121.198)	(105.850)	(83.100)	(71.958)
Non-Triplicate state*Post 2000	87.1	-93.8	79.9	31.5
	(109.085)	(95.271)	(74.795)	(64.766)
Mean y.	11402.32	7739	1976.35	1460.55
No. of Observations	187,639	187,639	187,639	187,639
R Squared	0.291	0.296	0.145	0.137

Notes: All specifications control for county and year-fixed effects with controls for state and year-wise demographic controls. Regressions are weighed using the female population count in each county, year, race, and age group. Errors are clustered at the state level. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels respectively.

Table 4: Interaction Effect Between Trade Liberalization Shocks and Non-triplicate Laws on Infant Health- By Race and Ethnicity of the Child

	(1)	(2)	(3)
	No of White preterm births per 100,000	No of Black preterm births per 100,000	No of Hispanic preterm births per 100,000
Non-Triplicate state*High trade shock*Post 2000	381.0**	57.3	-42.2
	(168.938)	(1133.752)	(761.317)
Mean y.	10363.22	13627	1857.68
No. of Observations	119,627	41,288	72,160
R Squared	0.217	0.152	0.086
	(1)	(2)	(3)
	No of White low BW births per 100,000	No of Black low BW births per 100,000	No of Hispanic low BW births per 100,000
Non-Triplicate state*High trade shock*Post 2000	296.0**	-464.3	371.2
	(144.658)	(957.140)	(578.646)
Mean y.	6730.67	13627	7019.03
No. of Observations	119,627	41,288	72,160
R Squared	0.202	0.166	0.098

Notes: All specifications control for county and year-fixed effects with controls for state and year-wise demographic controls. Regressions are weighed using the female population count in each county, year, and age group. Errors are clustered at the state level. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels respectively.

Table 5: Interaction Effect Between Trade Liberalization Shocks and Non-triplicate Laws on Maternal Risky Health Behaviors

	(1)	(2)
	Tobacco use per 100,000	Reported atleast 5 pre-natal visits per 100,000
Non-Triplicate state*High trade shock*Post 2000	516.5***	-276.1*
	(147.892)	(153.606)
High trade shock* Post 2000	-113.0	486.1***
	(121.899)	(126.608)
Non-Triplicate state*Post 2000	2933.5***	-637.9***
	(109.718)	(113.956)
Mean y.	9153.1	94979.55
No. of Observations	187,639	187,639
R Squared	0.616	0.407

Notes: All specifications control for county and year-fixed effects with controls for state and year-wise demographic controls. Regressions are weighed using the female population count in each county, year, race, and age group. Errors are clustered at the state level. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels respectively.

Table 6: Interaction Effect Between Trade Liberalization Shocks and Non-triplicate Laws on Infant Health- Using an alternate control group

	(1)	(2)	(3)	(4)
	% preterm (less than 37 weeks)	% low bw	% preterm (less than 32 weeks)	% very low bw
Non-Triplicate state*High trade shock*Post 2000	621.5***	459.5***	349.1***	206.4**
	(194.170)	(164.391)	(99.565)	(85.673)
High trade shock* Post 2000	-379.0***	-167.6**	-93.9**	-33.6
	(90.618)	(76.720)	(46.466)	(39.983)
Non-Triplicate state*Post 2000	348.0**	-57.9	-6.33	-13.0
	(147.419)	(124.810)	(75.592)	(65.045)
Mean y.	10818.81	7371.35	1725.39	1303.81
No. of Observations	47,306	47,306	47,306	47,306
R Squared	0.401	0.412	0.242	0.229

Notes: All specifications control for county and year-fixed effects with controls for state and year-wise demographic controls. Regressions are weighed using the female population count in each county, year, race, and age group. Errors are clustered at the state level. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels respectively.

Table 7: Interaction Effect Between Trade Liberalization Shocks and Non-triplicate Laws on Infant Health- Using the continuous Trade Liberalization Measure

	(1)	(2)	(3)	(4)
	% preterm (less than 37 weeks)	% low bw	% preterm (less than 32 weeks)	% very low bw
Non-Triplicate state*Continuous trade shock measure*Post 2000	55.2**	45.3*	32.9*	16.3
	(27.730)	(24.218)	(19.013)	(16.464)
Continuous trade shock measure* Post 2000	-41.6	-11.5	-13.5	-6.31
	(26.111)	(22.804)	(17.903)	(15.503)
Non-Triplicate state*Post 2000	22.2	-150.8	7.08	-19.6
	(169.025)	(147.617)	(115.892)	(100.354)
No. of Observations	187,639	187,639	187,639	187,639
R Squared	0.291	0.296	0.145	0.137

Notes: All specifications control for county and year-fixed effects with controls for state and year-wise demographic controls. Regressions are weighed using the female population count in each county, year, race, and age group. Errors are clustered at the state level. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels respectively.

Table 8: Interaction Effect Between Trade Liberalization Shocks and Non-triplicate Laws on Infant Health- Using the Common Support for NTR Gap

	(1)	(2)	(3)	(4)
	Preterm birth rate	Low bw birth rate	Extremely premature birth rate	Very low bw birth rate
Non-Triplicate state*High trade shock*Post 2000	415.0***	356.3***	189.1*	69.6
	(144.704)	(125.550)	(99.790)	(86.982)
High trade shock* Post 2000	-388.1***	-205.0**	-128.8	-70.5
	(118.799)	(103.073)	(81.925)	(71.411)
Non-Triplicate state*Post 2000	88.7	-91.9	80.1	30.7
	(106.930)	(92.776)	(73.740)	(64.276)
Mean y.	11396.04	7731.75	1971.1	1464.13
No. of Observations	177,942	177,942	177,942	177,942
R Squared	0.300	0.308	0.147	0.140

Notes: All specifications control for county and year-fixed effects with controls for state and year-wise demographic controls. Regressions are weighed using the female population count in each county, year, race, and age group. Errors are clustered at the state level. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels respectively.